RE-READING THE PAST FROM THE END OF PHYSICS: MAXWELL'S EQUATIONS IN RETROSPECT

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I. Introduction

For the working physicist, the past and future of physics are thoroughly intertwined. With each set of goals the discipline has posed for itself comes a new gloss on prior accomplishments. As a result there is no unique or simple fashion in which the history of physics (as viewed by physicists) is related to their research priorities. In this brief essay I would like to illustrate some examples of the many ways in which the past is re-read, and then to speculate on some of the functions this constant reinterpretation plays.

By the 'history of physics' I am not referring to the relatively recent professional history of physics of the sort that appears in journals like *Historical Studies in the Physical Sciences, Archive for History of Exact Sciences*, and so on. Physicists as a rule do not read this literature and in any case it has not yet existed for a long enough time for there to be any meaningful assessment of their effect. Rather, I have in mind history as it appears in textbooks, as it is repeated from generation to generation of physicists, and in general the version of the past physicists learn from those primary and secondary sources they actually read.

I take the self-thematization (*Selbstthematisierung* as it appears in the original title of the conference in preparation of this volume) to mean the establishment of programmatic ideals for physics: the articulation of what it would take to provide an adequate account of natural phenomena. This articulation has occurred not once but several times within modern physics. The argument to be presented here is that each of these reorderings of explanatory ideals has been accompanied by a new perception of past accomplishments, at least in the minds of working physicists.

Loren Graham, Wolf Lepenies, and Peter Weingart (eds.), Functions and Uses of Disciplinary Histories, Volume VII, 1983, 35–51.

Unfortunately in a mature science like physics, the participants in research are not given to explicit pronouncements on their over-arching explanatory goals. There is, however, a fascinating exception to this general tendency towards problem-solving rather than goal-defining. Several times in the history of modern physics there have been moments of an almost hubristic optimism about the future. Claims have been made that the fundamental laws of physics are all known and that the future will lie in applications of detail. In these moments we can glimpse the broad goals of physics: for to state that the end of physics is near is *a fortiori* to provide an idea of what would constitute such a triumph.

2. Histories and Futures: Maxwell's Equations through Continuum Physics

This discussion will be limited to examples of the reinterpretation of Maxwell's equations from the time of their invention by Maxwell to the present. Maxwell inherited two very different traditions which up until his time had coexisted in European thought without productively interacting. The first, Cartesian physics, supposed that all phenomena could be explained by the mechanical pushing and pulling that matter at one point exerts on matter at neighboring points. Within this scheme, Descartes contended, he "could set out here many rules to determine in particular, how and how much each object's motion is diverted, augmented or diminished by its collision with others. Taken together these would comprise all the effects of nature ..." (1).

The other mechanical legacy from the 17th century came of course from Newton. Unlike Descartes, in his theory of gravity Newton was unwilling to propose specific mechanisms by which gravity would operate, instead preferring to state simply the mathematical relations by which the motion of celestial objects could be calculated. So successful was his strategy that some Newtonians such as C. L. Berthollet were later moved to argue that even molecular attraction and gravitation were but "one and the same property" (2).

Newton's central force law formed the ideal for explanation in the 19th century as J.-B. Biot, F. Savart, and J.-M. Ampère turned to the newly discovered electrodynamic phenomena of the early nineteenth century. All of their discoveries were couched in the language of distant-action force laws. Biot and Savart sought laws for the action of magnetic poles on other magnetic poles; Ampère found the distant effects of current elements on other current elements. Maxwell, by contrast, formulated electrodynamics in such a way that he could maintain the precision of a Newtonian theory in a near-action form. Instead Maxwell proposed that charged objects act upon one another by first affecting states of an intervening substance which provided a continuum throughout space. For Maxwell his equations offered a comprehensive and quantitive measure of states of this continuum.

For example, in one formulation of his theory Maxwell represented the effects of magnetism as being analogous to an array of linked vortices through the ether. (See Figure 1.) Imagine a wire pq in which balls rotating clockwise

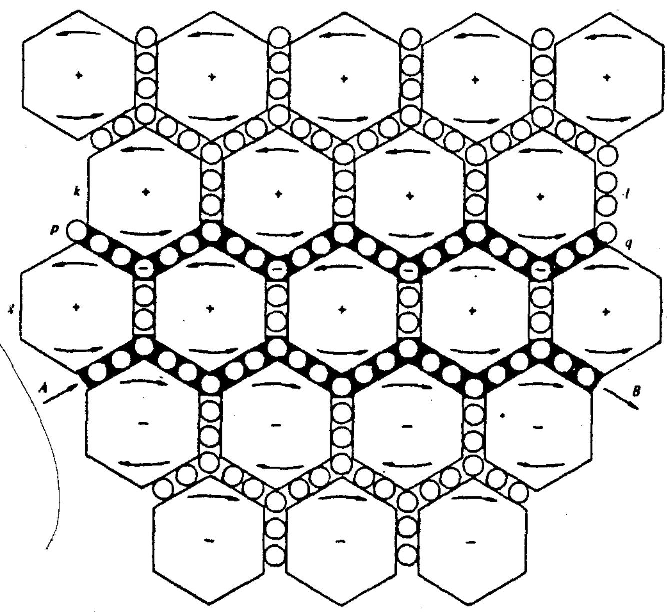

Fig. 1. Maxwell's Etherial Vortices (1861). Current in line pq causes vortices to form in the ether above and below the wire. Source: Maxwell, *Scientific Papers*, 489.

in place represent a current. The current causes motion of the vortices on either side of the wire. These motions can then have physical effects on other wires (such as AB) just as a magnetic field induces currents in moving wires (3). Quite generally Maxwell felt the mechanics of the ether "must be subject to the general laws of dynamics, and we ought to be able to work out all the consequences of its motion, provided we know the form of the relation between the motion of its parts" (4). In fact the dynamical analogies Maxwell established between the electromagnetic continuum and a mechanical continuum played a crucial role in his discovery of the electromagnetic nature of light (5). Maxwell's success in this regard led him and many of his followers to pursue the continuum mechanical analogies with great interest. In the course of his work Maxwell proposed many different models of the ether, leaving his followers to sort out what they took to be the essence of his theory. Here there was much disagreement.

J. H. Poynting, one of the first group of Cavendish students (6), studied Maxwell's work assiduously. He concluded that the real content of the theory was in a description of how energy was distributed in the ether (7). This approach was in sharp distinction to a contemporary of Maxwell's, William Thomson, who felt "one needed to know the system's structure" in detail in order to fully understand it (8). In the following years Thomson and others proposed model after model of the ether. But even as the models increased in complexity, it became more and more evident that Maxwell's equations were enormously successful. Hertz found the electromagnetic waves Maxwell had predicted, others were able to combine various mechanical interpretations of Maxwell's equations in such a way as to account both qualitatively and quantitatively for a host of electrical effects. What more could one want? Indeed, it seemed to some physicists in the closing year of the nineteenth century that taken together, Newton's celestial mechanics and Maxwell's equations indicated that the prospect of completing physics was in sight. As the pre-eminent American experimentalist on light, A. A. Michelson, declared in 1894:

> While it is never safe to affirm that the future of physical science has no marvels in store even more astonishing than those of the past, it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all phenomena which come under our notice. It is here that the science of measurement shows its importance – where quantitative results are more to be desired than qualitative work. An eminent physicist has remarked that the future truths of physics are to be looked for in the sixth place of decimals (9).

Many late nineteenth century Maxwellians saw Maxwell's electromagnetic synthesis as a triumph of dynamics. Of course they recognized problems, but these were expected to be surmountable. Not everyone was of such a mind. In 1900 Wilhelm Wien became a spokesman for a new movement that hoped to reverse the effort to explain electromagnetic effects by the dynamics of the ether. Wien argued that,

It is doubtless one of the most important tasks of theoretical physics to unite the up until now isolated fields of mechanics and electromagnetism, and to derive their relevant differential equations from a common foundation.

However Wien insisted that his predecessors' dream of finding a mechanically based synthesis was misguided.

More promising as a foundation for further theoretical work is the opposite task: to derive the mechanical from the more general electromagnetic equations (10).

For those physicists after 1900 who sought a unified 'electromagnetic world picture', such as H. Minkowski, H. Poincaré, H. L. Lorentz, and M. Abraham, Maxwell's equations were a starting and not an ending point. No longer did they view the ether as a mechanical object (or as fundamentally analogous to a mechanical object) in which stresses and strains could be identified with electromagnetic fields. Instead, as reformulated by Lorentz, the ether was completely distinct from charge. Thus electrical current simply became the movement of charge instead of a complicated state of the ether. Furthermore, the mechanical models of the ether were entirely abandoned and electrical and magnetic fields simply became specifications of states of a purely electromagnetic continuum.

In the years following Wien's program of 1900, Lorentz and others hoped to eliminate the concept of mechanical mass as a fundamental concept. They sought to show that what we call mass is nothing more than the inertia associated with electric fields in the ether. The mass of an ordinary object was ascribed to the aggregate inertia of the object's constituent charged particles. This was what was meant by explaining mechanics in terms of electrodynamics (11).

The ambitious program of the electromagnetic world view found a fascinated observer in the person of the young Albert Einstein. Even in his days as a student at the Eidgenossische Technische Hochschule (and despite the lack of interest displayed by his teachers) Einstein felt that

> The most fascinating subject at the time I was a student was Maxwell's theory. What made this theory appear revolutionary was the transition from forces at a distance to fields as fundamental variables .... It was like a revelation (12).

Even Planck's work of 1900 on energy quanta seemed of interest to Einstein principally insofar as it would clarify the problem of the electromagnetic foundation of physics (13).

But after considerable thought the young physicist concluded that "neither mechanics nor electrodynamics could ... claim exact validity", and thus neither should be derived from the other. "By and by", Einstein recalled, "I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts. The longer and the more despairingly I tried, the more I came to the conviction that only the discovery of a universal formal principle could lead us to assured results" (14).

Einstein's 'formal principle' became the foundation of the theory of special relativity. It can be stated briefly: Maxwell's equations and more generally all laws of physics should have the same form in all frames of reference. (This was already true for mechanics.) This meant that the Lorentz interpretation of Maxwell's electromagnetic theory of light would no longer do. For Maxwell's equations predicted the light wave to have a velocity of $c = 3 \times 10^{10}$ cm sec^{-1} in the rest frame of the ether. Therefore some other, different set of equations would have to replace Maxwell's equations in a frame of reference moving with respect to the ether. On philosophical-aesthetic grounds Einstein rejected this possibility. There should, he believed, only be one set of electromagnetic equations (15).

As can easily be understood, Einstein's epistemological criticisms of the usual interpretation of Maxwell's equations were not easily assimilated into physics. The person most responsible for the acceptance of Einstein's work was perhaps Hermann Minkowski, through his reformulation of Einstein's theory into a geometrical language. Minkowski's expectation was that one day physics could be expressed as a series of geometrical statements about lines and surfaces in four-dimensional space-time. As a start on this program, he re-expressed Einstein's formulation of electrodynamics in this goemetrical scheme (16).

At first Minkowski's highly mathematical (geometrical) point of view seemed alien and unnecessary to Einstein. Little by little though, Einstein came to Minkowski's point of view as he discovered that his own work on the general theory of relativity could not even get started without the idea of space-time. Eventually, the stunning success of general relativity made Einstein a thorough-going convert to the geometrical program.

From 1915 to the end of his life Einstein attempted to create an even more general geometrical theory that would incorporate both the general relativity theory of gravity and suitably generalized Maxwellian electrodynamics. Thus Einstein's vision of Maxwell's equations was of a component of a more general geometrical theory of space-time. Einstein even came to believe that such a generalized geometrical theory could be so formulated that quantum effects would be explained by a more fundamental physics of the continuum. As is well known, in this belief Einstein maintained a minority position against his contemporaries.

Some very interesting physics has been built upon the Einstein-Minkowski idea. One intriguing recent project has been the attempt by J. A. Wheeler *et al.* to pursue a 'geometrodynamics' in which all physics would be built upon the dynamics deduced from geometrical considerations. (They did not, however, expect to derive quantum effects as Einstein had hoped.) Part of their program included the writing of a now very popular text on gravitation which they began by summarizing their motives. First they wanted to display the results of much interesting astrophysics. They then added:

> Of quite another motive for the study of the subject, to contemplate Einstein's inspiring vision of geometry as the machinery of physics, we shall say nothing here because it speaks out, we hope, in every chapter of the book (17).

One of these chapters, naturally, is on Maxwell's equations which are interpreted as simple geometrical objects in space-time (akin to elongated wine bottle boxes). (See Figure 2.) Of course no physical object exists which looks like Figure 2; Wheeler and his colleagues are simply saying that such hypothetical objects can be used to calculate electrodynamic effects such as the force on a moving charged particle.

3. Histories and Futures: Maxwell's Equations and Quantum Physics

As discussed earlier, Einstein set his geometrical program against the view that held quantum mechanics to be the basis of a complete physical theory.

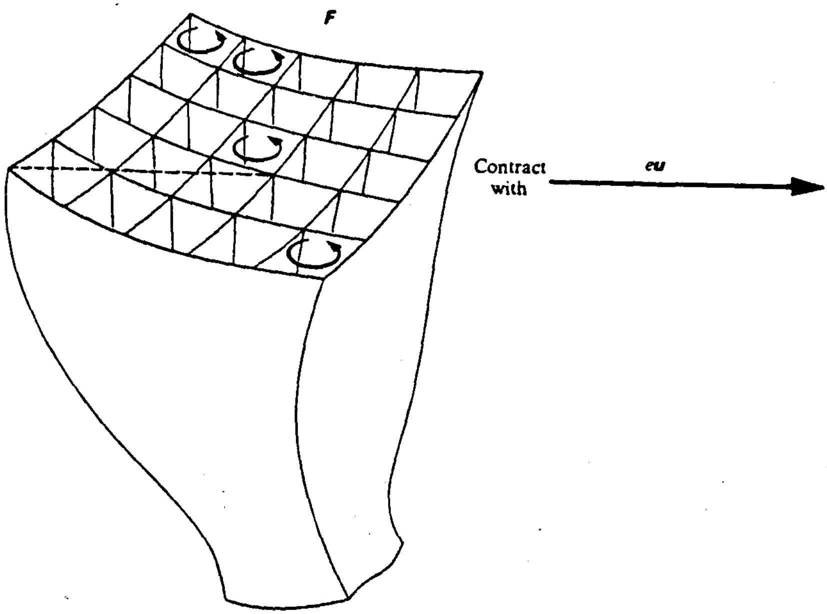

Fig. 2. Space-Time Geometrical Representation of Maxwell's Equations. Misner, Thorne and Wheeler show how a vector representing the velocity of a charged particle (the arrow) can be combined with a two-form (the wine-box object) to yield the force on on a moving particle. Source: Misner, Thorne and Wheeler, *Gravitation*, 104.

Since then the quantum mechanical approach has been accepted almost universally as the proper foundation of physics.

It was already clear in 1905 (when Einstein first hypothesized that light was made up of quanta) that discrete particles of light would wreak havoc on Maxwell's differential equations. How could these equations about continuous entities describe a granular reality? The answer at least for the then known massive particles – electrons and protons – came in the 1920's as quantum mechanics was developed. Since electrons and protons were thought to make up all matter, the future path of physics seemed well charted. Bertrand Russell was optimistic enough to claim in 1925 that:

Physical science is thus approaching the stage when it will be complete, and therefore uninteresting. Given the laws governing the motions of electrons and protons, the rest is merely geography – a collection of particular facts filling their distribution throughout the portion of the world's history. The total number of facts of geography required to determine the world's history is probably finite; theoretically they all could be written down in a log book to be kept at Somerset House with a calculating machine attached which, by turning a handle, could enable the inquirer to find out the facts at other times than those recorded. It is difficult to imagine anything less interesting or more different from the passionate delight of incomplete discovery. It is like climbing a high mountain and finding nothing at the top except a restaurant where they sell ginger beer, surrounded by fog but equipped with a wireless. Perhaps in the times of Ahmes the multiplication table was exciting (18).

For three years following Russell's prophecy there was extraordinary excitement in the physics community as it became clear that quantum mechanics was a very new kind of physical theory. Heisenberg, Schrödinger, and Max Born set out the elements of the new theory which was brought to what seemed a conclusion by Dirac in 1928. Dirac brought together relativity and quantum mechanics in the equation that bears his name. Max Born apparently was so exuberant over these heady developments that he announced to a group of visitors to Göttingen: "Physics, as we know it, will be over in six months" (19).

Part of Born's enthusiasm for the relativistic quantum mechanics was based on theoretical and experimental success that from the beginning promised to be spectacular. But another element of Dirac's theory that inspired statements such as Born's was grounded in a fundamental misunderstanding of what Dirac's equation meant. Dirac, it should be added, shared this misapprehension (20). The issue was this. Dirac's equation for the electron unambiguously also predicted the existence of a positively charged particle. This particle seemed to have the same mass as the electron (as well it should – we now call this particle the positron) but no such particle was known. It was thus widely assumed that someone would find a reason why this particle was the proton.

Naturally this would have been a success beyond anyone's expectation: a single equation would have accounted for both known particles and simultaneously reconciled quantum mechanics and relativity. It seemed obvious to many research physicists at the time that Maxwell's equations would easily be integrated into this system. Leon Rosenfeld later recalled that,

After Dirac's great paper on the theory of the electron one had the impression that all the fundamental features of atomic physics had been neatly incorporated into the new conceptual structure, and with characteristic eagerness the other pioneers of the atomic world Heisenberg and Pauli, leaving to lesser fry the polishing off of details, turned to the major remaining task of apply the new methods of quantization to the electromagnetic field. It is difficult to those who did not witness it to imagine the enthusiam, nay the presumptuousness, which filled our hearts in those days. I shall never forget the terse way in which a friend of mine (Now a very eminent figure in the world of physics) expressed his view of our future prospects: 'In a couple of years', he said, 'we shall have cleared up electrodynamics; another couple of years for the nuclei, and physics will be finished. We shall then turn to biology' (21).

Needless to say physics did not end – only a few years later the neutron, positron, and muon were discovered, none of which fit neatly into the naive interpretation of the Dirac equation. In addition Maxwell's equations were not quantized nearly so easily as Rosenfeld and his contemporaries expected. Both problems came to be seen as more, not less difficult during the late thirties – the number of particles continued to increase and the difficulties inherent in constructing a self-consistent and physically interpretable quantum electrodynamics became more evident.

After World War II, largely through the work of R. Feynman, J. Schwinger, S. Tomonaga, and F. Dyson, quantum electrodynamics was put in a form that was both predictively successful and (more or less) coherent. Together they presented an interpretation of quantum electrodynamics in part by jettisoning the 1930's hope that quantum equations could describe the motion of individual particles. In the new scheme any physical interaction could involve an arbitrary number of particles. Let me give an example. In electrodynamics before quantum mechanics one electron repels another by creating a field, the field causes the second electron to feel a force. In quantum electrodynamics one says the force between two electrons is due simultaneously to many processes, each of which occurs with a certain probability.

Most probable is that a photon (γ) travels from electron 1 (e_1) to electron 2 (e_2).

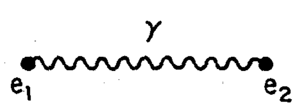

Fig. 3.

Less probable is that the photon, en route, creates an electron-positron pair

e^+e^- which then annihilates creating a second photon which is absorbed by electron 2.

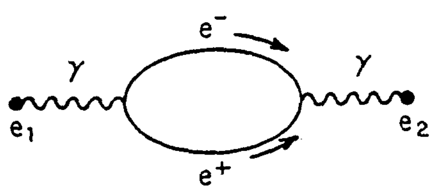

Fig. 4.

Indeed there is an infinite series of such possible processes which grow less probable as they grow more complicated. Adding together all possible diagrams gives us the total probability of electron 1 bouncing off electron 2. Maxwell's equations are thus given by infinite series, like the one just sketched of possible quantum exchanges between electrons.

Inspired by the success of quantum electrodynamics, physicists hoped that by analogy a quantum theory of the weak and the strong nuclear forces could be formulated. Some new particle or particles would take the place of the photon as carrier of the force. In 1967 S. Weinberg and A. Salam proposed a theory along these lines that explained both the weak and electrodynamic forces. They did so by making use of a new kind of symmetry.

The coordinate system in which one studies a phenomenon seems extrinsic to the phenomenon itself. For this reason Einstein postulated that it should be a goal of physics to make physical laws independent of changes of coordinate systems, even as one passed from a still to a moving system. Because coordinates are external to the objects of study, equations that remain unchanged when one changes coordinates are said to have an *external* symmetry.

Other kinds of symmetries are possible. For instance in the 1930's it was discovered that for some nuclear physics experiments nuclear effects were the same if one switched every neutron with a proton. This is an operation affecting the objects under study themselves. There the equations that remain unchanged even when one switches neutrons for protons are said to have *internal* symmetry.

Internal symmetries had been explored ever since Weyl's work in the

1920's, but in the 1960's a particular kind of internal symmetry, gauge symmetry, became the focus of a good deal of attention. Several physicists including Weinberg were very struck by a remarkable fact. If one demanded that the quantum theory of electrons had a gauge symmetry one had to add a new field to the equations. The simplest such field which ensured gauge symmetry gave Maxwell's equations! In a sense the gauge symmetry yielded the laws of electrodynamics.

Weinberg and Salam found this fact tremendously encouraging. They guessed that in order to obtain a quantum theory of nuclear forces it might suffice to demand a new, more general kind of gauge symmetry. Theories with this more general symmetry were hard to find, but not impossible. As Weinberg later recalled,

> Limitations of this sort are after what we most want; not mathematical methods which can make sense out of physically irrelevant theories, but methods which carry constraints, because these constraints may point of way toward the one true theory (22).

In 1967 Weinberg constructed a theory that satisfied these symmetric 'limitations', and properly yielded both Maxwell's equations and a weak interaction theory. Building on these ideas, H. Georgi, S. Glashow and others exploited an even larger gauge symmetry condition to build a theory that would encompass the Weinberg-Salam theory and the strong nuclear forces – in short all of fundamental physics except gravity. These theories have been dubbed GUTS for grand unified theories. Experimentalists are now working in many international groups to test these ideas. But already the theoretical virtues of these models have precipitated another period of great optimism among physicists. Glashow introduced the First Workshop on Grand Unification by arguing that

> .... we have for the first time an apparently correct theory of elementary particle physics. It may be, in a sense, phenomenologically complete. It suggests the possibility that there are no more surprises at higher energies, at least at energies that are remotely accessible .... Theorists do expect novel high-energy phenomena, but only at absurdly inaccessible energies. Proton decay, if it is found, will reinforce belief in the great desert extending from 100 GeV to the unification mass of 10^{14} GeV. Perhaps the desert is a blessing in disguise. Ever larger and more costly machines conflict with dwindling finances and energy reserves. All frontiers come to an end.
>
> You may like this scenario or not; it may be true or false. But it's neither impossible, implausible, nor unlikely (23).

But even before the grand unified field theories were developed it was clear that the basic machinery of quantum field theory, together with the new ideas about internal symmetries offered a profound new insight into the nature of matter. For the first time, in the early 1970's there was a real hope that a unification of all the fields could be accomplished without reducing physics to electrodynamics or geometry.

The pedagogical organization of physics has begun to reflect the new ideal of the gauge theorists. In his text on gravity and general relativity Weinberg, like Wheeler, began by acknowledging the intrinsic interest of astrophysical phenomena. He added that it was true that the geometrical approach,

> .... *was* Einstein's point of view, and his preeminent genius necessarily shapes our understanding of the theory he created. However, I believe that the geometrical approach has driven a wedge between general relativity and the theory of elementary particles. As long as it could be hoped, as Einstein did hope, that matter would eventually be understood in geometrical terms, it made sense to give Riemannian geometry a primary role in describing the theory of gravitation. But now the passage of time has taught us not to expect that the strong, weak, and electromagnetic interactions can be understood in geometrical terms, and too great an emphasis on geometry can only obscure the deep connections between gravitation and the rest of physics (24).

Stephen Hawking, whose work lies on just this boundary between gravity and particle physics has helped advance physics towards the still distant goal of uniting gravity with 'the rest of physics'. In fact he (more than some particle physicists) is concerned that any 'final' theory of physics include gravity. Nonetheless Hawking expects that GUTS can be expanded to do the job. In his inaugural lecture as Lucasian Professor at Cambridge University Hawking began by discussing,

> The possibility that the goal of theoretical physics might be achieved in the not too distant future, say, by the end of the century. By this I mean that we might have a complete, consistent and unified theory of the physical interactions which would describe all possible observations (25).

After cautioning that such hopes have been raised before he added:

> "Nevertheless, we have made a lot of progress in recent years and, as I shall describe, there are some grounds for cautious optimism that we may see a complete theory within the lifetime of some of those present here (26).

Like many physicist-prophets before him Hawking finished by contending that a sufficiently large computing machine would be capable of all future calculations of applied problems.

So maybe the end is in sight for theoretical physicists if not for theoretical physics (27).

If so, in the future Maxwell's equations will be viewed as the consequence of some small corner of the vast internal symmetry that determined the final theory of physics.

4. Re-Ordering the Past

Clearly there are a great many reasons why physicists reinterpret past accomplishments in physics. For purposes of discussion I want to call three of these functions pedagogic, heuristic, and justificatory. A pedagogic function is served for example, when earlier well-known results can be explained to students in the vocabulary of a new theory. Thus now virtually all texts on gauge theories and unified field theories begin by deriving Maxwell's equations from symmetry principles as a simple example of a much more powerful technique. One of the most popular of these pedagogic presentations adds almost apologetically after such an observation,

It is supererogatory to observe that the photon was not discovered by gauge invariance. Rather, gauge transformations were discovered as a useful property of Maxwell's equations (28).

Or, to give another example: in the Misner, Thorne, Wheeler text, *Gravitation*, the authors carefully develop Maxwell's theory in terms of differential geometry in order to point out the differences and similarities between the (geometrized) Maxwell equations and Einstein's geometrical gravity equations. Furthermore both in the case of quantum gauge theories and geometrodynamics the authors want to instruct the student in methods that will later be applied to more general and difficult cases. It would be interesting to trace systematically the entire pedagogical history of Maxwell's equations. At least at the best level of instruction I suspect such a history would closely parallel (though lag behind) research concerns.

Reformulation of the past often plays a role not just in the education of students but in the advancement of the discipline itself. Now we see the goals of the mechanical and electromagnetic world pictures as misguided; nonetheless, we can recognize that these older interpretations of Maxwell's equations led to much productive physics. To give another example: in later life Einstein commented that Lorentz's reformulation of Maxwell's equations in terms of charges and fields in the ether "simply had to lead to the special theory of relativity" (29). Elsewhere Einstein asserted,

> The special theory of relativity owes its origin to Maxwell's equations of the electromagnetic field. Conversely, the latter can be grasped formally in a satisfactory fashion only by way of the special theory of relativity (30).

Yet another example comes from the case of weak interactions. Weinberg recalls being very impressed by the possibility of deriving Maxwell's equations from a symmetry constraint. This contributed to his hope that some analogous constraint might prove productive in finding a theory of nuclear interactions.

Finally, there is a justificatory role played by re-reading the past. It always gives added weight to a current research program if older, established theories mesh with the new theories in a natural way. For the late nineteenth century ether-mechanicians Maxwell's equations fit into a larger mechanical world view. For the reductionist electromagnetic program of the early 1900's the history of physics was up until then a series of fortunate approximations, the true basis of which was only beginning to be understood. In their eyes there was an ever decreasing number of fundamental entities – they hoped to show ultimately that there would only be electricity in the world. When Einstein was developing the special theory of relativity, Maxwell's equations represented but one of several physical theories which, when properly reinterpreted, would co-exist with (not replace) relativistic mechanics.

In recent times we see the progress of physics very differently – as a long road marked by ever increasing symmetry. We have in mind an inexorable climb up a ladder of symmetries: Galilean, Lorentzian, global gauge, local Abelian gauge, local non-Abelian gauge. The study of the reinterpretation of the past in physics is therefore integrally linked to the ideal of progress in the physical sciences.

The example I have given here of Maxwell's equations is of course not typical of all older physical theories. But I do hope to have left the reader with some sense of how the past feels to working physicists. It is certainly not the past of the historian, but neither is it the stale textbook anecdote forcibly put in Baconian form. The past in physics is an ever-changing legacy, constantly reinterpreted at the forefront of physics.

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References

1. Descartes, *Oeuvres de Descartes*, C. E. Adams and P. Tannery (eds.), Paris: L. Cerf, 1897–1910, Volume 11, p. 47.
2. Cited in R. Fox, 'The Rise and Fall of Laplacian Physics', *Historical Studies in the Physical Sciences* 4 (1974), 89–136, on 98.
3. J. C. Maxwell, *Scientific Papers*, New York: Dover, 1965, 485ff. Two excellent secondary sources are the book by C. W. F. Everitt, *James Clerk Maxwell, Physicist and Natural Philosopher*, New York: Charles Scribners, 1975, and the article by J. Z. Buchwald, 'The Hall Effect and Maxwellian Electrodynamics in the 1880's Part I: The Discovery of a New Electric Field', *Centaurus* 23 (1979), 51–99, esp. 54ff. See also note 7.
4. J. C. Maxwell, *Scientific Papers*, 533 (Note 3).
5. E. W. F. Everitt, Maxwell, 98–99 (Note 3).
6. J. Z. Buchwald, Hall Effect, 66 (Note 3).
7. J. Z. Buchwald, *Matter, the Medium and Electrical Current*, unpublished Ph.D. dissertation, Harvard University, 1974, 427.
8. *Ibid*. See also the excellent article by M. Norton Wise, 'William Thomson's Mathematical Route to Energy Conservation: A Case Study of the Role of Mathematics in Concept Formulae', *Historical Studies in the Physical Sciences* 10 (1979), 49–83. Wise shows how Thomson's mathematical work led him to give the 'physical analogies more weight'.
9. A. A. Michelson, from an address of 1894 excerpted in a publication of 1898–1899. Cited in L. Badash, 'The Completeness of Nineteenth Century Physics', *Isis* 63 (1972), 48–58, on 52.
10. W. Wien, 'Über die Möglichkeit einer Elektromagnetischen Begrundung der Mechanik', *Annalen der Physik* 5 (1901), 501–513, on 502.

11. For more on the electromagnetic world view, see items in Note 15 and 'R. Mc-Cormmach, Einstein, Lorentz and the Electron Theory', *Historical Studies in the Physical Sciences* 2 (1970), 69–81; Tetu Hirosige, 'Theory of Relativity and the Ether', *Japanese Studies in the History of Science* 7 (1968), 37–58; P. Galison, 'Minkowski's Spacetime: From Visual Thinking to the Absolute World', *Historical Studies in the Physical Sciences* 10 (1979), 85–121.
12. A. Einstein, 'Autobiographical Notes', in P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist* 1, La Salle: Open Court, 1969, 32.
13. *Ibid.*, 47.
14. *Ibid.*, 53.
15. There is a vast literature on the history of special relativity. The reader should refer to A. I. Miller, *Albert Einstein's Special Theory of Relativity: Emergence (1905) and Early Interpretation (1905–1911)*, Reading: Addison Wesley, 1981, and G. Holton, *Thematic Origins of Scientific Thought*, Cambridge: Harvard University Press, 1973 for further references.
16. See P. Galison, 'Minkowski', (Note 11).
17. C. W. Misner, K. S. Thorne, and J. A. Wheeler, *Gravitation*, San Francisco: W. H. Freeman, 1973).
18. B. Russell, 'What I Believe', reprinted in R. E. Egner and L. E. Denonn (eds.), *The Basic Writings of Bertrand Russell*, New York: Simon and Schuster, 1961, 367–370, on 367–8.
19. Stephen Hawking, *Is the End in Sight for Theoretical Physics*? Cambridge: Cambridge University Press, 1980, 1.
20. See for example, P. A. M. Dirac, *The Principles of Quantum Mechanics*, Oxford: Clarendon Press, 1930, 236.
21. L. Rosenfeld, 'Niels Bohr in the Thirties', S. Rozenthal (ed.), *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues*, New York: Wiley, 1967, 118–119.
22. S. Weinberg, 'Conceptual Foundations of the Unified Theory of Weak and Electromagnetic Interactions', *Reviews of Modern Physics* 52 (1980), 515–523, on 517.
23. S. L. Glashow, 'The New Frontier', from *First Workshop on Grand Unification*, Brookline, Massachusetts: Math. Sci. Press, 1980, 3–8, on 3.
24. S. Weinberg, *Gravitation and Cosmology: Principles and Applications of the General Theory of Relativity*, New York: John Wiley, 1972, vii.
25. S. Hawking, 'End in Sight?' 1 (Note 19).
26. *Ibid.*, 2.
27. *Ibid.*, 26.
28. E. S. Abers and B. W. Lee, 'Gauge Theories', *Physics Reports C* 9 (1973), 1–141, on 10.
29. A. Einstein, 'H. A, Lorentz, His Creative Genius and His Personality', in H. A. Lorentz, *Impressions of His Life and Work*, Amsterdam: North Holland, 1957, 5–9, on 7.
30. A. Einstein, 'Autobiographical Notes', 62 (Note 12).