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The Ether Problem, the Mechanistic Worldview, and the Origins of the Theory of Relativity

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1. INTRODUCTION

Since the first systematic account by Max von Laue,[1] it has been, and still is, the common practice to introduce the theory of relativity with a survey of the nineteenth century ether problem. By "ether problem" I mean the theoretical and experimental investigations of possible influences of the earth's motion relative to the ether on optical and electromagnetic phenomena. I shall cite a few arbitrarily chosen examples from recent textbooks. Christian Møller begins his book with "a short historical survey of the numerous optical experiments which have been performed in an attempt to detect effects depending on the motion of the apparatus with respect to an absolute space."[2] He says that for Maxwell and his contemporaries "the ether was supposed to represent the absolute system of reference, thus giving a substantial physical meaning to Newton's notion of 'absolute space'."[3] But "the fruitless attempts to find out any influence of the motion of the earth on mechanical, optical, and electromagnetic phenomena gave rise to the conviction among physicists that the principle of relativity was valid for all physical phenomena."[4] W. G. V. Rosser, who aims at filling the gap between advanced textbooks and semi-popular books, gives a detailed account of the aberration of light from the stars and of experiments by Fizeau, Hoek, Airy, Michelson and Morley, and others "to illustrate how the theory of special relativity arose out of classical electromagnetism."[5]

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[1] M. von Laue, *Das Relativitätsprinzip* (Braunschweig, 1911), pp. 8–18.

[2] C. Møller, *The Theory of Relativity,* 2nd ed. (London, 1972), p. 5.

[3] *Ibid.,* p. 6.

[4] *Ibid.,* p. 30.

[5] W. G. V. Rosser, *An Introduction to the Theory of Relativity* (London, 1964), p. xiii.

Such a tradition has produced the common understanding that the theory of relativity was formulated as an answer to the ether problem. Scholars holding this view overlook that the ether problem had already received an answer before Einstein's theory in the work of H. A. Lorentz and Henri Poincaré, and that logic would therefore require them to admit Edmund Whittaker's much disputed view that the theory of relativity was formulated by Lorentz and Poincaré.[6] Since Whittaker's book appeared in 1953, many authors, beginning with Max Born,[7] have debated Whittaker's view. Heinrich Lange[8] and G. H. Keswani,[9] agreeing with Whittaker, have asserted that the main results of the theory of relativity were obtained by Poincaré. Charles Scribner Jr.,[10] although considering Whittaker's view too extreme, considers Poincaré's work a valuable contribution. Their opponents T. Kahan,[11] Gerald Holton,[12] Stanley Goldberg,[13] Kenneth Schaffner,[14] M. A. Tonnelat,[15] and Arthur I. Miller[16] have insisted on the difference between the Lorentz-Poincaré and Einstein's relativity theory and reject Whittaker's view. O. A. Starosel'skaya-Nikitina,[17] although without referring to Whittaker, has discussed the limitation of Poincaré's scientific thought which

[6]E. Whittaker, *A History of the Theories of Aether and Electricity. The Modern Theories 1900–1962* (London, 1953), Chap. 2.

[7]M. Born, "Physics and Relativity," a lecture given at the International Relativity Conference in Berne on 16 July 1955, in Max Born, *Physics in My Generation* (London, 1956), pp. 189–206.

[8]Heinrich Lange, *Geschichte der Grundlagen der Physik, Vol. 1: Die formalen Grundlagen—Zeit, Raum, Kausalität* (Freiburg/München, 1954), Chap. 10.

[9]G. H. Keswani, "Origin and Concept of Relativity," *British Journal for the Philosophy of Science, 15* (1965), 286–306; *16* (1965), 19–32, 273–294.

[10]C. Scribner, "Henri Poincaré and the Principle of Relativity," *Amer. Journ. Phys., 32* (1964), 672–678.

[11]T. Kahan, "Sur les origines de la théorie de la relativité restreinte," *Revue d'Hist. Sci., 12* (1959), 159–165.

[12]Gerald Holton, [a] "On the Origins of the Special Theory of Relativity," *Amer. Journ. Phys., 28* (1960), 627–636; Gerald Holton, *Thematic Origins of Scientific Thought. Kepler to Einstein* (Cambridge, 1973), pp. 165–195. [b] "On the Thematic Analysis of Science: The Case of Poincaré and Relativity," *Mélange Alexandre Koyré. II. L'aventure de la Science* (Paris, 1964), pp. 257–268; abridged under the title "Poincaré and Relativity," in *Thematic Origins,* pp. 185–195.

[13]Stanley Goldberg, [a] "Henri Poincaré and Einstein's Theory of Relativity," *Amer. Journ. Phys., 36* (1967), 934–944. [b] "The Lorentz Theory of Electrons and Einstein's Theory of Relativity," *ibid., 37* (1969), 982–994.

prevented him from reaching the theory of relativity. I, too, have briefly discussed the problem.[18]

These discussions have conclusively shown that Lorentz' and Poincaré's theory was *not* equivalent to the theory of relativity as properly understood, and that Lorentz and Poincaré did not accept the latter theory. But there still remains the question of the origin of the difference between the two theories, that is, the question of the root of Einstein's innovation. In this respect Gerald Holton has contributed the first step forward.[19] He has effectively criticized the traditional view that Einstein put forward his theory chiefly to surmount the difficulty caused by the negative result of the Michelson-Morley ether drift experiment. After a careful investigation he reached the conclusion that "the role of the Michelson experiment in the genesis of Einstein's theory appears to have been so small and indirect that one may speculate that it would have made no difference to Einstein's work if the experiment had never been made at all."[20] In contrast to Einstein, Lorentz, Poincaré, and most other contemporary physicists saw the Michelson-Morley experiment as one of the most urgent problems requiring their theoretical efforts. This difference of attitude toward the experiment between Einstein and the others stems from the difference between the problems which then preoccupied them. The problem that Einstein viewed as fundamental for physics at that time was different

[c] "Poincaré's Silence and Einstein's Theory of Relativity," *Brit. Journ. Hist. Sci., 5* (1970), 73–84.

14 Kenneth F. Schaffner, "The Lorentz Electron Theory of Relativity," *Amer. Journ. Phys., 37* (1969), 498–513.

15 M. A. Tonnelat, *Histoire du principe de relativité* (Paris, 1971), Chap. V.

16 Arthur I. Miller, "A Study of Henri Poincaré's 'Sur la Dynamique de l'Electron'," *Arch. Hist. Exact Sciences, 10* (1973), 207–328.

17 O. A. Starosel'skaja-Nikitina, "Rol' Anri Puankare v sozdanii teorii otnositel'nosti," *Voprosy Istorii. Estestvoznanija i Tekhniki, 5* (1957), 39–49.

18 Tetu Hirosige, [a] "A Consideration Concerning the Origins of the Theory of Relativity," *Japanese Studies in the History of Science,* No. 4 (1965), pp. 117–123. [b] "Electrodynamics Before the Theory of Relativity, 1890–1905," *Jap. Stud. Hist. Sci.,* No. 5 (1966), pp. 1–49, esp. pp. 44–45. [c] "Sôtairon wa doko kara umareta ka?" ("Where did the Theory of Relativity Emerge from?"), *Butsuri* (*Proceedings,* Physical Society of Japan), *26* (1971), 380–388.

19 Gerald Holton, "Einstein, Michelson, and the "Crucial" Experiment," *Isis, 60* (1969), 133–197; Gerald Holton, *Thematic Origins,* pp. 261–352.

20 *Ibid.,* p. 195; p. 327.

from the central issue of the ether problem which had been discussed by Lorentz, Poincaré, and other contemporary physicists. He saw the situation with a perspective that was quite distinctive. We therefore may say that a fundamental change in the aspect from which problems of physics were viewed was essential for the conception of the theory of relativity. What was the nature of that change? What were the factors that brought about the needed transformation of perspective? The origins of the theory of relativity must be sought in the answers to these questions.

To elucidate the origins of the theory of relativity it is necessary first to consider the actual nature and scope of the ether problem in the nineteenth century. The first part of the present paper is devoted to such a consideration. I do not, however, pretend to give a comprehensive history of the ether problem,[21] but intend only to clarify the nature of the problem with which Lorentz, Poincaré, and others wrestled at the turn of the century. The discussion of the ether problem will help to establish the novelty of Einstein's theory as compared with Lorentz' and Poincaré's theory. The consideration of the novelty of Einstein's approach, especially of his conceptual attitude towards physical problems, requires a reevaluation of the great influence of Mach on Einstein's thought. Differing from the common view, I find Mach's main influence upon Einstein, as far as the genesis of the theory of relativity is concerned, in his devastating criticism of the mechanistic world view. In the last part of the present paper I try to show, by discussing some aspects of Lorentz' and Poincaré's thoughts as well as some remarkable developments in the process by which Einstein's theory became accepted, that a complete emancipation from the mechanistic worldview was of crucial importance for the formation of the theory of relativity.

2. ABERRATION OF LIGHT AND THE VALIDITY OF THE WAVE THEORY OF LIGHT

Histories of the ether problem usually begin with the attempt to explain the aberration of light from the stars and the experiment by

[21] For a historical survey of optical problems in moving bodies, cf. U. I. Frankfurt and A. M. Frenik, "Ocherki razvitsiya optiki dvizhushchikhsya tel," *Trudy Instituta Istorii Estestvoznaniya i Tekhniki, 43* (1961), 3–49.

François Arago that showed that the refraction of light from the stars was not affected by the motion of the earth. Speaking of these investigations we are unconsciously inclined to believe that they were conducted in an attempt to prove the existence of the ether as an absolute reference system. Such a belief, however, is only a projection into the past of the prejudice of those who have already encountered the theory of relativity. In the early stages of the development of the ether problem, what absorbed the physicists' interest was not the issue of an absolute reference system, but rather the implications of the ether problem for the controversy over the nature of light.

When Arago, in 1810, performed his famous experiment,[22] he designed it on the basis of the emission theory of light, which was then predominant in France. In the wave theory the velocity of propagation of waves is determined exclusively by the properties of the medium, and consequently it has a constant value with respect to the medium irrespective of the motion of the source of light. On the other hand, in the emission theory the velocity of light, in general, depends on the initial velocity with which the light particles are emitted. It therefore must depend on the nature and state of the source of light. Astronomical determinations of light velocity by Ole Rømer and James Bradley had shown that it was constant irrespective of the distance over which the light was propagated. "Some astronomers, however," argued Arago, "doubted that stars having different sizes might emit rays with different velocities . . . ."[23] To test this conjecture it was necessary to determine the velocity of light from various stars with great precision. Arago proposed that this be done by observing the refraction of the light from stars. Some authors had also pointed out that such an experiment would give them a means of investigating the motions of the planets and the sun. Arago thought that "the result [of the experiment] must offer certain data concerning the true nature of light."[24] He used the translatory motion of the earth, "because the motion of our system [the earth] combined with the former [the motion of the whole solar system] would give rise to a sufficiently large inequality [of the light velocities]."[25] It was not his intention to determine

[22] F. Arago, "Mémoire sur la vitesse de la lumière" [1810], *Comptes rendus*, *36* (1853), 38–49.

[23] *Ibid.*, p. 40. [24] *Ibid.*, p. 43. [25] *Ibid.*, p. 43.

the velocity of the earth with respect to an absolute reference system.

Attaching a prism to the front of a meridian circle, Arago observed the deflection by the prism of bundles of light from stars moving toward and receding from the earth. The conclusion which he drew from his observation was not that it was impossible to detect the earth's motion. His conclusions were, first, that light rays are emitted by stars with different velocities, and, second, that, of rays emitted by bodies, only those having velocities within certain limits can be perceived by the human eye.[26] A scientist will generally draw conclusions from a scientific investigation according to the purpose for which he designed the investigation. Arago's conclusions clearly indicate that he intended to solve the currently debated problem of the properties of light particles.

Arago shortly turned to the wave theory of light. In 1815 he acknowledged that Augustin Fresnel's first attempt to explain diffraction by a wave theory was very promising, and he began to encourage and help Fresnel. Three years later, in 1818, he encouraged Fresnel to examine if the wave theory could be compatible with the result of his experiment as well as the aberration of light. In response to Arago's suggestion, Fresnel attempted to explain these experimental facts by means of hypotheses of a stationary ether and a drag coefficient of ether within refracting bodies.[27] In the same year, 1818, he finished his Academy Prize paper in which the theory of diffraction was developed to its full extent. His theory of transverse waves was put forward three years later, in 1821. Thus, in the year 1818, the emission theory still reigned and the wave theory was a heresy or, at most, an inferior competitor. Prominent members of the Paris Academy, when they chose the theory of diffraction as the subject of the Academy Prize, expected that a paper on the subject would provide a vindication of the emission theory.[28] Under these unfavorable circumstances Fresnel set out to show the superiority of the wave theory over the emission theory in the explanation of the

[26] *Ibid.*, p. 46.

[27] A. J. Fresnel, "Sur l'influence du mouvement terrestre dans quelques phénomènes d'optique," *Annales de chimie et de physique, 9* (1818); *Oeuvres complètes d'Augustin Fresnel* (Paris, 1866), *2,* 627–636.

[28] É. Verdet, "Introduction aux Oeuvres d'Augustin Fresnel," *Oeuvres complètes d'Augustin Fresnel, 1,* ix–xcix, esp. xxxvi.

aberration of light and of Arago's experiment. Referring to Arago's conclusions mentioned above, he stated that the necessity of the hypotheses of the diversity of velocities of light and the limited visibility of light "is not one of the smallest difficulties of the emission theory."[29]

Since the measurement of the velocity of light in a transparent body, which is often cited as the *experimentum crucis* for the wave theory,[30] was not performed until 1850, the wave theory of light still had not succeeded even in the 1840's. The theory of the aberration of light and Arago's experiment continued to be debated in relation to the legitimacy of the wave theory. When in 1842 Christian Doppler theoretically predicted the effect named after him by discussing the mechanism of propagation of longitudinal waves, he rejected the transverse wave theory of light and stated that, as the difficulties in explaining aberration showed, the assumptions of the transverse wave theory seemed "to contain great *inherent improbability*."[31] It is, to be sure, upon the hypothesis that light is a transverse wave and not upon the wave theory in general that Doppler cast doubt here. But no one can fail to recognize his strong distrust of Fresnel's theory. His criticism of Fresnel's theory was fully developed in a paper published in the following year, 1843.[32]

In his 1843 paper Doppler classified existing theories of the aberration of light into four groups, each of which, he asserted, had a difficulty of its own. To our eyes all four kinds of explanation seem to be based on the same kinematical principle, but Doppler considered them different from each other because of differences in the physical nature of the motion to which the principle was applied. The four kinds of explanation are: first, the analogy with the phenomenon that rain appears to fall obliquely when we see it from aboard a moving vehicle; second, the consideration of the path of

29 Fresnel, *op. cit.* (note 27), p. 628: "La nécessité de cette hypothèse n'est pas une des moindres difficultés du système de l'émission."

30 E. Whittaker, *A History of the Theories of Aether and Electricity, the Classical Theories* (London, 1951), pp. 126–127.

31 C. Doppler, "Ueber das farbige Licht der Doppelsterne und einiger anderer Gestirne des Himmels," *Abhandlungen kön. Böhm. Ges. Wiss., 2* (1841–1842), 465–482, esp. 468.

32 C. Doppler, "Ueber die bisherigen Erklärungsversuche des Aberrationsphänomens," *Abh. kön. Böhm. Ges. Wiss., 3* (1843–1844), 747–765.

light in the interior of the tube of a telescope, which requires us to tilt the telescope; third, explanation by the combination of the velocities of the earth and light; fourth, William Herschel's explanation that the eyeballs must be rotated forward in the direction of the motion of the earth for the light from a star to reach the center of the retina. Doppler argued that to make the second explanation acceptable one must assume that the ether does not change its position with respect to the solar system. To fill this requirement, however, one must assume that the earth traverses the ether freely without resistance, a hypothesis that is hardly tenable, particularly since it is incompatible with the opaqueness to light of the earth and of other terrestrial bodies. After pointing out the difficulties inherent in the other modes of explanation—immaterial for our present discussion—Doppler concluded that the transverse wave theory, however many facts it might be able to account for, could not be right simply because it clearly contradicted so simple a phenomenon as the aberration of light.[33]

George Gabriel Stokes, too, when he propounded his theory of aberration,[34] directed his criticism to the absurdity of the fundamental hypothesis of the Fresnel theory that the ether moved freely through the earth. But Stokes, contrary to Doppler, believed in the transverse theory of light. His theory of aberration was a part of his efforts to save the transverse wave theory from objections such as Doppler's. Stokes' theory is based on two assumptions: that the ether around the earth moves without the earth having any relative velocity at its surface, and that the motion of the ether is irrotational, that is, that it has a velocity potential. He thus approached the problem hydrodynamically, an approach which came naturally to Stokes who had begun his scientific career as a theoretical hydrodynamicist. On 14 April 1845, four weeks before his theory of aberration was presented at the Cambridge Philosophical Society, Stokes presented to the same society a long memoir on equations of

[33] C. Doppler, *ibid.*, p. 765: "Wenn eine Hypothese selbst eine so grosse Anzahl der complicirtesten Naturerscheinungen, für die sie gelten soll, ganz genügend erklärt, mit einer einzigen Erscheinung derselben Art aber in einem offenbaren Widerspruche steht, oder zum wenigsten sie überhaupt nicht erklärt: so ist dies ein ganz unleugbares Kennzeichen davon, dass diese Hypothese im Ganzen genommen nicht die wahre und richtige sein könne."

[34] G. G. Stokes, "On the Aberration of Light," *Phil. Mag.*, *27* (1845), 9–15; *Mathematical and Physical Papers, 1,* 134–140.

motion for viscous fluids and elastic bodies.[35] Toward the end of this memoir he asserted, on the supposition that solid bodies having small shear elasticity and large plasticity would vanishingly differ from fluids having large viscosity, that the ether, even if it is a fluid, would be able to transmit transverse waves of light.[36] He based his conclusion on the inference that the displacements of the ether particles are small because the wavelength of light is extremely short. It is clear that this argument is intended to solve the then urgent problem of the wave theory of light, that is, the contradiction that whereas the ether, as the medium of transverse light waves, must possess the elasticities of a solid body, it nevertheless exerts no resistance to the motion of the earth. Stokes' attention had been drawn to viscous fluids when, in 1842 or 1843, he learned of James South's experiment that suggested that the air around the plumb of a swinging pendulum moves with it. South's experiment occasioned Stokes to think that fluids might take part in the motion of solid bodies and would naturally have suggested to him the assumption of the "ether drag" in his theory of aberration.[37] If a moving body is to impart motion to the ether, however, there must be tangential stress acting across boundaries within the ether. Such stress would give rise to a shear elasticity and make possible the propagation of transverse waves. Thus, Stokes expected the model of the ether that provided a reasonable explanation of aberration to be, at the same time, the solution to a grave difficulty for the wave theory of light, namely, the enigma of how the fluid ether can transmit transverse waves.[38]

In later years he several times discussed the question of what physical properties should be ascribed to an ether that could satisfy the requirements deriving from his theory of aberration.[39] These discussions ultimately also threw light on the above enigma. He took

[35] G. G. Stokes, "On the Theories of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids," *Trans. Cambridge Phil. Soc., 8* (1849), 287-319; *Mathematical and Physical Papers, 1,* 75-129.

[36] *Ibid., Mathematical and Physical Papers, 1,* 126-127.

[37] David B. Wilson, "George Gabriel Stokes on Stellar Aberration and the Luminiferous Ether," *British Journal for the History of Science, 6* (1972), 57-72, esp. 61-62, 71.

[38] Cf. David B. Wilson, *ibid.,* p. 70: "Among other things, therefore, Stokes's theory of stellar aberration constituted a defence for the concept of an elastic-solid ether."

[39] G. G. Stokes, "On the Constitution of the Luminiferous Ether, Viewed with Reference to the Phenomenon of the Aberration of Light," *Phil. Mag., 29*

up the problem in response to James Challis' criticism[40] of his theory of aberration. Refuting Challis' criticism he argued as follows. No stable motion of incompressible fluids has a velocity potential. A fluid that has internal friction, however, can satisfy the requirement of a velocity potential. As he had shown in his earlier paper, a fluid ether having internal friction would behave as an ordinary fluid for the translatory motions of a gross material body and as a solid elastic body for extremely small vibrations. Hence "the astronomical phenomena of the aberration of light should afford an argument in support of the theory of transverse vibrations."[41]

To return to our theme, in the first half of the century all those who dealt with the ether problem approached it with the intention of establishing a legitimate theory of light. Arago's experiment and the aberration of light were considered the touchstone of such a theory. The theory of aberration later came to be discussed also with the expectation that it might furnish a model of the ether which could solve what seemed the most serious difficulty of the transverse wave theory of light. The ether problem in this period, therefore, should be viewed against the background of controversy over the validity of the theories of light. Apparently no one attempted to associate the ether with any privileged reference system for motion.

3. PROPAGATION OF LIGHT WAVES— THE ASTRONOMER'S PROBLEM

The attached diagram shows the number of papers, grouped according to publication dates in five year periods, that are listed under the headings "aberration of light" and "light propagation in moving media" in the *Catalogue of Scientific Papers 1800–1900* edited by the Royal Society.[42] Of course this diagram cannot be taken too seriously. It only gives a rough idea of the general trend, but it shows clearly that there was little discussion of the ether

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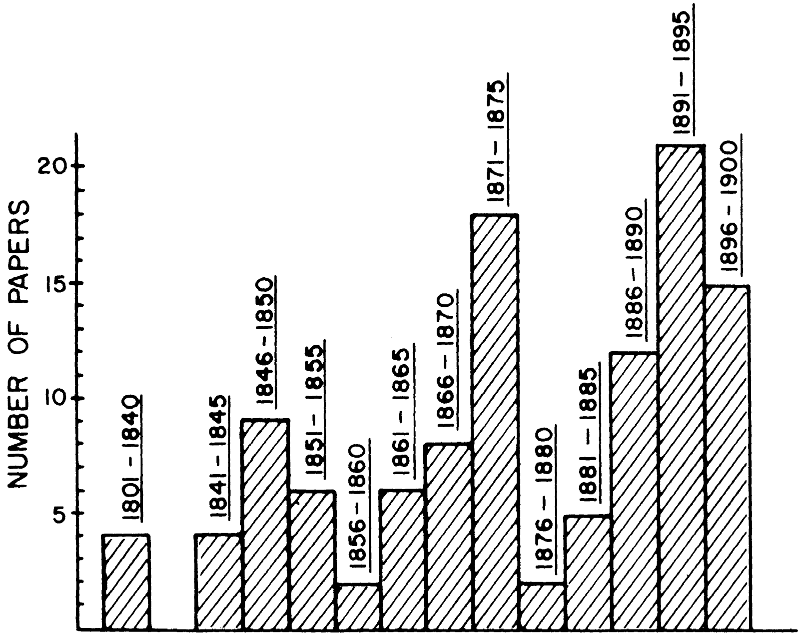
(1846), 6–10; *Mathematical and Physical Papers, 1,* 153–156. "On the Constitution of the Luminiferous Ether," *Phil. Mag., 32* (1848), 343–349; *Mathematical and Physical Papers, 2,* 8–13.

[40] J. Challis, "On the Aberration of Light," *Phil. Mag., 27* (1845), 321-327. On the controversy between Challis and Stokes, see Wilson, *op. cit.* (note 37).

[41] G. G. Stokes, *op. cit.* (note 39), *Mathematical and Physical Papers, 2,* 11.

[42] Royal Society of London, *Catalogue of Scientific Papers 1800–1900.* Subject Index, Vol. 3: *Physics,* Parts I and II (Cambridge, 1912, 1914).



NUMBER OF PAPERS
CONCERNING THE ETHER PROBLEM

problem in the 1850's and 1860's and increasing interest in the years around 1870.

Of course, even in the years of stagnation one finds a few significant investigations. Especially the confirmation of the Fresnel drag coefficient by Hippolyte Fizeau in 1851[43] furnished a cornerstone for discussion of the ether problem in subsequent years. In 1859 Fizeau made an experiment to see if differences in the direction of light rays with respect to the earth's motion alter the change of the azimuth of the plane of polarization of polarized light produced by refraction in a layer of glass.[44] Fizeau designed his experiment to

[43] A. H. L. Fizeau, "Sur les hypothèses relatives à l'éther lumineux, et sur une expérience qui paraît démontrer que le mouvement des corps change la vitesse avec laquelle la lumière se propage dans leur intérieur," *Comptes rendus, 33* (1851), 349-355.

[44] A. H. L. Fizeau, "Sur une méthode propre à rechercher si l'azimut de polarisation du rayon réfracté est influencé par le mouvement du corps ré-

confirm the Fresnel coefficient for solid bodies. Since the change of the azimuth of the plane of polarization depends on the refractive index of the glass, Fizeau expected to find from the change of the azimuth the change in the refractive index and, consequently, in the velocity of light in the transparent body. Fizeau alleged that the expected change had been found; his conclusion was later often doubted. In 1862 Jean Babinet predicted that the influence of motion would change the position of diffraction fringes.[45] He discussed this effect in connection with an astronomical problem. He imagined that if the velocity of the translatory motion of the solar system as a whole was determined, the distances to fixed stars could be measured with precision by triangulation, with the displacement of the solar system as the base. Probably because of his astronomical illustration, Babinet's paper was published in the *Comptes rendus* under the heading "astronomy." The many discussions of the ether problem around 1870 were also motivated by astronomical interests.

One of the achievements of nineteenth century astronomy was the publication of a number of voluminous catalogues of stars.[46] The first was Friedrich Wilhelm Bessel's compilation of the results of James Bradley's observations published in 1818. The results of Bessel's own observations during the years 1821 to 1833 were published after his death in 1846. The most famous, Friedrich Wilhelm August Argelander's *Bonn Durchmusterung,* was published in 1859–1862. Its extension to the stars of the southern skies by Eduard Schönfeld was published in 1875–1885. In 1885 John Macon Thome began to publish the *Cordoba Durchmusterung* covering the skies from twenty-two degrees south to the south pole. To compile catalogues of stars it was necessary to correct the observational data. The effect of atmospheric refraction had been investigated since Newton's time. Since it was also necessary to correct the effect of

fringent," *Comptes rendus, 49* (1859), 717–723; *Ann. de chim., 58* (1860), 129–163.

[45] J. Babinet, "De l'influence du mouvement de la Terre dans les phênomênes optiques," *Comptes rendus, 55* (1862), 561–564.

[46] I owe the general knowledge of astronomy in the mid-nineteenth century to the following two books: Robert Grant, *History of Physical Astronomy* (New York and London, 1966; originally London, 1852), Chaps. 14, 16, 19, and 21; Arthur Berry, *A Short History of Astronomy* (New York, 1961; originally 1898), Chaps. 12 and 13.

aberration, precise determination of the aberration constant became an important task in astronomy. Aberration also attracted attention in connection with the solar parallax which was determined by the diameter of the orbit of the earth, one of the fundamental constants of positional astronomy. Fizeau's experiment in 1849 proved it possible to measure by terrestrial means the velocity of light which had thus far been determined only by astronomical methods. It followed that, if, by combining the velocity of light measured by terrestrial means with the aberration constant, one obtained the orbital velocity of the earth, one could readily derive the diameter of the orbit.

The new correction factor which appeared in astronomical considerations in the nineteenth century was that due to the motion of the solar system as a whole. After Edmund Halley discovered the proper motion of the fixed stars in 1718, James Bradley, in 1748, was the first to point out the possibility that the apparent change in the position of fixed stars may be due not only to the motion of the stars but also to the motion of the solar system. In 1783 William Herschel, after analysis of the available data for the proper motion of the fixed stars, concluded that the solar system was in motion toward λ Hercules. He tried twice to correct the solar apex in 1805 and 1806. But his conclusion, based on many arbitrary hypotheses, especially concerning the distance to the fixed stars, could not immediately be accepted by his contemporaries. Jean Baptist Biot in 1812 and Bessel in 1818 denied the translatory motion of the solar system. In 1837 Argelander was the first to accept Herschel's conclusion. Not until the middle of the century did Otto Struve and others establish the motion of the solar system and the position of its apex. Since astronomical observations now had to take into account the proper motion of the solar system as well as the orbital motion of the earth, there arose the new problem of determining the path of light rays when not only the terrestrial observer but also the medium and the source of light, the fixed stars, are all in motion. But in the 1850's and 1860's there was still no general theory of light propagation. The wave theory of light had only recently been established, and the general theory of wave propagation, prompted by astronomical need, began to be developed only in the sixties and seventies.

We can see the immaturity of the theory of wave propagation in those days in, for example, the curious conclusion that the director of the Göttingen observatory, Ernst Friedrich Wilhelm Klinkerfues, drew in 1865–1866 from his discussion of the influence of the source of light upon refraction.[47] He concluded that the light emitted from a moving source changes its color without changing its wavelength. He reasoned as follows. Consider the vibrations of ether particles caused by the impact of propagating undulatory disturbances. If the source of light is at rest, the velocity with which the phase of oscillation is transmitted to successive particles—which Klinkerfues called the phase velocity—is the same as the velocity of transmission of the undulatory disturbances—the propagation velocity, as he called it. If the source moves, the rate of change of the phase of a particle will increase or decrease. Consequently, since the color of light is determined by the number of agitations per unit time of the visual nerve and therefore by the rate of change of the phase of the ether particles, the color of light emitted by a moving source should change. On the other hand, since the propagation velocity is determined by the properties of the medium, it does not depend on the motion of the source. Hence the spatial distance between ether particles having the same phase, that is, the wavelength, remains unchanged. Klinkerfues asserted that if one takes this effect of the motion of the source into consideration, it will be possible to explain, without assuming the Fresnel coefficient, the independence of the laws of reflection and refraction of the motion of the earth. However, he inferred that the angle of refraction would be influenced by the motion of the earth, and he attempted to detect the inferred effect. Though the result was negative, he did not give up the attempt to find the change of the angle of refraction caused by the motion of a light source. He also inferred a change in the aberration constant when he used a telescope filled with water, and he carried out an experiment.[48] Again he obtained a negative result, causing him to question the assumption of a stationary ether.

The problem Klinkerfues discussed could have been treated by a purely kinematical method based on Huygens principle. Incapable of

[47]W. Klinkerfues, "Ueber den Einfluss der Bewegung des Mittels und den Einfluss der Bewegung der Lichtquelle auf die Brechbarkeit der Strahls," *Gött. Nachr.* (1865), pp. 157–160, 210; "Weitere Mitteilungen über den Einfluss der Bewegung der Lichtquelle auf die Brechung des Strahls," *Gött. Nachr.* (1865), pp. 376–384; (1866), pp. 33–60; "Untersuchungen aus der analytischen Optik, insbesondere über den Einfluss der Bewegung der Lichtquelle auf die Brechung," *Astron. Nachr., 66* (1866), 337–366.

[48]W. Klinkerfues, *Die Aberration der Fixsterne nach der Wellentheorie* (Leipzig, 1867); "Versuche über die Bewegung der Erde und der Sonne im Aether," *Astron. Nachr., 76* (1870), 33–38.

perceiving this, he tried to derive the law of propagation dynamically, that is, by considering the physical mechanism of the propagation of light. His theory was received with sympathy and even stimulated a number of other investigations. G. B. Airy's famous experiment with a water telescope was performed under the stimulus of Klinkerfues' investigation. In a series of papers published in 1871–1872 Eduard Ketteler carried out a theoretical investigation of the influence of astronomical motions on optical phenomena, which was intended to correct the errors in Klinkerfues' theory.[49] He stated that "elucidation of the theoretical view seems to be desirable since Klinkerfues' conclusions have often been welcomed."[50]

Ketteler discussed reflection, refraction, diffraction, and interference of light on the basis of Huygens principle and showed that these phenomena would not be affected by astronomical motion. In his refutation of Klinkerfues' theory of the Doppler effect, however, Ketteler, too, had recourse to a model of the mechanism of propagation for vibrations proposed by Ernst Mach.[51] The model consisted of an infinitely long chain of metal cylinders connected by steel rings to each other. Ketteler explained that he used this model in order to discuss undulatory motion in an intuitive manner.[52] For us today his argument is rather complicated and turbid precisely because of his recourse to the mechanical model. To Ketteler, the development in terms of a mechanical model seemed to be intuitive and easy to understand because, first, it fit mechanistic modes of thinking, and, second, the lack of a general theory of wave propagation left him no alternative than to discuss the construction of wave fronts and rays in concrete terms.

In 1870 Wilhelm Veltmann gave the first general demonstration of aberration, based not on examinations of separate cases but on a

[49] E. Ketteler, "Ueber den Einfluss der astronomischen Bewegungen auf die optischen Erscheinungen," *Ann. d. Phys.*, *144* (1871), 109–127, 287–300; *144* (1872), 363–375, 550–563; *146* (1872), 406–430; *147* (1872), 404–429; "Nachträglicher Zusatz zu der Abhandlung über die Aberration," *Ann. d. Phys.*, *147* (1872), 478–479; "Ueber den Einfluss der astronomischen Bewegungen auf die optischen Erscheinungen. Nachtrag zu den letzten Abhandlungen," *Ann. d. Phys.*, *148* (1873), 435–448; *Astronomische Undulationstheorie oder die Lehre von der Aberration des Lichtes* (Bonn, 1873).

[50] E. Ketteler, *op. cit.* (note 49), *Ann. d. Phys.*, *144* (1871), 127.

[51] E. Mach, "Ueber eine Longitudinalwellenmachine," *Ann. d. Phys.*, *132* (1867), 174–176.

[52] E. Ketteler, *op. cit.* (note 49), *Ann. d. Phys.*, *144* (1871), 114.

general kinematical consideration.[53] Emphasizing that his theory aims at generality, he stated that his theory "covers not only the given cases but all possible cases, and is therefore exhaustive."[54] He added that "this hypothesis [the Fresnel coefficient] seems to have been utilized only for explaining separate special cases. . . . One obtains neither a clear understanding of the essence of Fresnel's hypothesis nor a general justification for the methods astronomers use in correcting the effect of aberration."[55] It may also be noted that Veltmann distinguished the propagation of light with respect to the ether from that relative to material bodies, calling the former "absolute motion" and the latter "relative motion."[56] At the same time he admitted the possibility that the ether itself moves with respect to space and called the propagation of light with respect to space the "real motion," though he did not discuss this "real motion." Veltmann's nomenclature shows that he did not identify the ether with absolute space in the Newtonian sense.

Veltmann's proof that, if the effects higher than the first order are disregarded, one cannot detect any influence of astronomical motions on optical phenomena is in essence the same as the general proof that H. A. Lorentz gave later. He considered the propagation of light in a system of transparent bodies sharing a common translation of velocity v. Let a ray starting from one point in the system, undergoing reflections and refractions, describe a polygonal path and return to the original point. If we denote the length of each side of the polygon by s_i, and the relative velocity of light with respect to the material body on this side by w_i, the time required for the ray to traverse the whole path will be $\Sigma s_i/w_i$. If the velocity of the ether

[53]W. Veltmann, [a] "Fresnel's Hypothese zur Erklärung der Aberrationserscheinungen," *Astron. Nachr.*, *75* (1870), 145–160; [b] "Ueber die Fortpflanzung des Lichtes in bewegten Medien," *Astron. Nachr.*, *76* (1870), 129–144; [c] "Ueber die Fortpflanzung des Lichtes in bewegten Medien," *Ann. d. Phys.*, *150* (1873), 497–535.

[54]*Ibid.* [c], p. 498: Veltmann states that his theory "nicht blos die vorstehenden, sondern sämmtliche möglichen Fälle umfasst, also wirklich erschöpfend ist."

[55]*Ibid.*, pp. 499–500: "Man scheint jedoch diese Hypothese bisher nur zur Erklärung einzelner specieller Fälle benutzt zu haben. . . . [Man] erhält . . . keine klare Einsicht in das eigentliche Wesen der Fresnel'schen Hypothese und keine allgemeine Begründung des Verfahrens der Astronomen bei der Correction wegen Aberration."

[56]*Ibid.*, p. 501.

relative to the body is u_i, then

$$w_i = c_i + u_i \cos\psi_i,$$

where ψ_i is the angle between the side i and the direction of the translation of the system. By Fresnel's hypothesis $u_i = v/n_i^2$, n_i being the refractive index of the body. Neglecting terms higher than the first order, we obtain

$$\frac{s_i}{w_i} \sim \frac{s_i}{c_i} - \frac{s_i}{c_i^2}\frac{v}{n_i^2}\cos\psi_i = \frac{s_i}{c_i} - \frac{v}{c^2}s_i\cos\psi_i,$$

$$\sum \frac{s_i}{w_i} = \sum \frac{s_i}{c_i} - \frac{v}{c^2}\sum s_i\cos\psi_i,$$

where c denotes the velocity of light in a vacuum. The first term on the right side of the last equation represents the time interval in which the ray traverses the whole path, and the sum in the second term, being the projection of the whole path on the direction of the translatory motion of the system, will vanish for a closed path. Hence the difference in optical path lengths for two rays connecting the same initial and end points remains the same regardless of the motion of the system. Now according to the Fresnel theory, the propagation of light can generally be described as a consequence of interference, and the interference is determined solely by the differences in the optical path lengths. In the approximation to the first order, therefore, no influence of the motion on optical phenomena appears.

Concurrently with Veltmann's theoretical exploration of the general theory of light propagation, experimental investigations led to a provisional accommodation concerning the relation between astronomical motion and optical phenomena. One of these was George Biddell Airy's experiment with a water telescope confirming Fresnel's prediction of 1818.[57] This experiment was motivated by the negative result of Klinkerfues' experiment and by Martinus Hoek's experiment,[58] which was alleged to have confirmed the

[57]G. B. Airy, "On a Supposed Alteration in the Amount of Astronomical Aberration of Light, Produced by the Passage of Light Through a Considerable Thickness of Refractive Medium," *Phil. Mag., 43* (1872), 310–313; "Additional Note to the Paper 'On a Supposed Alteration. . .'," *Phil. Mag., 45* (1873), 306.

[58]M. Hoek, "Détermination de la vitesse avec laquelle est entraînée une onde lumineuse traversant un milieu en mouvement," *Arch. néerl, 3* (1868),

Fresnel coefficient. Airy observed the star γ Draconis with a meridian circle whose tube was filled with water. He made two observations, one in spring and another in autumn after an interval of six months. Then he calculated from the observational results the latitude of the place of observation. If the water in the tube alters the aberration constant, the latitudes obtained by calculation should reveal discrepancies. Airy, however, could not find any. He carried out the experiment in 1871 and 1872, but the results were always negative. Another, far more sweeping, investigation was performed by Éleuthère Élie Nicolas Mascart,[59] who was awarded the 1873 Grand Prix of the Paris Academy of Sciences for this work. The Academy had offered the prize for an experimental investigation of "the modifications produced in the mode of propagation and the properties of light in consequence of motions of the luminous source and the observer."[60]

Mascart confirmed experimentally that no influence of the motion of the earth is observed in any of the examined phenomena such as diffraction by gratings, reflection by mirrors, chromatic polarization by doubly refracting substances, rotation of the plane of polarization by rock crystals, refraction by prisms, Newton rings, and interference by Young's mixed layer. He also repeated Hoek's experiment and confirmed the latter's conclusion. In performing these experiments he analyzed the phenomena by means of the Fresnel theory and the Doppler principle. He analyzed separately the two cases of a terrestrial and a celestial source of light, respectively, that is, the cases with and without relative motion between the observational instrument and the source of light, and predicted that in either case the results will always be negative. His experiments completely confirmed his prediction. Of the various phenomena investigated by Mascart, the propagation of light in a moving doubly refracting substance was the only one that Fresnel had not dealt with. In the analysis of this phenomenon Mascart assumed that the Fresnel coefficient, with

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180–185; "Détermination de la vitesse avec laquelle est entraîné un rayon lumineux traversant un milieu en mouvement," *ibid.*, *4* (1869), 443–450.

[59]E. Mascart, "Sur les modifications qu'éprouve la lumière par suite du mouvement de la source lumineuse et du mouvement de l'observateur," *Ann. de l'École norm.*, *1* (1872), 157–214; *3* (1874), 363–420.

[60]"Prix décerné. Année 1872.—Prix extraordinaires. Grand prix des sciences mathematique. Rapport lu et adopté dans la séance du 14 juillet 1873," *Comptes rendus*, *79* (1874), 1531–1534. Quotation is from pp. 1531–1532.

corresponding refractive indices, is valid for each of the ordinary and extraordinary rays. Having confirmed experimentally the result of his theoretical analysis, he concluded that if one supposes that ordinary rays behave in doubly refracting substances in much the same way as in isotropic substances, then the Fresnel coefficient can be used equally for both ordinary and extraordinary rays.[61] The Prize committee especially appreciated this extension of the Fresnel coefficient to doubly refracting substances. Its report states that "these last results [in the case of doubly refracting substances] have importance and novelty which, in concluding this report, may especially be emphasized."[62]

Mascart's application of the Fresnel coefficient to double refraction drew attention also because of its possible significance for the question of the physical explanation of the coefficient. The question had been discussed by several authors and there were two rival theories. The one supposes, as Fresnel's does, that only the excess of the ether contained in a material body, as compared with the surrounding ether, moves with the same velocity v as the material body, the rest remaining stationary. The other assumes the whole ether within the body to move with the velocity $(1 - 1/n^2)v$.[63] There was also the view that in a moving body the density of the ether is modified.[64] Mascart remarked that if different values of the Fresnel constant are applied for ordinary and extraordinary rays, Fresnel's interpretation of the drag coefficient that the excess ether moves with the body is not tenable.[65] These discussions are concerned with a new physical problem which is independent of the astronomical ones discussed so far in this section. The Paris Academy, explaining

[61] E. Mascart, *op. cit.* (note 59), *Ann. de l'École norm., 3* (1874), 418.

[62] *Op. cit.* (note 60), p. 1534: "Ces derniers résultats ont un caractère d' importance et de nouveauté qu'il convient de signaler d'une manière spéciale en terminant ce Rapport."

[63] For example, A. Beer, "Ueber die Vorstellungen vom Verhalten des Aethers in bewegten Mitteln," *Ann. d. Phys., 94* (1855), 428-434.

[64] J. Boussinesq, "Sur le calcul des phénomènes lumineux produits à l'intérieur des milieux transparents animés d'une translation rapide, dans le cas où l'observateur participe lui-même à cette translation," *Comptes rendus, 76* (1873), 1293-1296.

[65] E. Mascart, *op. cit.* (note 59), *Ann. de l'Ecole norm., 3* (1874), 420: "Il semble résulter de là que, pour calculer l'influence des milieux pondérables, il est nécessaire d'avoir recours à d'autres considérations que celle du transport partiel de l'éther, comme le faisait Fresnel."

the significance of its prize problem, stated that "now that the vibratory motion of light and the existence of the luminiferous ether are universally considered well established it appears of great interest to direct our research toward the properties of this elastic medium and its relation with the ponderable body."[66] In the next stage of the development of the ether problem it was just this relation of the ether with ponderable matter that became the central issue of the ether problem.

4. RELATION OF THE ETHER TO PONDERABLE MATTER–THE PHYSICIST'S PROBLEM

In the latter half of the 1880's physicists became interested in a new aspect of the ether problem. The immediate reason for their interest was the Michelson-Morley experiment, but we cannot forget another contributing factor, namely J. C. Maxwell's electromagnetic theory, which greatly enhanced the status of the ether in physics. In 1886, performing a thorough investigation of "the influence of the motion of the earth on the luminiferous phenomena," H. A. Lorentz wrote: "Examination of this question not only is interesting for the theory of light but has acquired a much more universal importance since it became probable that the ether plays a role in the phenomena of electricity and magnetism."[67] Maxwell, too, had been interested in the ether problem. He even performed an experiment in 1864 to see if the index of refraction was different for light rays moving in opposite directions and confirmed Arago's negative result.[68] In his

[66] *Op. cit.* (note 60), p. 1532: "Aujourd'hui que les mouvements vibratoires de la lumière et l'existence de l'éther lumineux lui-même sont considérés par tous comme des vérités bien établies, il paraît d'un grand intérêt de diriger nos recherches vers les propriétés de ce milieu élastique et ses relations avec la matière ponderable."

[67] H. A. Lorentz, "Over den invloed, dien de beweging der Aarde op de lichtverschijnselen uitoefent," *Versl. Kon. Akad. Wet., 2* (1886), 297–372 (French translation: "De l'influence du mouvement de la terre sur les phénomènes lumineux," *Arch. néerl., 21* (1887), 103–176; *Collected Papers, 4,* 153–214). Quotation from p. 153: "L'examen de cette question n'intéresse pas seulement la théorie de la lumière, il a acquis une importance bien plus générale depuis qu'il est devenu probable que l'éther joue un rôle dans les phénomènes de l'électricité et du magnétisme."

[68] J. C. Maxwell to W. Higgins, 10 June 1867. This letter was published in the latter's paper "Further Observations on the Spectra of some of the Stars and

article "Ether," written for the ninth edition of the *Encyclopaedia Britannica* (1879), after surveying some experimental attempts to find the velocity of the ether relative to terrestrial bodies, he concluded that "the whole question of the state of the luminiferous medium near the earth, and of *its connexion with gross matter,* is very far as yet from being settled by experiment" (italics are mine).[69] Maxwell here already perceived the question of the relation of the ether to ponderable matter.

It was George Francis FitzGerald, the ardent supporter of the Maxwellian theory, who first discussed the influence of the motion of the earth on electromagnetic phenomena in 1882.[70] He argued as follows. Henry Augustus Rowland's experiment had shown that the motion of electric charges produces physical effects. Since an "absolute" motion is meaningless, the motion here should be understood as the motion with respect to the ether. Fizeau's experiment concerning Fresnel's drag coefficient suggests that there exists a relative motion between terrestrial bodies and the ether. Hence one can expect terrestrial electrified bodies to produce electromagnetic effects as a result of the motion of the earth with respect to the ether. On this expectation FitzGerald theoretically examined interactions between terrestrial electric charges and magnets, and between two conducting currents, but he found that effects due to the common translatory motion cancel each other completely. He concluded his discussion by expressing hope for further investigations on possible effects in other, more general cases.

In the previous year, 1881, J. J. Thomson had made an attempt to deduce the drag coefficient from the electromagnetic theory.[71] He extended the Maxwell equations to the case of a moving body and obtained for the case of a dielectric moving with velocity v in the direction of the propagation of light, which is taken as the x-direction, the equation for the x-component of the dielectric

Nebulae. . . ," *Phil. Trans., 158* (1868), 529–564. Maxwell's letter is on pp. 532–535.

69J. C. Maxwell, "Ether," *The Scientific Papers of James Clerk Maxwell, 2,* 763–775. The quotation is from p. 770.

70G. F. FitzGerald, "On Electromagnetic Effects Due to the Motion of the Earth," *Trans. Roy. Dublin Soc., 1* (1882), 319; *The Scientific Writings of the Late George Francis FitzGerald,* pp. 111–118.

71J. J. Thomson, "On Maxwell's Theory of Light," *Phil. Mag., 9* (1880), 284–291.

displacement

$$c^2 \frac{\partial^2 f}{\partial x^2} = \frac{\partial^2 f}{\partial t^2} + v \frac{\partial^2 f}{\partial x \partial t},$$

where c is the velocity of light. If the velocity v of the body lies in the direction of the x-axis, putting $f = a \cos(qt - px)$, the relation for the velocity of light in the medium, q/p, is obtained:

$$\frac{q}{p} = \frac{v}{2} \pm \sqrt{c^2 - \frac{v^2}{4}} \sim \frac{v}{2} \pm c.$$

From this Thomson concluded that when the medium that the light traverses moves, the medium drags the light with half the velocity of the medium, a result that, Thomson asserted, agrees with Fizeau's experiment.

In 1889 Theodor DesCoudres made the first experiment to detect the effect of the motion of the earth on electromagnetic phenomena.[72] He designed the experiment to test if the electromagnetic induction between two coils is affected by the motion. The result was negative. What interests us more than the result of the experiment is the motivation and goal of DesCoudres' investigation. He had reason to hope that an attempt to find the effects of the motion on induction would now be successful: "Although up to now the fundamental assumption of our experiments that the electromagnetic induction propagates itself with a velocity that scarcely differs from that of light had been an unproved hypothesis, Hertz's experiments raised it to the rank of fact."[73] Noting the significance of his attempt, he expected that it would help to determine to "what extent the so-called luminiferous ether partakes in the motion of the ponderable mass of the terrestrial body."[74] These words show that the central interest in the ether problem then was no longer the propagation of light under various conditions nor the determination

[72] T. DesCoudres, "Ueber das Verhalten des Lichtäthers bei der Bewegung der Erde," *Ann. d. Phys.*, *38* (1889), 71–79.

[73] *Ibid.*, p. 72: "War die bei unserem Experimente gemachte Grundvoraussetzung, dass sich electrodynamische Induction mit einer von der Lichtgeschwindigkeit nicht sehr verschiedenen Geschwindigkeit fortpflanze, bislang eine unbewiesene Hypothese gewesen, so erhoben die Hertz'schen Experimente dieselbe zur Thatsache."

[74] *Ibid.*, p. 72: DesCoudres wanted to determine "inwieweit der sogenannte Lichtäther an den Bewegungen der ponderabelen Massen des Erdkörpers Antheil nimmt."

of the astronomical motion of the earth or the solar system, but the connection between matter and the ether, that is, the behavior of the ether within and around a mass of ponderable matter when the latter is in motion.

The shift of interest indicated above is also reflected in successive papers by Albert Abraham Michelson. In the report of the experiment that he performed in 1881 in Berlin, he stated that the experiment aimed at finding "the velocity of the earth's motion through the ether"[75] and concluded that the hypothesis of a stationary ether is not correct. Then in 1886 he and Edward Williams Morley repeated Fizeau's experiment on the drag coefficient.[76] In this investigation, as is natural in view of its subject, their attention was directed to the connection between ether and matter. Having confirmed Fizeau's result, they concluded that "*the luminiferous ether is entirely unaffected by the motion of the matter which it permeates*" (italics original).[77] By "entirely unaffected" they meant that, since "Fresnel's statement amounts . . . to saying that the ether within a moving body remains stationary with the exception of the portions which are condensed around the particles," that is, with the exception of the excess ether as compared to the surrounding ether, we may say that the remaining ether is entirely unaffected by the motion if we regard each particle of the body and the ether condensed around it as a single body.[78]

Michelson and Morley's most celebrated experiment of 1887 was, according to their words, intended to contribute to solving the following problem.[79] Their experiment in the preceding year had confirmed that the ether contained within a transparent body remains stationary when the body moves. However, in their view this result cannot be extended to the case of an opaque body such

[75] A. A. Michelson, "The Relative Motion of the Earth and the Luminiferous Ether," *Amer. Journ. Sci., 22* (1881), 120–129. For detailed accounts of this experiment and Michelson and Morley's successive experiments the reader is referred to Lloyd S. Swenson, *The Ethereal Aether. A History of the Michelson-Morley-Miller Aether-Drift Experiments, 1880–1930* (Austin and London, 1972). Swenson seems to take it for granted that Michelson viewed his experiment as a quest for absolute motion. I suspect this was not the case.

[76] A. A. Michelson and E. W. Morley, "Influence of Motion of the Medium on the Velocity of Light," *Amer. Journ. Sci., 31* (1886), 377–386.

[77] *Ibid.*, p. 386.

[78] *Ibid.*, p. 379.

[79] A. A. Michelson and E. W. Morley, "On the Relative Motion of the Earth and Luminiferous Ether," *Amer. Journ. Sci., 34* (1887), 333–345, esp. 334.

as the earth. That the ether can penetrate metal is shown by the change of volume of the Torricellian vacuum when a barometer tube is tilted; the ether freely passes through the wall of the tube. But free penetration does not necessarily indicate the absence of resistance. Much less can it be taken for granted that the ether can pass through so extended a body as the earth without resistance. Such an important problem as this should, as Lorentz had stated in his 1886 article, be decided not by supposition, but by experiment. The immediate purpose of Michelson and Morley's experiment was, therefore, to determine the relative motion of the earth and the luminiferous ether, and they concluded that "the ether is at rest with regard to the earth's surface."[80] However, their discussion of the background of the experiment shows that behind this purpose stood the more fundamental concern with the physical problem of the connection between the ether and matter.

Lorentz' 1886 article "On the Influence of the Motion of the Earth on Luminiferous Phenomena,"[81] to which Michelson and Morley referred in their paper, discusses from a unified theoretical point of view the results obtained thus far in the pursuit of the ether problem; it is the fundamental work to which later investigations would always have to refer. In this sense, it, together with the 1887 Michelson-Morley experiment, opened a new epoch in the history of the ether problem. The article clearly identifies "the connection between matter and the ether" as the central issue of the ether problem. Lorentz' article begins with the proof that the two fundamental assumptions of Stokes' theory of the aberration of light, that is, the existence of a velocity potential of the motion of the ether and the null relative velocity between the ether and the surface of the earth, are incompatible. However, of these two assumptions, Lorentz observed, only that of the existence of the velocity potential is indispensable for the explanation of aberration. He therefore examined the possibility of developing a theory in which the assumption of the relative velocity at the surface of the earth is dropped and replaced by Fresnel's hypothesis of the partial dragging of the ether by transparent bodies. He made the following assumptions:[82] first, that the ether surrounding the earth is in motion and that this ether has a velocity potential; second, that the motions of the ether and

[80] *Ibid.*, p. 339.
[81] H. A. Lorentz, *op. cit.* (note 67).
[82] *Ibid.*, § 8.

the earth can be different from each other at the earth's surface; third, that when the ether moves through a transparent body, the elementary waves of light in this body are dragged along the direction of the relative motion of the body with respect to the ether with the velocity kv. Here v denotes the relative velocity of the body to the ether, and $k = 1 - 1/n^2$, n being the refractive index of the body. Finally, Lorentz made no assumptions about opaque bodies. With these assumptions, and neglecting terms higher than the first order of v/c, Lorentz examined the path of light rays with regard to the earth—the relative rays, as he called them—to show that all phenomena occur as if the earth were at rest and the relative rays followed the path of light rays with regard to the ether, that is, the path of the absolute rays. In other words, except for the Doppler effect of light from the stars, there is no detectable effect of the motion of the earth upon optical phenomena. This result, agreeing with the conclusion from Fresnel's theory, accounts for all results of the experiments thus far performed. Since his conclusion depends to a large extent on the drag coefficient, Lorentz declared its experimental confirmation to be of special importance. Fizeau's 1851 experiment had confirmed the coefficient only qualitatively, leaving its numerical value undetermined. But Michelson and Morley's experiment had confirmed the numerical value assumed by Fresnel.

Lorentz' general theory, however, cannot determine whether or not the ether remains stationary, notwithstanding the motion of the earth, because of the second assumption above. Lorentz therefore, in the final part of the 1886 article, tried to approach the problem from another angle by emphasizing the "connection of matter and the ether."[83] He first considered the case of opaque bodies that do not permit the ether to penetrate them. In this case the ether within the telescope tube, together with the telescope, will take part in the motion of the earth. But some experiments suggest that opaque bodies, at least when they are not thick, permit the ether to pass through them freely. For example, when a barometer tube is inclined, the ether contained in the Torricellian vacuum goes out freely through glass and mercury. If we regard the atoms of matter as a local modification of the ether, we may expect that the ether freely penetrates material bodies however thick they might be. Lorentz considered this problem so important that he urged physicists not to be content with considerations of probability or simplicity, but to

[83] *Ibid.*, § 24.

decide on the basis of experiment whether the ether at the surface of the earth is at rest or in motion.[84] He referred to two experiments that had already been performed: Fizeau's 1859 experiment to see if the change of the azimuth of the plane of polarization caused by refraction of polarized light entering into a layer of glass is modified by the motion of the earth and Michelson's 1881 experiment to detect ether drift. The former experiment, Lorentz remarked, seems to have shown that the ether is at rest with respect to the surface of the earth but is not so definitive that it can determine the relative velocity. The latter he showed to be not sufficiently precise when Michelson's overestimation of the effect is corrected.[85]

Thus in his 1886 paper Lorentz reserved his conclusion about the motion of the ether. But there is little doubt that he was inclined to the hypothesis of a stationary ether. He based the theory of optical properties of matter which he had developed since 1875 on the fundamental assumption that the ether exists also in the interior of material bodies, permeating the intermolecular spaces, and can be treated separately from material particles as far as its electromagnetic effect is concerned.[86] Lorentz remarked that his assumption is also supported by the study of the effect of the motion of material bodies on optical phenomena.[87] In 1892 he laid the foundation of the electron theory by adopting the hypothesis of a stationary ether. Characterizing his theory as "the theory of electromagnetic phenomena based on the idea that ponderable matter is completely transparent to the ether and can move without communicating any motion to the latter," he stated that "one can adduce some facts in optics as the ground for this hypothesis."[88] These examples show that

[84]*Ibid., Collected Papers, 4,* 203: "Quoi qu'il en soit, on fera bien, à mon avis, de ne pas se laisser guider, dans une question aussi importante, par des considérations sur le degré de probabilité ou de simplicité de l'une ou de l'autre hypothèse, mais de s'addresser à l'expérience pour apprendre à connaître l'état, de repos ou de mouvement, dans lequel se trouve l'éther à la surface terrestre."

[85]*Ibid.,* § 26.

[86]Cf. T. Hirosige, "Origins of Lorentz' Theory of Electrons and the Concept of the Electromagnetic Field," *Historical Studies in the Physical Sciences, 1* (1969), 151-209.

[87]H. A. Lorentz, *Over de Theorie der Terugkaatsing en breking van het licht* (Academisch Proefschrift, Leiden, 1875); *Collected Papers, 1,* 1-192. See p. 87.

[88]H. A. Lorentz, "La théorie électromagnétique de Maxwell et son application aux corps mouvants," *Arch. néerl., 25* (1892), 363-552; *Collected Papers,*

Lorentz' concern for the ether problem was related to his interest in the theory of optical and electromagnetic properties of matter, that is, it was closely connected with the inception and emergence of his electron theory. In turn he could not formulate the electron theory without picturing clearly the connection of material molecules with the ether. It is, therefore, not fortuitous that Lorentz' 1886 paper closes with the suggestion that the ether problem will eventually be reduced to the problem of the connection between matter and the ether, that is, to the question whether or not moving ponderable bodies communicate their motion to the ether within and around them.

Rayleigh also considered the ether problem in the light of the question of the connection between the ether and ponderable matter. In a paper written in 1887 but published in *Nature* in 1892, he stated that the ether problem "must evidently turn upon the question whether the aether at the earth's surface is at rest, absolutely or relatively to the earth."[89] Insofar as effects of the first order in v/c are concerned, Fresnel's theory agrees with all the facts. Michelson's experiment of 1881, as Lorentz noticed, leaves some doubt about its precision. Although the hypothesis of a stationary ether seems, at the present, to be advantageous, Rayleigh agreed, the problem cannot be considered settled. Thus, to decide the problem, he proposed to test experimentally whether or not the path of light passing near a heavy mass moving at high speed is affected by the motion of the mass.[90] If an effect is detected, then moving bodies can more or less communicate their motion to the surrounding ether.

The proposed experiment was carried out by Oliver Lodge.[91] Sending two bundles of light between two horizontal steel disks

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2, 164–343, esp. 228: "Il m'a semblé utile de développer une théorie des phénomènes électromagnétique basée sur l'idée d'une matière pondérable parfaitement perméable à l'éther et pouvant se déplacer sans communiquer à ce dernier le moindre mouvement. Certains faits de l'optique peuvent être invoqués à l'appui de cette hypothèse. . . ."

[89] Rayleigh, "Aberration," *Nature, 45* (1892), 449–502; *Scientific Papers, 3,* 542–553, esp. 544.

[90] *Ibid., Scientific Papers, 3,* 551.

[91] O. J. Lodge, [a] "Aberration Problems.—A Discussion concerning the Motion of the Ether near the Earth, and concerning the Connexion between Ether and Gross Matter; with some new Experiments," *Phil. Trans., A184* (1893), 727–804; [b] "Experiments on the Absence of Mechanical Connexion between Ether and Matter," *Phil. Trans., A189* (1897), 149–166.

along the same path but in opposite directions, he investigated whether or not putting the steel disks into rapid rotation produces a difference in the speeds of the two bundles of light. He also tested the effect of an electric or magnetic field applied perpendicularly to the disk. The experiment, which he repeated several times from 1891 through 1894, gradually improving the instrument, always gave negative results. What is important for us here is how Lodge viewed the problem situation of the experiment. He said that "the nature of the connexion between ether and gross matter is one of the most striking physical problems which now appear ripe for solution."[92] Explaining his intention he asked if, when a material body moves, the ether within the body moves as a whole with the latter or if only the modifications of the ether produced by the presence of matter travel, the ether itself remaining stationary.[93] Fizeau's 1851 experiment indicates the intermediate case which Fresnel supposed: in Fresnel's theory the ether surrounding material bodies is assumed to be always stationary. All the negative results of the experiments to detect an ether drift can be accounted for by assuming either that the ether is completely connected with matter, or, if FitzGerald's contraction hypothesis is granted, that it is entirely independent of matter. It is therefore desirable, argued Lodge, to test—with his experiment of rotating steel disks—whether the ether outside material bodies remains stationary or not. The conclusion he drew was that "the experiment proves, I think, that by the motion of ordinary masses of matter the ether is appreciably undisturbed, and raises a presumption in favour of the earth's motion being equally impotent."[94]

In 1895 Ludwig Albert Zehnder made an experiment to see whether the ether within opaque solid bodies moves with the bodies or not.[95] He attempted to test by optical methods whether the ether inside an iron cylinder is condensed or not by moving an iron piston back and forth in the cylinder. Having obtained a negative result, Zehnder concluded that solid bodies, like fluid matter, are transparent to the ether. Two years earlier, in 1893, Richard August Reiff had obtained an equation expressing the propagation of light within

[92] *Ibid.*, [a], p. 729.

[93] *Ibid.*, p. 731.

[94] *Ibid.*, p. 753.

[95] L. Zehnder, "Ueber die Durchlässigkeit fester Körper für den Lichtäther," *Ann. d. Phys.*, *55* (1895), 65–81.

moving dielectrics on the basis of Helmholtz' electrodynamics,[96] from which he derived the Fresnel coefficient.[97] In the same year Helmholtz had shown that integration of the Maxwellian stress over a closed surface in the ether gives a finite value and asserted that in an excited electromagnetic field a non-vanishing resultant force acts upon a portion of ether having finite volume, giving rise to a flow of the ether.[98] By bringing up the question of the movability of portions of the ether, Helmholtz' paper seems to have promoted interest in the connection between the ether and matter. A few years later Joseph Larmor, in his historical survey of the ether problem, wrote that Lodge's experiment denied any such motion of the ether as Helmholtz had predicted.[99] In 1897 William Craig Henderson and John Henry performed an experiment to detect directly by means of an interferometer the flow of the ether predicted by Helmholtz' theory to occur when the electric displacement and magnetic force are not zero.[100] They naturally obtained a negative result.

With the exception of Michelson's futile 1897 experiment to detect differences in the relative velocities of the ether and the earth at different altitudes,[101] most of the experiments on the ether problem made in the 1890's thus focused on the connection between the ether and matter. The same tendency is noticeable in theoretical treatments of this period.

It is evidently H. A. Lorentz who, in the period considered, studied most thoroughly the ether problem on the basis of electromagnetic theory. He augmented his investigations after 1892[102] to form the

96 For Helmholtz' electrodynamics see T. Hirosige, *op. cit.* (note 86), pp. 161-167.

97 R. Reiff, "Die Fortpflanzung des Lichtes in bewegten Medien nach der electrischen Lichttheorie," *Ann. d. Phys., 50* (1893), 361-367.

98 H. von Helmholtz, "Folgerungen aus Maxwell's Theorie über die Bewegung des reinen Aethers," *Ann. d. Phys., 53* (1893), 135-143.

99 J. Larmor, *Aether and Matter. A Development of the Dynamical Relations of the Aether to Material Systems, on the Basis of the Atomic Constitution of Matter, Including a Discussion of the Influence of the Earth's Motion on Optical Phenomena* (Cambridge, 1900), p. 19.

100 W. C. Henderson and J. Henry, "Experiments on the Motion of the Ether in an Electromagnetic Field," *Phil. Mag., 44* (1897), 20-26.

101 A. A. Michelson, "The Relative Motion of the Earth and the Ether," *Amer. Journ. Sci., 3* (1897), 475-478.

102 H. A. Lorentz, "Over de terugkaating van licht door lichamen die zich bewegen," *Versl. Kon. Akad. Wet., 1* (1892), 28-31; "De relative beweging van de aarde en den aether," *ibid., 1* (1892), 74-79; "De aberratietheorie van

monograph *An Essay on the Theory of Electrical and Optical Phenomena in Moving Bodies* published in 1895.[103] In this essay he accounted for the absence of the effect of the motion of the earth by proving the theorem of corresponding states within the first order approximation.[104] I shall discuss Lorentz' theory in section six; in the present section I am concerned with his view of the nature of the problem with which he was wrestling. He began the *Essay* with the words: "The question whether the ether takes part in the motion of ponderable bodies or not has not yet found an answer which satisfies all physicists."[105] He then explained why he had long thought Fresnel's stationary ether to be preferable. His reasons were, first, that the ether cannot be confined within solid or liquid walls and, second, that the Fresnel coefficient had been experimentally confirmed.[106] Lorentz now aimed to develop a theory on the fundamental hypothesis of a stationary ether which would account for all known facts. For this purpose he found the electron theory most suitable, because it enabled him to introduce the penetration of ether into matter into the equations in a satisfactory way.[107] Lorentz' words again show that in his conception the ether problem was almost synonymous with the question of the connection between ether and matter.

Before closing this section, a few words may be devoted to the theory of the propagation of light in moving bodies which Woldemar Voigt developed in 1887 on the basis of the elastic wave theory of light.[108] Voigt's theory is remarkable because of its success in deriv-

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Stokes," *ibid., 1* (1892), 97–103; "Over den infloed van de beweging der aarde op de voortplanting van het licht in dubbelbrekende lichamen," *ibid., 1* (1893), 149–154. English translations of these papers appear in *Collected Papers, 4,* 215–218; 219–223; 224–231; 232–236, respectively.

[103] H. A. Lorentz, *Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern* (Leiden, 1895); *Collected Papers, 5,* 1–138.

[104] For Lorentz' theorem of corresponding states, see T. Hirosige, "Electrodynamics before the Theory of Relativity, 1890–1905," *Japanese Studies in the History of Science,* No. 5 (1966), pp. 1–49, esp. pp. 14–18.

[105] H. A. Lorentz, *op. cit.* (note 103), *Collected Papers, 5,* 1. "Die Frage, ob der Aether an der Bewegung ponderabler Körper theilnehme oder nicht, hat noch immer keine alle Physiker befriedigende Beantwortung gefunden."

[106] *Ibid.,* pp. 1–3.

[107] *Ibid.,* p. 7.

[108] W. Voigt, "Theorie des Lichtes für bewegte Medien," *Gött. Nachr.* (1887), pp. 177–238; *Ann. d. Phys., 35* (1888), 370–396; 524–551.

ing the Fresnel coefficient and other various experimental results by taking into consideration mechanical forces acting between the ether and material particles and by assuming that the ether always remains stationary without sharing the motions of material bodies. We may call it the translation of Lorentz' electron theory into the language of elastic wave theory.

5. APOGEE OF THE ETHER PROBLEM

Physicists studied the connection between the ether and matter with still greater eagerness from the end of the nineteenth century until well into the first decade of the twentieth. This last rise of interest sprang from, among other things, the 1898 Düsseldorf meeting of the Society of German Scientists and Physicians. On this occasion, members of the society organized a special conference to discuss the ether problem. For a few years previous to this, the physics sections of these meetings had held special colloquia on selected topics for which younger scientists were asked to prepare review papers. The preparatory committee of the physics section of the 1898 meeting, consisting of Ludwig Boltzmann, Georg Hermann Quincke, and Emil Gabriel Warburg, adopted "the problem concerning translatory motion of the ether" as the subject of the special discussion.[109] In accordance with this decision, Boltzmann asked Lorentz, who had "deeply studied this problem," to participate in the discussion of the "behavior of the ether in moving media."[110] At the 1897 meeting it had also been decided to invite Dutch scientists to the 1898 meeting, and Lorentz was among those invited.[111] He accepted the invitation with great pleasure.[112] For him this was the first occasion to participate in an international scientific meeting, and he experienced much delight in getting acquainted with German colleagues.[113] The German physicists, too, to judge from the exalted vein of some of their letters, seem to have been greatly stimulated by

[109] F. Klein to Lorentz, 20 October 1897, Algemeen Rijksarchief, den Haag, Lorentz Papers 1.

[110] L. Boltzmann to Lorentz, 13 October 1897, Algemeen Rijksarchief, den Haag, Lorentz Papers 1.

[111] Klein to Lorentz, *op. cit.* (note 109).

[112] Lorentz to Boltzmann, 20 October 1897, Algemeen Rijksarchief, den Haag, Lorentz Papers 1.

[113] G. L. de Haas-Lorentz, ed., *H. A. Lorentz–Impressions of His Life and Work* (Amsterdam, 1957), p. 89.

the discussion of the ether problem with Lorentz. They concluded that it was desirable to repeat experiments on the movability of the ether.[114] DesCoudres was entrusted to repeat Fizeau's experiment on the change of the azimuth of the plane of polarization produced when polarized light is refracted.[115] Stimulated by conversations with Lorentz at the meeting, Max Planck developed the idea of rescuing the Stokes theory of aberration by attributing compressibility to the ether.[116]

Willy Wien prepared the review paper for the discussion. He began his paper with the words: "The questions whether the luminiferous ether takes part in the motion of bodies or not, and whether or not movability can be ascribed to the ether at all have long occupied physicists . . . ."[117] He next discussed successively the questions whether or not, one, the motion of the ether, if it exists at all, expends energy, two, the ether possesses inertial mass, and, three, the motion of solid bodies is communicated to the ether. It is clear that what is of central interest here is not the question of an absolute frame of reference for motion, but the problem of the physical properties of the ether and its connection with ponderable matter. Having examined existing theories and experiments concerning these questions, Wien set forth a program for further research.[118] He noted that there is little chance of achieving a successful theory that is based on the concept of a movable ether without inertia. On the other hand, the use of the notion of a stationary ether leads to a

[114] *Verhandlungen der Gesellschaft deutscher Naturforscher und Ärzte, 70* (1898), 2. Teil, 1. Hälfte, p. 83.

[115] DesCoudres to Lorentz, 18 November 1898, Algemeen Rijksarchief, den Haag, Lorentz Papers 1.

[116] M. Planck to Lorentz, 21 October 1898, Algemeen Rijksarchief, den Haag, Lorentz Papers 1. Planck's idea was not published as a paper but is discussed in some detail in H. A. Lorentz, "De aberratietheorie van Stokes in de onderstelling van een aether niet overal dezelfde dichtheid heeft," *Versl. Kon. Akad. Wet.,* 7 (1899), 523–529. The French translation appears in *Collected Papers, 4,* 245–251.

[117] W. Wien, "Ueber die Fragen, welche die translatorische Bewegung des Lichtäthers betreffen," *op. cit.* (note 114), pp. 49–56. The full paper is published in *Ann. d. Phys., 65* (1898), Beilage, i–xviii. Quotation on p. i: "Die Frage, ob der Lichtäther an den Bewegungen der Körper theilnehme oder nicht, und ob ihm überhaupt Beweglichkeit zuzuschreiben ist, hat die Physiker seit langem beschäftigt und zahllos sind die Annahmen und Vermuthungen, die man für die Eigenschaften des Trägers der electromagnetischen Erscheinungen aufzustellen für nöthig hielt."

[118] *Ibid., Ann. d. Phys., 65* (1898), Beilage, xvii–xviii.

violation of the principle of action and reaction and to theoretical conclusions that disagree with some experimental results. The experimental results contradicting a stationary ether are those of the Michelson-Morley 1887 ether drift experiment, of E. Mascart's experiment on rotation of the plane of polarization by rock crystal, which gave a negative result in contradiction to Lorentz' theoretical prediction that charged condensers do not induce a magnetic field notwithstanding the motion of the earth, and of Fizeau's experiment on the change of the azimuth of the plane of polarization when light is refracted. It is therefore very desirable to repeat these experiments. If they give results refuting the notion of a stationary ether, the only alternative would be to take into account the effect of gravity upon the ether. This is equivalent to bestowing an inertial mass on the ether and therefore will, at the same time, dissolve the difficulty concerning the principle of action and reaction. In this case, the result of Lodge's experiment would have to be disposed of by assuming that a small terrestrial body does not appreciably drag the ether because of its small gravity. The explanation of aberration will be invalidated if the earth puts the surrounding ether in motion by the action of its gravitational force, but the difficulty might be solved by reconsidering the hydrodynamics of a gravitational fluid. The task imposed on theoreticians is to predict cases where motion of the ether is expected to be detected.

Wien's paper was followed by Lorentz' supplementary paper.[119] Lorentz agreed with Wien that the issue was the physical properties of the ether and its connection with ponderable matter. He said: "Ether, ponderable matter, and, we may add, electricity are the building stones from which we compose the material world, and if we could know whether matter, when it moves, carries the ether with it or not, then the way would be opened before us by which we could further penetrate into the nature of these building stones and their mutual relations."[120] As to the movability of the ether, Lorentz

[119]H. A. Lorentz, "Die Fragen, welche die translatorische Bewegung des Lichtäthers betreffen," *op. cit.* (note 114), pp. 56-65; *Collected Papers,* 7, 101-115.

[120]*Ibid.,* p. 56: "Aether, ponderable Materie, und wir wollen hinzufügen Elektricität, sind die Bausteine, aus denen wir die materielle Welt zusammensetzen, und wenn wir einmal wüssten, ob die Materie bei ihrer Bewegung den Aether mit sich fortführe oder nicht, so wäre uns ein Weg gegeben, auf dem wir etwas weiter in das Wesen dieser Bausteine und ihrer gegenseitigen Beziehungen eindringen können."

opposed Wien and defended the stationary ether: the difficulty of the Stokes theory is a kinematical one and can by no means be solved by assuming gravitational action. On the other hand, there are many facts that support the view that material bodies are transparent to the ether. Therefore, "the main question is and will be that of the relation of the theory of a stationary ether, after it has disposed of aberration, to the other facts of the electrical as well as of the optical domain."[121] For Lorentz, who admitted a stationary ether, there doubtlessly existed relative motion between terrestrial bodies and the ether. It is therefore quite natural that he considered it the fundamental problem to find reasons why physical effects of the motion do not reveal themselves. In the paper under consideration, he put forward his plan of explaining the experiments adduced by Wien by means of the theory of electrons. As to the principle of action and reaction, he asserted that since it is a principle obtained within the limits of daily experience it need not be valid for the elementary interaction between the ether and ponderable matter.[122]

Without doubt, the Düsseldorf meeting greatly promoted interest in the ether problem among German speaking physicists. They proposed, discussed theoretically, and carried out various experiments designed to probe into the connections between moving bodies and the ether. The experiment on the change of the azimuth of the plane of polarization produced when light enters a glass layer, which was entrusted to DesCoudres, seems to have been eventually abandoned. There is no report of its result as far as I know. Gustav Mie remarked on the flow of the ether predicted by Helmholtz' theory at the Düsseldorf meeting[123] and later published two papers discussing hydrodynamical motion of the ether on the basis of Helmholtz' theory.[124] The Dutch physicist Hermann Haga, who had attended the meeting, repeated Klinkerfues' 1870 experiment immediately after the Düsseldorf meeting.[125] Klinkerfues'

[121] *Ibid.*, p. 59: "Die Hauptfrage ist und bleibt, wie sich die Theorie des ruhenden Aethers, nachdem sie mit der Aberration abgerechnet hat, zu den sonstigen Thatsachen sowohl auf electrischem wie auch auf optischem Gebiete verhält."

[122] *Ibid.*, p. 64.

[123] *Ibid.*, p. 65.

[124] G. Mie, "Ueber mögliche Aetherbewegungen," *Ann. d. Phys.*, *68* (1899), 129–134; "Ueber die Bewegung eines als flüssig angenommenen Aethers," *Phys. Zeits.*, *2* (1801), 319–325.

[125] H. Haga, "Ueber den Versuch von Klinkerfues," *Arch. néerl.*, *5* (1900), 583–586; "L'expérience de Klinkerfues," *ibid.*, *6* (1901), 765–772.

experiment supposedly had shown a change of the positon of the absorption line of bromine with a change in the relative direction of rays with respect to the motion of the earth; Haga confirmed that such an effect was absent.[126] As to the experiment on the rotation of the plane of polarization by rock crystal, Richard Wachsmuth and Otto Schönrock asserted in 1902 that the instrument used by Mascart had not been perfect and hence the experiment should be repeated.[127] In the same year Egon von Oppolzer proposed to observe the deflection of light from the stars which might be produced by the rotation of the ether that is caused by the diurnal rotation of the earth.[128] Physicists also discussed detection of differences in the intensities of light travelling in different directions by means of a bolometer, a method Fizeau had once proposed.[129] In 1902 H. A. Lorentz and Alfred Heinrich Bucherer debated the feasibility of this experiment and eventually agreed that it would give a negative result.[130] Bucherer then directed his student Paul Nordmeyer to carry out the experiment, and Nordmeyer obtained the predicted negative result.[131] In 1904 W. Wien and Alfred Fritz Schweitzer independently of each other proposed another method of detecting differences in the speeds of light travelling westward and eastward.[132] A. A. Michelson criticized their proposal, indicating that a factor

[126]W. Klinkerfues, *op. cit.* (note 48), *Astron. Nachr.*, *76* (1870), 33–38.

[127]R. Wachsmuth und O. Schönrock, "Beiträge zu einer Wiederholung des Mascart'schen Versuches," *Verh. Deutsch. Phys. Ges.*, *4* (1902), 183–188.

[128]Egon v. Oppolzer, "Erdbewegung und Aether," *Ann. d. Phys.*, *8* (1902), 898–907.

[129]A. H. L. Fizeau, "Constatation du mouvement de la terre par les radiations calorifiques," *Cosmos*, *1* (1853), 689–692; *Ann. d. Phys.*, *92* (1854), 652–655.

[130]A. H. Bucherer to Lorentz, 15 February 1902, 6 April 1902, and 8 December 1902, Algemeen Rijksarchief, den Haag, Lorentz Papers 2. H. A. Lorentz, "The Intensity of Radiation and the Motion of the Earth," *Proc. Roy. Acad. Amsterdam*, *4* (1902), 678–681; *Collected Papers*, *5*, 167–171. The original Dutch version, *Versl. Kon. Akad. Wet.*, *10* (1902), 804–808; A. H. Bucherer, "Über den Einfluss der Erdbewegung auf die Intensität des Lichtes," *Ann. d. Phys.*, *11* (1903), 270–283.

[131]P. Nordmeyer, "Über den Einfluss der Erdbewegung auf die Verteilung der Intensität der Licht- und Wärmestrahlung," *Ann. d. Phys.*, *11* (1903), 284–302.

[132]W. Wien, "Über einen Versuch zur Entscheidung der Frage, ob sich der Lichtäther mit der Erde bewegt oder nicht," *Phys. Zeits.*, *5* (1904), 585–586. A. Schweitzer, "Über die experimentelle Entscheidung der Frage, ob sich der Lichtäther mit der Erde bewegt oder nicht," *Phys. Zeits.*, *5* (1904), 809–811.

they had overlooked in their plan would invalidate the proposed experiment.[133]

From about 1900 English speaking scientists, too, became remarkably active pursuing the inquiry into the ether problem. To Kelvin, speaking at the Royal Institution in 1900, the question of "how could the earth move through an elastic solid, such as essentially is the luminiferous ether," was one of the two clouds hanging over nineteenth century physics.[134] In 1901 Frederick Thomas Trouton performed an experiment which had been proposed by G. F. FitzGerald, who died on 22 February 1901.[135] In the experiment a condenser whose plates are laid in the direction of the motion of the earth is charged with electricity. Since a moving electric charge is equivalent to an electric current, the condenser must acquire, in addition to electrostatic energy, a certain amount of magnetic energy. Since this magnetic energy may come from the kinetic energy of the earth, the condenser must receive an impulse when it is charged with or discharges electricity. Trouton's experiment showed no effect. He then investigated the consequence of another assumption, namely, that the energy in question is supplied by the source of electricity for charging the condenser. If this is the case, a couple which tends to turn the plates of the condenser into the direction perpendicular to the earth's motion should act upon the condenser when it is charged. Trouton and Henry R. Noble tried to find such a couple in 1903 but failed.[136]

In 1902, preceding Wachsmuth's proposal to repeat Mascart's experiment, Rayleigh carried out an experiment on the rotation of the plane of polarization and obtained a negative result.[137] Rayleigh

[133] A. A. Michelson, "Relative Motion of Earth and Aether," *Phil. Mag., 8* (1904), 716–719.

[134] Kelvin, "Nineteenth Century Clouds over the Dynamical Theory of Heat and Light," *Phil. Mag., 2* (1901), 1–40; *Journ. Roy. Inst., 16* (1902), 363–397; reproduced in *The Royal Institution Library of Science, Physical Series* (London, 1970), *5,* 324–358, esp. 324.

[135] F. T. Trouton, "The Results of an Electrical Experiment, Involving the Relative Motion of the Earth and Ether, Suggested by the late Prof. FitzGerald," *Trans. Roy. Soc. Dublin, 7* (1902), 379–384; *The Scientific Writings of G. F. FitzGerald,* pp. 557–565.

[136] F. T. Trouton and H. R. Noble, "The Mechanical Forces Acting on a Charged Condenser Moving through Space," *Phil. Trans., A202* (1904), 165–181.

[137] Rayleigh, "Is Rotatory Polarization Influenced by the Earth's Motion?" *Phil. Mag., 4* (1902), 215–220; *Scientific Papers, 5,* 58–62.

then attempted to detect the double refraction that would result from the motion of transparent bodies if the Lorentz-FitzGerald contraction was a real effect. This experiment, too, was fruitless.[138] In 1904 D. B. Brace repeated it and concluded from his negative result that the contraction hypothesis was not tenable.[139] Larmor contradicted him, asserting that Brace's result could be accounted for by the theorem of corresponding states.[140] In the following year Brace repeated Fizeau's experiment on the change of the azimuth of the plane of polarization[141] and Mascart's experiment on the rotation of the plane of polarization.[142] Obtaining negative results in both experiments he concluded that the absence of the first order effect was established.

As for the second order effect, E. W. Morley and Dayton Clarence Miller in 1905 repeated Michelson-Morley's 1887 experiment and confirmed the negative result.[143] It should here be noted that their experiment was intended not only to detect an ether drift but, in addition, to test the contraction hypothesis. They reasoned that since the contraction should probably depend on physical properties of the solid body, it would be possible to test the contraction hypothesis by detecting different contractions of mounting beds made of different materials.[144]

All the investigations at the turn of the century thus suggested two conclusions that contradict each other: that the ether is mechanically independent of ponderable matter, and that one cannot in any way

[138] Rayleigh, "Does Motion through the Aether Cause Double Refraction?" *Phil. Mag., 4* (1902), 678–683; *Scientific Papers, 5,* 63–67.

[139] D. B. Brace, "On Double Refraction in Matter Moving through the Aether," *Phil. Mag., 7* (1904), 317–329.

[140] J. Larmor, "On the Ascertained Absence of Effects of Motion through the Aether, in Relation to the Constitution of Matter, and on the FitzGerald-Lorentz Hypothesis," *Proc. Phys. Soc. London, 18* (1904), 253–258; *Mathematical and Physical Papers, 2,* 274–280.

[141] D. B. Brace, "The Aether 'Drift' and Rotary Polarization," *Phil. Mag., 10* (1905), 383–396.

[142] D. B. Brace, "A Repetition of Fizeau's Experiment on the Change Produced by the Earth's Motion in the Rotation of a Refracted Ray," *Phil. Mag., 10* (1905), 591–599.

[143] E. W. Morley and D. C. Miller, [a] "On the Theory of Experiments to Detect Aberrations of the Second Degree," *Phil. Mag., 9* (1905), 669–680; [b] "Report of an Experiment to Detect the FitzGerald-Lorentz-Effect," *ibid.,* pp. 680–685.

[144] *Ibid.,* [a], p. 669.

detect the effects of the ether drift that must exist at the surface of the earth if the ether is independent of the motions of material bodies. Lorentz and other theoretical physicists made strenuous efforts to reconcile these conclusions. Lorentz had already in 1895 been able to account for the absence of observable effects of the first order in his *Essay.* As to the Michelson-Morley experiment which was of second order, however, he had had to be satisfied with explaining it by introducing the *ad hoc* hypothesis of contraction. He considerably advanced his theory in 1899.[145] Introducing a transformation of coordinate variables which in its form was identical with the relativistic transformation, he proved that the contraction of moving bodies is necessarily required to secure the theorem of corresponding states in the first order effects. In the same paper he also showed that the mass of any material body will always be altered by its motion.

The "dynamical theory of luminiferous ether" that J. Larmor had developed since 1893 was very similar to Lorentz' theory in many respects. In one of a series of papers on this subject published in 1897, Larmor proved the theorem of corresponding states to the second order by using the same transformation of coordinate variables as Lorentz.[146] Then, in his book *Aether and Matter,* completed by the end of 1898 and published in 1900, he introduced transformations of coordinate variables and field variables of the same form as those used in the theory of relativity.[147] If he had made full use of these transformations, he could have proved the strict correspondence of states between two physical systems of which one is at rest and the other in motion and contracted. Larmor indeed remarked that the electromagnetic equations in the moving system, written in terms of the new variables, have the same form as the Maxwell equations in the stationary system. Nevertheless he called his theory an "approximation carried to the second order"

[145] H. A. Lorentz, "Vereenvoudige theorie der electrische en optische verschijnselen in lichamen die zich bewegen," *Versl. Kon. Akad. Wet.,* 7 (1899), 507–522. The French translation appears in *Collected Papers, 5,* 139–155. For a brief account of the theory developed in this paper see T. Hirosige, *op. cit.* (note 104), pp. 24–27.

[146] J. Larmor, "A Dynamical Theory of the Electric and Luminiferous Medium. Part III: Relations with Material Media," *Phil. Trans., A190* (1897), 205–300; *Mathematical and Physical Papers, 2,* 11–132. For Larmor's theory see T. Hirosige, *op. cit.* (note 104), pp. 10–14.

[147] J. Larmor, *op. cit.* (note 99), pp. 173–179.

and was satisfied with instituting "a correspondence which will be correct to the second order."[148] Larmor's failure to fully exploit his results must be due to his failure to recognize the problem situation that Henri Poincaré had emphasized since 1895. Poincaré occasionally expressed the view that no influence of the earth's motion on optical phenomena will be detected in any order of approximation and that a theory will one day be formulated from which physicists can derive this prediction. At the International Congress of Physics held in Paris in 1900, he praised Lorentz' theory as the most satisfactory one among existing theories but expressed discontent that it had required new hypotheses for each new experimental result.[149] Poincaré's criticism and the negative results of the experiments by Trouton and Noble, Rayleigh, and Brace motivated Lorentz to make fresh efforts to reach, in 1904, a theory that was able "to show, . . . without neglecting terms of one order of magnitude or another, that many electromagnetic actions are entirely independent of the motion of the system."[150] This theory of 1904, refined and augmented by Poincaré in the following year, furnished a satisfactory answer to the question of the "relation. . .[of] the theory of a stationary ether, after it has disposed of the aberration, with other facts in electrical as well as optical domains" which Lorentz had posed at the 1898 Düsseldorf meeting. It may therefore be viewed as the end of the ether problem.

6. CHARACTER OF LORENTZ' THEORY

The Lorentz-Poincaré theory has in recent years been the subject of active discussion by historians of physics. It might therefore be superfluous to describe its content in detail. It is, I believe, nonetheless desirable to reconsider its character in relation to the ether

[148] *Ibid.*, p. 173.

[149] H. Poincaré, "Relations entre la physique expérimentale et la physique mathématique," *Rapports présentés au Congrès international de Physique de 1900* (Paris, 1900), *1,* 1–29; *La science et l'hypothèse* (Paris 1902), Chaps. 9 and 10.

[150] H. A. Lorentz, "Electromagnetische verschijnselen in een stelsel, dat zich met willekeurige snelheid kleiner dan die van het licht bewegt," *Versl. Kon. Akad. Wet. Amst., 12* (1904), 986–1009. The English version: "Electromagnetic Phenomena in a System Moving with Any Velocity Smaller than That of Light," *Proc. Roy. Acad. Amsterdam, 6* (1904), 809–831; *Collected Papers, 5,* 172–197.

problem, because it will be necessary to evaluate Lorentz' and Poincaré's theoretical efforts against the background of the contemporary problem situation to demonstrate the novelty of Einstein's approach.

Looking back on the history of the ether problem as we have considered it in the foregoing sections, we may safely conclude that the ether problem had not been viewed as a search for an absolute frame of reference for motion. Throughout the whole period considered we can find almost no argument that the ether provides the absolute frame of reference supposed by Newton. The sole exception, it seems, is the view Lodge expressed in 1898. He wrote in a letter to the *Philosophical Magazine,* criticizing W. Sutherland's objection to the Michelson-Morley experiment, that "the whole of this subject [the ether problem] indicates that the aether is a physical standard of rest; and that motion relative to it, which is becoming cognisable by us, is in that sense an ascertained absolute motion."[151] He arrived at his view by discussing the question of the correct expression for the kinetic energy of a moving body.[152] He argued that it seems physically absurd that the amount of kinetic energy of a body, $1/2\ mv^2$, depends on the coordinate system in which the velocity v is measured. Instead, physicists should attach real meaning to an absolute velocity. On the one hand, the ether freely penetrates material bodies and, in turn, exerts no resistance to bodies moving through it, since it lacks viscosity at its boundaries. On the other hand, all interactions between portions of matter are thought to be mediated by the ether. It may therefore be asserted, Lodge declared, that the kinetic and potential energies differ categorically from each other: the former is a property only of matter and the latter only of the ether. Lodge concluded from this that it is reasonable to measure the kinetic energy by taking the ether as the reference of rest. In this sense the ether provides the absolute reference system for velocity. Lodge's argument shows that he based his assertion on the results of contemporary discussion of the connection between the ether and ponderable matter. In other words, his view of the ether as an abso-

[151] O. J. Lodge, "Note on Mr. Sutherland's Objection to the Conclusiveness of the Michelson-Morley Aether Experiment," *Phil. Mag., 46* (1898), 343–344, esp. 344.

[152] O. J. Lodge, "On the Question of Absolute Velocity and on the Mechanical Function of an Aether, with Some Remarks on the Pressure of Radiation," *Phil. Mag., 46* (1898), 414–426.

lute system of reference for motion was a consequence of the pursuit of the ether problem, but not the motive for it. Further, Lodge's view seems to have provoked no serious response.

The foundation of Lorentz' theory is the assumption of a stationary ether and Maxwell's equations describing the electromagnetic state of the stationary ether. Since the interaction between charged particles constituting material bodies and an electromagnetic field depends on the motion of the charged particles with respect to the ether, electromagnetic phenomena occurring in experimental devices that share the motion of the earth are naturally affected by this motion. Lorentz showed that the effects produced compensate each other, thus giving no detectable trace of the influence of the motion. I shall cite an example from Lorentz' article on the electron theory in the *Encyclopedia of Mathematical Sciences* edited by Felix Klein.[153] When a conductor carrying an electric current $I = \rho u$ moves with the earth, it exerts the electric force

$$\mathrm{d}' = \frac{1}{c}\,\mathrm{grad}\,(\mathrm{w}\cdot\bar{\mathrm{a}}') \tag{1}$$

on an external electric charge. Here w denotes the velocity of the earth, $\bar{\mathrm{a}}'$ is determined by the equation

$$\Delta\bar{a}' - \frac{1}{c^2}\,\ddot{\bar{a}}' = -\frac{1}{c}\,\rho u. \tag{2}$$

(A bar over a letter indicates an averaged value.) Such a force, however, is not observed. Lorentz explained this by noting that force (1) also acts upon electric charges within the conductor. The force induces within the conductor a charge density $\bar{\rho} = 1/c^2\ (wI)$, and this charge in turn gives rise to a scalar potential whose value is $1/c\ (w\bar{a}')$. The contribution of this potential to the electric force cancels out force (1).

It is, however, impossible to prove universally the absence of effects of motion by confirming cancellation in each separate case. Lorentz sought to surmount this limitation by having recourse to the theorem of corresponding states. The theorem allowed him to discuss phenomena occurring in a physical system that is moving

[153] H. A. Lorentz, "Weiterbildung der Maxwellschen Theorie. Elektronentheorie," *Encyklopädie der mathematischen Wissenschaften* (Leipzig, 1904), *5*, Nr. 14. The example is cited from pp. 260–261.

with respect to the ether in terms of phenomena in a system fixed with respect to the ether by carrying out a suitable transformation of variables. But in 1903 the theory was still essentially an approximate theory to the first order. In his *Encyclopedia* article which was finished in December 1903, Lorentz summarized the outlook of theoretical inquiry.[154] If we assume that the molecular forces are modified by motion in the same way as the electric force, then we can derive the contraction of material bodies and account for the result of Trouton-Noble's experiment. But this manner of explanation has the defect that the thermal motion of molecules is entirely neglected. If we admit that material mass, too, undergoes the same change as electromagnetic mass when the body moves, the difficulty might be solved. At the same time Lorentz accepted Poincaré's criticism that the present theory is forced to introduce *ad hoc* hypotheses for each new phenomenon. He urgently hopes, he said, that a theory can be found which can show from fundamental hypotheses and in a general manner that electromagnetic phenomena on the earth are independent of the motion of the earth. He realized this hope himself with his 1904 theory.[155]

Lorentz' 1904 theory is based on the Maxwell equations

$$\left.\begin{aligned} &\operatorname{div} \mathrm{d} = \rho, \ \operatorname{div} \mathrm{h} = 0, \\ &\operatorname{curl} \mathrm{h} = \frac{1}{c}\left(\frac{\partial \mathrm{d}}{\partial t} + \rho \mathrm{v}\right), \\ &\operatorname{curl} \mathrm{d} = -\frac{1}{c}\frac{\partial \mathrm{h}}{\partial t}, \end{aligned}\right\} \tag{3}$$

which are valid in a coordinate system (x, y, z) fixed with respect to the ether, and on the expression of the force exerted on electric charge

$$\mathrm{f} = \mathrm{d} + \frac{1}{c}[\mathrm{v}\,\mathrm{h}]. \tag{4}$$

In these equations v is the velocity of an electric charge in the stationary coordinate system which is fixed with respect to the ether. Lorentz' aim is to investigate the electromagnetic phenomena that occur in a physical system travelling with a uniform velocity w in the

[154] H. A. Lorentz, *ibid.*, pp. 277–279.
[155] H. A. Lorentz, *op. cit.* (note 150).

direction of the x-axis and to demonstrate that the phenomena do not exhibit any influence of the motion. Lorentz found that to treat the problem by making use of equations (3) and (4) in the stationary system involves enormous complications. To avoid them he introduced the following variables as a mathematical device:

$$x' = kl(x - wt),\ y' = lu,\ z' = lz,\ t' = kl\left(t - \frac{w}{c^2}x\right), \tag{5}$$

where

$$k = \frac{1}{\sqrt{1 - w^2/c^2}}$$

and the coefficient l is to be considered a function of w, whose value is 1 for $w = 0$ and which, for small values of w, differs from unity no more than by an amount of the second order. Further,

$$\begin{aligned} d'_x &= \frac{1}{l^2} d_x,\ d'_y = \frac{k}{l^2}\left(d_y - \frac{w}{c}h_z\right),\ d'_z = \frac{k}{l^2}\left(d_z + \frac{w}{c}h_y\right), \\ h'_x &= \frac{1}{l^2} h_x,\ h'_y = \frac{k}{l^2}\left(h_y + \frac{w}{c}d_z\right),\ h'_z = \frac{k}{l^2}\left(h_z - \frac{w}{c}d_y\right), \end{aligned} \tag{6}$$

and, for electric charge and the relative velocity of the electron with respect to the physical system considered, $u\,(v = w + u)$,

$$\left.\begin{aligned} \rho' &= \frac{1}{kl^3}\rho \\ u'_x &= ku_x,\ u'_y = ku_y,\ u'_z = ku_z. \end{aligned}\right\} \tag{7}$$

By substituting these expressions for variables in (3) and (4), he obtained equations in primed variables that are of nearly the same from as (3) and (4), that is, the Maxwell equations in the rest system of the ether. This is a result which we can easily anticipate because equations (5) and (6) are of the same form as the relativistic Lorentz transformations. That the result is not "exactly the same form" but "nearly the same form" is due to the difference between equation (7) and the relativistic transformations of electric charge and velocity.

Lorentz next expressed the positions of particles constituting the physical system Σ in terms of relative coordinates $x_r = x - wt$, $y_r = y$, $z_r = z$. He assumed system Σ' to consist of particles that are the same as in Σ but at rest relative to the ether and that have the coordinates $x = klx_r$, $y = ly_r$, and $z = lz_r$. The system Σ' may be obtained by enlarging the system Σ by the ratios 1 to lk in the x-direction, and 1 to l in the y- and z-directions. Then electromagnetic phenomena in the system Σ' will be described by the same equations as those obtained from equations (3) and (4) by the transformation above. This result enabled Lorentz to treat electromagnetic phenomena in a physical system moving relative to the ether by considering phenomena in the system Σ' which is at rest with respect to the ether and associated with the system Σ by definite relations. He arrived at the theorem of corresponding states without neglecting any terms of any order of magnitude. However, to settle the problem completely, Lorentz still needed to introduce a certain number of hypotheses. First, he assumed that an electron moving with velocity w relative to the ether is contracted by the fraction $1/kl$ in the direction of the motion and $1/l$ in the directions perpendicular to it. Second, he assumed that intermolecular forces are modified by motion in the same way as the electrostatic force. This hypothesis leads to the contraction of macroscopic bodies. Third, he assumed that the mass of the electron is entirely electromagnetic. From the requirement that the states of the imaginary system Σ' obtained by his transformation are the ones which occur in reality, he then concluded that $l = 1$. The conclusion implies that the contraction of moving electrons and the deformation of physical systems when transformed from Σ to Σ' occur only in the direction of motion. Fourth, Lorentz assumed that the entire mass of all kinds of particles is modified by motion in the same way as the electromagnetic mass of the electron. This hypothesis assures the correspondence of states even in the case when molecules are in thermal motion.

In his 1904 theory Lorentz demonstrated the absence of effects of motion in quite a general manner by means of the theorem of corresponding states rather than showing directly a mutual compensation of effects. The underlying idea, however, was still that the motion does produce certain effects but that they cannot be detected because they cancel each other. Lorentz' own words show this. For example, in *The Theory of Electrons*, which was published in 1909 on the basis of his 1906 lectures delivered at Columbia University, after a detailed description of his 1904 theory Lorentz stated that the "chief difference" between his theory and Einstein's consists in the latter "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."[156]

7. POINCARÉ'S "PRINCIPLE OF RELATIVITY"

Poincaré received Lorentz' 1904 theory with great enthusiasm. In the next year, 1905, Poincaré gave a mathematically refined form to Lorentz' theory, discussed the stability of the deformable electron, and attempted to extend it so as to include gravity.[157] He not only demonstrated that the so-called Lorentz transformation formed a group, but even, though implicitly, used four-dimensional representation.[158] More remarkably, he had for about ten years been proposing "the principle of relativity." He was fascinated with Lorentz' 1904 theory because it seemed to him to embody his principle of relativity. It is because of these facts that Whittaker and his followers regard Poincaré as the founder of the theory of relativity. But what Poincaré called the "principle of relativity," to judge by its purport, cannot be regarded as identical with the principle of relativity as we understand it in terms of the theory of relativity. It is not given the status of a postulate as is the latter in the theory of relativity. From the analysis of the situation of the ether problem as it was understood at the time, Poincaré anticipated the principle of relativity as an empirical law, looking forward to a theory which could explain or prove the principle.

Poincaré's first statement concerning the principle of relativity appears in his 1895 paper dealing with Larmor's electromagnetic theory. There he stated that the conclusions drawn from various empirical facts can be summarized by the assertion that "it is impossible to make manifest the absolute motion of matter, or rather the motion of ponderable matter relative to the ether."[159] In 1899, in

[156] H. A. Lorentz, *The Theory of Electrons and Its Applications to the Phenomena of Light and Radiant Heat* (Leipzig, 1909), p. 230.

[157] H. Poincaré, [a] "Sur la dynamique de l'électron," *Comptes Rendus, 140* (1905), 1504–1508; *Oeuvres de Henri Poincaré, 9,* 489–493. [b] "Sur la dynamique de l'électron," *Rendiconti del Circolo matematico di Palermo, 21* (1906), 129–176; *Oeuvres, 9,* 494–550. The content of paper [b] is carefully analyzed by Arthur I. Miller, *op. cit.* (note 16).

[158] Arthur I. Miller, *op. cit.* (note 16), p. 252.

[159] H. Poincaré, "A propos de la théorie de M. Larmor. (3)," *L'éclairage électrique, 5* (1895), 5–14; *Oeuvres, 9,* 395–413. Quotation is from p. 412: "Il est impossible de rendre manifeste le mouvement absolu de la matière, ou mieux le mouvement relatif de la matière pondérable par rapport à l'éther."

the lecture at the Sorbonne, he said: "I regard it very probable that optical phenomena would depend only on the relative motion of material bodies . . ., and this would be valid not disregarding quantities of the second or third order in the aberration constant, but rigorously. As experiments become more and more exact, this principle will be verified with increasing precision."[160] Poincaré expected that "a well constructed theory should be able to demonstrate the principle at once and with perfect rigor."[161] He expressed the same view again at the Paris International Congress of Physics in the following year: "I do not believe . . . that more exact observations will ever make evident anything else but the relative displacement of material bodies."[162] For all orders "the same *explanation* must be found. . . . [Everything] tends to show that this explanation would serve equally well for the terms of the higher order, and that the *mutual destruction* of these terms will be rigorous and absolute" (italics mine).[163] That Poincaré's desire for rigor motivated and guided Lorentz' efforts toward his 1904 theory is seen from the previously cited conclusion of his *Encyclopedia* article as well as from the introduction of his 1904 paper.[164] At the same time Poincaré's words show that in the physical interpretation of the theory he completely agreed with Lorentz. For him, too, effects of motion exist but do not manifest themselves because of mutual compensation.

[160] H. Poincaré, *Électricité et optique. La lumière et les theories électrodynamiques. Leçon professees à la Sorbonne en 1888, 1890 et 1899* (Paris, 1901), p. 536: "Je regarde comme très probable que les phénomènes optiques ne dépendent que des mouvements relatifs des corps matériels en presence. . . et cela non pas aux quantités près de l'ordere de carré ou du cube de l'aberration, mais rigoureusement. A mesure que les expériences deviendront plus exactes, ce principe sera vérifié avec plus de precision."

[161] *Ibid.:* "Une théorie bien faite devrait permettre de démontrer le principe d'un seul coup dans toute sa rigueur."

[162] H. Poincaré, *op. cit.* (note 149), *Rapports, 1,* 22; *La science et l'hypothèse,* p. 201: "Je ne crois pas, . . . que des observations plus précises puissent jamais mettre en évidence autre chose que les déplacements relatifs des corps matériels." The English quotation is from *Science and Hypothesis* (New York, 1952), p. 172.

[163] H. Poincaré, *ibid., Rapports, 1,* 23; *La science et l'hypothèse,* p. 202: "Il faut trouver une même explication pour les autres, et alors tout nous porte à penser que cette explication vaudra également pour les termes d'ordre supérieur, et que la destruction mutuelle de ces termes sera rigoureuse et absolue."

[164] H. A. Lorentz, *op. cit.* (note 150), *Collected Papers, 5,* 173–174.

The term "principle of relativity" was used by Poincaré for the first time in September 1904 in his address at the International Congress of Arts and Science at St. Louis. He formulated the "principle of relativity" as follows: "The laws of physical phenomena should be the same, whether for an observer fixed, or for an observer carried along in a uniform movement of translation."[165] At first glance, this expression might remind us of Einstein's principle of relativity. But, in fact, it is an "empirical truth"[166] which might some day be denied by an experiment. This address, in which Poincaré acknowledged indications of a crisis in physics,[167] has also attracted attention because it contains a discussion of the synchronization of clocks by means of light signals[168] which resembles that in Einstein's paper on the theory of relativity. But this discussion, too, quite differs in spirit from Einstein's and goes along with Lorentz' attitude. Poincaré wanted to show that, since the velocity of light differs according to its direction of propagation, when the observer is in motion the observer's clock synchronized by means of light signals is advanced or retarded, indicating only the local time of his position, and that he cannot know the disorder of his clock because he has no means other than his clock.

In his article "Dynamics of the Electron" of 1908,[169] Poincaré discussed in detail the connection of the principle of relativity with the Lorentz theory. He first stated the principle in the following form: "Whatever be the method employed, we shall never succeed in disclosing any but relative velocities; I mean the velocities of certain material bodies in relation to other material bodies."[170] After

165 H. Poincaré, "L'etat actuel et l'avenir de la physique mathématique," *Bulletin des sci. math., 28* (1904), 302–324, esp. 306; *La valeur de la science* (Paris, 1905), pp. 170–211, esp. pp. 176–177: "Le principe de la relativité, d'après lequel les lois des phénomènes physiques doivent être les mêmes, soit pour un observateur fixe, soit pour un observateur entraîné dans un mouvement de translation uniforme."

166 H. Poincaré, *ibid., Bulletin,* p. 307; *La valeur de la science,* p. 179.

167 H. Poincaré, *ibid., Bulletin,* p. 302; *La valeur de la science,* p. 171: "Je répugne à donner un prognostic, je ne puis pourtant me dispenser d'une diagnostic; eh bien, oui, il y a des indices d'une crise sérieuse." The English quotation is from *The Value of Science* (New York, 1958), p. 91.

168 H. Poincaré, *ibid., Bulletin,* p. 311; *La valeur de la science,* pp. 187–188.

169 H. Poincaré, "La dynamique de l'électron," *Revue gén. des sci., 19* (1908), 386–402; *Oeuvres, 9,* 551–586; *Science et méthode* (Paris, 1908), pp. 215–272.

170 H. Poincaré, *ibid., Oeuvres, 9,* 563; *Science et méthode,* p. 235: "Quel que soit le moyen qu'on emploie, on ne pourra jamais déceler que des vitesses

demonstrating that in the Lorentz theory the difference between the local and true times exactly cancels out the effect of the contraction of length and that therefore there occurs no manifest change in the velocity of light through motion, he concluded that "it is impossible to escape the impression that the Principle of Relativity is a general law of Nature."[171] In Poincaré's conception, the validity of the principle of relativity, which has been inferred from experience, must be given an explanation,[172] a demand he repeated in his public lecture at the 1909 meeting of the French Association for the Advancement of Sciences. His lecture delivered at the École Supérieure des Postes, Télégraphes et Téléphones shortly before his death in 1912[173] is especially interesting since it shows that to the end of his life he was faithful to the spirit of Lorentz' compensation theory. In this lecture Poincaré illustrated in detail that the Lorentz theory based on the local time and the contraction hypothesis explain "the perfect compensation which is observed in all the experiments of optics," and that "there is compensation equally in electric phenomena. . . ." He "came to believe that the principle of relativity was perfectly exact."[174]

Once physicists had received the Lorentz theory as the long sought satisfactory solution of the ether problem, they turned their attention quite naturally to the question whether the fundamental hypotheses of the Lorentz theory were acceptable or not. We have already seen that Morley and Miller partly intended their 1905 experiment to verify the contraction hypothesis. In the same year D. B. Brace noted that no valid reason had yet been found for the contraction hypothesis and called attention to Fritz Hasenöhrl's 1904

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relatives, j'entends les vitesses de certains corps matériels, par rapport à d'autres corps matériels."

[171] H. Poincaré, *ibid., Oeuvres, 9,* 567; *Science et méthode,* p. 240: "Il est impossible d'échapper à cette impression que le principe de relativité est une loi générale de la Nature." The English quotation is from *Science and Method* (New York, n.d.), p. 221.

[172] H. Poincaré, "La mécanique nouvelle," *Revue électrique, 13* (1910), 23–28, esp. 24: "Il faut expliquer pourquoi."

[173] H. Poincaré, "La dynamique de l'électron," *Supplément aux Annales des Postes, Télégraphes et Téléphones* (1913).

[174] *Ibid.,* p. 47: "Ceci explique la compensation parfaite que l'on observe dans toutes les experiences d'optique. On a également la compensation dans les phenomenes electriques. . . . On est arrivé à croire que le principe de relativité était parfaitement exact."

theoretical discussion of the thermodynamics of radiation contained in a moving cavity.[175] Hasenöhrl had shown that one obtained a contradiction of the second law of thermodynamics unless one introduced the contraction hypothesis.[176] That Walter Kaufmann's experiments to determine the velocity dependence of the mass of the electron[177] attracted much attention in the years following 1904–1905 was due, at least partly, to their close connection with the question of the validity of the fundamental physical assumptions of the Lorentz theory. It should also be mentioned in this context that Poincaré paid serious attention to Kaufmann's experiments and on several occasions emphasized the significance of a new "dynamics of the electron."[178] In fact, Poincaré thought of the dynamics of the electron as the theory that could solve the ether problem, as can be seen from the titles of his articles discussing the Lorentz theory, "The New Mechanics" and "The Dynamics of the Electron."

8. THE FUNDAMENTAL PROBLEM FOR EINSTEIN

Einstein's theory of relativity, unlike the Lorentz-Poincaré theory, did not aim at explaining why effects of motion could not be made manifest. Accordingly, in his theory the principle of relativity is not a law to be deduced from the fundamental principles of the theory, but a postulate. In the introduction to his 1905 paper, Einstein stated that we are led to "the conjecture . . . that . . . for all coordinate systems for which the mechanical equations are valid, the same laws of electrodynamics and optics will also be valid. . . . We will raise this conjecture (the substance of which will hereafter be called the 'principle of relativity') to the status of a postulate. . . ."[179] From

[175] D. B. Brace, "The Negative Results of Second and Third Order Tests of the 'Aether Drift' and Possible First Order Methods," *Phil. Mag., 10* (1905), 71–80, esp. 72.

[176] F. Hasenöhrl, "Zur Theorie der Strahlung in bewegten Körpern," *Ann. d. Phys., 15* (1904), 344–370; *16* (1905), 589–592.

[177] For Kaufmann's experiments, see section 12, *op. cit.* (notes 264, 265, and 268).

[178] For example, H. Poincaré, *op. cit.* (notes 169, 172, and 173).

[179] A. Einstein, "Zur Elektrodynamik bewegter Körper," *Annalen der Physik, 17* (1905), 891–921, esp. 891; Einstein speaks of the "Vermutung, . . . dass . . . für alle Koordinatensysteme, für welche die mechanischen Gleichungen gelten, auch die gleichen elektrodynamischen und optischen Gesetze gelten. . . . Wir wollen diese Vermutung (deren Inhalt im folgenden 'Prinzip der Relativität' genannt werden wird) zur Voraussetzung erheben. . . ."

this principle and from the second postulate of the constancy of the velocity of light Einstein logically derived the whole of his theory. The problem that the Lorentz-Poincaré theory set for itself is, of course, also solved by Einstein's theory, or rather it ceases to exist at all, because the unsuccessful experiments associated with the ether problem are only expressions of the fundamental postulate of Einstein's theory. Einstein, indeed, alluded to them only in vague terms as "examples of a similar kind."[180]

If the ether problem as understood by Lorentz, Poincaré, and other contemporary physicists was not the goal of Einstein's theory, then what was the fundamental problem for Einstein when he created the theory of relativity? To answer this question we should first consider Einstein's own statements. Einstein several times expounded the development of his thought which led him to the theory of relativity. His accounts are not consistent to the finest points, but when conflated they are very revealing. I first enumerate them in roughly chronological order of their publication or recording:

[1] Conversation with psychologist Max Wertheimer, which Wertheimer has reported in his *Productive Thinking.*[181] The conversation was held in 1916 or soon after.

[2] Obituary of Ernst Mach which Einstein wrote in March 1916.[182]

[3] "How did I create the theory of relativity,"[183] an improvised account Einstein gave to students of the University of Kyoto on 14 December 1922.

[4] The biography by Anton Reiser published in 1930.[184] Einstein prefaced it with the words: "The author of this book is one who knows me rather intimately in my endeavour, thoughts, beliefs. . . . I found the facts of the book duly accurate. . . ." Anton Reiser is, according to Gerald

180 *Ibid.*

181 Max Wertheimer, *Productive Thinking,* enlarged edition edited by Michael Wertheimer (New York and London, 1959), pp. 213-226.

182 A. Einstein, "Ernst Mach," *Phys. Zeits., 17* (1916), 101-104.

183 Jun Ishiwara, *Einstein Kyôzyu Kôen-roku* (*The Record of Professor Einstein's Lectures*) (Tokyo, 1923), pp. 131-151. Reprint (Tokyo, 1971), pp. 78-88.

184 Anton Reiser, *Albert Einstein. A Biographical Portrait* (London, 1931).

Holton's investigation,[185] the pseudonym of the husband of Einstein's stepdaughter Ilse.

[5] Letters to his friend Michele Besso dated 6 January 1948 and 6 March 1952.[186]

[6] "Autobiographical Notes" published in 1949.[187]

[7] Interviews by R. S. Shankland.[188]

[8] Letters quoted by Carl Seelig in his biography of Einstein.[189]

[9] Letter to Carl Seelig dated 19 February 1955.[190]

[10] Speeches and letters quoted and examined by Holton.[191]

According to these records Einstein took his first step toward the theory of relativity with a conceptual experiment he carried out at the age of sixteen ([1], [6], [7]). While still a student of the cantonal school in Aarau, Switzerland, he asked himself what phenomena would be seen by an observer who ran after propagating light with a velocity equal to that of light. Would he see a standing electromagnetic field which varied only from point to point? From the very beginning it seemed intuitively clear to Einstein that, judged from the standpoint of such an observer, everything would have to happen according to the same laws as for an observer who was at rest ([1], p. 218; [6], p. 52). In other words, he already possessed conception that was destined to develop into the principle of relativity. However, he was at that time "a pure empiricist" who "expected to approach the major questions of physics by observation and experiment" ([4], p. 54). In his second year at the Federal Institute of

185 Gerald Holton, "Influences on Einstein's Early Work in Relativity Theory," *The American Scholar, 37* (1967–1968), 59–79. A slightly condensed version appears in *Thematic Origins of Scientific Thought,* pp. 197–217, esp. p. 211.

186 Albert Einstein/Michele Besso, *Correspondence 1903–1955,* translation, notes, and introduction by Pierre Speziali (Paris, 1972), pp. 390–392; 464–465.

187 Albert Einstein, "Autobiographisches," Paul Arthur Schilpp, ed., *Albert Einstein: Philosopher-Scientist* (New York, 1949 and 1951), pp. 1–95.

188 R. S. Shankland, "Conversations with Albert Einstein," *Amer. Journ. Phys., 31* (1963), 47–57.

189 Carl Seelig, *Albert Einstein. Eine dokumentarische Biographie* (Zurich, 1954).

190 A. Einstein to C. Seelig, 19 February 1955. Published by Seelig in *Technische Rundschau* (1955), and partially quoted by Max Born, *op. cit.* (note 7), p. 193.

191 Gerald Holton, *op. cit.* (note 19).

Technology in Zurich he planned to perform an experiment to detect changes of the velocity of light caused by the earth's motion ([1], [3], [4]). He designed an experiment to find the difference in the energies carried by two bundles of light travelling in opposite directions by means of thermopiles ([3], p. 79), but "there was no chance to build this apparatus" because "the scepticism of his teachers was too great, the spirit of enterprise too small" ([4], p. 53). Here again it should be noted that, although attempting the experiment, Einstein did not expect it to be successful. "His wish to design such experiments was always accompanied by some doubt that the thing was really so" ([1], p. 214). At this time he did not know the Michelson-Morley experiment, but even when he later got acquainted with it, its "results were no surprise to him, . . . [but] seemed to confirm . . . his ideas" ([1], p. 217; [10]). It may be said that Einstein had prefigured the principle of relativity from the earliest time.

Pondering over the question of the relation of the laws of optical and electromagnetic phenomena to the motion of the observer, young Einstein spent some time trying to modify Maxwell's equations. "If the Maxwell equations are valid with regard to one system, they are not valid in another. They would have to be changed. . . . For years Einstein tried to clarify the problem by studying and trying to change the Maxwell equations. He did not succeed . . ." ([1], p. 216). He tried to treat Fizeau's experiment concerning the drag coefficient with the Maxwell-Lorentz equations and "believed that they were correct and express rightly the facts. That they are valid also in moving coordinate systems indicates the relation of the so-called constancy of the velocity of light. . . . [But] this is not compatible with the law of composition of velocity known in mechanics" ([3], pp. 81–82). "In whatever way he tried to unify the question of mechanical movement with the electromagnetic phenomena, he got into difficulties" ([1], p. 216). Thus he "had to spend nearly one year with fruitless thinking" ([3], p. 82). During that time he even considered the possibility of an emission theory of light ([7], p. 29).

To restate in our terms, the problem with which Einstein was wrestling in these years was to modify Maxwell's theory in such a way that he would obtain a theory of electromagnetic and optical phenomena in which only relative motion had physical meaning. In other words, Einstein was seeking to bring about a unification of mechanics and electromagnetism with regard to the relativity of motion. He had set himself a problem concerned with the form, rather than the content, of theory, but Einstein, empiricist as he was then, did not become clearly aware of this until he came to reflect upon the consequences of Planck's radiation formula. His "Autobiographical Notes" tell us that, looking at Planck's formula, he found that "radiation must . . . possess a kind of molecular structure in energy, which of course contradicts Maxwell's theory. . . . Reflections of this type made it clear to me as long ago as shortly after 1900, i.e., shortly after Planck's trailblazing work, that neither mechanics nor thermodynamics could (except in limiting cases) claim exact validity. By and by 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. The example I saw before me was thermodynamics. . . . After ten years of reflection such a principle resulted from a paradox upon which I had already hit at the age of sixteen" ([6], pp. 51, 53). Again pointing out the relationship between the theory of relativity and his commitment to the quantum theory of radiation, Einstein wrote to Carl Seelig two months before his death: "The insight that the 'Lorentz invariance' is a general condition for any physical theory . . . was for me of particular importance because I had already previously found that Maxwell's theory did not account for the micro-structure of radiation and could therefore have no general validity" ([9], p. 193). As a result of his reflection on the necessity of a reconstruction of physics, Einstein had come to recognize the profound significance of the principle of relativity.

It is of course impossible to reconcile the Maxwell theory with the principle of relativity without modifying traditional notions of time. "The type of critical reasoning which was required for the discovery of this central point was decisively furthered, in my [Einstein's] case, especially by reading of David Hume's and Ernst Mach's philosophical writing" ([6], p. 53; [2], p. 102; [5], pp. 391, 464; [8], pp. 59–60). The first paper of the theory of relativity was completed five to six weeks after he hit upon the modification of the concept of time ([1], pp. 214, 219; [3], pp. 82–83; [8], p. 82).

The most remarkable point in Einstein's first relativity paper of 1905 is that it begins with the discussion of asymmetries with respect to motion involved in the current form of electromagnetic theory. It then proceeds to introduce the principle of relativity as a postulate to remove asymmetries with regard to motion from the electromagnetic theory. By doing so it aims at securing the relativity of motion in electrodynamics as well as in mechanics. The mode of presentation of the 1905 paper completely corresponds to the foregoing presentation extracted from Einstein's writings.

To summarize, Einstein had speculated on the relation between motion and electromagnetic phenomena since as early as the mid-1890's. When he began to ponder the consequences of Planck's radiation theory, he came to consider the problem in broader perspective. He felt the necessity of rebuilding physics on some formal principle. He was especially concerned about formal incongruities between physical theories. The theory of relativity was a fruit of his efforts to eliminate such incongruities. However, the incongruity between electrodynamics and mechanics with respect to the relativity of motion with which he was concerned here was not the only incongruity between these two theories that worried him. Holton has pointed out,[192] Einstein also found a formal incongruity between them in their respective fundamental entities. His other great achievement of 1905, the theory of light quanta, was also intended to remove "a fundamental formal difference"[193] between mechanics and electromagnetism, namely, the difference of having a discrete fundamental entity, the mass point, in mechanics and a continuous one, the field, in electromagnetism. In contrast to Einstein, both Lorentz and Poincaré, and indeed all other contemporary physicists, did not give any consideration to the formal incongruity between physical theories. The allegation that Einstein's theory might have been suggested by Lorentz' 1904 theory has been refuted by Holton.[194] I may add that even if Einstein had been acquainted with Lorentz' 1904 paper the course of events would not have been changed essentially, for he saw the state of physics in those days

[192] Gerald Holton, *op. cit.* (note 12), [a], pp. 629-630.

[193] A. Einstein, "Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt," *Ann. d. Phys., 17* (1965), 132-148, esp. 132. As early as 1949 Takehiko Takabayasi noted that "the theory of light quanta had been derived from the formal antithesis of the point mechanics and the field theory." T. Takabayasi, "Koten Buturigaku no Hôkai Katei ni tuite" ("On the Process of the Decline of Classical Physics"), *Kagakusi Kenkyu,* No. 11 (1949), pp. 1-9, esp. p. 5.

[194] Gerald Holton, *op. cit.* (note 185), *Thematic Origins of Scientific Thought,* pp. 204-205.

quite differently than Lorentz, Poincaré, and others, and consequently perceived an entirely new problem which none of them had recognized.

Einstein could not have formed his fruitful conception of the unification of mechanics and electromagnetism by a universal formal principle, however, if the two theoretical systems had not been confronting each other on the same footing. The unification of two theories, unlike the reduction of one to the other, presupposes that both theories are equally privileged or, rather, equally unprivileged. To consider mechanics and electromagnetism as two theories having equal status seems quite natural and easy to us today. But such a viewpoint and the problems it set for physicists were just what Lorentz, Poincaré, and others failed to acquire. The difference in this respect between Einstein and Lorentz, Poincaré, and others is so great that it can hardly be considered accidental. It is not unreasonable to seek its roots in the differences between their epistemologies of physics or worldviews. We must consider the influence of Hume and Mach on Einstein which he himself acknowledged.

9. HUME AND MACH

The period beginning in 1902, when Einstein settled in Berne as an officer of the Federal Patent Office, was especially fruitful for the development of his thought because of the evenings he spent with his friends Maurice Solovine and Conrad Habicht reading scientific and philosophical books such as Mach's writings and Hume's *Treatise of Human Nature.* He professed that Hume exerted more direct influence on his work than Mach,[195] but he does not say precisely what he learned from Hume. According to Solovine's recollection, Einstein and his friends "discussed for several weeks David Hume's particularly sagacious criticism of notions of substance and causality."[196] Hume disclaimed the notion of *substantia,* both material and spiritual, and replaced it with bundles of ideas. His criticism of the notion of causality is one of the best known topics in the history of philosophy. Hume asserted that the causal relation merely meant that an object had occurred always in conjunction with another object; it did not express a necessary relation between the two objects. As far as these assertions are concerned, it is difficult to establish a

[195] A. Einstein to M. Besso, 6 January 1948, *op. cit.* (note 186), p. 391.

[196] Albert Einstein, *Lettres à Maurice Solovine* (Paris, 1956), p. viii.

direct, specific connection with Einstein's theory of relativity. Their influence on Einstein would have been a general one.

Hume's ideas of space and time, on the contrary, would probably have had considerable direct influence on the development of Einstein's.[197] Hume stated that "*the idea of space or extension is nothing but the idea of visible or tangible points distributed in a certain order*" (italics original),[198] and that "we have no idea of any real extension without filling it with sensible objects."[199] As to time, it "is always discover'd by some *perceivable* succession of changeable objects" (italics original).[200] We have no "idea of time without any changeable existence. . . ."[201] Asserting that physical theories are "based on the kinematics of rigid bodies"[202] Einstein developed the theory of relativity in his first relativity paper by beginning with definitions of space and time by means of a scale and a clock. This approach to the concepts of space and time immediately reminds us of Hume's assertions that the idea of space is based on an arrangement of tangible objects and that the idea of time is based on a perceptible succession of changeable objects.

However, for Einstein to find fruitful suggestions in Hume's discussion of the notions of space and time, it was necessary that the adaptation of electromagnetic theory to the principle of relativity should first have become the desideratum. The idea of modifying the notions of space and time may have emerged in the effort to solve the problem of adaptation. We are thus led to the question of what conceptual factor enabled Einstein to set himself the problem of adapting the electromagnetic theory to the principle of relativity. To answer this question we must turn to Mach.

As to Mach's influence on Einstein, it has been taken for granted that Einstein's commitment to Machian positivism contributed to the innovations in the concepts of space and time. For example, Philipp Frank has stated: "The definition of simultaneity in the special theory of relativity is based on Mach's requirement that every statement in physics has to state relations between observable quan-

[197]David Hume, *A Treatise of Human Nature* (Oxford, 1888); reprint (Oxford, 1968), pp. 26–68.

[198]*Ibid.*, p. 53.

[199]*Ibid.*, p. 64.

[200]*Ibid.*, p. 35.

[201]*Ibid.*, p. 65.

[202]A. Einstein, *op. cit.* (note 179), p. 892.

tities. . . . Mach's requirement, the positivistic requirement, was of great heuristic value to Einstein."[203] Hans Reichenbach, too, has said: "The physicist who wanted to understand the Michelson experiment had to commit himself to a philosophy for which the meaning of a statement is reducible to its verifiability. . . . It is this positivist, or let me rather say, empiricist commitment which determines the philosophical position of Einstein. . . . He merely had to join a trend of development characterized, within the generation of physicists before him, by such names as Kirchhoff, Hertz, Mach. . . ."[204] Gerald Holton, who has made an interesting, detailed investigation of encounter and deviation in the thought of Einstein and Mach, has found a "Machist component" in the birth of the theory of relativity in Einstein's insistence that the fundamental problems of physics could not be understood before an epistemological analysis, and in his identification of reality with the product of sensations.[205] The Japanese philosopher Wataru Hiromatu has discussed the connection between Mach's philosophical thought and the theory of relativity, especially in their premises of understanding the external world, and has asserted that Mach's ideas, such as the monistic world view, the conception of science as description, the principle of thought economy, and his theoretical investigation in physics as the embodiment of these ideas, pioneered in many respects the theory of relativity, the special as well as the general.[206] Hiromatu's discussion, however, is concerned only with conceptual links, not with any genetic link between Mach's philosophy and the theory of relativity. To find the actual contribution of Mach's thought to the development of Einstein's during the years around 1900 that were crucial for the genesis of the theory of relativity, we have to look at Einstein's own words.

[203] Philipp G. Frank, "Einstein, Mach, and Logical Positivism," P. A. Schilpp, ed., *Albert Einstein: Philosopher-Scientist,* pp. 269–286, esp. pp. 272–273.

[204] Hans Reichenbach, "The Philosophical Significance of the Theory of Relativity," *ibid.,* pp. 287–311, esp. pp. 290–291.

[205] Gerald Holton, "Mach, Einstein, and the Search for Reality," *Daedalus* (1968), pp. 636–673; *Thematic Origins of Scientific Thought,* pp. 219–259, esp. p. 224.

[206] Wataru Hiromatu, "Mach's Philosophy and the Theory of Relativity—In Referring to His Criticism of Newtonian Physics" (in Japanese), in W. Hiromatu and H. Kato, eds. and trans., *Mach: Ninsiki no Bunseki* (*Mach: Analysis of Knowledge*) (Tokyo, 1966); reprint (Tokyo, 1971), pp. 136–173.

Urged by his friend Michele Besso,[207] Einstein in 1897 read Mach's *Mechanics in Its Development, Historically and Critically Described,*[208] which made a strong impression on him. In 1947 Besso asked Einstein if it is permissible to say that Mach's thought played the decisive role in drawing Einstein's attention to "observable quantities—perhaps indirectly to 'clock and scale'."[209] Einstein's answer[210] to Besso's specific question was rather negative; he said that Mach's influence on the development of his thought was surely great, but that it is not clear how much it affected his research directly. However, as to Mach's general influence, his answer was very definite: "I see his great service in that he loosened the dogmatism about the foundation of physics that had been dominant during the eighteenth and nineteenth century. He has tried to show especially in mechanics and heat theory how concepts arose out of experience. He has convincingly advocated the point of view that these concepts, even the most fundamental, receive their justification only from experience, that they are in no way *logically* necessary" (italics original).[211] This statement exactly corresponds to the fol-

207Carl Seelig, *op. cit.* (note 189), p. 39.

208E. Mach, *Die Mechanik in ihrer Entwicklung historisch-kritisch dargestellt,* 3rd ed. (Leipzig, 1897). It may be convenient to devote a few words to the successive editions of the *Mechanics.* The first edition was published in 1883 and the second in 1888, the text remaining unaltered except for corrections of printer's errors. The third through ninth editions appeared in 1897, 1901, 1904, 1908, 1912, 1921, and 1933, respectively. From the third through the seventh edition Mach made revisions and additions to each new edition. When in 1897 Einstein first read Mach's *Mechanics,* nearly ten years had passed since the publication of the second edition. It would have been difficult for Einstein to purchase a copy of the second edition. The third edition appeared most probably in the first half of 1897, since the author's preface is dated January 1897. It is not unreasonable to assume that Einstein read the newly issued third edition. Upon this assumption I will refer in the following to the third edition. Of course we cannot exclude the possibility that Einstein by some means read the second, or even the first, edition, but the difference between the various editions is not significant for the following discussion, because the difference consists mainly in separate examples and the addition or deletion of references to other authors who discussed related topics in the interim. The fundamental purport is unchanged throughout all editions.

209M. Besso to A. Einstein, 12 October, 4 and 23 November, and 8 December 1947, *op. cit.* (note 186), p. 386; Besso is inquiring if "die Mach'schen Gedankengänge entscheidend auf das Beobachtbare hinwiesen—vielleicht eben indirekt, auf 'Uhren und Massstäbe'."

210A. Einstein to M. Besso, 6 January 1948, *op. cit.* (note 186), p. 391.

211*Ibid.,* pp. 390–391: "Ich [sehe] sein grosses Verdienst darin, dass er den lowing passage in his "Autobiographical Notes" which was perhaps written shortly before the above letter. There Einstein writes: "Even Maxwell and H. Hertz . . . in their conscious thinking adhered throughout to mechanics as the secured basis of physics. It was Ernst Mach who, in his history of mechanics, shook this dogmatic faith; this book exercised a profound influence upon me in this regard while I was a student."[212] We may assume that Mach's influence upon Einstein consisted essentially in undermining the mechanistic worldview by showing that even the fundamental concepts of mechanics are, in the last analysis, rooted in experience. In physics in general, this was the very goal that Mach himself sought to attain in his *Mechanics* and other writings.[213]

10. CRITICISM OF THE MECHANISTIC WORLDVIEW

To understand the origin of Mach's thought, it is expedient first to examine the essay *History and Root of the Axiom of the Conservation of Work* (hereafter abbreviated as *History and Root*),[214] which is based on a lecture delivered in 1871. Mach stated in his *Mechanics* that the view developed there was first propounded in *History and Root*. The purpose of the essay *History and Root* was to repudiate the mechanistic worldview through examination of the foundation of the principle of conservation of energy. Mach's criticism was directed especially at Hermann von Helmholtz and Wilhelm Wundt.

In the introduction of *On the Conservation of Force* (1847)[215]

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im 18. und 19. Jahrhundert herrschenden Dogmatismus über die Grundlagen der Physik aufgelockert hat. Er hat besonders in der Mechanik und Wärmelehre aufzuzeigen gesucht, wie die Begriffe aus den Erfahrungen heraus entstanden sind. Er hat überzeugend den Standpunkt vertreten, dass diese Begriffe, auch die fundamentalsten, ihre Berechtigung nur von der Empirie aus erhalten, dass sie in keiner Weise *logisch* notwendig sind."

212 A. Einstein, *op. cit.* (note 187), p. 21.

213 Alfonsina D'Elia analyzes Mach's writings, paying special attention to the latter's criticism of the mechanistic world view, in A. D'Elia, *Ernst Mach* (Firenze, 1971). John T. Blackmore's biographical study *Ernst Mach. His Work, Life, and Influence* (Berkeley, 1972) fails to evaluate Mach's insistence on refuting the mechanistic worldview.

214 E. Mach, *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit. Vortrag gehalten in der K. Böhm. Gesellschaft der Wissenschaften am 15. Nov. 1871* (Leipzig, 1909). This is a reprint of the first edition which appeared in Prague in 1872.

215 Hermann Helmholtz, *Über die Erhaltung der Kraft, eine physikalische Abhandlung, vorgetragen in der Sitzung der physikalischen Gesellschaft zu Berlin am 23sten Juli 1847* (Berlin, 1847); reprint (Bruxelles, 1966).

Helmholtz formulated the mechanistic worldview with its philosophical foundation by stating that, in view of the principle of sufficient reason, the "ultimate goal of theoretical natural science is to find the ultimate unchangeable causes of the processes in nature."[216] The external objects of science are matter and force which are inseparable from each other, and the ultimate causes that science seeks to discover ought to be motive forces. The actions of the motive forces are determined only by spatial relations between bodies, since motion is the change in the mutual spatial relation between at least two bodies, and consequently the motive force as the cause of motion is deduced only for mutual relations between bodies. Material bodies may be resolved into mass points, and there is no other spatial relation between points than their mutual separations. "The task of physical science, therefore, is defined as consisting in reducing natural phenomena to unchangeable, attractive and repulsive forces."[217] As these words clearly show, Helmholtz not only asserted that all physical phenomena should be explained in terms of material points and central forces, but furthermore reasoned that the reduction of all physical phenomena to mechanics was a necessity on *a priori,* metaphysical grounds.

Helmholtz shared this view with many nineteenth century scientists. One conspicuous proponent of a similar view was Wundt, who, six years before Mach's *History and Root,* tried to establish an epistemological foundation for the necessity of the mechanistic worldview in *The Axioms of Physics and Their Relation to the Causal Principle.*[218] In Mach's words, Wundt was "the proponent of the tendency of modern natural science," and "no objection was raised to Wundt's view."[219] Wundt asserted that all causes in nature are causes of motion for the following reason. The qualitative changes in external objects, judged on the information furnished by

[216]*Ibid.,* p. 2: "Das endliche Ziel der theoretischen Naturwissenschaften ist also, die letzten unveränderlichen Ursachen der Vorgänge in der Natur aufzufinden."

[217]*Ibid.,* p. 6: "Es bestimmt sich also endlich die Aufgabe der physikalischen Naturwissenschaften dahin, die Naturerscheinungen zurückzuführen auf unveränderliche, anziehende und abstossende Kräfte. . . ."

[218]W. Wundt, *Die physikalischen Axiome und ihre Beziehung zum Causalprinzip. Ein Kapitel aus einer Philosophie der Naturwissenschaft* (Erlangen, 1866). See Ernst Cassirer, *The Problem of Knowledge. Philosophy, Science, and History since Hegel* (New Haven, 1950), pp. 87–88.

[219]E. Mach, *op. cit.* (note 214), p. 19.

our senses, is described by the statement that one object has disappeared and another object with partly different qualities has taken its place. Such disappearance and appearance contradict the identity of being and the indestructibility of matter. However, there "is one single case where an object does change before our eyes and yet still remains the same, and this is the case of motion. Here the change consists merely in the alteration of an object's spatial relationships to other objects. . . . [During a change of position, objects] remain identical. . . . We must trace every change back to the only conceivable one in which an object remains identical: motion."[220]

This sort of argument does not appeal to present-day readers, but it appealed to nineteenth century scientists. They thought that it was not accidental and matter-of-fact that every natural phenomenon had to be accounted for by mechanics, but logical and necessary. In their view, the axioms of mechanics were not merely empirical, factual laws, but, like axioms and theorems of geometry, *a priori* or necessary truths. We can find an example in Bernhard Riemann's manuscript "Gravity and Light," inferred to have been written after 1858, which shows from the critic's point of view the current conception. Riemann criticized the attempt to elevate the laws of mechanics to *a priori* truths, remarking especially that the law of inertia cannot be accounted for by the principle of sufficient reason.[221]

In his *History and Root,* Mach, tracing the origins of the principle of conservation of energy to the knowledge that "it is impossible to produce work from nothing," tried to show that this knowledge was rooted far more deeply in human experience over an immensely long period than in modern mechanics. He wanted to assert that the efforts to derive the laws of mechanics *a priori* from the general principle of causality were meaningless. His *Mechanics,* published nearly ten years later, was the fruit of his efforts to widen and deepen the criticism of the *a priori* view of mechanics which he had outlined in *History and Root.* In the preface to the first edition of the *Mechanics,* which he retained throughout all succeeding editions, Mach stated that "the tendency [of this book] is rather an enlightening

[220]Quoted by Cassirer, *op. cit.* (note 218), p. 88.

[221]B. Riemann, "Gravitation und Licht," *Gesammelte mathematische Werke und wissenschaftlicher Nachlass,* 2nd ed. (1892); reprint (New York, 1953), pp. 532–538. Cf. Yôitu Kondô, *Sin Kikagaku Sisôsi* (*A Conceptual History of Geometry.* Revised) (Tokyo, 1966), p. 189.

one or, to put it more clearly yet, an anti-metaphysical one."[222] In *History and Root* Mach defined metaphysical concepts as concepts of which "we have forgotten how we reached them."[223] Hence, in his *Mechanics* he attempted to elucidate "the questions of the *scientific* content of mechanics, of *how* we obtained it, from what *sources* we have derived it, and to what extent it can be considered our assured possession" (italics original).[224] In other words, Mach's purpose in the *Mechanics* was to put an end to a priority in mechanics and thus to strike a blow at the mechanistic worldview.

Mach's *Mechanics* is divided into five chapters. The first chapter deals with statics. Examining closely the "proofs" of the lever principle by Archimedes and Galileo, the deduction of the theorem of equilibrium of force on an inclined plane by Simon Stevin, the "geometrical proof" of the parallelogram of forces by Daniel Bernoulli, and the derivation of the principle of virtual displacement by Joseph Louis Lagrange, Mach unveiled that behind all these "proofs" are presuppositions of certain intuitive knowledge, which are no more than generalizations of repeated experience obtained in the long history of the human race. The second chapter of the *Mechanics* is devoted to the consideration of dynamics. Analyzing the reasoning by which Galileo inferred the law of inertia (on a horizontal plane), Mach concluded that behind Galileo's reasoning lies the intuitive knowledge that any body having weight never ascends by itself, and also that "it is at all events entirely erroneous to express the inertia as self-evident, or to try to derive it from the general theorem that 'the action of a cause persists'."[225] Similarly Mach pointed to the important part played by intuitive knowledge in Christian Huygens' determination of the center of oscillation of extended bodies. He then proceeded to a detailed discussion of Isaac Newton's conceptions of mass and action and reaction and made it clear that these

[222] E. Mach, *op. cit.* (note 208), p. v: "Ihre Tendenz ist vielmehr eine aufklärende oder, um es noch deutlicher zu sagen, eine antimetaphysische."

[223] E. Mach, *op. cit.* (note 214), p. 2: "Metaphysisch pflegen wir diejenigen Begriffe zu nennen, von welchen wir vergessen haben, wie wir dazu gelangt sind."

[224] E. Mach, *op. cit.* (note 208), p. v, posed "die Fragen . . . worin der *naturwissenschaftliche* Inhalt der Mechanik besteht, *wie* wir zu demselben gelangt sind, aus welchen *Quellen* wir ihn geschöpft haben, wie weit derselbe als ein gesicherter Besitz betrachtet werden kann. . . ."

[225] *Ibid.,* p. 135: "Die Trägheit als selbstverständlich darzustellen, oder sie aus dem allgemeinen Satz 'die Wirkung einer Ursache verharrt' abzuleiten, ist jedenfalls durchaus verfehlt."

two kinds of concepts, mass on the one hand and action and reaction on the other, depend on each other, and that a certain amount of intuitive knowledge and experience underlies the process by which the concepts were formed. This part of the book is written with the greatest ardor and persuasiveness. It is followed by the most famous section of the book, the criticism of Newton's concepts of time and space, which is so well known that we may pass on without giving a detailed account. But one point is worth emphasizing. After illustrating that there is no need to associate the law of inertia with a special absolute space, Mach stresses that "the most important result of our considerations is, however, *that even the apparently simplest laws of mechanics are very complicated in nature, that they rest on unfinished, even never completely terminable experience,* . . . [but] *that they should by no means be regarded as mathematically determined truths, but rather as theorems that not only can be controlled by experience but even need to be*" (italics original).[226] Mach's celebrated opposition to the concepts of absolute space and time must, therefore, be understood in the broader context of his criticism of a priority in mechanics. In the general observation at the end of the chapter Mach stressed that descriptions of Newtonian mechanics should distinguish the parts based on experience from those that are arbitrary convention and that the present form of mechanics is determined by historical contingency. In the third chapter of the *Mechanics,* Mach discussed formal principles of mechanics such as the principle of least action. Here again he rejected a priority. Mach noted that "the largest fault of Descartes, which spoils his study of nature, is that he thinks those propositions to be self-evident and clear that only experience can determine."[227] In the fourth chapter Mach proposed the concept of "economy of scientific thinking," and in the fifth he used the concept as a basis for asserting

[226] *Ibid.,* pp. 231–232: "Das wichtigste Ergebniss unserer Betrachtungen ist aber, *dass gerade die scheinbar einfachsten mechanischen Sätze sehr complicirter Natur sind, dass sie auf unabgeschlossenen, ja sogar auf nie vollständig abschliessbaren, Erfahrungen beruhen,* . . . *dass sie aber keineswegs selbst als mathematisch ausgemachte Wahrheiten angesehen werden dürfen, sondern vielmehr als Sätze, welche einer fortgesetzten Erfahrungscontrolle nicht nur fähig, sondern sogar bedürftig* sind."

[227] *Ibid.,* pp. 274–275: "Der grösste Fehler des Descartes aber, der seine Naturforschung verdirbt, ist der, dass ihm Sätze von vornherein als selbstverständlich und einleuchtend erscheinen, über welche nur die Erfahrung entscheiden kann."

that the mechanistic worldview is unfounded. Mach argued that, from the viewpoint of the economy of thinking, the mechanical hypothesis has no priority over other kinds of hypothesis, and that therefore "we consider as prejudice the view that mechanics should be considered the foundation of all other branches of physics and that all physical processes ought to be explained mechanically."[228]

Since we are preoccupied with the theory of relativity, we are liable to see in Mach's *Mechanics* above all the critical discussion of the concepts of space and time as an early expression of the spirit of the relativity theory. Mach's discussion of space and time could certainly have been suggestive to Einstein in the gestation of the relativistic conception of space and time. Einstein in fact praised Mach's critical mind in his obituary of Mach, quoting at considerable length the passages concerning the concepts of space and time from the *Mechanics.*[229] Nonetheless, Mach's first purpose of the *Mechanics* as a whole was, as he added in later editions, "to convince readers that *properties of nature* cannot be fabricated with the aid of self-evident hypotheses but should be drawn from *experience*" (original italics).[230] Einstein was quite right when he regarded the destruction of the dogmatism of the mechanistic worldview as the greatest merit of Mach's *Mechanics.*

Mach's extended criticism of the mechanistic worldview was not without justification in the late nineteenth century, since the mechanistic worldview was then still the prevalent view. For example, J. C. Maxwell stated in his *Matter and Motion* that "the first part of physical science relates to the relative position and motion of bodies,"[231] because physical science should deal with the simplest and most abstract phenomena in nature and the simplest of all natural phenomena is the change in the arrangement of material bodies. He also asserted that the law of inertia is understandable *a priori.* He said that if a body that is not subject to any influence were to

[228] *Ibid.,* p. 486: "Die Anschauung, dass die Mechanik als Grundlage aller übrigen Zweige der Physik betrachtet werden müsse, und dass alle physikalischen Vorgänge *mechanisch* zu erklären seien, halten wir für ein Vorurteil.

[229] A. Einstein, *op. cit.* (note 182), pp. 102–103.

[230] E. Mach, *Die Mechanik,* 8th ed. (Leipzig, 1921), p. 20: "Mein ganzes Buch verfolgt aber das Ziel, den Leser zu überzeugen, dass man *Eigenschaften* der Natur nicht mit Hilfe sebstverständlicher Annahmen aus den Fingern saugen kann, sondern dass diese der *Erfahrung* entnommen werden müssen."

[231] J. C. Maxwell, *Matter and Motion* (1877); reprinted, with notes and appendices by Joseph Larmor (London, 1920), p. 2.

change its velocity spontaneously, then by the maxim that "the same causes will always produce the same effects"[232] we would be led to a conclusion which "is in contradiction to the only system of consistent doctrine about space and time which the human mind has been able to form."[233] Helmholtz, who in the middle of the century had formulated the mechanistic worldview, later modified his original view of the causal principle,[234] but to the end of his life maintained the view that mechanics occupied the primary place in the whole of physics. In his lecture on mechanics given in 1893–1894, in which he characterized forces as causes that always persist and act according to immutable laws, he declared that "the whole of theoretical physics may be constructed with the aid of the concept of force."[235] A letter by the Japanese physicist Hantaro Nagaoka, written in Berlin in 1893, portrays the state of physics there at that time. Nagaoka wrote that "physicists here seem to believe that it is the modern way to reduce every thing to the mechanical ground."[236] To young Einstein, who "had read with enthusiasm Ludwig Büchner's *Force and Matter*"[237] which emphasizes the inseparability of force and matter and asserts that all forces and actions in nature consist in the conditions or movements of the particles of matter, Mach's radical criticism of the mechanistic worldview must have been a revelation.

11. EPISTEMOLOGICAL STATUS OF MECHANICS IN LORENTZ' AND POINCARÉ'S VIEWS OF PHYSICS

Having considered Mach's criticism of the mechanistic worldview and its general significance for late nineteenth century physics, we now turn to the question of what its specific bearing was on the birth of the theory of relativity. To answer this question we have to return to the scientific thought of Lorentz and Poincaré.

232 *Ibid.*, p. 13.
233 *Ibid.*, pp. 28–29.
234 See the note added in 1881 when "On the Conservation of Force" was included in *Wissenschaftliche Abhandlungen von Hermann Helmholtz* (Leipzig, 1882–1895), *1*, 12–75, on 68.
235 H. von Helmholtz, *Vorlesungen über die Dynamik discreter Massenpunkte*, 2nd ed. (Leipzig, 1911; 1st ed. 1898), p. 24.
236 Kiyonobu Itakura, Tosaku Kimura, and Eri Yagi, *Nagaoka Hantarô Den* (*A Biography of Hantaro Nagaoka*) (Tokyo, 1973), p. 170.
237 Carl Seelig, *op. cit.* (note 190), p. 14.

If "mechanistic worldview" is understood to be a world picture "in which the laws of physics are reduced to those of mechanics,"[238] then Lorentz and Poincaré did not share the "mechanistic worldview." Much less did they attempt to make a mechanical model for electromagnetic phenomena. At the beginning of his *Theory of Electrons* Lorentz rejected such attempts, saying that "we can develop the theory to a large extent and elucidate a great number of phenomena, without entering upon speculations of this kind. Indeed, on account of the difficulties into which they lead us, there has of late years been a tendency to avoid them altogether and to establish the theory on a few assumptions of a more general nature."[239] The ether as Lorentz conceived it was, to use Einstein's words,[240] deprived of all mechanical properties but "rest." Poincaré, too, said: "The end we seek is not the mechanism; the true and only aim is unity."[241] Since the electromagnetic theory can be formulated in such a way that it satisfies the principles of conservation of energy and of least action, mechanical explanation is always and in infinitely many ways possible. We have to be satisfied with the abstract possibility, he asserted. He even declared: "Whether the ether really exists matters little. . . . That, too, is only a convenient hypothesis."[242] Rejecting mechanical explanations, Lorentz and Poincaré sought instead to unify physics under the electromagnetic view of nature.[243] In spite of their denial of the "mechanistic worldview," however, careful examination of their thought reveals that they, too, had not emancipated themselves from the prevalent view that mechanics should occupy the primary position in the logical structure of the edifice of all physics.

Lorentz' greatest contribution to the development of electromagnetic theory is that he took the electromagnetic field, which Maxwell, Hertz, and others had considered a state of the dielectric,

[238] Arthur I. Miller, *op. cit.* (note 16), p. 212, footnote 11.

[239] H. A. Lorentz, *op. cit.* (note 156), p. 2.

[240] A. Einstein, *Aether und Relativitätstheorie* (Berlin, 1920), p. 7.

[241] H. Poincaré, *op. cit.* (note 162), *Rapports, 1,* 26; *La science et l'hypothese,* p. 207: "Le but poursuivi; ce n'est pas le mécanisme, le vrai, le seul but, c'est l'unité."

[242] H. Poincaré, *La science et l'hypothèse,* pp. 245–246: "Peu nous importe que l'éther existe réellement. . . . Ce n'est là aussi qu'une hypothèse commode."

[243] Russell McCormmach, "Einstein, Lorentz, and the Electron Theory," *Hist. Stud. Phys. Sci., 2* (1970), 41–87; Stanley Goldberg, *op. cit.* (note 13); Arthur I. Miller, *op. cit.* (note 16).

as an independent physical reality; he considered it to be the state of the ether.[244] Lorentz' ether is a nonmechanical entity in the sense that its physical state is entirely determined by electromagnetic excitation. It is almost synonymous with the electromagnetic field as we understand it today. It is the substance of electromagnetic phenomena. For Lorentz, however, a substance must be endowed with some mechanical characteristics, however abstract and limited they may be. At the end of his *Theory of Electrons,* defending his theory against Einstein's theory of relativity, he wrote: "I cannot but regard the ether, which can be the seat of an electromagnetic field with its energy and its vibrations, as endowed with a certain degree of substantiality, however different it may be from all ordinary matter. In this line of thought, it seems natural not to assume at starting that it can never make any difference whether a body moves through the ether or not, and to measure distances and lengths of time by means of rods and clocks having a fixed position relatively to the ether."[245] Lorentz believed, in other words, that if something is a substance, then *motion* relative to it can be conceived and this motion must have some physical consequence. He expressed the same thought also in the series of lectures which he delivered in Göttingen in 1910: although the ether in the theory of electrons "is still left with substantiality to such an extent that a coordinate system can be defined thereby," the theory of relativity has attacked even this last substantiality.[246] In other words, in Lorentz' view, if motion or rest cannot be determined relative to the ether, the ether cannot be the substance of electromagnetic phenomena. Lorentz stressed the same point of view in his address at the Royal Academy in Amsterdam in 1915: "To the ether all substantiality is denied to the extent that we cannot speak of rest or motion with respect to it."[247] That substantiality meant for Lorentz the possession of mechanical characteristics

[244] T. Hirosige, "Origins of Lorentz' Theory of Electrons and the Concept of the Electromagnetic Field," *Hist. Stud. Phys. Sci., 1* (1969), 151–209.

[245] H. A. Lorentz, *Theory of Electrons,* p. 230.

[246] H. A. Lorentz, "Alte und neue Fragen der Physik," *Phys. Zeits., 11* (1910), 1234–1257; *Collected Papers, 7,* 205–257. Quotation is from p. 210: "Schliesslich ist ihm nur noch soviel Substantialität geblieben, dass man durch ihn ein Koordinatensystem festlegen kann. Selbst dieser letzte Rest der Substantialität wird durch das *Relativitätsprinzip* angegriffen. . . ."

[247] H. A. Lorentz, "De lichtaether en het relativiteitsbeginsel," *Jaarboek Kon. Acad. Wet.* (1915); *Collected Papers, 9,* 233–243. Quotation is from p. 238: "Aan den aether wordt in die mate alle substantialiteit ontzegd, dat men van rust of beweging to opzichte van hem selfs niet kan spreken."

is apparent also from the following discussion concerning the electromagnetic mass of the electron: "By our negation of the existence of material mass, the negative electron has lost much of its substantiality. We must make it preserve just so much of it, that we can speak of forces acting on its parts, and that we can consider it as maintaining its form and magnitude."[248]

Since the ether, as the substance of electromagnetic phenomena, must possess mechanical characteristics and hence serve as reference system for rest or motion, it is evident that the foundation of electromagnetic theory should be the electromagnetic equations in the coordinate system that is fixed with respect to the ether. Although highly appreciative of Einstein's theory, Lorentz, therefore, held to his theory to the end of his life.[249] He thought that physicists were free to choose either the theory based on the ether or the theory of relativity,[250] and that "each physicist can adopt the attitude which best accords with the way of thinking to which he is accustomed."[251] We must bear in mind that these statements by Lorentz all belong to the period after the advent of the theory of relativity. Earlier, Lorentz had, to my knowledge, never explicitly expressed the idea that the ether determined a physically privileged coordinate system, or that, in other words, the ether furnished an absolute frame of reference, although the ether did play such a role in his theory. The absence of such a statement by Lorentz tends to corroborate my conclusion about the ether problem, namely, that its issue was not finding an absolute frame of reference. Lorentz, I suppose, did not become aware of the kinematical significance of the role played by the ether in his theory until he compared his theory of electrons with Einstein's theory. His statements show that he was unable to reach the theory of relativity and later continued to reject it because he had deeply committed himself to the mechanistic worldview—the view that mechanics should be assigned the primary place in the edifice of physics—without being clearly aware of his commitment.

[248]H. A. Lorentz, *op. cit.* (note 156), p. 43.

[249]Max Born wrote: "When I visited Lorentz a few years before his death, his scepticism [about the theory of relativity] had not changed." M. Born, *Physics in My Generation* (London, 1956), p. 192.

[250]H. A. Lorentz, *op. cit.* (note 247), *Collected Papers, 9,* 241.

[251]H. A. Lorentz, "Considération élémentaire sur le principe de relativité," *Revue gén. des sci., 25* (1914), 179; *Collected Papers, 7,* 147–165. Quotation is from p. 165: "Chaque physicien pourra prendre l'attitude qui s'accorde le mieux avec la façon de penser à laquelle il s'est accoutumé."

The privileged position of mechanics in Poincaré's scientific thought, too, is closely connected with his conception of the principle of relativity as an empirical law rather than a postulate. The principle of relativity here is the principle that it is impossible to experimentally detect motion relative to the ether. Poincaré first formulated the "principle of relative motion" as a general principle of mechanics and then extended it to the "principle of relativity."[252] The principle of relative motion states that "the motion of any system whatever ought to obey the same laws, whether it is referred to fixed axes or to the moving axes drawn by rectilinear and uniform motion."[253] It is, in other words, the principle of relativity as we understand it, within the limit of mechanics. This principle, in Poincaré's view, is not merely a general expression of empirical facts but implies certain elements which transcend experience. It therefore claims a different epistemological status than his "principle of relativity."

As is well known, Poincaré considered axioms of geometry to be conventions.[254] He did not mean, however, that axioms of geometry are an entirely arbitrary invention of the human mind. In the process of selection "our choice among all possible conventions is *guided* by experimental facts" (italics original).[255] The criterion according to which they are chosen is convenience. They are subject to the requirement that they do not contradict each other. From the chosen axioms we then logically construct the whole theory of geometry. By their origin, the axioms are only conventions or "definitions in disguise," but at the same time they are therefore absolutely true. Poincaré thought that nearly the same was true for mechanics: "The principles of this science, although more directly based on experience,

[252]H. Poincaré, [a] "La theorie de Lorentz et le principe de reaction," *Arch. néerl.*, *5* (1900), 252–278; *Oeuvres, 9,* 464–488. The relevant place is on p. 482. [b] *La science et l'hypothèse,* p. 135. [c] *Op. cit.* (note 169), *Oeuvres, 9,* 552; *Science et méthode,* p. 217. Poincare's distinction between *principe du mouvement relatif* and *principe de relativité* is also noted by Arthur I. Miller, *op. cit.* (note 16), pp. 233–234.

[253]H. Poincaré, *La science et l'hypothèse,* p. 135: "Le mouvement d'un système quelconque doit obéir aux mêmes lois, qu'on le rapporte à des axes fixes, ou à des axes mobiles entraînes dans un mouvement rectiligne et uniforme."

[254]H. Poincaré, *La science et l'hypothèse,* Chaps. 3–5.

[255]*Ibid.,* p. 66: "Notre choix, parmi toutes les conventions possibles, est *guidé* par des faits expérimentaux." The English quotation is from *Science and Hypothesis,* p. 50.

still share the conventional character of the geometrical postulates."[256] In one respect, the principles of mechanics are based on experiments. For an almost isolated system, they can be approximately confirmed by experiment. In the system of mechanics, however, they are generalized so that they become postulates to be applied to the whole universe and are regarded as exactly true, "because they reduce in final analysis to a simple convention that we have the right to make, because we are certain beforehand that no experiment can contradict it."[257]

In the physical sciences other than mechanics, "the scene changes. We meet hypotheses of another kind."[258] A hypothesis in physics "should always be submitted to verification as soon as possible and as many times as possible."[259] It goes without saying that, if it cannot stand this test, it must be abandoned without any hesitation. The most general of the hypotheses that have stood the test are, according to Poincaré, the principle of the conservation of energy, Carnot's principle (the second law of thermodynamics), and the principles of action and reaction, of relativity, of the conservation of mass, and of least action. In mechanics, all but Carnot's principle have the character of a convention and are therefore exactly true. As a consequence of the extensive development of physics during the nineteenth century, the principles have been extended to fields other than mechanics, confirmed there too, and now are considered "experimental truths."[260] Poincaré recognized that, as the other physical principles, the "principle of relativity" "is no longer a convention. It is verifiable, and consequently it need not be verified."[261] In fact, the rapid and unexpected development of physics around the turn of the century seemed to Poincaré to undermine the fundamental principles

[256] *Ibid.*, p. 5: "Les principes de cette science, quoique plus directement appuyés sur l'expérience, participent encore du caractère conventionnel des postulats géométriques." The English quotation is from *Science and Hypothesis,* p. xxvi.

[257] *Ibid.*, pp. 162–163: "C'est qu'ils se réduisent en dernière analyse à une simple convention que nous avons le droit de faire, parce que nous sommes certains d'avance qu'aucune expérience ne viendra la contredire." The English quotation is from *Science and Hypothesis,* p. 136.

[258] *Ibid.*, p. 6.

[259] *Ibid.*, p. 178: "Elle doit toujours être, le plus tôt possible et le plus souvent possible, soumise à la vérification."

[260] H. Poincaré, *op. cit.* (note 166).

[261] H. Poincaré, "L'espace et le temps," *Scientia, 12* (1912), 159–171, esp. 168; *Dernières pensées* (Paris, 1913), p. 105: "Il est vérifiable et par conséquent il pourrait n'être pas verifié."

of physics. He therefore tried, in his St. Louis lecture in 1904, to diagnose the situation in physics and to seek a way out of the crisis of the principles. Although in 1904 he felt that the principle of relativity had been saved by the Lorentz theory, the result of Kaufmann's 1905 experiment showing that the electromagnetic mass of the electron varies according to Abraham's formula rather than Lorentz' once again caused him anxiety. Referring to Kaufmann's result in 1908, he admitted that "it would seem that the Principle of Relativity has not the exact value we have been tempted to give it."[262]

Poincaré's anxiety about the validity of the principle of relativity originated in the very distinction he made between the epistemological status of mechanics and that of other branches of physics. According to his philosophy of science, mechanics provides the frames for describing the processes in nature, which, in mechanics, are conventions and consequently claim strict validity; the theories of other branches of physics are developed so as to conform to these frames. In a lecture delivered in London in the spring of 1912, Poincaré asserted about the most fundamental frame, space, that its definition was reduced to the proposition that the form of the equations of *dynamics* should not be altered by transformations of the coordinate axes.[263] As far as he maintained such a point of view, it must have been inconceivable for him to postulate a universal principle of relativity for both mechanics and electromagnetism that treated the two sciences as equals. Several historians of physics have discussed reasons why Poincaré was not able to reach the theory of relativity. I claim that, as with Lorentz, the most fundamental reason is his mechanistic worldview: as Lorentz, Poincaré believed that mechanics must be assigned primacy in the epistemological structure of the whole of physics.

12. EMANCIPATION FROM THE MECHANISTIC WORLDVIEW AND THE THEORY OF RELATIVITY

The discussion in the preceding section suggests that for the emergence of the theory of relativity a complete emancipation from the mechanistic worldview was the essential prerequisite. Einstein's

[262] H. Poincaré, *op. cit.* (note 169), *Oeuvres, 9,* 572; *Science et méthode,* p. 248: "Le Principe de Relativité n'aurait donc pas la valeur rigoureuse qu'on était tenté de lui attribuer." The English quotation is from *Science and Method,* p. 228.

[263] H. Poincaré, *op. cit.* (note 261), p. 169; *Dernières pensées,* p. 107.

theory of relativity did not intend to reduce either mechanics or electromagnetism to the other, or to assign primacy to one over the other as did Lorentz' and Poincaré's. Einstein reached the theory of relativity by searching for a unification of mechanics and electromagnetic theory at a higher level. For the idea of postulating a universal principle of relativity to arise it was of crucial importance that mechanics and electromagnetism were considered to be of equal standing. Unification of the two theories had to take precedence even over the modification of the concepts of space and time.

The process by which Einstein's theory was gradually accepted during the latter half of the first decade of this century confirms the importance of the complete emancipation from the mechanistic worldview. In fact the purport and significance of Einstein's theory had been misunderstood. Accordingly, physicists did not generally accept it, until they recognized that it was concerned not only with electrodynamics but also with mechanics, that is, that the fundamental postulates of the theory of relativity were universal principles to which mechanics as well as electrodynamics was to be subjected. Such a recognition contradicted the mechanistic worldview.

Walter Kaufmann was the first to cite Einstein's 1905 relativity paper in his article on the mass of the electron. For several years Kaufmann had been engaged in experiments to determine the change of the mass of the electron with change in velocity. In 1901, measuring the electric and magnetic deflection of Becquerel rays, he confirmed that the electron mass increased with its velocity and estimated that the electromagnetic mass of the electron was comparable in its magnitude to the mechanical mass.[264] In the following two papers published in 1902 and 1903, respectively,[265] Kaufmann concluded that the mass of electrons in Becquerel and cathode rays is entirely electromagnetic. These results greatly interested physicists in connection with the electromagnetic view of nature, which was advocated by Wilhelm Wien and Max Abraham.[266] Lorentz, A. H.

[264] W. Kaufmann, "Die magnetische und electrische Ablenkbarkeit der Becquerelstrahlen und die scheinbare Masse der Elektronen," *Gött. Nachr., Math.-phys. Kl.* (1901), pp. 143–155.

[265] W. Kaufmann, "Ueber die electromagnetische Masse des Elektrons," *Gött. Nachr., Math.-phys. Kl.* (1902), pp. 291–296; "Ueber die 'Elektromagnetische Masse' der Elektronen," *Gött. Nachr., Math.-phys. Kl.* (1903), pp. 90–103.

[266] W. Wien, "Ueber die Möglichkeit einer elektromagnetischen Begründung der Mechanik," *Verh. d. Deutsch. Phys. Ges., 8* (1906), 136–141; *Physika-*

Bucherer, and Paul Langevin joined the discussion about electromagnetic mass and the constitution of the electron.[267] Late in 1905 Kaufmann arrived at a definite conclusion about the constitution of the electron. Judging his measurement of the deflections of beta-rays to favor Abraham's rigid sphere electron, he declared that the theory of Lorentz and Einstein was definitely rejected.[268] Kaufmann's conclusion was challenged in the following year by Max Planck.[269] Planck tried to derive the velocity dependence of the electron mass by making use of Einstein's theory and asserted that Kaufmann's result could not refute the Lorentz-Einstein theory conclusively. It was in 1908 that Bucherer for the first time obtained an experimental result in favor of Lorentz' and Einstein's formula of the electron mass,[270] but the experiment was so delicate that the result did not convince all physicists. Disputes continued for a few more years. As late as August 1910 Jakob Johann Laub, in his review article "On the Experimental Foundation of the Relativity Principle," had to admit that there did not yet exist an unequivocal conclusion.[271]

Thus the velocity dependence of the electron mass became one of the topics that was most actively discussed by both experimental and theoretical physicists for nearly ten years after 1905. In textbooks the researches on this subject are often cited as the first experimental verification of the theory of relativity, but, in reality,

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lische Abhandlungen und Vorträge, 2, 115–120. [b] "Die Kaufmannschen of the electromagnetic view of nature see Russell McCormmach, *op. cit.* (note 243).

267 A. H. Bucherer, *Mathematische Einführung in die Elektronentheorie* (Leipzig, 1904); P. Langevin, "La physique des électrons," *Revue gén. des sci., 16* (1905), 257–276; *La physique depuis vingt ans* (Paris, 1923), pp. 1–69.

268 W. Kaufmann, "Über die Konstitution des Elektrons," *Sitzb. preuss. Akad. Wiss.* (1905), pp. 945–956.

269 Max Planck, [a] "Das Prinzip der Relativität und die Grundgleichungen der Mechanik," *Verh. d. Deutsch. Phys. Ges., 8* (1906), 136–141; *Physikalische Abhandlungen und Vorträge, 2,* 115–120. [b] "Die Kaufmannschen Messungen der Ablenkbarkeit der β-Strahlen in ihrer Bedeutung für die Dynamik der Electronen," *Phys. Zeits.,* 7 (1906) 753–761; *Phys. Abhandlungen und Vorträge, 2,* 121–135.

270 A. H. Bucherer, "Messungen an Becquerelstrahlen. Die experimentelle Bestätigung der Lorentz-Einsteinschen Theorie," *Phys. Zeits., 9* (1908), 755–762; "Die experimentelle Bestätigung des Relativitätsprinzips," *Ann. d. Phys., 28* (1909), 513–536.

271 J. Laub, "Über die experimentellen Grundlagen des Relativitätsprinzips," *Jahrb. d. Rad. u. Elekt.,* 7 (1910), 405–463, esp. 462.

contemporary physicists, being concerned primarily with the constitution and mass of the electron, did not ascribe this meaning to their work. By scrutinizing their earlier discussions we find that none of them were conscious of the fundamental difference between Einstein's and Lorentz' theories. Even Planck, who was the first to appreciate and encourage Einstein, did not clearly distinguish the two theories. The only difference he recognized was that Einstein's method was more general than Lorentz'; he spoke of "the 'principle of relativity' recently introduced by H. A. Lorentz and in a more general manner by A. Einstein."[272] Discussing Kaufmann's deflection experiment of 1905, he called the theory of the deformable electron "Lorentz-Einstein's theory."[273] Planck's expression for the principle of relativity, "the postulate that the absolute motion can never be detected,"[274] reminds us rather of Poincaré's definition. Poincaré never mentioned Einstein's name in his many articles referring to the electron mass.[275] His concern in those articles was exclusively with the questions if "the dynamics of the electron" compel us to modify one of the principles of mechanics, the invariability of mass, and how this should be done. Abraham, Kaufmann, Arnold Sommerfeld, and others who defended Abraham's rigid sphere model of the electron, supporting the electromagnetic view of nature as the new mode of physics, refuted Lorentz' and Einstein's formula saying that it upheld the old-fashioned mechanistic view.[276] To our eyes, however, their alleged new mode, the electromagnetic view of nature, is as limited as the mechanistic view of nature because it attempted to substitute electromagnetism for mechanics instead of denying a privileged position to any branch of physics. As the mechanists had tried to reduce the whole of physics to mechanics, so

[272]M. Planck, *op. cit.* (note 269), [a], *Phys. Abhandlungen und Vorträge, 2,* 115; Planck used "das vor kurzem von H. A. Lorentz und in noch allgemeinerer Fassung von A. Einstein eingeführte 'Prinzip der Relativität'."

[273]M. Planck, *op. cit.* (note 269), [b], *Phys. Zeits.,* 7 (1906), 761; this discussion following Planck's paper is omitted from the *Phys. Abhandlungen und Vorträge.*

[274]*Ibid.,* p. 756.

[275]H. Poincaré, *op. cit.* (notes 169, 172, and 173), and "La mécanique nouvelle," in Poincaré, *Sechs Vorträge über ausgewählte Gegenstände aus der reinen Mathematik und mathematischen Physik* (Leipzig, 1910), pp. 49–58.

[276]See the discussion of Planck's paper at the meeting of the Society of German Natural Scientists and Physicians. *Op. cit.* (note 269), [b], *Phys. Zeits.,* 7 (1906), 760–761.

holders of the electromagnetic view of nature tried to reduce physics to electromagnetism. They never thought of subordinating both mechanics and electromagnetism to one universal principle. Accordingly, they regarded the principle of relativity only as a mechanical principle and thus failed to realize the universal significance of Einstein's principle of relativity.

Apart from Einstein, Hermann Minkowski was the first to state clearly that the principle of relativity was to be postulated universally for the whole of physics and, consequently, that the theory of relativity required a radical transformation of the fundamental concepts of physics. On this insight he tried in December 1907 to formulate a relativistic mechanics for an extended body.[277] Minkowski argued that, although many physicists believe that classical mechanics contradicts the postulate of relativity which he adopted in his paper as the foundation of electrodynamics, "it would be quite unsatisfactory if the new concept of time . . . were considered valid only in a limited domain of physics."[278] His recognition of the universal implication of the principle of relativity must have been closely connected with his discovery that Einstein's theory could be expressed in a four-dimensional form. In fact, he almost simultaneously put forth the idea of formulating the theory of relativity in a four-dimensional space. A month before he presented his paper on relativistic mechanics to the Göttingen Academy, on 5 November 1907, he outlined the four-dimensional formulation of the theory of relativity in an address at the Mathematical Society of Göttingen. He began his address with the words: "Starting from the electromagnetic theory of light, a complete transformation seems about to happen in our notions of space and time."[279] No physicist before Minkowski had spoken of a transformation of the notions of space

[277]H. Minkowski, "Mechanik und Relativitätspostulat," appendix to "Die Grundgleichungen für die elektromagnetischen Vorgänge in bewegten Körpern," *Gött. Nachr., Math.-phys. Kl.* (1908), pp. 53-111; *Zwei Abhandlungen über die Grundgleichungen der Elektrodynamik* (Leipzig, 1910), pp. 5–57; appendix, pp. 45–57.

[278]*Ibid.,* p. 45: "Es wäre höchst unbefriedigend, dürfte man die neue Auffassung des Zeitbegriffs, die durch die Freiheit der Lorentz-Transformationen gekennzeichnet ist, nur für ein Teilgebiet der Physik gelten lassen."

[279]H. Minkowski, "Das Relativitätsprinzip," *Ann. d. Phys., 47* (1915), 927–938, esp. 927: "Von der elektromagnetischen Lichttheorie ausgehend, scheint sich in der jüngsten Zeit eine vollkommene Wandlung unserer Vorstellungen von Raum und Zeit vollziehen zu wollen. . . ."

and time.[280] Minkowski, being a mathematician, must have been able to focus his attention on the mathematical, formal side of the theory of relativity without being troubled by any special physical view of nature, whether mechanistic or electromagnetic. And because he focused his attention on the formal relations he was able to grasp the full implication of Einstein's theory.

Historical analysis of the reception of Einstein's theory of relativity still remains to be further advanced before we can make a conclusive pronouncement. In such an analysis historians would have to give due consideration to the parts played by the work of Max Planck, Ebenezer Cunningham, A. H. Bucherer, Gilbert Newton Lewis and others.[281] Even without such historical analysis, however, we may reasonably conclude that Minkowski's papers cited above as well as his famous lecture "Space and Time" in 1908[282] played a fundamental part in drawing physicists' attention to the conceptual transformation involved in the theory of relativity. In 1910 Lorentz, in his course of lectures delivered in Göttingen, stated that to deny the existence of a "true" time means to follow Einstein's and Minkowski's thought.[283] Minkowski's work had the remarkable effect of giving a major stimulus to the study of mechanics from the relativistic point of view. Max Born, who had been Minkowski's assistant for a few weeks just before the latter's premature death, developed some relativistic mechanical concepts along the lines pioneered by Minkowski.[284] Philipp Frank, too, was induced by Minkowski's

[280]Of course Einstein here, too, is the exception. In the spring of 1905 he wrote to his friend Conrad Habicht about his current studies: "Die vierte Arbeit liegt im Konzept vor und ist eine Elektrodynamik bewegter Körper unter Benützung einer *Modifikation der Lehre von Raum und Zeit*" (italics mine). Carl Seelig, *op. cit.* (note 189), p. 89.

[281]Papers thus far published that partially meet this need are: T. Hirosige, "Syoki no Sôtaironteki Rikigaku" ("Relativistic Mechanics in its Early Stage"), *Buturigakusi Kenkyû, 4* (1968), 39-54; *5* (1969), 55-70; *6* (1970), 27-61. Stanley Goldberg, "In Defense of Ether: The British Response to Einstein's Special Theory of Relativity, 1905-1911," *Hist. Stud. Phys. Sci., 2* (1970), 89-125.

[282]H. Minkowski, "Raum und Zeit," *Jahresber. d. Deutsch. Math. Verein., 18* (1908), 75-88; *Phys. Zeits., 10* (1909), 104-111.

[283]H. A. Lorentz, *op. cit.* (note 246), *Collected Papers, 7*, 211.

[284]M. Born, "Die träge Masse und das Relativitätsprinzip," *Ann. d. Phys., 28* (1909), 571-584; "Die Theorie des starren Elektrons in der Kinematik des Relativitätsprinzips," *Ann. d.* Phys., *30* (1909), 1-56, 840; "Über die Dynamik des Elektrons in der Kinematik des Relativitätsprinzips," *Phys. Zeits., 10* (1909), 814-817.

paper to attempt a systematization of the theory of relativity comprising both electrodynamics and mechanics. In a paper presented to the Viennese Academy in March 1909[285] he succeeded in showing that, if the principle of relativity is accepted as a universal principle, then from this starting point both electrodynamics and mechanics can be systematically developed. He stated in the introduction to his paper that he had been motivated by the "wealth of ideas" in Minkowski's paper of December 1907. Fróm 1908 on, physicists began to publish many papers in which they applied the theory of relativity to problems in not only electrodynamics or optics but also mechanics. The problems in mechanics included subjects such as the equation of motion of a point mass and relativistic definitions of a rigid body. Needless to say, if physicists discuss mechanical problems from the relativistic point of view, they must assume that relativistic conceptions are valid also in mechanics. The rising interest in relativistic mechanics after 1907, therefore, shows that the universal importance of the theory of relativity was rapidly becoming understood correctly and the theory widely accepted.

Side by side with the general acceptance of the theory of relativity went the recognition that it invalidates the mechanistic worldview. In September 1910 Planck, who now had clearly recognized the fundamental difference between Lorentz' and Einstein's theories and the significance of the modification of the concepts of space and time, declared at the Königsberg meeting of the Society of German Natural Scientists and Physicians: "He who considers the mechanistic view of nature the postulate of the physical way of thinking will never be able to make friends with the relativity theory."[286]

13. CONCLUDING REMARKS

Einstein's achievement and the failure of Lorentz and Poincaré to reach his understanding of relativity raise questions about the growth of science. Scientific research problems are not forced upon

[285] P. Frank, "Die Stellung des Relativitätsprinzips im System der Mechanik und der Elektrodynamik," *Sitzungsb. Wiener Akad. Wiss., 118* (1909), 373–446, esp. 376.

[286] M. Planck, "Die Stellung der neueren Physik zur mechanischen Weltanschauung," *Verh. Ges. Deutsch. Naturf. u. Ärzte, 82* (1910), part 1, pp. 58–75; *Phys. Abhandlungen und Vorträge, 3,* 30–46, esp. 39: "Wer daher die mechanische Naturanschauung als ein Postulat der physikalischen Denkweise ansieht, wird sich mit der Relativitätstheorie nie befreunden können."

scientists automatically by nature. They are the questions that scientists ask nature on the basis of their views of nature and science. The problems of scientific research can be formulated only in correlation with the views of nature and science. Historians, whose view of science is affected by the science of their own time, have not always been able to change their perspective when studying scientific developments that differed greatly from the current circumstances of science. The contrast between most of today's scientific research—the puzzle solving in normal science, as Thomas S. Kuhn has called it[287]—and the creative period in physics around the turn of the century and the first decades afterwards is reflected in the manner in which historians have considered the origin of the theory of relativity.

As the integration of science into the systems of society has advanced, various institutions and the legislation to promote and organize scientific research have been further and further augmented, and under the stimulus of these changes the instruments and facilities for scientific research have undergone rapid technological innovation and an enormous growth in size and complexity. Such recent trends affect the practice of research so that the part occupied by routine work in scientific research is continually on the increase.[288] Project research dominates today's science, and in project research the work is divided into separate parts that are assigned to different scientists. Work on these fragments of projects, performed in by far the majority of cases according to prescription, is not a creative intellectual adventure. In these cases, scientific research is reduced to the manipulation of instruments, data, formulas, and so on, and consequently such factors as philosophical inclination, worldview, and the idiosyncracies of scientists become of little significance. What is important here is only the technical skill of the scientists.

If the history of science is likely to reflect the view of science held

[287]Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd ed. (Chicago, 1970), Chap. 4.

[288]For an introductory discussion of the integration of science into the systems of society and its influence on the qualities of contemporary scientific research, see Tetu Hirosige, *Kagaku no Syakaisi: Kindai Nihon no Kagaku Taisei* (*A Social History of Science: The Social System of Science in Modern Japan*) (Tokyo, 1973), the introductory and final chapters. In English literature I refer the reader to Jerome R. Ravetz' discussion of what he calls the industrialized science: Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (Oxford, 1971), esp. Chap. 2.

by the historian, as Alexandre Koyré has suggested,[289] then the modern historian, responding to the trends in science characterized above, will inevitably confine his attention to technical details in the science of the past. Limited by the point of view that corresponds to the characteristics of present-day science, he will neglect to pay due attention to the changes in aspect that give rise to scientific innovation.

The most important feature of scientific innovation is that, to achieve it, the scientist has to bring innovation to the problem itself and to abandon preconceptions. Scientific innovation begins when he perceives a problem to be studied that formerly has not existed as a scientific problem. The theory of relativity, needless to say, was an innovation in the sense just emphasized. It is therefore no surprise that Lorentz and Poincaré, who pursued the ether problem in its traditional formulation, could not create the theory of relativity, eminent though they were in vision and competence. The ether problem did not contain the factor that alone could cause the transformation of the problem structure. For the transformation to be effected, the premise that had made the ether problem the central concern of physicists had to be doubted and abandoned. We have found that premise in the worldview that holds that any physical substance ought to be characterized only by mechanical categories. According to this view, motion of and relative to the ether must always have physical consequences. It was this view that had to be changed.

In view of the close correspondence between the scientist's views of nature and science and his formulation of the problems of scientific research, Mach's refutation of the mechanistic worldview was of crucial importance for the formation of the theory of relativity. Certainly, Mach's criticism of the concepts of absolute space and time, holding that determinations in space and time are no more than the determinations of an event by other events, must have been suggestive to Einstein. But it could be suggestive only after he, viewing the problem situation from a new aspect, had discovered the new problem to be attacked, that is, only after Mach's refutation of the mechanistic worldview had provided him with the new perspective. In this sense I see Mach as having made the most fundamental contribution to the emergence of Einstein's theory of relativity.

[289] A. Koyré, *Étude d'histoire de la pensée scientifique* (Paris, 1966), pp. 71–72 and *passim.*

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