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Essay Review: A Physicists' Philosopher—James Clerk Maxwell on Mathematical Physics

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1. The year 1986 saw two important new additions to the growing (though still surprisingly sparse) literature on James Clerk Maxwell. The two books are very different. John Hendry's *James Maxwell and the Theory of the Electromagnetic Field*[(1)] endeavors to tell the story of the formation of Maxwell's views on the theory of the electromagnetic field, and to set it against a rich and well researched account of his life, his philosophy, and the major scientific and philosophical trends of its day. Elizabeth Garber, Stephen G. Brush, and C. W. F. Everitt's *Maxwell on Molecules and Gases*,[(2)] on the other hand, together with its forthcoming sequel, is a careful and comprehensive compilation of Maxwell's writings, both public and private, related to his kinetic theory of gases. The 55 texts comprising the volume are organized chronologically under two major headings—those related to atomic and statistical physics, and those related to the kinetic theory of gases—which allows the reader to follow Maxwell's development in each area. The editors set the stage for Maxwell's texts with a lengthy and detailed introductory chapter entitled, Kinetic Theory and the Properties of Gases: Maxwell's Work in its Nineteenth-Century Context. Both books are extremely valuable contributions to our knowledge and understanding of one of the greatest physicists of all time. And both break new ground: Hendry in his novel contextualization of Maxwell's *magnum opus*, i.e., in the new story he has to tell, and Garber, Brush, and Everitt in the new material they assemble and in the new story they thereby invite to be told. However, both books, in different ways, fail to highlight sufficiently that aspect of Maxwell's work which, to my mind, is the most relevant to today's reader. Let me elaborate a little.

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2. As a matter of course, the working physicist encounters two related, yet quite different sets of problems: those to do with understanding the world without, and those to do with *going about* such an understanding. Physics, of course, is primarily concerned with the former, with furthering our understanding of the physical. But a firm stand with respect the latter is mandatory for any advance to be made with respect the former. Indeed, the very notion of advance turns on what we take a good piece of physics to consist of. And what should be regarded a good piece of physics is as much a question in dispute as the problems of physics themselves. The history of physics is at once also the history of the second-order, more philosophical debate regarding the very nature of physics. Much philosophy of science has been written with little if any sensitivity to what scientists are actually up to. But there is no such thing as nonphilosophical physics. Each piece of research—theoretical or experimental—in the very act of declaring itself a step forward, implicitly takes a philosophical stand on the question of the nature of good physics. Subsequently, there are works of physics that are paradigmatic exemplifications of definite philosophies of science (e.g., those of Laplace), others that break new philosophical ground (e.g., those of Faraday), and others that in their philosophical ambiguity pose a challenge to current philosophical trends (e.g., Newton's *Principia*).

But more times than none the philosophical bias of a work of physics is not made explicit in the text. More so it is frequently only tacitly assented to by the physicist himself. And it is left to the historian and philosopher of science to render it explicit. Nevertheless, the reflexive deliberation of the aims and methods of physical enquiry is not a merely rear-guard descriptive enterprise. It turns on more than a philosophical examination of what physicists have already produced. In crucial instances in the history of physics novel reassessments of the nature of scientific investigation gave rise to revolutionary advances in physics rather than vice versa. The study of nature and the study of the *study* of nature share a history of intriguing dialectic, at times going their separate ways and at times working closely together in fruitful dialogue. But again, despite the inevitable "philosophy ladenness," to paraphrase Popper, of all physical research, the philosophical reflection on physics is, as a matter of course, left to the philosopher. In a sense such a division of labor is understandable. The working physicist concentrating on specific problems usually lacks the wider perspective necessary for viewing his or her work in more general terms. It is like trying to play the game, coach the team, and provide a running commentary on it all at once. On the other hand, the philosopher as a rule lacks first-hand knowledge of day-by-day physics. He or she usually *knows* physics, perhaps at times more than the working physicist concentrating on a specific area. But he or she lacks the feel of new physics *in the make*; the problems, the false starts, the dead ends, the hunches, the intuitions, the wild shots in the dark, and the pigheadedness, that combine to breed new physical knowledge. Rarely does one come across an individual whose work combines both the insight of the working physicist and the scope, breadth, and reflexivity of the philosopher. Not only was James Clerk Maxwell such an individual but his contributions to both physics and the philosophy of physics were of the very first order.

Maxwell's place in the history of physics is beyond dispute. And the readers of this journal need neither the books under review nor a review of them to substantiate such a claim. As opposed to his work *in* physics, however, Maxwell's writings *on* physics are less known, and I shall return to them shortly. But before doing so, I would like to state my main claim. Despite the uncontested quality of Maxwell's trail-blazing physics, and despite the fact if not for Maxwell's work much of today's physics would most probably not be what it is, *qua* physicist Maxwell has been wholly superseded. The physical problems he grappled with are, largely due to him, problems no more. And though incorporated into today's physics, his ideas no longer participate, as they once did, in the creative forefront of physical enquiry. On the other hand, and this is the crucial point, the problems he was grappling with concerning the nature, aims, and method of physics are still very much with us today. *Qua* philosopher of science, Maxwell is as relevant now as he ever was—not merely from the perspective of the historian. And it is in relation to his topicality as a keen and observant philosopher of (and at the same time first-rate contributor to) mathematical physics that I wish to assess the two books under review.

3. First to Hendry. Hendry's book is not only the best analysis of Maxwell to date, but it offers one of the finest overviews available of early 19th century physics. In a word, Hendry portrays Maxwell's scientific outlook as taking form against the backdrop of two major conflicting approaches to science current at the time—approaches he dubs (following Immanuel Kant): "mechanistic" and "dynamistic" (p. 5 and *passim*). Hendry's basic differentiation is widely acknowledged by both historians of physics and of the philosophy of science, and is described by some as the basic difference between Platonic and Aristotelian science (see, e.g., Ref. 3). The two approaches manifest themselves paradigmatically in the mechanics of Laplace and Lagrange and of their respective followers. The former school, represented, according to Hendry, apart from Laplace, by Biot, Navier, Cauchy, and Poisson, strove to ground a comprehensive physical account of reality upon a hypothesized and mechanistic ontology of particles acting at a distance, whereas the latter school, represented, apart from Lagrange, by Fresnel, Fourier, Ohm, and Ampere, proceeded analytically, striving to "save" the observed phenomena by means of a set of general and dynamical equations devoid of "any visual, geometrical or mechanical representations of constructions" (pp. 33ff). Philosophically speaking, the two approaches in physics pertain in the extreme to two fundamentally different philosophies of science, the former viewing scientific enquiry as striving to achieve an explanatory and causal account of the phenomena in terms of a deeper and more fundamental ontology, and the latter, denying such a task for physics, viewing the aims of physical theory as *representational* rather than explanatory, as striving "to establish among diverse experimental laws *a logical coordination* serving as a sort of image and reflection of the true order" according to which reality is organized.(4)

Hendry's analysis of the dynamistic and mechanistic trends culminating in 19th century physics is masterly. His account, on the other hand, of the *philosophies* of science at play at the time is, in my opinion, debatable at least in part. (The Scottish Common Sense philosophers he mentions, for example, are, I think, far less "dynamistic" than he makes them out to be, and both Whewell and William Rowan Hamilton, portrayed by Hendry as dynamistic hardliners, advocated philosophies of science that consciously endeavored, each in its own way, to somehow bridge the dynamistic–mechanistic divide.) But that is beside the point. Where I find Hendry's otherwise excellent study lacking is not in the manner it portrays the philosophical background to Maxwell's work, but in its failure to properly *relate* it to the formation of his views.[2] Put bluntly, the two extremely well thought-out studies that comprise Hendry's book—that of the history of 18th and 19th century theories of science and that of the day-by-day formation of Maxwell's scientific outlook—to an extent remain studies apart. What Hendry does achieve is a *description* of the formation of Maxwell's views *in terms* of the scientific and philosophical trends of his day. But he seems to be aiming at more, namely at an *explanation* of what Maxwell was up to, the historiographic question being: what should count as an explanation of the emergence of a system of thought? Hendry himself (p. 51) cautiously warns his readers against taking too simplistic a view of the matter:

> ...whether we look at the teachers whom Maxwell most respected—William Hamilton, Forbes, Whewell and Stokes—or the scientists upon whose own electromagnetic researches he built—Oersted, Ampere, Faraday and Thompson —there is no escaping the dominance of a dynamistic attitude. All eight philosophers and scientists were indeed clearly associated with aspects of the

[2] Domb(5) registers a similar complaint in his review of Hendry.

> dynamical tradition and the two British philosophers, Hamilton and Whewell, were two of the most prominent spokesmen for that tradition. We should not jump from this observation to any conclusions concerning Maxwell himself. He could have acted against, or independently of this background. The concept of influence as applied from individual to individual is historically very suspect.

But if not by personal influence, how is the formation of Maxwell's point of view to be accounted for? Hendry evidently has no alternative historiography to offer, for he continues:

> But given that Maxwell was sympathetic to aspects of the dynamical attitude, and we shall see that he was, its prominence in the background to his work suggests strongly that we should take is as the starting point for our discussion...

In other words, claims Hendry, the two traditions are to serve us in *retrospect* as a frame of reference in our dealings with Maxwell. (For similar remarks see also pp. 115–114.) But from the historian's perspective, a thinker's intellectual context counts for more than that. People operate not merely *in terms of*, but *as a function of* their context. And a truly *prospective* study of the emergence of a system of thought should capture just that aspect of its formation. What we are seeking, however, and Hendry is certainly right here, is not a *causal* contextualization of Maxwell's thought. Works of science and philosophy, especially as innovative as those of Maxwell, are not *determined* by their context. What are determined by context, and this is my main point, are the *problems* they endeavor to solve. To account historically for Maxwell's writings in physics and the philosophy of science, I submit, is to view them in relation to the scientific and philosophical problems he elected to take issue with. And these were a direct consequence of his intellectual context. Thus, to fully appreciate Maxwell's philosophical deliberations, one must first ask what, in view of the philosophical trends of his day, were the philosophical *difficulties* he encountered. This, at least with respect to the philosophy of science, is the missing link in Hendry's story.

4. At one point in the book (p. 40), Hendry acknowledges the fact that whereas on the continent the situation in mathematical physics was effectively mappable in terms of the two traditions, "in Britain, however, things developed rather differently." A variety of factors, ranging, according to Hendry, from a patriotic devotion to Newtonian fluxional analysis to the Napoleonic wars, combined to produce in Britain "rather different divisions from those we have discussed here." He goes on, however, to review the peculiar situation that developed as result in British physics, but fails to even ask whether British *philosophy of science* might also have been affected. I suggest that it was, and that an understanding of the way in which it was affected is the key to understanding the problems to which Maxwell's philosophical writings were addressed.

An important turning point in the history of 19th century British science was the formation in 1811 of the Analytical Society in Cambridge. The founders of the Society, notably John Herschel, Charles Babbage, and George Peacock, all still undergraduates in their early 20s, formed it with a view to introduce into the Cambridge curriculum the powerful multivariate and variational algebraic methods of continental analysis, in place of the geometrical methods of Newton's fluxions still taught at the university. In particular, they called for the adoption of Lagrange's algebraic and wholly formalistic version of the calculus. Lagrange based his version of the calculus on a definition of the "derived functions" of a given function as the coefficients of its Taylor power-series expansion. It was a move designed primarily to sidestep the problems of foundations which had plagued the development of the calculus since the publication of George Berkeley's devastating *The Analyst* in 1734. And it was a deliberately *instrumentalistic* move at that, one that sought to purge the calculus of the as-of-yet ill-defined notions of limit and continuity. However, with respect to physics England remained, despite the Lagrangean turn it was undergoing in mathematics, by and large committed to a Baconian philosophy of science. Namely, to one that viewed the study of nature as an empirical, realist, and inductive enterprise. A position to which bore witness two of the most influential philosophical tracts of the era: John Herschel's *Preliminary Discourse on the Study of Natural Philosophy* [1830] and John Stuart Mill's *A System of Logic* [1843]. Mathematical physics, in particular analytic mechanics, was hence rendered problematic from a philosophical point of view, in the way it incorporated (as it now appeared) a vacuous and totally formal mathematical scheme in order to yield an empirical and realist account of nature. Only two thinkers in Britain seriously addressed the issue during the 1830's—William Whewell at Cambridge and William Rowan Hamilton in Dublin. Others simply ignored it. Pure mathematics was pursued by Babbage, Peacock, and others with no real concern for its reapplication to physics, while thinkers like Herschel and Mill published philosophies of science that were intentionally irrelevant to contemporary analytical physics.

Whewell's story was different. Fully sympathetic toward the objectives of the Society, he embarked, during the late 1810s, on a series of mechanics textbooks designed to replace the current "Senate books" and that presented mechanics in as analytical form as he thought his readership would allow. Didactic deliberations took him swiftly to the heart of the problem. And as a result, he gradually retreated from both a Lagrangean view of mathematics (publishing a textbook in 1838 entitled *The Doctrine of*

Limits) and a Baconian theory of natural science. Whewell's alternative philosophy of science stated briefly the following: All real knowledge comprises both an objective empirical element given by the world without "colligated," as he had it, by a purely conceptual element given by the mind. Science at its best, he claimed, takes the form of a well-formed Euclidean conceptual scheme that on the one hand explicates what he called a "fundamental idea" (such as that of space in the case of geometry, that of force in the case of mechanics, and that of limit in the case of the calculus), while perfectly "colligating" an entire body of fact on the other. His differences with Hamiltonian centered on the question of the *emergence* of such science. Whewell viewed its formation as an essentially *unitary* process by which both elements—the conceptual and the empirical—emerged in union, and he formulated his methodology of science accordingly. William Rowan Hamilton, on the other hand, a keen follower of Coleridge and Kant, and thus, from the very start, free of traditional empiricist considerations, held mathematical science to be the miraculous culmination of two quite separate efforts. "There are, or may be imagined, *two* dynamical sciences," he wrote to Whewell in 1833,

> one subjective *a priori*, metaphysical, deducible from meditation on our ideas of Power, Space, Time; the other objective, *a posteriori*, physical discoverable by observation and generalization of facts or phenomena: ...these two sciences are distinct in kind, but ultimately and wonderfully connected, in consequence of the ultimate union of the subjective and objective in God,...

Hamilton's extreme idealism was too much for someone coming from Whewell's background. "The world which Newton constructed (*sic*)," observed Hamilton playfully in a lecture delivered in 1830, "was like the outward world; but had it not been so, he might still have chosen to contemplate it." For Whewell, despite its twofold, or as he had it, antithetical structure, mathematical physics took form in the course of a deliberate negotiation of phenomena, and not by mere meditating on our ideas. The process of induction itself, he maintained, involved the construction of conceptual structures by which the facts considered were thereafter seen in a new light, and which at one and the same time could be shown to constitute a meaningful step in both reasoning *upward* from the facts to laws of greater generality, and in reasoning *downward* from the axioms of the conceptual scheme. This of course raised weighty epistemological problems alongside problems of methodological procedure. There was also the problem of the nature of the *truth* of such science. Whewell's conclusion that the laws of physics were at once both necessary (*qua* theorems of a Euclidean system) and empirical (*qua* colligators of phenomena) baffled his critics, to say the least. Yet, on the other hand, Whewell's philosophy of science stood out as the only serious and fully worked out philosophical account of mathematical physics available. This, I submit, was the philosophical problem situation faced by the young Maxwell.

Unlike any other, Whewell's philosophy was explicitly formulated to account for the type of physics (both dynamistic or mechanistic) developed by the great researchers of electromagnetism on whose work Maxwell was building. And it was a system of thought Maxwell was well acquainted with both directly and *via* J. D. Forbes at Edinburgh, who had been tutored by Whewell by correspondence during the summer of 1831.

It is within the context of the problems initially taken up by Whewell and those raised thereafter by Whewell's novel solutions to them that, I propose, Maxwell's philosophical writings should be read. In other words, in his essays concerning the differences between mathematical and physical knowledge, the precise nature of mathematical and physical truths, the merits of formal mathematical analogies in physics, the role of experiment, or the nature of physical constants the locus of tension is not to be sought in the differing dynamistic and mechanistic attitudes to science, but rather, as with Whewell, in the problematic interface between the mathematical and experiential aspects that combine to make science at its best.

Once aware of the role of Whewell's philosophy in the formation of Maxwell's views, one cannot fail to notice the Whewellian ring to almost all of Maxwell's writings in the philosophy of science—"that hidden and dim region where Thought weds fact, where the mental operation of the mathematician and the physicial action of the molecules are seen in their true relation."(6)

First are the little asides such as his remark, in his prize-winning essay on Saturn's rings, to the effect that "Huygens discovered *that what he saw* was a thin flat ring etc."(7) (in the empiricist tradition to discover *is* to see, but according to Whewell it is to see in the new light of a concept), or his remark in a letter to R. B. Litchfield about his work in optics that,(8)

> It is hard work grinding out "appropriate ideas" as Whewell calls them. However, I think they are coming out at last, and by dint of knocking them against all the facts and 1/2-digested theories afloat, I hope to bring them to shape, after which I hope to understand something more about inductive philosophy than I do at present.

The letter, note, is also indicative of Maxwell's keen philosophical reflection on the science he was engaged in.

Second, and more important, are the essays and lectures that clearly expound upon Whewellian themes. One good example is his delightful 1860 Kings College lecture on the nature of natural philosophy (Garber *et al.*,(2) document 2, pp. 68–89). In it Maxwell attempts to characterize the various physical sciences as "bounded on the mechanical side by Mathematics, and on the Physical side by Chemistry" (p. 69). Mechanics, he states, "differs from Mathematics only by involving the ideas of matter, time, and force, in addition to those of Quantity and Space. The methods employed are the same as in mathematics and the axions, or laws of motion, upon which the science is founded are of the same kind as those of geometry" (p. 69). All branches of physics, he states, agree with mathematics "in using ... ideas as the foundation of systematic science," and in the absence of the idea appropriate to the science in question no progress can be made:

> If a man understands what Force means, I have only to secure his attention, and I can prove to him as many propositions as I please, but if he has not the *fundamental idea*, no amount of demonstration will give it him. He must think for himself till he gets it. (p. 73; italics added)

Natural philosophy, however, involves more than the mathematical explication of fundamental ideas:

> ...in the study of Natural Philosophy we shall endeavour to put our calculations into such a form that every step may be capable of some physical interpretation, and thus we shall exercise powers far more useful than those of mere calculation —the application of principles, and the interpretation of results. (p. 77)

All this, of course, is party-line Whewell. However, Maxwell continues, viewed thus, as theorems of mathematical systems interpreted physically, the nature of the truth of the laws of physics becomes a problem. For, as Maxwell put it, "When we examine the truths of science, [we] find that we can not only say 'This *is* so' but 'This *must be* so for otherwise it would not be consistent with the first principles of truth'" (p. 75). Maxwell on this chooses not to pass philosophical judgement:

> I shall now not enter upon the question whether the fundamental truths of Physics are to be regarded as mere facts discovered by experiment, or as necessary truths, which the mind must acknowledge as true as soon as its attention has been directed to them. Questions of this kind belong to Metaphysics. (p. 74)

He goes on to talk of the method of the physical sciences, and again, like Whewell, stresses the value of hypotheses in the process of discovery.

Finally, there are texts in which Whewellian themes are critically built on, with regard to which Maxwell goes a step beyond Whewell. The most important issue, dealt with in detail by Hendry, but, again, without reference to the problems afloat, is that of the role of analogy in science.

Whewell's evaluative criteria of scientific theories were wholly retrospective. Central among them was what he had dubbed: "consilience of inductions," namely, when a theory surprisingly yields an explanation of phenomena quite different from those it was initially designed to explain, in which, he claimed "we have a criterion of reality, which has never yet been produced in favour of falsehood."(9) True theories, claimed Whewell, converged, simplifying entire areas of knowledge as they developed. This was, as he had it, "the stamp of truth," and it becomes evident as time goes by only in retrospect. Maxwell was not satisfied with Whewell's analysis, on two accounts. First, he was reluctant to accept Whewell's view that the discovery of the consilient virtues of a theory were to be left to mere chance, and he sought ways to *test* theories for their convergent properties. Second, the notion of consilience itself, he claimed, was not at all simple. "Suppose," he says, "that we have successfully introduced certain ideas belonging to an elementary science by applying them metaphorically to some new class of phenomena. It becomes an important philosophical question," and one not dealt with by Whewell,[10] "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" (Ref. 6, p. 102). Moreover, he pointed out (Ref. 6, p. 103), at times *rival* theories exhibit similar consiliences, and arrive apparently independently at the same numerical results. The Whewellian test of consilience is evidently not enough in such cases. And yet the whole project of mathematical physics seems to be motivated by and geared toward a search for formal analogies between phenomena. For "as in a pun two truths lie hid under one expression," he wrote in a witty and insightful essay written at Cambridge in 1856, "so in an analogy one truth is discovered under two expressions" (Ref. 8, p. 235). Maxwell, as is well known, made the notions of physical and mathematical analogy the cornerstones to the methodology he both preached and practiced. "The criterion of the value of a theory, that it explains quite other phenomena besides those on which it is based," wrote Max Planck, "has never been so well satisfied as with Maxwell's theory" (cited in Ref. 11). His notion of analogy is perhaps his main claim on philosophical posterity, but to fully appreciate his achievement, a full and detailed analysis of the problems he faced is called for. And those, I suggest, have their roots in the philosophy of William Whewell.

5. Read thus, in their problematic context, Maxwell's philosophical and methodological writings acquire new meaning and special relevance. Not only are the philosophical problems Maxwell addressed still very much with us today, but the solutions he offered them, unlike his science, are far from superseded. As such, I believe, they merit both a comprehensive scientific edition and a full philosophical and historical analysis such as that offered with respect to aspects of his scientific work in the books under review. His texts on kinetic theory and the properties of gases as collected by Elizabeth Garber and her collaborators, and the story of the emergence of his theory of the electromagnetic field as told by Hendry, are valuable contributions to the literature on Maxwell precisely *because* they clearly convey the scientific difficulties he was addressing. But with respect to his philosophical writings, such is not the case in either of the books. The list of texts of philosophical interest included in *Maxwell on Molecules and Gases* is partial, and was evidently compiled for the reference they make to his thoughts on molecules and gases rather than for their purely philosophical value. Similarly, as I have shown, Hendry, too, fails to tell the full story of Maxwell's profound deliberations regarding the nature of mathematical and physical knowledge, the nature of their truths, and that of the manner in which they are best achieved. Nonetheless, both books, in their different ways, constitute important steps toward such an account of one of the greatest thinkers of all time.

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