Duhem on Maxwell: A Case-Study in the Interrelations of History of Science and Philosophy of Science

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Since the revival of historicist philosophy of science in the 1960s many philosophers have acknowledged a debt to Duhem. But Duhem's opinions are imperfectly understood and, as McMullin has shown in his (1970) and (1979), there are many strands in the current revival of historicism. We consider here Duhem's views on the role of history in the appraisal of scientific theories. However, there is no single text offering Duhem's views on the subject; rather, they are revealed during their application to various historical and contemporary cases. Duhem's most sustained examination of a contemporary case is his critique of Maxwell's science and scientific methodology.

Duhem's critique of Maxwell is not a set of isolated or incidental dicta, but the expression of Duhem's mature thought over the last quarter-century of his life, ranging from (at least) 1893 to his death in 1916. Other than some minor differences in doctrine arising from differences in exposition among his various works, this critique does not seem to have changed during that period. Among the various parts of Duhem's critique are his complaints that i) Maxwell's theory is too bold or not systematic enough, that ii) it is too dependent on models, and that iii) its concepts are not continuous with those of the past.[1] Interestingly, these aspects of Duhem's critique of Maxwell are interconnected; all three are based on Duhem's insistence on a historical criterion for the evaluation of physical theories.

The justification Duhem gives for the accusation of boldness — imprudence inouie or audace téméraire (1902, p. 8) — is that

> when a physicist discovers facts unknown until then, when his experiments have allowed him to formulate new laws that the theory had not foreseen, he must first try with the greatest care to represent these laws, to the required degree of approximation, as consequences of admitted hypotheses. Only after having acquired the certainty that the magnitudes treated until now by the theory cannot serve as symbols for the observed

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> quantities, that received hypotheses cannot flow from the established laws, is he authorized to enrich physics with a new magnitude, to complicate it with a new hypothesis (1902, p. 7).

According to Duhem, the founders of electrodynamics – Coulomb, Poisson, and Ampère – followed these principles, but Maxwell did not. Duhem cites with approbation Ampère's attempt to represent the attraction or repulsion of currents in wires using Coulomb's formula. He also praises the fact that Ampère abandoned the attempt only after experimental facts about the magneto-optical rotation, discovered by Faraday, showed clearly that Ampère could not succeed in the attempt. But, again, according to Duhem, Maxwell does not follow these principles. In fact, he goes so far as to accuse Maxwell of following the "inverse path":

> At the moment when Maxwell introduced a new magnitude in electrodynamics, the displacement current, at the moment when he marked, as essential hypotheses, the mathematical form of the laws to which this magnitude should be submitted, no properly observed phenomenon required this extension of the theory of currents; that theory was sufficient for representing, if not all phenomena known until then, at least all those whose experimental study had achieved a sufficient degree of clearness. No logical necessity pressed Maxwell to imagine a new electrodynamics (Duhem 1902, p. 8).

Hence Maxwell reversed, "with an incredible rashness [avec une imprudence inouie]", the natural order according to which physical theory evolves – he broke with the past before he needed to, that is, of course, given the assumption that the past demonstrates a natural order, an evolution.

Duhem even gives Maxwell a motive for his boldness, his reversal of natural order; that motive is Maxwell's alleged desire to endow Faraday's work with its own research tradition, an extension of Faraday's work similar to the one which the work of Coulomb and Poisson received through Ampère's electrodynamics (Duhem 1902, p. 8). According to Duhem, Maxwell's desire to extend Faraday's work provided Maxwell with some analogies and perhaps also an instinctive feeling about the electrical nature of light. We shall now consider Duhem's judgment concerning such analogies and instincts, where such analogies and instincts derive from the use of models.[2]

Duhem is well-known for his attack on the use of models in physical science. According to Duhem, model-building is an occupation pursued by scientists with "ample, but shallow" minds, a trait of English scientists (to be contrasted with the "deep, but narrow" minds of French scientists). At first, it seems that Duhem tries to link his attack on model-building with his espousal of an instrumentalist methodology for the physical sciences. Duhem accuses the English scientists of believing that "to understand physical phenomena is to compose a model" (1954a, pp. 71-72; 1893, p. 351) and of "confusing model with theory" (1954a, p. 71; 1893, p. 350). He specifically accuses Maxwell of these misunderstandings, indicating that Maxwell, in his 1855-56 essay proposed only "to illustrate" the theory of dielectrics, whereas in his 1861-62 essay he proposed "to represent or to explain" the electrical and magnetic actions by a mechanical model (1902, p. 9). Duhem even seems scornful of Maxwell's interchangeable use of the verbs "to represent" and "to explain". He states, "for an English physicist, the two words have the same meaning" (1902, p. 9) and he refers his reader to his discussion of English physics in his 1893 article.

For Duhem there is a crucial difference between representing and explaining. He divides theories into two large categories, explanatory and purely representative theories (1954a, p. 80 and elsewhere), and argues that physical theories should not be considered as explanatory, but as purely representative or classificatory. The argument is that, in order for physical theory to be explanatory, it would have to be subordinate to metaphysics and not autonomous (1954a, pp. 7-14). Since no metaphysical system is sufficient as a basis for physical theory, and since the acceptance of several competing metaphysical systems would destroy the apparent consensus achieved in physical theory, Duhem argues for autonomy as a virtue of physical theory — that is, Duhem argues that physical theory must be independent of any particular metaphysics, and so must be representative or classificatory, not explanatory. In consequence, he argues for instrumentalism or conventionalism, as opposed to realism, as the only adequate methodology, consistent with what he perceives is a great consensus in physical theory. The reference to the two words having the same meaning for Maxwell and English scientists is therefore a reference to what Duhem would consider a confusion about the aim of physical theory, one that arises, perhaps, in the identification of the model with the theory, in thinking that what is represented by the theory and/or model is real.

But although Duhem hints at this conclusion, he does not accept it ultimately. He broadens the critique of model building to include mathematical models along with mechanical models; for Duhem the use of vector algebra and quaternions is just as objectionable as the use of analogical models and for the same reasons.[3] Duhem decides that, for the English scientists, theory is not "an explanation of physical laws, but a model of these laws; it is constructed not for the satisfaction of reason, but for the pleasure of the imagination." (1893, p. 360). When this phrase is repeated in Duhem 1954a, it is carefully reworded to get Duhem's main point across: "Theory is for him [the English physicist] neither an explanation nor a rational classification of physical laws, but a model of these laws, a model not built for the satisfying of reason but for the pleasure of the imagination." (p. 81). The complaint is clear: model-building, what the English school inherited from Faraday, has no real place in physical theory (beyond a minor heuristic role); it can neither be grounded in realism, in the thought that physical theories are explanatory structures, nor in instrumentalism, in the thought that physical theories are classificatory or representative structures. In fact, model-building is not even connected to the higher intellectual faculty of reason but to the lower intellectual faculty of imagination.[4] So, ultimately, Duhem's attack on model-building must be rooted in something more fundamental than his instrumentalist methodology for the physical sciences.

Duhem banishes model-building from physical theory (as he previously banished boldness) because model-building breaks with historical continuity; in fact, model-building is not only historically non-continuous, but present models are even often "non-continuous" among themselves.[5] Some model-builders even find pleasure in building two or more models of the same law.[6] The fact that the English physicist can accept disparate models, breaking up the historical continuity of science and even its present unification, is what shocks Duhem; it is what reconfirms for him that English physics is not the work of reason, but the work of imagination (Duhem 1893, p. 361 and 1954b, p. 81).

Therefore, underlying Duhem's criticisms of Maxwell is a principle of historical continuity. In fact, such a principle is invoked in the conclusion of Duhem's primary work on Maxwell. There Duhem evaluates an interpretation of Maxwell's work he attributes to Heaviside, Hertz, and Cohn, among others. Duhem quotes Hertz as stating that "what is essential in Maxwell's theories is Maxwell's equations."[7] He takes this to be Hertz's way of salvaging what is valuable in Maxwell from the midst of illogisms and incoherences, which are not only difficult to correct, but which have frustrated many illustrious mathematicians (Duhem 1902, pp. 221-222; cf. also Duhem 1919, pp. 119-120). But Duhem cannot accept Hertz's implied criterion of identity for physical theories. He asserts, in a far-sighted passage, that he might accept such a criterion for algebra but that "a physicist is not an algebraist":

> An equation does not simply bear on letters; such letters symbolize physical magnitudes which must be either measurable experimentally or formed from other measurable magnitudes. Therefore, if a physicist is given only an equation, he is not taught anything. To this equation must be joined rules by which the letters that the equation bears upon are made to correspond to the physical magnitudes they represent. And that which allows us to know these rules is the set of hypotheses and reasonings by which one has arrived at the equations in question. [The set of rules] is the <u>theory</u> that the equations summarize in a symbolic form: <u>in physics, an equation, detached from the theory that leads to it, has no meaning</u> (1902, p. 223).

Duhem proceeds to detail two complete theories that can recover Maxwell's equations within a logical and coherent structure, the theories of Boltzmann and Helmholtz. The only criterion he gives for choosing between these two theories, though without elaborating upon it or justifying it, is that Helmholtz's theory is to be preferred over Boltzmann's theory because it is a natural "extension of the doctrines of Poisson, Ampère, Weber, and Neumann; it leads logically from the principles posited at the start of the 19th century to the most seductive consequences of Maxwell's theories, from the laws of Coulomb to the electromagnetic theory of light; [it does so] without losing any of the recent victories of electrical science; it reestablishes the continuity of tradition."[8]

What is Duhem's justification for a criterion of historical continuity? Duhem does not attempt to answer this question in his Maxwell book, perhaps because he previously broached the question in a digression of his 1893 article.[9] There Duhem acknowledges that "IF REASONS OF PURE LOGIC ALONE ARE INVOKED, <u>then a physicist cannot be prevented from representing various sets of laws, or even a unique set of laws, by several irreconcilable theories; the incoherence of the development of physical theory cannot be condemned [by pure logic alone]</u>" (1893, p. 366; the emphasis is original; cf. also, 1954b, pp.

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294-295). The reasons one invokes are outside pure logic and outside reasons of physics proper, but deal with the relation between physics and metaphysics, what Duhem calls cosmology in this context.[10] Surprisingly, if one considers Duhem as strictly an instrumentalist, for Duhem, we must judge a physical theory in comparison to an ideal and perfect theory which provides the total and adequate metaphysical explanation of the nature of material things, that is, the natural classification of laws.[11] For that reason, a coherent physical theory is more perfect than an incoherent set of incompatible theories.

A further clue for understanding Duhem's criterion of historical continuity is provided by Duhem when, in his 1905 article he elaborates upon the digression from his 1893 article. Commenting upon the connection between cosmology, that is, metaphysics, natural classification, or the ideal form of physical theory, and physical theory itself, Duhem asserts,

> It is not enough for the cosmologist to know very accurately the present doctrines of theoretic physics; he must also be acquainted with past doctrines. In fact, it is not with the present theory that cosmology should be analogous, but with the ideal theory toward which the present theory tends by a continual progress. It is not the philosopher's task, then, to compare present-day physics to his cosmology by congealing science at a precise moment of its evolution, but rather to judge the tendency of theory and to surmise the goal toward which it is directed. Now, nothing can guide him safely in conjecturing the path that physics will take if not the knowledge of the road it has already covered (1954b, p. 303).

Duhem supports this statement using an analogy with the trajectory of a ball. We cannot guess its end point with an instant glance at the ball, but we can prolong its trajectory if we followed the ball from the moment it was struck. So we cannot guess the end point of physical theory, the natural classification, by looking at any particular theory. We must appeal to the trajectory of physical theory, to its history, to enable us to tell whether any particular theory is likely to contribute toward the ultimate natural classification.[12] That is why Duhem insists on a historical criterion of continuity for the evaluation of physical theories.

Duhem's insistence on history as a criterion for the acceptance of physical theories sets his work apart from most Anglo-American twentieth-century philosophers of science. Some (i.e., Hempel and positivists) believe that historical factors do not enter into the appraisal of scientific theories which are discussed in purely logical terms in the context of justification. Others (i.e., Kuhn and historicists) while emphasizing the importance of historical studies as a basis for the philosophy of science, do not accept an historical criterion for the evaluation of scientific theories of the sort employed by Duhem. The work of Kuhn, and of later historicist philosophers of science like Lakatos, has interesting similarities and differences to Duhem's view. In one sense, the importance of the historical context in which scientific theory emerges is recognized by these writers. In another sense, they deviate from Duhem's position by relativizing the application of this criterion to groups of theories forming coherent traditions.[13] These new entities are of exactly the same sort as the coherent tradition which Duhem sees linking Coulomb, Poisson, and Ampère. But a difference is immediately apparent. Modern historicist philosophers of science acknowledge the possibility of the existence of more than one such tradition. Given that Maxwell's sequence of theories in electricity and magnetism is a particularly clear example of the sequence of theories replacing one another in the manner required by modern historicists, why was Duhem unable to accord to Maxwell's research tradition the same status he evidently recognized in the work of French electricians? The points made so far suggest an answer in three parts. First, Duhem, for philosophical reasons, failed to recognize Maxwell's theories as part of a legitimate research tradition. Second, Maxwell's theories were not effectively connected to the only <u>other</u> legitimate tradition in electricity and magnetism (defined by Ampère, Coulomb, and Poisson). And third, an acceptable alternative to Maxwell's theories existed which <u>was</u> effectively connected to the previous tradition.

It is important to distinguish two <u>sorts</u> of models in the work of English physicists of Duhem's time. An example of the first sort of model is William Thomson's gyrostatic model of McCullagh's aether. This model is intended to demonstrate only that the physical properties of McCullagh's aether are consistent with Newtonian mechanics. The actual constituents of the model (the springs, axles, and gyrostats derided by Duhem) are not intended as candidates for reality.[14] The second sort of model, used extensively by Maxwell in describing electricity and magnetism, consists of hypothetical motions in a perfect fluid – usually some form of vortex. Although these models are also treated using Newtonian mechanics, it is clear that Maxwell himself, and his followers, regarded them as potential candidates for reality. Successive theories developed by Maxwell and his followers (like J. J. Thomson) were attempts to articulate a model of a perfect fluid aether which would account for all known electromagnetic phenomena.

Maxwell died before he could complete his own research program in a satisfactory manner. His last major work, the <u>Treatise on Electricity and Magnetism</u>, is clearly not a finished product.[15] Because of Maxwell's premature death, the connecting thread between his various theories, and the models they contained, was by no means obvious to readers outside the group of his closest students.[16] In the light of recent research on Maxwell,[17] it is now again possible to recognize his works as an unfinished attempt to explain electro-magnetism from real structures in a fluid aether. It is this goal that unites Maxwell's work into a coherent tradition. But the goal is not well stated by Maxwell himself. Moreover Duhem would not have recognized that a tradition could be unified by such a goal on the grounds that this would compromise the autonomy of physics from metaphysics.

Given that Duhem was unable to connect Maxwell's work into a coherent, self-contained tradition, he naturally attempted to connect it with the major existing tradition in electrical theory. But Maxwell's goal of recovering electromagnetic phenomena from the motions of a perfect fluid is incompatible with the tradition of Coulomb, Poisson, and Ampère, as Duhem correctly concluded.[18] This was not to deny that Maxwell's theories had certain attractive features in their own right, and correctly predicted the existence of hitherto unknown phenomena.

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Duhem's rejection of Maxwell's theories was completed by the identification of an alternative, the theory of Helmholtz, which could be consistently connected with the tradition of Ampère et al. Given suitable adjustments of parameters, Helmholtz's theory yielded identical results to Maxwell's later theories in all empirically testable situations.[19]

To summarize, then, Duhem rejects the possibility of alternative ontologies in physics on the grounds that only metaphysics may pronounce on the nature of reality. Physical theories must therefore form a single continuous tradition with no abrupt discontinuities in a conceptual or ontological content. This prevents him from being able to recognize Maxwell's work as forming a coherent whole, the goal of which is to produce an alternative ontology for electricity and magnetism. Duhem is clearly correct that Maxwell's theories cannot be consistently connected with the theories and ontology of Coulomb, Poisson, and Ampère. If we are to find fault in his critique it must be on the grounds that we reject his division of physics from metaphysics, and admit as reasonable practice in scientific research the construction of alternative ontologies.

Duhem's justification for his separation of physics and metaphysics – his desire for physics to be autonomous from metaphysics – depends upon his characterization of science as achieving consensus (in contrast with metaphysics, in which consensus is not achieved). This assumption might also be called into question. Duhem's own science was not accepted in his day (or today) and his writing on scientific methodology was and is still controversial. It is clear that many aspects of Duhem's critique of Maxwell were rejected, even by the physicists of his day. For example, when Duhem sent his study of Maxwell to Pierre Curie, Curie responded (in a letter to Duhem), using Duhem's exact words to punctuate his disagreement, that "it would be good if our physicists demonstrated Maxwell's <u>incredible rashness</u> [<u>imprudence inouie</u>]."[20] The heresy of which Duhem accused Maxwell, incredible rashness, the breaking of historical continuity, was accepted by Curie as a virtue for physicists.

Notes

[1] Duhem also objects that Maxwell makes mistakes in constants (1902, pp. 98-101, 145-147) and that his theory does not account for obvious entities such as magnets (1902, p. 145). We will not be discussing these complaints about constants and about the theory's inability to account for magnets; they turn out not to be terribly interesting in the final analysis. There seems to have been little data to suggest the need for an additional constant to Maxwell's theory, one corresponding to a constant generated by Helmholtz's theory, which Duhem defended. The same can be said about Duhem's complaint that Maxwell's theory cannot account for entities such as permanent magnets. For more information about the assessment of both these complaints, see L. Roy (1923a). More interesting is Duhem's complaint that Maxwell makes too many mistakes in mathematics and logic (see, for example, Duhem 1902, pp. 62n, 74, 81-82, 85, 87, 98-101, 106, and 221). Although these are not complaints about a finished product, about mistakes that Maxwell could not avoid, Duhem does attempt to generalize and to connect this

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criticism with his other criticisms. For example, Duhem implies that it is his boldness that pushes Maxwell forward, disregarding any difficulties (Duhem 1902, pp. 11-12). But Duhem also rejects Henri Poincaré's more serious accusation that the mistakes in Maxwell's demonstrations are intentional, or minor attempts at cheating - "des coups de pouce" (Duhem 1902, p. 12). However, Duhem repeats the accusation that Maxwell's mathematical mistakes are too numerous when he quotes Poincaré's diatribe against Maxwell's lack of orderliness and his willingness to put forth provisional structures, what Duhem interprets as Maxwell's overdependence on models. (Duhem often quotes Poincaré's attack on Maxwell, an attack he is clearly pleased to cite and to endorse in part - Duhem 1954a, p. 85, from H. Poincaré, 1892.) By discussing the issue of mathematical errors in this context, Duhem implies that it is because of his penchant for models that Maxwell is unconcerned with logic or mathematical exactitude (Duhem 1954a, p. 86). In general, though, Duhem seems willing to recognize that Maxwell's mistakes are detachable from Maxwell's theory (1902, pp. 74 and 85). In any case, there is enough evidence that Duhem ultimately accepts Maxwell's equations, even though he finds faults in their historical derivations (cf. 1902, pp. 221 et seq).

[2] In both (1893) and (1954a) Duhem claims that what characterizes the English school - the followers of Faraday - is their use of models: "the English physicist materializes these [abstract] lines [of force having no thickness or real existence] and thickens them to the dimensions of a tube which he will fill with vulcanized rubber. In place of a family of lines of ideal forces conceivable only by reason, he will have a bundle of elastic strings, visible and tangible . . . Such is the famous model of electrostatic action imagined by Faraday and admired as a work of genius by Maxwell and the whole English school" (1954a, p. 70; cf. Duhem 1893, p. 349).

[3] "Thus it is only among English men of science that symbolic algebras, the calculus of quaternions, and 'vector analysis' are customary, most of the English treatises making use of these complex and shorthand languages. French and German mathematicians do not learn these languages readily. . . . Whereas the French and German physicist intends the algebraic part of a theory to replace just the series of syllogisms used to develop this theory, the English physicist regards the algebra as playing the part of a <u>model</u>." (1954a, p. 77-79).

[4] The distinction between the higher intellectual faculty of reason and the lower faculty of imagination is a standard philosophical distinction from Descartes and Pascal on; it does not carry the same implications for empiricists, for whom images might be equated with ideas. Clearly Duhem intends to evoke the French tradition; that is how he is to be understood in the following passage (and other similar passages): "thus, in English theories we find those disparities, those incoherencies, those contradictions that we are driven to judge severely because we seek a rational system where the author has sought to give only a work of the imagination" (Duhem 1893, p. 361 and 1954a, p. 81; see also Duhem 1902, p. 358).

[5] "The various theories of the Scottish physicist [Maxwell] are irreconcilable with traditional doctrine; they are irreconcilable among themselves." (1902, p. 11).

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[6] "To a physicist of the school of Thompson or Maxwell, there is no contradiction in the fact that the same law can be represented by two different models" (1893, pp. 360-361; 1954a, p. 81).

[7] (1902, p. 222, quoting H. Hertz, p. 21). Duhem also quotes p. 23 of the same work: "To the question, 'what is Maxwell's theory?,' I do not know any reply which is more brief and more precise than the following: 'Maxwell's theory is Maxwell's system of equations.'"

[8] Duhem (1902, p. 225). On the previous page, Duhem asserts that "if, in order to proceed logically to Maxwell's equations, we follow the methods proposed by Boltzmann, we would be required to abandon in part the work of Poisson and his successors on the distribution of electricity and magnetism." (1902, p. 224). It is interesting to find, as late as 1923, another physicist defending the doctrine of Helmholtz-Duhem as the superior doctrine, on the grounds that it explains all of Maxwell's results without breaking the tradition: "We have recently shown [in Louis Roy (1923a)] how Helmholtz's electrodynamics, which was completed by Duhem, explains in an extremely sure manner, without breaking the tradition, all the essential results obtained by Maxwell for mediums at rest. It was therefore natural, in spite of the actual vogue of electrical theories, to attempt to extend this electrodynamics to mediums in motion and to compare its results with those of Hertz and Lorentz." (Roy, 1923b, p. 199).

[9] Pp. 364-370; interestingly, Duhem did not reproduce these pages when he wrote Duhem 1954a though he came back to this topic when he attempted to defend the Duhem 1954a in Duhem 1905, his response to Abel Rey's "La philosophie scientifique de M. Duhem."

[10] This theory, by definition, would not permit the negative consequences of metaphysics in physics described when Duhem defends the autonomy of physics.

[11] Duhem (1893, p. 368). McMullin might see this as evidence for Duhem's realism, just as he would see Duhem's espousal of T1 (that scientific theories are historical entities) as evidence of realism: "An instrumentalist is unlikely to admit that the sequence of the historical development of a theory has any other than an accidental significance, since the only legitimate criteria for him are immediate predictive power and simplicity (construed as pragmatic convenience)." (McMullin 1979, p. 61). Duhem might still be viewed as an instrumentalist (though this may just be a question of semantics), since he claims that we have no reasons of pure logic for the belief that the natural classification actually corresponds to real relations among things; Duhem, quoting Pascal, asserts that this is "one of those reasons of the heart that 'reason does not know'." (1954a, p. 27, and elsewhere).

[12] In his 1954b article, Duhem also sketches what he perceives as an evolution in physical theory tending toward a natural classification. Rejecting the possibility of any evolution that includes the mechanistic revolution of the 17th century, he proposes an evolution from Aristotelian cosmology to the principles of thermodynamics (pp. 305-311). The most illuminating passage in which Duhem talks about the evolution

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of physical theory is the extremely theological passage from the conclusion of the second volume of his 1905-06 book: "However, while all these efforts contributed to the advancement of a science that we contemplate today as fully completed, none who exerted these efforts suspected the greatness of the form of the monument they were building. . . . . Masons clever at cutting and laying stones, they worked on a monument whose plan was not revealed to them by the architect. How could all these efforts have contributed so precisely to the materialization of a plan unknown to the laborers, if a clearly perceived plan did not exist before in the imagination of an architect and if this architect did not have the power of directing and coordinating the labor of the masons? The development of statics shows us, as much and even more than the development of a living being, the influence of a guiding idea. In the complex facts making up this development we detect the continuous action of a Wisdom that foresees the ideal form the science must aim for and we detect a Power directing all the efforts of the thinkers toward this goal. To sum up, we recognize there the work of Providence" (pp. 194-195).

[13] These coherent bodies of scientific work are called "paradigms" in Kuhn (1970), "research programmes" in Lakatos (1978), and (in a technical sense) "research traditions" in Laudan (1977).

[14] Duhem does make a place for some models in his physical theory (1954a, pp. 93-104) as heuristic devices. One might therefore wonder why he would then object to models as conceived in this first way.

[15] At times Duhem seems to recognize that his critique of Maxwell may be unfair, and that Maxwell is attempting to fashion a single coherent theory. Duhem even quotes Maxwell as saying in print "I have not been able to make the next step" (Duhem 1954a, p. 86), and Duhem himself attributes to Maxwell the intention to formulate a coherent whole: "What is intended and desired by Maxwell is a coordinated theory" (Duhem 1954a, pp. 102-103).

[16] As an indication, see the elaborate apology and explanation offered by J. J. Thomson in his (1885).

[17] For a survey, see Wise (1982); the most important material is now presented in Buchwald (1985).

[18] This incompatibility arises most clearly in the ontology of electricity. For the French school, electricity is a fluid, that is, a substance. For Maxwell's school, electricity is an epiphenomenon of the motions of the aether. It is not a substance, but a consequence of the motions of a substance.

[19] See O'Rahilly (1938), chap. 5.

[20] Curie even underlines the words in his letter. P. Curie, Letter to Duhem, 11 January 1902; published in P. Brouzeng (1978).

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