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After the Baltimore Lectures: Some Philosophical Reflections on the Subsequent Development of Physics

Kelvin appears to be associated, in the historical mythology of science, with the opinion that the science of physics was nearly at an end—that there only remained some small clearing up of difficulties, to be followed by a process of determining physical constants to increasing numbers of decimal places. James Clerk Maxwell alluded to this view—of course without the slightest thought of implicating his friend Sir William Thomson—in his Inaugural Lecture at the Cavendish Laboratory; Maxwell remarked: "If this is really the state of things to which we are approaching, our Laboratory may perhaps become celebrated as a place of conscientious labour and consummate skill, but it will be out of place in the University, and ought rather to be classed with the other great workshops of our country, where equal ability is directed to more useful ends." How far Kelvin stood from the complacent view that Maxwell deprecated is immediately apparent from the first page of the preface he supplied when the *Baltimore Lectures* were published by the Cambridge University Press in 1904:

> Having been invited by President Gilman to deliver a course of lectures in the Johns Hopkins University after the meeting of the British Association in Montreal in 1884, on a subject in Physical Science to be chosen by myself, I gladly accepted the invitation. I chose as subject the Wave Theory of Light with the intention of accentuating its failures; rather than of setting forth to junior students the admirable success with which this beautiful theory had explained all that was known of light before the time of Fresnel and Thomas Young, and had produced floods of new knowledge splendidly enriching the whole domain of physical science. My audience was to consist of Professorial fellow-students in physical science; and from the beginning I felt that our meetings were to be conferences of coefficients, in endeavours to advance science, rather than teachings of my comrades by myself. I spoke with absolute freedom, and had never the slightest fear of undermining their perfect faith in ether and its light-giving waves: by anything I could tell them of the imperfection of our mathematics; of the insufficiency or faultiness of our views regarding the dynamical qualities of ether; and of the overwhelmingly great difficulty of finding a field of action for ether among the atoms of ponderable matter. We all felt that difficulties were to be faced and not to be evaded; were to be taken to heart *with the hope of solving them if possible*; but at all events with the certain assurance that there is an explanation of every difficulty though we may never succeed in finding it.

Among the "coefficients" at the lectures were Rowland, Michelson, and Morley. The eighty-year-old Kelvin, in his preface, after noting with satisfaction that the difficulties of 1884 had (in his opinion) all been resolved in the ensuing two decades, wrote with a certain relish of pride that "two of ourselves, Michelson and Morley, have, by their great experimental work on the motion of ether relatively to the earth, raised the one and only serious objection against our dynamical explanations." (It should not be supposed that this one remaining serious objection represented, in Kelvin's view, the last step that would need to be taken; on the contrary, he immediately proceeded to indicate what advances he expected toward "the grand object ... of finding a comprehensive dynamics of ether, electricity, and ponderable matter, which shall include electrostatic force, magnetostatic force, electromagnetism, electrochemistry, and the wave theory of light.")

Yet for all the restless energy of his mind, and despite his generous attitude toward the work of colleagues, it cannot be denied that Kelvin's view of physics was, in a way, curiously stiff and hidebound. Indeed, the advances he celebrated in the preface of 1904 (in which he asserted that the difficulties posed in his lectures of 1884 had all been resolved) have left no trace on the physics of our century, whereas the work that from some years before those lectures were delivered to the time of their publication had achieved the greatest advance in the subject to which the lectures were themselves devoted—work that had laid the groundwork for the profound developments of the next two decades—was passed over almost entirely in both the original lectures and published volume. There is in this something of more than merely biographical interest.

The full title of Thomson's course (it was not until some eight years later that he was baronized as Lord Kelvin) was "Lectures on Molecular Dynamics and the Wave Theory of Light"; the subject, in other words, was to be the problem of the dynamical interrelations of ordinary matter—treated as molecular in its structure—and that extraordinary matter the ether (the carrier of the waves constituting light). To resolve this problem completely, one would require a theory that gave a satisfactory representation of the propagation of light in free space (the "free ether"), in the interior of ordinary transparent or partially transparent matter, and at the surface of separation between two ordinary bodies of different constitution, or between free ether and an ordinary body. Beyond this, one would require an account of the dynamical interactions involved when bodies *move through* the ether. This inventory of desiderata essentially defines what deserves to be called "the classical ether problem for light." In Kelvin's opinion, by 1904 he had satisfactorily resolved this problem, except for the difficulty posed by the Michelson-Morley experiment. There was, in the latter part of the nineteenth century, a further and more intricate set of issues concerning the ether, which may be called "the classical ether problem for the electromagnetic field." Kelvin fully recognized this; in the preface from which I have already quoted, he goes on to say:

My object in the Baltimore Lectures was to find how much of the phenomena of light can be explained without going beyond the elastic-solid-theory. We have now our answer: *everything non-magnetic; nothing magnetic.* The so-called "electromagnetic theory of light" has not helped us hitherto: but the grand object is fully before us of finding a comprehensive dynamics of ether, electricity, and ponderable matter, which shall include electrostatic force, magnetostatic force, electromagnetism, electrochemistry, and the wave theory of light.

Kelvin had already adverted to this object in 1889, in a paper on ethereal dynamics:

... to give anything like a satisfactory material realisation of Maxwell's electro-magnetic theory of light ... essentially involves the consideration of ponderable matter permeated by, or imbedded in ether, and a *tertium quid* which we may call electricity, a fluid go-between, serving to transmit force between ponderable matter and ether.... I see no way of suggesting properties of matter, of electricity, or of ether, by which all this, or any more than a very slight approach to it, can be done, and I think we must feel at present that the triple alliance, ether, electricity, and ponderable matter is rather a result of our want of knowledge, and of capacity to imagine beyond the limited present horizon of physical science, than a reality of nature.[1]

Some eleven years before those words were written, H. A. Lorentz published a paper on the propagation of light in a material medium, treated on the basis of the electromagnetic theory of light.[2] Although an abridged German version was published in 1880 in a prominent journal,[3] this work appears to have remained largely ignored; Helmholtz, for instance, was evidently unaware of it in 1892, when he published a paper on the electromagnetic theory of dispersion, and it is accorded only passing reference, with inadequate appreciation of its contents, in Whittaker's *History of the Theories of Ether and Electricity.*[4] In fact, Lorentz's paper of 1878, which contains the first development of a theory of dispersion on the basis of the electromagnetic theory, initiated the line of research that eventuated in what we now know as Lorentz's theory of electrons, and it is this theory that provided the framework for all progress on the question of molecular dynamics and the theory of light.

It is of interest, for the understanding of Lorentz's point of view and procedure, and also for the sake of certain general lessons I wish to preach, to go back a little further—to Lorentz's doctoral thesis of 1875, which itself has as background and foundation the critical discussion of electrodynamic theories that Helmholtz had published in 1870. Helmholtz's purpose was to examine the relationships among, and to lay the basis for comparing the merits of, the several electrodynamic theories then in competition, with somewhat special concern for the status of Maxwell's theory. To this end, Helmholtz followed one of the leading ideas of Maxwell: that the ether—presumed to fill otherwise free space (as the carrier of light waves)—was essentially a polarizable dielectric medium. But, unlike Maxwell, Helmholtz treated this medium after the manner of the Poisson-Mossotti theory, according to which polarization consists in a separation of charges resulting from the ordinary action at a distance of external exciting charges. Thus, it may be said that Helmholtz's version of the theory employs an ether whose fundamental constitution is intrinsically electrical rather than elastic.

The Helmholtz electrodynamics contains two parameters—let me call them κ and λ—whose values serve to discriminate a spectrum of possible specific theories. The parameter κ appears in the expression for the electrodynamic interaction at a distance between two current elements; $\kappa = 1$ yields the electrodynamic law of Franz Neumann, $\kappa = -1$ yields the once-celebrated law of Weber, and, according to Helmholtz, $\kappa = 0$ corresponds to the theory of Maxwell. This last statement, however, is not right, as Poincaré seems to have been the first to point out.[5] The parameter λ may be regarded as defining the dielectric coefficient of the ether; more exactly, λ is the ratio of the electrostatic force between two given charges at given distance apart *in an ideal nonpolarizable medium* to the force between the same charges at the same distance in ordinary empty space (i.e., "in the ether"). Thus the value $\lambda = 1$ gives the "pure" action-at-a-distance theories, in which the ether itself is nonpolarizable, whereas any value greater than 1 makes the ether electrically polarizable. The very interesting results of Helmholtz's investigation were the following:

- that any polarizable medium will be a carrier of electromagnetic waves;
- that there will occur in general both longitudinal and transverse waves, propagated with different velocities;
- that the velocity of the transverse waves will depend upon the polarizability of the medium, that of the longitudinal waves upon both the polarizability of the medium and the value of the parameter κ; and
- that, on the other hand, for $\kappa = 0$ the velocity of longitudinal waves will be infinite—hence, there will be no such waves—and, on the other hand (in view of the value found by measurement for the ratio of the electromagnetic unit of charge to the electrostatic unit), in order for the propagation of transverse electromagnetic waves in a given medium to have the velocity of light the polarizability of this medium must be practically infinite.

In the last case (more strictly, in the limit in which the polarizability of the ether goes to infinity) one arrives at Maxwell's theory in full detail: the infinite polarizability of the ether corresponds to Maxwell's hypothesis that "all electric currents are closed" (that is, that the current distribution is solenoidal); and the parameter κ, if one supposes only that it is held bounded during the limiting process, drops out of the theory altogether—once again the longitudinal waves disappear.

It was, then, upon Helmholtz's formulation that Lorentz based his treatment of reflection and refraction (the subject of his thesis) and his treatment of the dependence of the optical properties of bodies on their physical constitution (the subject of his paper of 1878). In doing so, he did not commit himself to the strictly infinite polarizability of the ether, or to the strict nonexistence of longitudinal waves, but only to the very high polarizability of the ether and the very great velocity of longitudinal waves. The thesis concluded with an indication of problems that remained to be investigated and that seemed ripe for investigation from the vantage point already attained; in this connection, Loretz remarked: "If it is true that light and radiant heat are constituted by electric vibrations, it is natural to admit that the molecules of bodies, which give rise to such vibrations in the ambient medium, are likewise the seat of electric oscillations, whose intensity increases with the temperature. This conception, which is not new but which derives from the electromagnetic theory a high degree of probability, seems to me very fruitful."[6] Just that conception is employed in the paper of 1878, in which material dielectric bodies are regarded not (like the ether) as continuous polarizable media, but as molecular in structure, the space between the molecules being pervaded by ether. In the chapter on dispersion,[7] Lorentz finds it necessary to specialize his assumption about the molecules by positing that they contain particles possessing both charge and mass. This is the first characteristic assumption of the theory of electrons. Although introduced very quietly, in the midst of a painstaking piece of work that can be seen as a paradigm of what Thomas Kuhn calls "normal science,"[8] the assumption in its context is momentous. Lorentz, in addition to opening his scientific career with an investigation devoted, in however cautious a fashion, to the theory of Maxwell (whose status among physicists in the 1870s was as insecure as was that of the theory of relativity in the years immediately following 1905), had combined that theory with an element superficially quite foreign to it: the *tertium quid* referred to so unenthusiastically by Kelvin—electricity—not in its old guise as a fluid, but in the form of intrinsic charge of a particle of "ordinary" matter, which charge serves to mediate the interaction of the particle with the ether.

The problem of the nature of electric charge deserves to be regarded as the main crux of Maxwell's theory; thus Poincaré wrote in 1890 in the preface to his published lectures on Maxwell's theory: "It is in electrostatics that my task has been the hardest; it is there above all in fact that precision is wanting [in Maxwell]. One of the French scientists who have most deeply studied the work of Maxwell said to me one day: 'I understand everything in his book except this: what is a charged sphere?'"[9] Lorentz's postulating of intrinsically charged particles can be regarded as a cutting of the Gordian knot. But such cutting is in itself a crude act: remember Kelvin's suggestion that "the triple alliance, ether, electricity, and ponderable matter is rather a result of our want ... of capacity to imagine beyond the limited present horizon of physical science, than a reality of nature."

The postulate of charged massive particles in Lorentz's 1878 paper does, in a sense, lack depth; it serves simply to tie charge and mass together, to provide something that can resonate or fail to resonate with the oscillations of the electric field in a light wave. But in Lorentz's subsequent work a radically new thing comes to pass. Put simply, it is the more far-reaching assumption that the charges of such particles constitute the *sole* mediators of interaction between ordinary "ponderable" matter and the ether. This extended assumption requires, to make it precise, a fuller analysis of the nature of the interaction in question; that is, in the terms that prevailed at the time, an analysis of the mechanical interconnection of an intrinsically charged particle and the ether.[10]

Just such an analysis was provided by Lorentz in his famous memoir of 1892 on the electrodynamics of moving bodies;[11] but what Lorentz gave was a mechanical analysis with a difference. The introductory section of the memoir begins as follows:

> In one of the most beautiful chapters of his *Treatise on Electricity and Magnetism*, Maxwell shows how the principles of mechanics can serve to elucidate the questions of electrodynamics and the theory of induced currents, without the need to penetrate the secret of the mechanism that produces the phenomena. The illustrious scientist limits himself to a small number of hypotheses, which are known to all physicists and of which I may be permitted here to recall the principal ones.

Lorentz then reviews the chief assumptions upon which Maxwell, in part IV, chapter VI of the *Treatise* ("Dynamical Theory of Electromagnetism") had based his own subsumption of his theory under the principles of Lagrangian mechanics; he continues:

> After having posited the principles I have summarized, Maxwell applies the equations of Lagrange; he arrives thus at well-known formulas for the electrodynamic forces and for the induction of currents....
>
> [But t]he equations that determine the motions of electricity in three-dimensional bodies do not follow, in the book of Maxwell, from a direct application of the laws of mechanics; they rest upon the results that have been obtained for linear conductors.

Lorentz next adverts to the work of Heaviside and of Hertz in simplifying the form of Maxwell's field equations, and remarks that Hertz had hardly concerned himself with a mechanical account of processes in the field. He adds:

> Needless to say, this method has its advantages.
>
> Nevertheless, one is always tempted to revert to mechanical explications. That is why it has seemed useful to me to apply directly to the most general case the method of which Maxwell has given the example in his study of linear circuits.

There follows a crucial statement:

> I had yet another motive to undertake these researches. In the memoir in which M. Hertz treats of bodies in motion, he takes the ether they contain to move with them. Now, optical phenomena have long since demonstrated that this is not always so. I therefore wished to know the laws that govern electrical motions in bodies that traverse the ether without its entrainment; and it seemed to me difficult to achieve this aim without having as guide some theoretical idea. The views of Maxwell may serve as foundation for the theory sought.

The exposition of the results here adumbrated occurs in chapter IV of Lorentz's memoir, which begins: "It has seemed to me useful to develop a theory of electromagnetic phenomena based upon the idea of a ponderable matter completely permeable to the ether, and able to move without communicating the slightest motion to the latter." But how, Lorentz asks, is one to form a precise idea of a body that, moving within the ether and thus traversed by it (in effect, by an "ether wind"), is at the same time the seat of an electric current or a dielectric process?

> To overcome the difficulty as far as it was possible for me, I have sought to reduce all the phenomena to a single one, the simplest of all, namely just to the motion of a charged body. It will be seen that, without a deeper analysis of the relation between ponderable matter and the ether, one can establish a system of equations suited for the description of what happens in a system of such bodies.

Lorentz's fundamental hypotheses (presented, with the greatest simplicity and clarity, in section 75 of the memoir[12]) are these:

(i) All charges are carried by particles of "ponderable matter"—i.e., bodies in the ordinary sense, having mass and subject to the application of "ponderomotive" forces. (I shall give these particles the name later adopted for them by Lorentz: *electrons*.) For convenience, it is assumed that the distribution of charge is given by a charge-density function that is, at each instant, a smooth function on space. Further, it is assumed that the individual electrons are rigid, and that the charge density of each one is rigidly associated with the particle.

(ii) Electrons and ether mutually penetrate one another. Ether, in particular, is strictly ubiquitous; not only does it occupy all spaces *between* charges, it occupies all of space *simpliciter*.

(iii) Charge density is connected with the "components of the dielectric displacement in the ether"—that is, with the vector function of electric field intensity—by the divergence equation of Maxwell.

(iv) The "total electric current," in the sense of Maxwell, is the sum of the current of convection (determined by the motions of the electrons) and the "displacement current" (determined by the rate of change of the electric intensity). Lorentz remarks that, in consequence of these two assumptions, the distribution of total current is solenoidal. In comparison with the theory of Helmholtz, this result is now obtained in a quite different (and in a sense more abstract) way: there is no longer any question of the circuits' being completed by complementary motions of charges in the dielectric medium; the ether itself—which has now become, from a fundamental point of view, the only dielectric medium—is not assumed to be a Mossotti-type polarizable body or to contain any electric charges other than those of the "electrons."

(v) The total current is connected with the vector function of magnetic field intensity by the pertinent curl equation of Maxwell (a postulate that is consistent with the foregoing, in view of the solenoidal character of the current). The magnetic field itself is source-free. The electromagnetic energy associated with the total current—given by an energy-density function proportional to the square of the intensity of the magnetic field—is, following Maxwell, to be regarded as *kinetic energy of the ether*.

(vi) The *configuration* of an electromagnetic system (in the sense of generalized dynamics) is determined by the configuration in the ordinary sense of the system of the electrons involved, together with the vector function that defines the dielectric displacement. The potential energy of the ether is distributed with a density proportional to the square of the electric field intensity.

The application of d'Alembert's principle to these hypotheses leads, in the first place, to the remaining set of Maxwell equations: those for the curl of the electric intensity. (An analogous result had been established, so far as the free ether is concerned, by G. F. FitzGerald some thirteen years earlier.[13]) Beyond this, however, Lorentz obtained the new basic result he sought: the formula that gives the force per unit charge in the electromagnetic field, and so completes the account of the role of electricity as intermediator between ponderable matter and the ether. Indeed, the influence of ordinary matter upon the ether is now expressed, through the divergence equation of the electric field and the curl equation of the magnetic field, as determined by the distribution of charge carried by material particles and by the convection of charge as those particles move, and the influence of the ether upon ordinary matter is expressed as the "ponderomotive force" exerted upon the charged particles. A notable circumstance in this result is the asymmetry it introduces in the treatment of the "displacement current" of Maxwell; the latter is on a par with the convection current, so far as its role in influencing the magnetic field is concerned, but it is not involved (as the convection current is) in determining ponderomotive forces. The theory of a polarizable ether makes no such distinction.

I have called Lorentz's account a mechanical analysis "with a difference." By this I meant to refer not just to the fact that the mechanics involved is of the generalized kind that allows one to avoid detailed hypotheses about microstructure (Maxwell himself had invoked such a procedure twenty-eight years before), nor just to the fact that no elastic medium is posited (that is equally the case in the Helmholtz theory to which Lorentz's earlier investigations had attached themselves). The profound difference lies, rather, slightly beneath the surface of Lorentz's characterization of his theory as "based on the idea of a ponderable matter perfectly permeable to the ether and able to move without communicating the least motion to the latter."

In one sense, of course, this phrase explicitly contains a notion that stands in striking contrast with traditional ideas; impenetrability has often been considered one of the essential properties of body, so the notion of two sorts of matter that are literally co-present at the same points of space is rather a radical departure. It is therefore of some interest that such a noted traditional "mechanist" as Kelvin subscribed, toward the end of his life, to a theory of the ether that assumed mutually penetrable forms of matter. It is this theory that constitutes his proposed solution of the problems of his Baltimore Lectures in the 1904 edition.[14] But Kelvin's ether, although it does not interact *by contact* with the particles with which it interpenetrates, does interact with these particles by *distance* forces, so that motion is communicated between the ether and ponderable matter. In Lorentz's theory, no such effect occurs; instead, the ether appears as a system with mechanical attributes; it is the seat of energy (a part of which is formally identified as kinetic), and it is able to exert forces on bodies, yet is itself incapable of motion.

In putting the case so, I am perhaps open to a legitimate objection. Lorentz himself certainly does not present the conception he has introduced as "revolutionary." He characterizes his attempt as one "to surmount the difficulty as far as it was possible for me," and "without a deeper analysis of the relation between ponderable matter and the ether"; he leaves us free to envisage the possibility that such a deeper analysis might yet identify the energy of the magnetic field as genuine energy of motion within the ether and reconcile small-scale motion within the latter with its immobility in the large. On the other hand, it is quite clear that Lorentz was aware of the formidable difficulties in the way of any such classically mechanical theory.[15] What in effect he did deserves comparison with such other cuttings of Gordian knots as that by Einstein in his 1905 quantum paper and that by Bohr in his 1913 papers on the constitution of atoms and molecules: in his quiet way, he had the boldness and the insight to combine those parts of existing physical theory that could lead to definite and interesting results, while simply ignoring—as in the cases of Einstein and Bohr, tentatively and in the spirit of heuristic inquiry—what he saw no way to incorporate consistently. Perhaps the chief difference between Lorentz and the other two in this respect is that in the later period there was in the background far more definite evidence of an out-and-out contradiction between the classical notions and natural phenomena, so that the expectation of an ultimate clarification on strictly classical principles was more clearly excluded. This may be enough to render Lorentz "normal" and the other two "revolutionary," but if one attends to the objective consequences of the respective investigations the distinction appears of limited interest.

Furthermore, in his next large work on the subject Lorentz made his departure from classical principles quite definite:

> Why should we, having once assumed that the ether does not move, ever talk of a force acting upon this medium? It would be simplest to assume that no force ever acts upon a volume element of the ether, considered as a whole; or even to refrain from applying the very concept of force to such an element, which indeed never moves from its place. To be sure, this conception would conflict with the theorem of the equality of action and reaction—since we have grounds for saying that the ether *exerts* force upon ponderable matter; but, so far as I see, nothing compels us to elevate that theorem to a fundamental law valid without restriction.[16]

Perhaps, after all, we should recognize Lorentz as a *quiet* revolutionary.

The consequences that flowed from Lorentz's treatment of the electromagnetic ether problem may be divided into two classes. The first of these surely deserves the epithet "normal": rapid and impressive progress, partly at the hands of Lorentz himself and partly at the hands of others, in the theoretical representation—both "explanation" and "prediction"—of optical, electrical, magnetic, and thermal properties of bodies. One exemplar of this class is the work for which, in 1902, Lorentz shared with Pieter Zeeman the second Nobel Prize in physics: the theory of the so-called "normal Zeeman effect." This theory, which Lorentz furnished immediately upon Zeeman's discovery, allowed both the prediction of the finer structure of the phenomenon (doublet when emission is in the direction of the applied magnetic field, triplet when emission is perpendicular to the field; circular polarization of the emitted light) and the determination of the charge-to-mass ratio of the particles responsible for it; in effect, then, it led to the discovery of the electron, in the current sense of that word.[17]

This "discovery of the electron" by Zeeman and Lorentz was independent of, and essentially contemporaneous with, the corresponding discovery made by J. J. Thomson in his work on cathode rays; together with the latter, it constitutes the beginning of the acquisition of genuine information about subatomic particles. In calling this work an exemplar of the class of "normal" consequences of Lorentz's theory, I had in mind not only that it illustrates the power of that theory in advancing our understanding of natural processes, but also its noteworthy limitation: The "normal" Zeeman effect is in point of fact exceptional. What is called the anomalous effect is far more common, and of this Lorentz's theory proved unable to give a satisfactory account. This case is typical, in that the processes the theory of electrons deals with all lie on or over the borderline that separates the domain of competence of classical physics from the domain in which recourse to the quantum theory is essential; thus, the "normal science" involved tends to arrive rapidly at "anomalies" in the sense of Kuhn, and may in this respect be called pre-revolutionary. If the terminology appears frivolous—and I have already indicated my reservations about any such sharp distinction as Kuhn has suggested—there is nevertheless a quite serious point involved: The power of a scientific theory does not consist solely in its success in explanation and in prediction; the *failures* of a theory can be of the greatest importance when they serve to bring deep problems into sharp focus.[18]

The second class of consequences I have mentioned is related to the genuine Lorentzian revolution. That is to say, these consequences involve directly the new fundamental conception Lorentz had introduced: that of the immobile ether that nevertheless has attributes of a mechanical system interacting with charged particles. Here the most obvious development is the one Lorentz is best remembered for in the folk tradition: his reconciliation of the postulate of immobility with the failure of all attempts to detect the motion of ordinary bodies—in particular, the earth—relative to the ether. As is well known, this achievement was attained rather painfully, step by step, starting from the introduction in 1892 of the hypothesis of a contraction of bodies in the direction of their motion relative to the ether.[19] The near-final stage was reached by Lorentz in 1904; the definitive formulation was given by Poincaré in 1905–06 and—with a difference—by Einstein in 1905.[20] But parallel to that development, and not altogether separate from it, was another, whose result was to consolidate into the conceptual frame of physics the notion of the ether as what I shall now call a *non-Newtonian mechanical system.* In this connection, let me mention especially the introduction into the theory of the notion of *electromagnetic* (linear and angular) *momentum,* first as a kind of *jeu d'esprit* by Poincaré in 1900[21] and then, on the basis of Poincaré's suggestion (but now treated as having serious and far-reaching import), by Max Abraham in 1903.[22] This move, which served to rescue the conservation principles of momentum and angular momentum despite the absence of any "force of reaction" to the Lorentz force on the electrons, constituted the first extension of a specific concept of Newtonian mechanics to a domain strictly outside its original home; one now spoke of momentum with no necessary reference to any mass in motion.

I fear I may have dwelt too long upon historical details that are by now fairly familiar; but I wished to present them in a particular form. In the remainder of this essay I shall depart from that detailed historical mode in order to make some more general comments on the matters I have spoken of and their sequel. First I want to say how I construe the difference that (as everyone now appears to agree) subsists between the Lorentz-Poincare theory and that of Einstein.

In the perfected from given it by Poincaré, Lorentz's theory may be described as follows:

(1) All the postulates of the theory of electrons, as formulated in the memoir of 1892, remain in effect when (as in that memoir) the coordinate system is taken

as one with respect to which the ether is at rest—except that the assumption of the rigidity of the electron is replaced by the assumption of a distribution of stresses that just equilibrate the mutual repulsions of its charge elements.

(2) If one introduces, for a system of reference in uniform translatory motion through the ether, an array of auxiliary spatial and temporal coordinates and of electrodynamic variables related to those for the system at rest by the set of transformations given by Poincaré, then exactly the same postulates hold in the moving system. This result is a purely mathematical theorem; it is what Hermann Weyl, in his splendid exposition of the theory of relativity, calls Lorentz's Theorem of Relativity.[23]

(3) If (as Lorentz does) one postulates further that, in Poincaré's words, "all forces, of whatever origin, are affected by a translation in the same manner as the electromagnetic forces,"[24] then the spatio-temporal, electromagnetic, and dynamical quantities introduced formally under (2) will be just the ones determined in empirical measurement by an observer in a state of uniform translatory motion.

The physical content of the theory, therefore, is determined by the postulates I have mentioned in (1) and (3). The theorem cited in (2), with the further remark in (3), yields the conclusion that uniform motion with respect to the ether is in principle undetectable by any physical means.

Now, in comparing this theory with that of Einstein, I want to say emphatically that the physical contents of the two—as I understand the notion of "physical content"—are identical. The distinction between them resides, as I see it, not at all in their physical content but in two aspects of the point of view with which they are presented by their respective authors—aspects that, since they do not belong to the *objective content* of physics and yet have important bearings upon our *understanding* of the physics, may appropriately be called "meta-physical."

The first difference lies simply in the degree of conviction with which the theories are presented. In Lorentz's case, one finds an entirely characteristic expression of caution: "It need hardly be said that the present theory is put forward with all due reserve. Though it seems to me that it can account for all well established facts, it leads to some consequences that cannot as yet be put to the test of experiment. One of these is that the result of Michelson's experiment must remain negative, if the interfering rays of light are made to travel through some ponderable transparent body."[25] Lorentz made analogous statements on many subsequent occasions; what they come to is that he was not entirely convinced that phenomena would never be found that are sensitive to uniform translatory motion through the ether, and he therefore considered the theory *tentative*.

Poincaré's case is rather different. For some ten years he had been setting forth, with increasing emphasis, the conviction that motion relative to the ether was a strictly fictitious concept, and that a really satisfactory physical theory should reveal this in all rigor.[26] He could not, therefore, have shared Lorentz's doubts about *this* aspect of the theory. His misgiving, rather, was that the desired objective had been obtained by artificially complicated means: "We cannot be contented with formulas that are simply juxtaposed, and which agree only by a happy chance; we require these formulas to come as it were to penetrate one another mutually. The mind will not be satisfied until it believes itself to perceive the reason for this agreement, to the point of having the illusion that it could have foreseen the same."[27] Moreover, he suspected that this artificial complication was connected with an inappropriate relation of the concepts of the theory to methods of measurement: "In this theory, two equal lengths are, by definition, two lengths that light takes the same time to traverse. Perhaps it will suffice to abandon this definition, for the theory of Lorentz to be overthrown as completely as was the system of Ptolemy by the intervention of Copernicus."[28] It was, therefore, with an expression of considerable diffidence that Poincaré introduced his paper; the passage I have just quoted continues: "If this occurs one day, that will not prove that the effort made by Lorentz was useless; for Ptolemy, whatever one thinks of him, was not useless to Copernicus. Therefore I have not hesitated to publish these partial results, even though at this very moment the whole theory may seem placed in jeopardy by the discovery of the magneto-cathodic rays."[29]

Einstein, it need hardly be said, had no such misgivings; he possessed in full the "illusion" desiderated by Poincaré, and it is a famous fact that the conviction thereby engendered was strong enough to sustain his confidence through a period in which serious experimental counterevidence—nothing so vague as the mysterious "magneto-cathodic rays" mentioned by Poincaré—seemed to have refuted his theory.

The second difference I have in mind is suggested by the contrast between Poincaré's reasons for doubt and Einstein's for conviction. It has, of course, to do with what has often been called the *interpretation* put upon the theory. The word is apt enough; but I think it demands some elucidation, and with this aim I want to compare the case at hand with a famous hypothetical example offered, with a philosophical purpose, by Poincaré.

In chapter IV of *Science and Hypothesis*, Poincaré describes a fictitious world, concerning which he makes the claim that beings essentially like us who are native to it would be led to describe the "space" in which they live as non-Euclidean; but that we ourselves, should we find ourselves there someday, would continue to use Euclidean goemetry in our representation of its physical phenomena. The example is based upon the conformal mapping—due to Poincaré—of Bolyai-Lobachevsky space onto the interior of a Euclidean sphere; Poincaré in effect just supposes that solid bodies move so as to remain congruent with themselves in the non-Euclidean metric and that light is propagated with constant "non-Euclidean velocity" along non-Euclidean "straight lines," and he argues that this very description of the hypothetical world shows that we, if we entered it, would be able to describe it in Euclidean language.

Now, there is an interesting oversight in Poincaré's account. His "world," by his own assumption, can be described by a physics that admits the Bolyai-Lobachevsky congruence group as a group of symmetries. The Euclidean description he invokes must postulate systematic deformations of physical bodies and a systematic variation, over space, of the velocity of light. Reichenbach had Poincaré's discussion in mind when he tried to formulate a principle of "ruling out universal forces," but this, I think, was a misstep, although a sound motive can be seen behind it. The point is that the deformations required for the Euclidean description break the symmetry of the world. But again, this can be misconstrued. What is important is not the "esthetic" loss from breach of symmetry; it is, rather, a serious implication which that breach has for the practice of physics. Because the Euclidean description breaks the symmetry, it requires that one postulate a difference in physical character among the points or regions of the space itself. In Poincaré's model, this difference is expressed in terms of the distance from the center of the (Euclidean) sphere. But because the physics is symmetric, this immediately implies that the Euclidean description is far from unique; any point could have been chosen as "center," and so there are infinitely many ways to choose a Poincaré-style Euclidean description. (The case is in fact still worse. Poincaré's own alternative conformal model for Lobachevskian geometry, using a half-space instead of the interior of a sphere, would entail another form of the law of deformation of bodies and velocity of light. This model *has* no center. And if one renounces conformality of the mapping, the possibilities grow outlandishly.)

How, then, is our "Euclidean" physicist to proceed? He may begin by choosing the Poincaré representation and declaring some place he has reason to prefer to be "the center"; but then he will be forced to keep track of his spatial relation to that center (which may be hard to do), wherever he happens to wend his way in that world, and to use that relation constantly to correct the account of his experiences—and his experiments—for the postulated distortions (which will be onerous in the extreme). Our physicist will also be in an awkward position in relation to his sister and brother Euclideans, who agree with him in their ideology but have elected different realizations thereof. In the end, surely, this would be an intolerable state of affairs; Poincaré surely ought to regard the theory as "Ptolemaic," and as based upon an inappropriate relation of concepts to methods of measurement.

The parallel to the case of Poincaré versus Einstein is exact. The place of the non-Euclidean symmetry is taken by the "Lorentz Theorem of Relativity"—proved, in strictness, not by Lorentz but by Poincaré. The place of the artificially elected center of the world is taken, not (of course) by a point, but by a (space-time) direction—a state of motion: the postulated state of motion of the immobile ether.

The most far-reaching consequence of the acceptance of Einstein's point of view is its consequence for physical investigation—its "heuristic" significance, as one says. Having recognized Lorentz symmetry as (to use Poincaré's words once more) not "a happy chance," but something for which there is a fundamental "reason," we shall expect all physical laws yet to be discovered to conform to this symmetry until reasons to the contrary are found. Notice that this heuristic principle is already implied by the Lorentz-Poincaré theory, if it is taken in all strictness; the difference is that Lorentz and Poincaré were free not to take their theory strictly—to leave open the door (as Lorentz deliberately chose to do) for the eventuality that motion relative to the ether might yet be made manifest. Einstein's form of the theory definitely closed that door.

There is an issue of methodology here. In general, leaving doors open when possible ought certainly not be regarded as a vice. For Lorentz, that was his procedure of choice on all occasions and at all stages of his career. In this, he stands in rather instructive contrast to Kelvin, although a superficial view might regard both men simply as "scientific conservatives." We have seen the cautious young Lorentz choosing to base his first investigations into the electromagnetic theory of light, not on the more decisive formulation of its creator, Maxwell, but on the more "open" formulation of Helmholtz (a formulation that has been attacked as "rather heavy-handed" and as "spoiling the subtle harmony" of Maxwell's conceptions[30]). Clearly, his caution did not prevent Lorentz from making fundamental contributions to Maxwell's theory and fundamental advances beyond it. What is still more striking is that as late as 1903, when Lorentz wrote the article on Maxwell's theory for the *Enzyklopädie der mathematischen Wissenschaften*, he included in it a brief discussion of the distance-action theory of Helmholtz and its relation to the "field-action" theory.[31] This ended, to be sure, with the comment that "no one will dispute that the merit of simplicity and of greater perspicuity [*Anschaulichkeit*] and intelligibility [*Verständlichkeit*] in physical regard lies with Maxwell's theory,"[32] but it also contained the statement that "should the need ever appear of dropping the law of the solenodial distribution of current, further developments could attach themselves to the Helmholtz theory with a finite value of ε_0 [i.e., of the constant I called λ above]."[33]

The point I want to make about this is that Lorentz's conservatism *was* just caution. He did not commit himself obstinately to "received" views, any more than he rushed to embrace "revolutionary" views (even his own). It is sometimes urged that such caution is a drawback to the energetic pursuit of a program. So it can be; and so can the deep commitment to a program be a drawback to the open-minded entertainment of fruitful alternative suggestions. The last is what Kelvin surely lacked. He was willing to consider the most radical conceptions, and the most startling combinations of structure (mutually penetrating kinds of matter; an ether engaged in fundamental action at a distance, a "rotationally elastic" ether, a "quasi-labile" ether with negative compressibility, an ether that was in part quasi-labile and in part nearly incompressible[34]), so long as these fell within the scope of classical mechanical constructions. He was also quite capable, as his pathbreaking work in thermodynamics shows, of working fruitfully outside that mechanical framework. However, for a certain species of theory—one that involved fundamentally dynamical conceptions but that transcended, or even just tentatively abstracted from, the strict Newtonian frame—his mind seems to have been simply closed; he did not *pay attention* to the successes of such theories.

That one cannot say of Lorentz. The literature contains some really grotesque statements about his attitude toward Einstein's theory, and even about his lack of capacity to understand that theory. But Lorentz's remarks on the subject in his *Theory of Electrons*[35] ought to be enough to convince anyone that in 1909 Lorentz both understood and fully appreciated Einstein's work, although he maintained his characteristic attitude of caution (judiciously moderated, although still not abandoned, in the notes he added for the second edition of 1915). And if those remarks about the special theory of relativity do not suffice, the case is put beyond the cavil of a doubt by the circumstance that Lorentz made significant contributions to the *general* theory of relativity—and did so in the period from his sixty-first to his sixty-ninth year.[36]

I rehearse these facts not merely in the interest of justice to Lorentz (although I do believe that justice should be done, to the best of one's ability, in historical commentary) but for the sake of a larger point about the dialectic of scientific investigation. It seems to me that too many clichés, and too many oversimplifications, are propagated both in the historical and in the historically oriented philosophical literature about science—clichés and oversimplifications about programmatic divisions, about national divisions, about generation gaps. All these divisions (and others as well) are really present, and have their influence on research and on the combat of theoretical views; they are an inescapable part of the human comedy; but exactly *what* influence they have is a subtler question than it is often taken for.

To add a dimension to the evidence I have already cited, let me make a brief comment on the issue of national differences. One encounters over and over again the statement that Maxwell's views were long neglected on the Continent, and especially in Germany. This is undoubtedly true to a certain, not insignificant, extent. But there are *local* nuances. Berlin was far more sympathetic than was Göttingen; in fact the first significant experimental testing of, and evidence for, the theory of Maxwell came from Helmholtz's laboratory in Berlin (and from Boltzmann in Vienna). Again, the Scotsman Kelvin—a compatriot and friend of Maxwell—was cool to his theory and rather uncomprehending of its content. This is explained in terms of the famous British penchant for "mechanical models" (a penchant, however, shared in superlative degree by the Austrian Boltzmann, who was one of the earliest and most enthusiastic of Maxwell's adherents). And finally we have our Dutchman Lorentz,

who pursued the theory of the Scot Maxwell, initially under the banner of the German Helmholtz; who developed his own theory of the electrodynamics of moving bodies with important stimulation and constructive criticism from the Frenchman Poincaré; and whose work found one of its culminations in what during the time of infamy was called "Jewish physics."

On the subject of these clichés, I cannot omit to mention one particular tidbit: In 1906, Planck delivered an address to the Congress of German Scientists and Physicians on Kaufmann's measurements of the deflection of β-rays by electric and magnetic fields and the significance of those measurements for the dynamics of the electron.[37] Planck's conclusion was that Kaufmann's results did not suffice to decide between the Lorentz-Einstein theory of the deformable electron and the theory of Max Abraham, which held the electron to be a rigid sphere. Abraham's theory made it possible to suppose that the electron mass was exclusively electrodynamic in origin, whereas Lorentz's theory did not; the former was accordingly preferred by the adherents of the "electromagnetic world-view." In the discussion following Planck's talk, Arnold Sommerfeld said: "On the question of principle formulated by Herr Planck I should like to express the conjecture that the gentlemen under forty years old will prefer the electrodynamic postulate [i.e., the rigid electron of Abraham] and those over forty the mechanical-relativistic postulate. I give the preference to the electrodynamic."[38] The report of the proceedings indicates amusement in the audience; Sommerfeld was thirty-eight at the time. In the previous decade Sommerfeld had worked on mechanical models of the ether; in the next decade (when, it must be conceded, he was past forty) his application of the special theory of relativity to Bohr's atomic model led to the explication of the fine structure of spectral lines—a success that served to establish the theory of relativity as one of the most firmly grounded parts of physics.[39]

Besides suggesting that the sociological contemplation of science might profit from caution in respect of large conclusions (and from a more relaxed enjoyment of the human comedy), the example of Sommerfeld, when set beside that of Lorentz, illustrates another important fact about the role in science of "commitment" to theories or to programs. I have argued before, from Lorentz (and Helmholtz provides another notable instance here), that a principled attitude of caution is no necessary bar to the pursuit of fruitful work. The case of Sommerfeld illustrates the complementary point: that enthusiastic commitment to a program need not be an insuperable bar to the serious entertainment of alternatives; for such commitment can be revised.

I have called the difference in point of view between Lorentz and Einstein "meta-physical." I should like now to say something about a matter that may deserve that epithet without the hyphen: the question whether the ether has been eliminated from physics. A few years ago, Larry Laudan, citing a statement by Hilary Putnam to the effect that the ether does not exist (or, in current jargon, that the term 'ether' occurring in nineteenth-century physical theories does not genuinely "refer"), used this in an argument against the position, then held by Putnam, that "terms in a mature science typically *refer*,"[40] and more generally against a certain version of "theoretical realism." In the same connection, Putnam had used the term 'atom' as an example of one that did "refer." If we replace 'atom' here by 'molecule', we have the two elements of Kelvin's subject in Baltimore: the dynamical relations of molecules and the ether.

I am not going to take sides on the issue of realism, which I do not consider to be well posed; however, I want to quote Lorentz on the question of the ether, from an article he wrote on Maxwell's theory and the theory of electrons for the volume on physics in the series *Die Kultur der Gegenwart*, published in 1914:

> In conclusion I should like just to say something about the significance of the ether for theoretical physics. Although, as the foregoing exposition shows, the role of this medium has continually gained in significance, on the other hand the attempts to penetrate further into its nature have fallen increasingly into the background. Since the development of the theory of relativity many physicists have indeed gone so far as to speak no longer of an ether at all, but just of the electromagnetic field propagating itself in space. The question, to what extent this is expedient, must here be left undecided. In part it reduces itself to a verbal question; if one does not want to say that all forces are transmitted *through the ether*, one will nonetheless have to explain, according to the theory of relativity, that they are all propagated in space with the speed of light.[41]

The "explanation" Lorentz calls for here, applied specifically to the force of gravity, led in the year or two following his remark to the definitive formulation of the general theory of relativity. In the physics of our own time, the problem of the transmission of force has received one partial solution in that theory of Einstein and another partial solution in the special-relativistic quantum theory of fields and their associated particles. How to resolve the still outstanding difficulties of the latter, and beyond this how to find a framework in which both of these partial solutions have their place, is certainly the greatest issue facing theoretical physics today. It seems to me entirely appropriate to say, with Lorentz, that whether or not one calls this "the problem of the ether" is a merely verbal question.

Something else ought not to be lost sight of, however. In this transformation of the ether problem, the presuppositions of Kelvin have undeniably been left behind; it would be absurd to raise the question of whether the ether, as anything like an elastic medium, "really exists." But it is equally true, although often ignored, that the old notion of "space," that empty and quiescent container within which bodies exist and forces are propagated, has also been left behind. First, with special relativity, we were led to space-time as the frame whose structure constrained the form of all interactions; then, with general relativity, we were led to the view that space-time is not a quiescent container but is itself interactive; finally, with quantum electrodynamics, we have been led to the view that even "empty" regions of space-time are seething with—I almost said "physical activity," but I suppose it would be more correct to say physical possibilities that have to be reckoned with. It is therefore, in an important sense, more accurate to say that space and time themselves have been assimilated to the ether than that the ether has been eliminated from our view of the world.

And what kinds of properties does this ether have? The answer is that it has the properties of a dynamical system—in the generalized and transformed sense that term itself has acquired in general relativity (where the curvature tensor and the mass-energy tensor play the central role) and in the quantum theory (where Lagrangian densities and Hamiltonian operators are the recognizable descendants of the concepts which Lorentz was one of the first to generalize to the electromagnetic field).

As to the other pole of Kelvin's pair, the atoms or molecules, it is at least arguable that their status has undergone a quite parallel transformation, and that the sense in which they have been retained in our own physics is quite analogous: Some of what had seemed their most fundamental properties have fallen away, but their recognizable conceptual descendants have continued to play a basic role in our theories.

Finally, a word as to the character of this "recognizable conceptual descent": What is in fact "recognizable" is a distinct relationship, from older to newer theory, of *mathematical forms*—not a resemblance of "entities." This has always seemed to me the most striking and important fact about the affiliations of scientific theories. I do not suggest a philosophical "explanation" of this fact; I cite it, on merely historical evidence, just as a fact. But I think that, in its turn, this fact helps to "explain" why such a "conservative" as Lorentz, who was willing to borrow the mathematical structures suggested by older theories and to explore their application in contexts where the presumed "substrates" of those structures were lacking (Should one call this "realism," or should one call it a purely "instrumentalist" use of theory?), was able so greatly to advance our understanding of the world.

Notes

1. W. Thomson, "Motion of a Viscous Liquid; Equilibrium or Motion of an Elastic Solid; Equilibrium or Motion of an Ideal Substance Called for Brevity Ether; Mechanical Representation of Magnetic Force," in *Mathematical and Physical Papers*, vol. III (London: C. J. Clay and Sons, Cambridge University Press Warehouse, 1890), pp. 436–465, at 465.

2. H. A. Lorentz, *Collected Papers*, vol. II (The Hague: Martinus Nijhoff, 1936), pp. 1–119: "Concerning the Relation Between the Velocity of Propagation of Light and the Density and Composition of Media." (This is an English translation of the Dutch original.)

3. "Über die Beziehung zwischen der Fortpflanzung des Lichtes und der Körperdichte," *Wiedemanns Annalen der Physik und Chemie* 9 (1880): 641–665.

4. See H. Stein, "'Subtler Forms of Matter' in the Period Following Maxwell," in *Conceptions of Ether*, ed. G. N. Cantor and M. J. S. Hodge (Cambridge University Press, 1981), pp. 325–326.

5. H. Poincaré, *Electricité et optique*, vol. II, *Les théories de Helmholtz et les expériences de Hertz* (Paris: George Carré, 1891), pp. 50, 103, 111. In the single-volume second edition (Paris: Gauthier-Villars, 1901), the corresponding pages are 275, 329, and 337–338.

6. Lorentz, *Collected Papers*, vol. I, p. 383.

7. Ibid., vol. II, pp. 70–87; see especially pp. 79 ff.

8. Indeed, the work of Lorentz *tout ensemble* is so characterized by Kuhn himself. He remarks, in commenting on the views of Popper, that the latter's emphasis upon the "occasional revolutionary parts" of the scientific enterprise is "natural and common": that "the exploits of a Copernicus or Einstein make better reading than those of a Brahe or Lorentz." See Thomas Kuhn, "Logic of Discovery or Psychology of Research," in *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave (Cambridge University Press, 1970), p. 6; also in Kuhn, *The Essential Tension* (University of Chicago Press, 1977), p. 272.

9. Poincaré, *Electricité et optique*, vol. I (Paris, 1890), pp. xvi–xvii.

10. In the 1878 paper, Lorentz dispensed with such a fuller account and simply postulated that the particle is subject to a force equal to the product of its charge and the intensity of the electric field at its location—or, more strictly, to the volume integral of the product of charge density and field intensity. There was no issue of motion through the ether, because he located the charged particles at the centers of cavities in the ether and considered only oscillations small enough so that each particle stayed inside its cavity. (In the theory of dispersion, contained in chapter III of the work, no magnetic forces were taken into account.)

11. "La théorie électromagnétique de Maxwell et son application aux corps mouvants," in Lorentz, *Collected Papers*, vol. II, pp. 164–343.

12. Ibid., pp. 230–233. (In stating Lorentz's assumptions, I have deviated slightly from the arrangement he gives to them.)

13. G. F. FitzGerald, "On the Electromagnetic Theory of the Reflection and Refraction of Light," *Philosophical Transactions of the Royal Society*, 1880; reprinted in *The Scientific Writings of the Late George Francis FitzGerald*, ed. J. Larmor (Dublin and London, 1902). See pp. 45–49 and 41–42. See also Stein, "'Subtler Forms of Matter'" (note 4 above), pp. 312–315.

14. The assumptions of this theory are stated in Lecture 19 (rewritten 1903) of Kelvin's *Baltimore Lectures* (Cambridge University Press, 1904). See also what appears to be Kelvin's last published paper, "On the Motions of Ether Produced by Collisions of Atoms and Molecules, Containing or not Containing Electrons," in *Mathematical and Physical Papers by the Right Honorable Sir William Thomson, Baron Kelvin*, vol. VI, ed. J. Larmor (Cambridge University Press, 1911), pp. 235–243, in particular p. 237, §8.

15. The following remark is from a paper of some eight years later (1903): "The physicists who have endeavoured, by means of certain hypotheses on the mechanism of electromagnetic phenomena, to deduce the fundamental equations from the principles of dynamics, have encountered considerable difficulties, and it is best, perhaps, to leave this course, and to adopt the equations (I)–(VII) [that is, the Maxwell-Lorentz equations]—or others, equivalent to them—as the simplest expression we may find for the laws of electromagnetism." ("Contributions to the Theory of Electrons," in *Collected Papers*, vol. III, p. 136.)

16. Lorentz, *Versuch einer Theorie der elektrischen und optischen Erscheinungen in bewegten Körpern* (Leiden: E. J. Brill, 1895); reprinted in *Collected Papers*, vol. V (The Hague: Martinus Nijhoff, 1937). See, in the latter, p. 28.

17. See P. Zeeman, "On the Influence of Magnetism on the Nature of the Light emitted by a Substance," *Philosophical Magazine*, 5th series, (January–June 1897): 226–236. Note in particular the following (pp. 230 ff.):

> §15. The train of reasoning ... by which I was induced to search after an influence of magnetism, was at first the following:—If the hypothesis is true that in a magnetic field a rotatory motion of the aether is going on, the axis of rotation being in the direction of the magnetic force (Kelvin and Maxwell), and if the radiation of light may be imagined as caused by the motion of the atoms, relative to the centre of mass of the molecule, revolving in all kinds of orbits, suppose for simplicity circles; then the period ... will be determined by the forces acting between the atoms, and then deviations of the period to both sides will occur through the influence of the perturbing forces between aether and atoms.... The deviation will be the greater the nearer the plane of the circle approximates to a position perpendicular to the line of force.
>
> §16.... [T]he above-mentioned considerations are at most of ... value as indications of somewhat analogous cases....
>
> §17. A real explanation of the magnetic change of the period seemed to me to follow from Prof. Lorentz's theory.
>
> ... Prof. Lorentz, to whom I communicated these considerations, at once kindly informed me of the manner in which, according to his theory, the motion of an ion in a magnetic field is to be calculated, and pointed out to me that, if the explanation following from his theory be true, the edges of the lines of the spectrum ought to be circularly polarized. The amount of widening might then be used to determine the ratio between charge and mass, to be attributed to a particle giving out the vibrations of light.
>
> The above-mentioned extremely remarkable conclusion of Prof. Lorentz relating to the state of polarization in the magnetically widened lines I have found to be fully confirmed by experiment (§20).
>
> . . . .
>
> §23. The experiments 20 to 22 may be regarded as a proof that the light-vibrations are caused by the motion of ions, as introduced by Prof. Lorentz in his theory of electricity. From the measured widening (§14) ... the ratio e/m may now be deduced. It thus appears that e/m is of the order of magnitude of 10^7 electromagnetic C.G.S. units.

18. Maxwell remarked that "the scientific or science producing value" of a piece of work is to be measured by its effect in stimulating investigation. See *The Scientific Papers of James Clerk Maxwell*, ed. W. D. Niven (Cambridge University Press, 1890; New York: Dover, 1965), vol. II, p. 486. The passage occurs in Maxwell's article "Attraction," written for the ninth edition of the *Encyclopaedia Britannica*. Lorentz's own far-seeing comment on the unresolved problems connected with Zeeman's phenomenon, from his Nobel Lecture of 1902 (*Collected Papers*, vol. VII [1934], p. 84) is worth quoting:

> I am convinced that the theory will not make significant advances, until it directs attention not merely upon a single spectral line, but upon the totality of all lines of a chemical element.

If we once succeed in understanding theoretically the constitution of the spectra, then, and not sooner, one will be able to tackle the more complicated forms of the Zeeman phenomenon with success. Or to express my thought better: in the future the investigations concerning the regularities in the spectra and those concerning the Zeeman effect will have to proceed hand-in-hand; so they will be able someday to lead to a theory of the emission of light, to attain which is one of the finest goals of contemporary physics.

19. This hypothesis is the only aspect of Lorentz's work to which I have found a reference in Kelvin: he cites it, from Lorentz's monograph of 1895, in two papers of 1900, reprinted as Appendices A and B in *Baltimore Lectures* (1904). (The second of these is the celebrated article on the "two clouds" over the dynamical theory of heat and light.) The two passages, verbally identical (see *Baltimore Lectures* [1904], pp. 485 and 492), refer to "a brilliant suggestion made independently by FitzGerald and by Lorentz of Leyden" that may serve to reconcile with the Michelson-Morley experiment the hypothesis of the "free motion of ether through space occupied by the earth." No mention is made by Kelvin of the general theoretical context into which Lorentz incorporated this suggestion.

20. The chief stages were the following:

- the introduction, in the *Versuch* of 1895 (see note 16 above), of spatial coordinate transformations (§23), of associated transformations of the electric and magnetic field vectors and the charge-density (§§20, 23), and of a "local" time coordinate (§30), serving to simplify the electrodynamic equations when these are referred to a system moving relative to the ether; the use of the resulting formulation to show the undetectability of the earth's motion by optical means, in so far as the square of the ratio of the earth's velocity to that of light can be regarded as negligibly small; and the indication (§§90–92) of the way in which the contraction hypothesis, which extends the nondetectability to the case of the Michelson-Morley experiment, can be plausibly incorporated into the theory
- a modification (or, rather, two successive modifications) of the previous transformations, in a paper of 1899 ("Théorie simplifiée des phénomènes électriques et optiques dans des corps en mouvement," in Lorentz, *Collected Papers*, vol. V, pp. 139–155; see §§4 and 10); the most important new feature, introduced toward the end, is the incorporation into the "local time" of a factor that represents, in effect, the "time dilatation," which here makes its first appearance
- the incorporation, in a 1904 paper ("Electromagnetic Phenomena in a System Moving with any Velocity Smaller than that of Light," in Lorentz, *Collected Papers*, vol. V, pp. 172–197), of the second set of transformations of 1899—with one change respecting the components representing (or, rather, formally replacing) the velocity of an electron in the equations referred to a moving system—into a theory showing the close correspondence of all electromagnetic processes in such a moving system with those in a system at rest
- the perfection of the theory by Poincaré, who achieved the definitive form of the transformations (in particular, of those for charge-density and velocity) and exhibited the strict symmetry that then results between the representation of processes in systems at rest and those in uniform motion (H. Poincaré, "Sur la dynamique de l'électron," in *Oeuvres de Henri Poincaré*, vol, IX [Paris: Gauthier-Villars, 1954], pp. 494–550; see pp. 499–503).

The paper of Poincaré was published in 1906, but an abstract containing the equations of transformation appeared the year before (ibid., pp. 489–493). Einstein's contribution, of course, is to be found in his famous paper "Zur Elektrodynamik bewegter Körper," *Annalen der Physik* 17 (1905): 891–921.

21. Poincaré, "La théorie de Lorentz et le principle de réaction," *Oeuvres*, vol. IX, pp. 464–488.

22. M. Abraham, "Prinzipien der Dynamik des Elektrons," *Annalen der Physik*, 4th series, 10 (1903): 105–179; see p. 110.

23. Weyl, *Space—Time—Matter*, tr. H. L. Brose (New York: Dover, 1950), pp. 160–166. I have praised Weyl's account of the theory; unfortunately, this translation is badly defective, and the reader interested in reading his exposition must be referred to the German original, preferably its fifth or sixth edition: *Raum—Zeit—Materie: Vorlesungen über allgemeine Relativitätstheorie*, sixth edition (Berlin: Springer-Verlag, 1970).

24. Poincaré, *Oeuvres*, vol. IX, p. 491.

25. Lorentz, *Collected Papers*, vol. V, p. 190.

26. See, e.g., Poincaré, *Oeuvres*, vol. IX, pp. 381–382, 412–413; and *Rapports du Congrès de Physique de 1900*, vol. I, pp. 22–23.

27. Poincaré, *Oeuvres*, vol. IX, p. 497.

28. Ibid., p. 498.

29. The phenomenon referred to by Poincaré was first described by André Broca (*Comptes Rendus, Paris* 126 (1898): 736–738, 823–826), who reported that in a sufficiently intense magnetic field there are produced two distinct sorts of cathode rays: those "of the first kind," which wind about the lines of magnetic force, and those "of the second kind," which follow the field lines. The subject was taken up again in 1904 by Paul Villard (*ibid.* 138 (1904): 1408–1411), who concluded that the rays "of the second kind," to which he gave the name "magneto-cathodic rays," were *uncharged* (p. 1410: "Les rayons magnéto-cathodiques ne sont pas électrisés.... Leur charge, si elle existe, est... incomparablement moindre que celles des premiers. Il est fort probable que cette charge est nulle et que ces rayons sont autre chose qu'électrisés.") and that their properties were "inverse to those of the rays of Hittorf" (i.e., the ordinary cathode rays): "Le champ électrique agit sur les premiers comme le champ magnétique sur les seconds, et reciproquement." It is clearly to Villard's account, with its suggestion that the phenomenon represents a fundamentally new kind of process, that Poincaré refers. The same volume of the *Comptes Rendus*, however, contains a discussion by Charles Fortin in which it is pointed out that Villard's observations are consistent with the behavior to be expected of cathode rays of the ordinary kind if these wind about the magnetic lines of force in helices of very small radius.

In the *Comptes Rendus* for the years 1909 and 1911 (vols. 148 and 152), discussion of the magneto-cathodic rays was continued by G.-L. Gouy, who, finding bright and dark fringes at positions for which the lengths of the rays were integral multiples of a certain length (itself inversely proportional to the intensity of the magnetic field), suggested that an appropriate modification of the Newtonian theory of periodic "fits" might have to be invoked for the rays. I find no subsequent reference to the phenomenon in the *Comptes Rendus* through 1914; the whole matter has by now lapsed into such obscurity that in the English translation of Poincaré's paper by H. M. Schwartz, which appeared in the *American Journal of Physics*, (39 [1971]: 1287; 40 (1972): 862), Poincaré is made to say that the whole theory is put in jeopardy by the discovery of cathode rays! (These had of course been discovered several decades before, and they fit very well indeed into the structure of Lorentz's theory; but clearly the translator did not know what to make of the "rayons magnéto-cathodiques.")

30. See L. Rosenfeld, "The Velocity of Light and the Evolution of Electrodynamics," *Nuovo Cimento*, supplement to vol. IV, series X, no. 5 (September 1956): 1664.

31. *Enzyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen*, vol. V, part 2 (Leipzig: B. G. Teubner, 1904–1922), article 13: "Maxwells elektromagnetische Theorie," by H. A. Lorentz, §§44–45.

32. Ibid., p. 144.

33. Ibid., p. 143.

34. See the references given in note 13 above; also Stein, "'Subtler Forms of Matter,'" pp. 320–321 and 329–330.

35. H. A. Lorentz, *The Theory of Electrons and Its Applications to the Phenomena of Light and Radiant Heat* (reprint of second edition, 1915; New York: Dover, 1952); see preface and pp. 223–230; also (added in second edition) notes 72*–74, 75*–76 (pp. 321–327, 328–334). In particular, compare with the passage earlier cited from Poincaré about the chance agreement of formulas the following remarks of Lorentz's.

- on the last page of the main text of 1909: that Einstein "may certainly take credit for making us see in the negative result of experiments like those of Michelson, Rayleigh and Brace, not a fortuitous compensation of opposing effects, but the manifestation of a general and fundamental principle" (p. 230)
- in a note added in 1915: "If I had to write the last chapter now, I should certainly have given a more prominent place to Einstein's theory of relativity ... by which the theory of electromagnetic phenomena in moving systems gains a simplicity that I had not been able to attain. The chief cause of my failure was my clinging to the idea that the variable *t* only can be considered as the true time and that my local time *t′* must be regarded as no more than an auxiliary mathematical quantity." (p. 321)

36. See a series of articles reprinted in Lorentz, *Collected Papers*, vol. V, pp. 229–245, 246–313, 330–355, 363–382.

37. Planck, "Die Kaufmannsche Messungen der Ablenkbarkeit der β-Strahlen in ihrer Bedeutung für die Dynamik der Elektronen," *Physikalische Zeitschrift* 7 (1906): 753–761.

38. Ibid., p. 761.

39. Note the following passage in Sommerfeld's book *Atomic Structure and Spectral Lines*, tr. (from third edition, 1922) H. L. Brose (New York: Dutton, 1923), p. 531: "In this chapter we have seen how the theory of relativity, just as it has remodelled all our physical thought and ideas, has also been able to help forward spectroscopy in a decisive manner. Conversely, we note that, in return, spectroscopy is in a position to lend support to one of the main pillars of the theory of relativity and to decide in its favor the question of the variability of mass of the electron."

40. L. Laudan, "A Confutation of Convergent Realism," *Philosophy of Science* 48 (1981): 19–49; see in particular p. 24 and p. 21, n. 1.

41. Lorentz, "Die Maxwellsche Theorie und die Elektronentheorie," in *Physik*, ed. E. Warburg (Berlin: B. G. Teubner, 1915), pp. 311–333; the quotation is from the closing sentences of the article.

Abner Shimony

The Methodology of Synthesis: Parts and Wholes in Low-Energy Physics

1 Aspects of the Problem

One of the most pervasive features of the natural world is the existence of reasonably stable systems composed of well-defined parts which are to a large extent unchanged by entering into composition or leaving it. The problem of parts and wholes is to understand with the greatest possible generality the relation between the components and the composite system.

The parts-wholes problem has an ontological aspect, which concerns the properties of the components and the composite system without explicit consideration of how knowledge of them is obtained. Among the ontological questions are the following: Is there an ultimate set of entities which cannot be subdivided and which are therefore "atomic" in the etymological sense? If the properties of the components are fully specified, together with the laws governing their interactions, are the properties of the composite system then fully determined? In particular, are those properties of composite systems which are radically different from those of the components, and which might properly be characterized as "emergent," also definable in terms of the latter? Do composite systems belong, always or for the most part, to "natural kinds"? Is the existence of natural kinds explicable in terms of the laws governing the components? Are both the possible taxonomy and the actual taxonomy of natural kinds thus explicable? Is there a hierarchy of "levels of description"—i.e., microscopic, macroscopic, and possibly intermediate—such that laws can be formulated concerning a coarser level without explicit reference to the properties at a finer level of description?

The parts-wholes problem also has an epistemological aspect. Suppose that the most precise and best-confirmed laws turn out to govern relatively simple systems—as is indeed mostly the case in physics—but that the systems of interest are enormously complicated combinations of simple components. Then there will be insuperable experimental difficulties in gathering knowledge about all the initial conditions of the parts, and insuperable mathematical difficulties in deducing from the basic laws the properties of the composite system.