Getting rid of the Ether

Could Physics have achieved it sooner, with better assistance from Philosophy?

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ABSTRACT: Assuming, with Hasok Chang, that the history and philosophy of science can contribute to scientific knowledge, particularly when it is a matter of disposing of groundless or useless notions, I examine the case of the luminiferous ether, and seek to ascertain what factors may have kept it alive until 1905, when Einstein declared it superfluous.

Keywords: ether, lines of force, Maxwell-Lorentz electrodynamics, special relativity.

Die Einführung des Namens 'Äther' in die elektrischen Theorien hat zur Vorstellung eines Mediums geführt, von dessen Bewegung man sprechen könne, ohne daß man wie ich glaube, mit dieser Aussage einen physikalischen Sinn verbinden kann.

Einstein to Mariç, August 1899.[1]

Hasok Chang (2004) bids us regard the history and philosophy of science (HPS) as a *continuation of science by other means*. In the particular case of physics this entails that we ought to expect to achieve epistemic advances beyond the reach of the ordinary scientific methods of experiment and observation, theoretical modeling and calculation, through the HPS methods of critical reflection about physical concepts, propositions and theories, and of intelligent gathering and reading of physics' historical records. I substantially agree with Chang's views. Indeed, it is obvious to me that the enormous literature concerning the interpretation of quantum mechanics mainly relies on the HPS methods I have just mentioned and that, if the effort invested on it ever comes to fruition, this success should be counted as a triumph of philosophy. While we wait, seemingly without end, for such a triumph, it is perhaps advisable to do some case studies —at a historico-critical "metalevel", so to speak—, to see if any intellectual forces may have inhibited or hindered the application of HPS methods in past episodes of science in which it appears to us now that they could have promptly suc-

[1] "The introduction of the noun 'ether' in the theories of electricity has led to the idea of a medium, of whose motion one could speak, without, I believe, being able to attach a physical meaning to one's utterance" (Einstein 1987- , 1: 226). I owe this striking quotation to John Stachel (2002), p. 171, whose English translation I reproduce with minor changes.

ceeded. The lessons we get from such studies might be of some help to us in our present quandary.

HPS methods would appear to be particularly apt for spotting any concepts in a scientific theory for which there is actually no need, thus making it easier for science to get rid of them. One such concept figured prominently in classical electrodynamics, namely, the luminiferous ether, until Einstein (1905) eliminated it. It is generally agreed that Einstein's timely reading of philosophers —Hume and Kant, Mach and Poincaré— contributed to this and other momentous decisions he took at that time. However, one may wonder whether the philosophers, if they had thought more intently and more knowledgeably on these matters, could not have disposed of the ether much sooner. After all, the ether is a chimera whose acceptance blatantly defied Newton's banning of hypotheses, and Faraday had said already in 1846 that the electric and magnetic fields countenanced by him did not require any material support.

In the introduction to the famous paper, "Zur Elektrodynamik bewegter Körper", in which Einstein laid down the foundations of Special Relativity (SR), he wrote: "The introduction of a 'light-ether' will prove to be superfluous".[2] This statement reminds me of the little child's cry "But he's got nothing on!" in Andersen's tale about the emperor's new clothes. Just as the false tailors disappear from the tale as soon as the child has spoken, so ether, which for centuries had filled the universe (in the dreams of Reason), vanished like an exorcised ghost after that short remark by the junior patent official in Bern. Well, I admit there is some exaggeration in this statement of mine, for Relativity was stiffly resisted at first by numerous scientists and philosophers. Still, it does not appear that they thought Relativity was false *because* it denied the ether.[3] By contrast, *ca.* 1905 a new physical theory that denied radioactivity would have been laughed out of court. Surely, then, the existence of the light-ether was not supported by any hard facts. Indeed, it would seem that there was not much to be said for it, and that, therefore, a team of philosophers of science could have promptly got rid of it through conceptual criticism. This would have spared 19th-century science many fruitless efforts.

However, even a superficial glance at the history of ideas makes it doubtful that mere philosophers could have succeeded in suppressing the ether. For, as G.N. Can-

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[2] "Die Einführung eines 'Lichtäthers' wird sich insofern als überflüssig erweisen, als nach der zu entwickelnden Auffassung weder ein mit besonderen Eigenschaften ausgestatteter 'absolut ruhender Raum' eingeführt, noch einem Punkte des leeren Raums, in welchem elektromagnetische Prozesse stattfinden, ein Geschwindigkeitsvektor zugeordnet wird" (Einstein 1905, p. 277). Kilmister translates 'Lichtäther' as 'luminiferous ether'. I have preferred the more literal translation of the same expression by D.E. Jones in his authorized translation of Hertz (1893), p. 24.

[3] At least I am not aware of anyone having raised this objection. H.A. Lorentz insisted to the end of his life that his own ether-based electrodynamics of moving bodies was, in its mature version of 1904, just as good as Einstein's, with which it is empirically equivalent (both yield *exactly* the same predictions); but he never claimed that the latter was disqualified by its denial of ether. On the other hand, I learnt recently that J.J. Thomson said as late as 1909 that ether is as essential to us as the air we breathe, and that the study of this substance is the most important task of physics (quoted in French translation by Samueli and Boudenot 2005, p. 107.)

tor (1981) showed, the existence of one or more ethers was regarded by many scientists as a material aid to their Christian or spiritualistic beliefs, and we know all-too-well, after 350 years of Enlightenment, that ideas benefiting from this connection are dreadfully resilient and impervious to criticism. The religious associations of ether — or *æther*— are implicit in its name, a transcription of the Greek word αἴθηρ, which to Homer meant 'heaven' (*Iliad*, 16.365), and which Aristotle adopted as the name of the fifth element, the changeless stuff that the heavens are made of.

After Tycho, tracking the comet of 1572, showed that the planets are not affixed to impenetrable spheres, the word 'ether' became redundant and was ready to acquire a new meaning. The conception of ether as a fluid, transparent, extremely subtle form of matter that thoroughly fills the vast interstellar and interplanetary spaces is usually attributed to Descartes.[4] In the *Principles of Philosophy*, Descartes consistently refers to this form of matter as 'the second element' (1644, III, §§ 52, 70, 82, 123; 1996, 8: 105, 121, 137, 172).[5] However, in the same year in which this book was published, Sir Kenelm Digby used 'aether' in English to designate the transparent interstellar matter (*Oxford English Dictionary*, s.v. **ether**, 5.a), and the term was later employed in this sense, as a matter of course, in the British Isles and in the Continent.[6] According to Descartes, this form of matter is immensely more abundant than the two other kinds acknowledged by him: the opaque matter of the Earth and the other planets, and the radiant matter of the Sun and the stars.

The Cartesian ether, which lacked even the faintest trace of empirical evidence, was postulated on purely a priori grounds, namely, that the existence of an empty space is a contradiction in terms.[7] It was held to be the vehicle of light, either as a rigid solid that transmits it instantly (according to Descartes 1637, pp. 3-6; 1996, 6: 83-86), or as an elastic fluid through which light propagates with finite speed in the guise of longitudinal waves (according to Huygens, 1690). But the ether also played a central role in the explanations of planetary motion and free fall proposed by Descartes himself and other Cartesian physicists. These explanations rest on the assumption that any portion of ether in motion will act by impulse on other portions of it and on other kinds of matter. So the planets are carried around the Sun by ether whirlwinds, like logs driven by a river. On the other hand, terrestrial bodies are made heavy by the

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4 For instance, by Whittaker 1951/53, 1: 5: "Space is thus, in Descartes' view, a *plenum*, being occupied by a medium which, though imperceptible to the senses, is capable of transmitting force, and exerting effects on material bodies immersed in it—the *aether*, as it is called". The diligent reader will notice that Whittaker does not say that Descartes himself used this word to call it.

5 In the early, posthumously published book *Le monde*, he called it 'air', but not without warning the reader that it must not be confused with "this gross air we breathe" (Descartes 1996, 10: 28).

6 See Newton's posthumous papers in Hall & Hall (1962), pp. 94, 112, 220. Also Huygens (1888-1950), 19: 472, 473, 573; 21: 454; cf. 19: 463; Leibniz (1965), 4: 469.

7 "If you wish to conceive that God takes away all the air in a room, without replacing it with another body, then by the same token you must conceive that the walls of this room come together, or else there will be a contradiction in your thought" —Descartes to Mersenne, 9 January 1639 (in Descartes 1996, 2: 482).

relative lightness of celestial matter, which, however, is not an irreducible quality, but a consequence of the greater speed with which this subtle kind of matter moves upward, through the pores of gross terrestrial bodies, leaving them behind: "This celestial matter has more force to move away from the centre around which it turns than any of the parts of the Earth, which makes that it is light relatively to them" (Descartes 1644, IV, §22; 1996, 9-2: 211).[8]

In Newton's *Principia*, much of Book II was designed to prove that the phenomena of the Solar System cannot be accounted for by ether vortices (not, at any rate, under Newton's Laws of Motion). And Newton's well-known declaration that he does not feign hypotheses, coming as it does right after his admission that he has been unable to assign a cause to gravity, can surely be read as a gibe at the ether-based Cartesian theories. In Part III, §44, of his *Principia,* Descartes declared that he did not claim to have found the "genuine truth" concerning the physical questions he dealt with, and that what he would henceforth write about them should be understood "as an hypothesis" (the French translation adds: "that is perhaps very far from the truth"; as I pointed out in footnote 8, this is probably from Descartes own hand). Nevertheless, if every consequence inferred from such an hypothesis fully agrees with experience, "we shall gather from it no less utility for life than from the knowledge of truth itself" (1644, III, §44; 1996, 8: 99).[9] In unwitting —or was it deliberate?— opposition to Descartes' words, Newton's First Rule of Philosophy prescribes that no causes of natural things should be admitted *unless they are true* (Newton 1726, p. 387; 1999, p. 794). For the purposes of human science, it is enough that a natural thing like gravity "really exists and acts according to the laws that [one has] set forth and is sufficient to explain all the motions" that one links to it.[10] "To tell us that every Species of Things is endow'd with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phænomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd" (Newton 1721, p. 377).

Newton's methodological attitude did not favor the ether, which, being intangible and invisible, can only exist hypothetically. During the 18th century, as Newton's in-

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[8] I quote from Picot's French translation, which is more explicit than the Latin. Many of the additions in Part III, §§ 41ff. and in Part IV, were probably written by Descartes himself (see Adam's *Avertissement* in Descartes 1996, 9-2: ix-xix). For a sympathetic exposition of Cartesian theories of gravity, see Aiton 1972, or the shorter presentation in Aiton 1989.

[9] Again, the French translation explains: "…for one will be able to employ it in the same way to dispose natural causes to produce the effects one will desire"; see Descartes 1996, 9: 123.

[10] In the century following Newton, the finest physicists, such as d'Alembert or Fourier, insisted that they did not seek for causes but for laws. But the run-of-the-mill scientific practitioners obscurely thought of gravity as a force of attraction exercised by, say, the Sun as it were through a bodiless arm that stretches all the way from it to the Earth to grab it and pull it, while at the same time prompting the arm of the Earth to stretch out as well and to grab and pull the Sun in turn.

fluence grew, the ether became increasingly discredited. By 1771 the enlightened founders of *Encyclopedia Britannica* thought it appropriate to explain 'ether' as "the name of an imaginary fluid, supposed by several authors […] to be the cause […] of every phenomenon in nature".[11] Not without irony, Joseph Priestley extolled the "fine scene" that ether afforded "for ingenious speculation":

> Here the imagination may have full play, in conceiving of the manner in which an invisible agent produces an almost infinite variety of visible effects. As the agent is invisible, every philosopher is at liberty to make it whatever he pleases, and ascribe to it such properties and powers as are most convenient for his purpose. (Priestley 1775, vol. 2, p. 16; quoted by Laudan 1981, p. 159)

In 1784, LeSage, author of the last great ether theory of gravity (which many decades later was favorably judged by James Clerk Maxwell), declared he would stop publishing about it: "Since your physicists are so prejudiced against the possibility of solidly establishing the existence of my imperceptible agents, which nevertheless are most suitable (*très-propres*) for making intelligible the attractions, affinities and extensibilities (*expansibilités*) which currently constitute all of physics, I shall suspend still for some time the publication of the works I prepared about these agents."[12]

Newton's undisputed authority presided also over Ampère's construction of electrodynamics in terms of attractive and repulsive forces acting instantaneously at a distance between centers of force (which, however, to save the phenomena, Ampère had to conceive, paradoxically, as oriented line elements—not points!). From then on, until the experimental discovery of electromagnetic waves by Hertz late in the 19th century, French and German electrodynamics remained wedded to action at a distance —instantaneous or deferred—, while "la physique anglaise"[13] persistently took the contrary view, namely, that electric and magnetic action properly has its seat, not in the ordinary massive bodies that display it to our eyes, but in the intangible, pervasive ether in which these bodies are embedded. The "English" (in fact, mainly Scottish and Irish) approach was inspired by the wish to produce a physico-mathematical theory that would embody the ideas of Michael Faraday, whose mind-boggling discoveries had revolutionized the science of electricity, but who, for lack of a formal education, had been unable to convey his views in precise mathematical form. Yet Faraday's views —as far as we, assisted by our hindsight, can judge them now— were much closer to Einstein's than the ether-based theories of Maxwell and his followers. At any rate, Faraday did not hide his dislike for the ether hypothesis and openly favored the

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[11] First edition (1771), 1: 31, *s.v.* **ether**; quoted by Laudan (1981), p. 170. The article **éther** in Diderot's *Encyclopédie* is only slightly less sarcastic: "L'*éther* ne tombant pas sous les sens & étant employé uniquement ou en faveur d'une hypothèse, ou pour expliquer quelques phénomenes reels ou imaginaires, les Physiciens se donnent la liberté de l'imaginer à leur fantaisie". (This article is signed by the British authors Harris and Chambers.)

[12] Quoted in French by Laudan (1981, p. 166), from Prevost 1805, p. 242. My translation.

[13] Duhem (1914), 1[e] partie, Ch. IV, §§IV-IX. For Duhem, "English physics", as practiced by the Victorian electrodynamicians from Maxwell to Larmor, is a paradigm of broadmindedness (*amplitude d'esprit*), which is not always a virtue; according to Duhem, a man in whom this form of intelligence, "which Pascal calls broadness and weakness of mind, was developed to an almost monstrous degree" was the un-French emperor of the French, Napoleon I (1914, p. 81).

attribution of physical existence to curves in space that he termed electric and magnetic *lines of force*, but which, in today's mathematical jargon, we would describe as the integral curves of the electric and the magnetic vector fields.[14]

Faced with this curious combination of facts, a thoughtful student of history cannot help wondering what could motivate the persistent attachment of Victorian physicists to the electromagnetic ether. To me at least, now that a full century has elapsed since Einstein declared it "superfluous", the 19th-century revival of this pre-Newtonian fantasy appears as a step back, unworthy of such refined mathematicians as William Thomson (Lord Kelvin), James Clerk Maxwell, George Francis FitzGerald and Joseph Larmor. Why did they so unhesitatingly take it for granted that, if electric and magnetic action happens chiefly in the space between the observable bodies, then that space must be filled by a material substance of a special kind, in which all other substances are soaked but which remains impervious to chemical analysis? More on line with our present inquiry, we may well ask the following question: Could an energetic, efficient HPS department, working in Cambridge, England, *ca.* 1850, have prevented this seemingly wrongheaded move? Questions of counterfactual history should no doubt be addressed only in a sportive mood. Still, by toying with the purely speculative answers that such questions allow, it is sometimes possible to improve our grasp of the concepts we use.

To begin with, let us recall that the luminiferous ether, which Einstein found superfluous, had been revived, together with the wave theory of light, by Thomas Young, some fifty years before Maxwell's earliest publications on electromagnetism.[15] Young's proposal was strongly resisted, especially by the French scientific establishment, in the name of the corpuscular conception of light, which was thought to be backed by the authority of Newton. However, as far as I know, nobody doubted that *if* light is transmitted by waves, *then* there must exist a vibrating body that fills interstellar space and also permeates air, water, glass, and generally all transparent bodies. It should be noted that the same French scientists that opposed the wave theory of light and, with it, the light-ether, had no reservations against other imponderable fluids, capable of permeating ordinary bodies, notably caloric, which they held responsible for heat, in opposition to the kinetic conception defended about the same time by Rumford (1798). They all embraced the ether hypothesis together with the wave theory when Poisson, wishing to embarrass Fresnel, inferred from the latter's wave optics that a bright spot would be seen at the center of the shadow of a small circular screen and this seemingly implausible prediction was fulfilled (Whittaker 1951/53, 1: 108). Indeed, no less a mathematician than Cauchy took it upon himself to dream up the

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[14] Given a vector field V on a smooth manifold M, every smooth curve in M whose tangent vector at each point of its range is identical with the value of V at that point is an integral curve of V. Cf. Kobayashi and Nomizu (1963), 1: 12; Choquet-Bruhat *et al.* 1977, p. 141.

[15] Young 1800, 1802a, 1802b, 1804. I owe these references to Buchwald 1981, p. 235.

molecular structure of ether in various ways.[16] His constructions allowed him to infer the optical phenomena of dispersion and double refraction, but little else. In a spirit very close to that of our present inquiry, Jed Buchwald asks why it was "necessary for Cauchy to embrace the hypothesis of a molecular ether if (as was almost certainly the case) he was primarily interested in discovering and solving differential equations for light?" Buchwald's reply deserves attention:

> Without the hypothesis of a molecular ether there would, at the time, simply have been no route at all to the mathematics. For, although the ultimate aim for Cauchy was always a mathematical proposition from which calculable phenomena could be deduced, this aim could only be achieved throughout most of the 1830s by deductions founded on a molecular ether. Moreover, there was then little to object to, because the hypothesis fitted so well into contemporary physical ideas; that is, it utilised the widely accepted concepts of material points and central forces. (Buchwald 1981, pp. 223-24)

Similar considerations can probably account also for other contemporary work on ether dynamics, notably by James MacCullagh (1839).[17]

About the same time, the great Cambridge philosopher and historian of science William Whewell moved away from Newton's Rules of Philosophy and expressed his willingness to countenance hypotheses in the "inductive sciences", provided that they meet certain conditions. To be acceptable, an hypothesis must not only (*a*) be "consistent with *all* the observed facts"; but (*b*) it ought to correctly foretell "phenomena which have not yet been observed" (Whewell 1847, 2: 62). "Such a coincidence of untried facts with speculative assertions cannot be the work of chance, but implies some large portion of truth in the principles on which the reasoning is founded" (ibid., 2: 64). The corroboration of wave optics by Poisson's bright spot at the center of the circular screen's shadow admirably illustrates condition (*b*). Larry Laudan (1981, pp. 176ff.) therefore claims that Whewell's restoration of the hypothetico-deductive method was inspired in particular by the success of wave optics and the ether hypothesis supposedly inherent to it. If true, this claim entails a negative answer to my question about the possible role of a counterfactual Cambridge HPS department in exorcising the ether; for Whewell, Master of Trinity since 1841, surely would have exercised an enormous influence on the composition and orientation of such a department. Indeed he probably played the opposite role in real life, by instilling in

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[16] "Within the space of ten years the great French mathematician produced two distinct theories of crystal-optics and three distinct theories of reflection, almost all yielding correct or nearly correct final formulae, and yet mostly irreconcilable with each other and involving incorrect boundary conditions and improbable relations between elastic constants" (Whittaker 1951/53, 1: 137). As Poincaré (1901*b*) remarked in connection with a later ether theory: "Les hypothèses, c'est le fonds qui manque le moins" ("There is no dearth of hypotheses"; quoted from Poincaré 1968, p. 182).

[17] For further references, see Whittaker (1951/53), 1: 137n. MacCullagh's paper of 1839 is discussed in Darrigol (2005), pp. 237-239 (followed by an extract in French translation, pp. 241-248). See also Stein (1981), pp. 310-315. Darrigol (2000), pp. 190f, 334f., explains the importance of MacCullagh's ether for FitzGerald and Larmor. Although it differed drastically from any material ever considered in the received physics of elastic bodies, MacCullagh's medium agrees with the boundary conditions required by Fresnel's laws of reflection and refraction; its equations of motion follow from Hamilton's principle and, suitably interpreted, they yield the Maxwell equations (in the absence of sources).

Maxwell —who was a student and later a fellow at Trinity in the 1850s— an enduring loyalty to ether, which seems unnecessary to us, given the mathematical resources at Maxwell's command. However, I am not sure that Laudan's claim is warranted, for even if Whewell's attitude to hypotheses had something to do with the successful prediction of unsuspected phenomena by Fresnel's wave optics,[18] that would not yet say anything about the assumption of an ether, which certainly did not occur as a premise in the deduction of those phenomena, but served rather as a means of reconciling the mathematical theory with the ordinary metaphysical prejudices of the time.[19]

Be that as it may, Faraday, who remained free from scholastic prejudices thanks to the same contingencies that prevented him from receiving a good mathematical education, had little sympathy for the light-ether and did not conceal his inclination to do without it when the Faraday effect[20] he discovered in 1845 confirmed his suspicion that light might be intimately related to electromagnetism. In a letter he wrote to Richard Phillips on 15 April 1846 and which was published as "Thoughts on Ray-Vibrations" (Faraday 1846), he explains that by regarding the "ultimate atoms" of matter "as centres of force, and not as so many little bodies surrounded by forces […] and capable of existing without them", he was gradually led to look at the lines of force, which issue from every atom on this view, "as being perhaps the seat of vibrations of radiant phenomena". This notion "will dispense with the aether, which in another view, is supposed to be the medium in which these vibrations take place." After briefly discussing the "lines of gravitating force" between ponderable particles, Faraday proceeds:

> The lines of electric and magnetic action are by many considered as exerted through space like the lines of gravitating force. For my own part, I incline to believe that when there are intervening particles of matter (being themselves only centres of force), they take part in carrying on the force through the line, but that when there are none, the line proceeds through space. Whatever the view adopted respecting them may be, we can, at all events, affect these lines of force in a manner which may be conceived as partaking of the nature of a shake or lateral vibration.
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> It may be asked, what lines of force are there in nature which are fitted to convey such an action and supply for the vibrating theory the place of the aether? I do not pretend to answer this question with any confidence; all I can say is, that I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted. The lines of weight or gravitating force are, certainly, extensive enough to answer in this respect any demand made upon them by radiant phaenomena; and so, probably, are the lines of magnetic force.
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[18] They certainly inspired the similar attitude of John Herschel (1830, cf. pp. 32-33, 196-197, 207, 261-262).

[19] Laudan (1981) does not produce any textual evidence for the connection between Whewell's attitude toward hypotheses in general and the ether hypothesis in particular and I have not been able to find any in Whewell's *Philosophy of the Inductive Sciences*, where, by the way, he lists Cauchy's ether among several "precarious hypotheses" (1847, 1: 491).

[20] The Faraday effect is the rotation of the plane of polarization experienced by a beam of plane polarized light when it passes in the direction of the magnetic lines of force through certain materials —e.g. water, heavy flint glass, quartz— exposed to a strong magnetic field. See Faraday (1855), pp. 1-11.

> The view which I am so bold to put forth considers, therefore, radiation as a kind of species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavors to dismiss the aether, but not the vibration. The kind of vibration which, I believe, can alone account for the wonderful, varied, and beautiful phaenomena of polarization, is not the same as that which occurs on the surface of disturbed water, or the waves of sound in gases or liquids, for the vibrations in these cases are direct, or to and from the centre of action, whereas the former are lateral. It seems to me, that the resultant of two or more lines of force is in an apt condition for that action which may be considered as equivalent to a lateral vibration; whereas a uniform medium, like the aether, does not appear apt, or more apt than air or water. (Faraday 1855, pp. 450-451)

Lines of force made their first appearance within the field-theoretic approach to gravity and electrostatics developed earlier by some Continental mathematicians,[21] and Faraday is plainly alluding to them. However, such lines and their tangent vectors were apparently regarded by everyone except Faraday as powerful aids for calculating the actual and virtual effects of forces acting at a distance, and not as fitting representations of physical realities. Only the mathematically untutored English genius endorsed the now common view that physical realities may and indeed ought to be conceived as models (in the model-theoretic sense, i.e. realizations) of mathematical structures. By contrast, the most mathematical among the British physicists paid obeisance —or was it only lip service?— to Kelvin's criterion of intelligibility: "It seems to me that the test of 'Do we or not understand a particular subject in physics?' is 'Can we make a mechanical model of it?'" (Kargon and Achinstein 1987, p. 111).[22] Coming from someone who about the same time had declared that "we have no right to assume that there may not be something else that our philosophy does not dream of" (ibid., p. 41),

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[21] Duhem (1914, p. 99) observes that, in dealing with electrostatics, "a French or German physicist, such as Poisson or Gauss, mentally places in space […] this abstraction called a material point, accompanied by that other abstraction called an electric charge, and thereupon seeks to calculate a third abstraction, viz., the force to which the material point is subjected; he gives formulas that allow one to determine the magnitude and the direction of that force, for every possible position of this material point, and infers a series of consequences from these formulas. He shows, in particular, that at each point of space, the force is directed following the tangent of a certain line, the *line of force*; that all the lines of force meet at right angles certain surfaces, whose equation he gives, the *equipotential surfaces*." A significant counterexample to Duhem's ethnography is the English mathematician George Green (1828), who generalized and extended Poisson's investigations concerning electricity and magnetism and even anticipated Gauss's use of the term 'potential' for the scalar field whose gradient is the field of vectors tangent to the lines of force.

[22] The two occurrences of 'model' in the above sentences illustrate two current meanings of the word that should not be confused, for they are not merely different but point, if I may say so, in opposite directions. In the branch of logic known as model theory, a *model* of a structure of a given species is any set endowed with structural features that satisfy the requirements of that species. In the more ordinary acceptance in which the word is used in the Kelvin quotation, a *model* is a representation of an individual or generic object by a real or ideal object of a different sort, such as a cardboard model of a Greek temple, or the familiar model of a pendulum, consisting of a weightless inextensible string with a massive dimensionless particle at one end, affixed by the other end to a frictionless nail. The common inclination to confuse both meanings may be due to the fact that today theoretical physics generally represents physical processes and situations by models in the second sense which are models in the first sense of specific mathematical structures (generally not the streamlined ones that are studied by the several branches of pure mathematics, but ad hoc combinations of them).

this was indeed an amazingly restrictive demand; but it held Maxwell and the Maxwellians in awe.[23]

Mechanical modeling, as understood by Kelvin, involved looking for "the explanation of all phenomena of electro-magnetic attraction or repulsion, and of electromagnetic induction […] simply in the inertia and pressure" of matter (Thomson 1857, p. 200; quoted by Harman 1998, pp. 99-100). In the same Baltimore lectures of 1884 in which he stated the said criterion, Kelvin told his audience they "must not listen to any suggestion that we must look upon the luminiferous ether as an *ideal* way of putting the thing. A real matter between us and the remotest stars I believe there is, and that light consists of *real* motions of that matter" (Kargon and Achinstein 1987, p. 12; my italics).

In his three major papers on electromagnetism, Maxwell (1856, 1861/62, 1864) carried to different lengths the enterprise of modeling, screwing "the focussing glass of theory" —as he said— "sometimes to one pitch of definition, and sometimes to another, so as to see down into different depths" (Maxwell, 1990-2002, 1: 377; quoted by Harman 1998, p. 82). But he never flinched in his adhesion to the ether hypothesis. The paper "On Faraday's Lines of Force" (1855/56) seeks only "to shew how, by a strict application of the ideas and methods of Faraday, the connexion of the very different orders of phenomena which he has discovered may be clearly placed before the mathematical mind" (Maxwell 1890, 1: 157-158). Although Faraday's style of thought was "very generally supposed to be of an indefinite and unmathematical character" (p. 157), Maxwell regarded Faraday's lines of force as evidence that he was "in reality a mathematician of a very high order" (Maxwell 1890, 2: 360). In Maxwell's formulation the electric (or, respectively, magnetic) lines of force form a congruence of curves in space, i.e. a set of curves such that one and only one of them passes through each point of space, and represents the direction of the force that would act on a positively electrified particle (or, respectively, an elementary north pole) placed at that point (Maxwell 1890, 1: 158). According to Maxwell, we "thus obtain a geometrical model of the physical phenomena, which would tell us the *direction* of the force, but we should still require some method of indicating the *intensity* of the force at any point" (ibid.).[24] However, "if we consider these curves not as mere lines, but as fine tubes of variable section carrying an incompressible fluid, then, since the velocity of the fluid is inversely as the section of the tube, we may make the velocity vary according to any given law, by regulating the section of the tube, and in this way we might represent the

[23] The index to Hunt (1991), *s.v.* 'ether models', mentions Oliver Lodge's cogwheel ether, FitzGerald's paddlewheel ether, Poynting's turbine-spring ether, Maxwell's vortex and idle-wheel ether, and FitzGerald's wheel and band ether; pp. 96-104 of the same book describe FitzGerald's vortex sponge ether of 1885. Chapter IX of Whittaker (1951/53), vol. 1, is devoted to "Models of the ether". See also in Buchwald (1985), pp. 146-150, the discussion of "ether rupture" and Larmor's difficulty in dealing with it.

[24] By a suitable parametrization of the lines of force one could ensure that the vector tangent to each such curve at each point reflects not only the *direction* of the force at this point but also its *intensity*. But in 1856 any parameter other than arc length would probably have seemed ungeometrical to Maxwell's readers.

intensity of the force as well as its direction by the motion of the fluid in these tubes" (pp. 158-159). Despite any appearances to the contrary, this is not intended to be a mechanical model of the electrical (or magnetic) field. On this point, Maxwell is emphatic:

> The substance here treated of must not be assumed to possess any of the properties of ordinary fluids except those of freedom of motion and resistance to compression. It is not even a hypothetical fluid which is introduced to explain actual phenomena. It is merely *a collection of imaginary properties* which may be employed for establishing certain theorems in pure mathematics in a way more intelligible to many minds and more applicable to physical problems than that in which algebraic symbols alone are used. (Maxwell 1856, in Maxwell 1990, 1: 160; my italics)

Thus, the "substance" in question is neither more nor less than a bundle of precisely defined properties (and relations), that is, the bare embodiment of a mathematical structure; and the familiar word 'fluid' is here stripped of "every meaning except that which is warranted by the phenomena themselves" (Maxwell 1890, 2: 359).
In his second paper, "On Physical Lines of Force" (1861/62), Maxwell valiantly proposes a mechanical model of the ether. Electricity and magnetism depend on the presence of molecular vortices in it.[25] The model allowed the existence of transverse waves propagating in the ether with a speed equal to the ratio between the electrostatic and the electromagnetic unit. Back to town from the country house where he worked out this result, Maxwell verified that this value, as established by Weber and Kohlrausch (1857), differed by less than 1.5% from the speed of light, as it was then known. Maxwell concluded:

> The velocity of transverse undulations in our hypothetical medium […] agrees so exactly with the velocity of light […] that we can scarcely avoid the inference that *light consists in the transverse undulations of the same medium with is the cause of electric and magnetic phenomena.* (Maxwell 1890, 1: 500)

At the time Maxwell did not draw the further conclusion that light itself is an electromagnetic phenomenon. He conceived optical and electromagnetic processes in mechanical terms, the relation between them being "expressed by the molecular connection between ether and matter, in terms of different motions in the ether" (Harman 1998, p. 109). Thus, the generally acknowledged reality of the optical ether could be regarded as supporting the ether hypothesis in the theory of electromagnetism.[26]

Maxwell's mechanical model was clever but coarse. On 23 December 1867, he wrote to Tait that "the nature of this mechanism is to the true mechanism what an orrery[27] is to the solar system" (Maxwell, 1990-2002, 2: 337; quoted by Darrigol 2000,

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[25] We need not go here into details. The model has been repeatedly explained in the literature. In particular, the quaint illustration printed facing Maxwell (1890), 1: 488 (Plate VIII, fig. 2), can also be found in Tricker (1966), p. 118, Siegel (1991), p. 41, Harman (1998), p. 104, and Darrigol (2000), p. 150.

[26] In the paper of 1864, Maxwell proceeded "to investigate whether [the] properties of that which constitutes the electromagnetic field, deduced from electromagnetic phenomena alone, are sufficient to explain the propagation of light through the same substance" (1890, 1: 577), and he tentatively concluded that yes, "light and magnetism are affections of the same substance, and that light is an electromagnetic disturbance propagated through the field according to electromagnetic laws" (1: 580).

[27] "**Orrery** [Named after Chas. Boyle, Earl of Orrery, for whom a copy of the machine invented by George Graham c 1700 was made by J. Rowley, an instrument-maker.] A piece of mechanism devised

p. 154). In "A Dynamical Theory of the Electromagnetic Field" (1864) he made no attempt at creating a more accurate model. Instead he based the theory on Hamilton's principle.[28] By showing that his field equations comply with the requirements of this principle Maxwell proves that the electromagnetic phenomena described or predicted by the equations *can be* explained by the mechanical behavior of a peculiar substance that permeates all ordinary bodies and completely fills the space between them.[29] Never mind that he does not give us an inkling of how exactly such a mechanical explanation could be carried out. "To demonstrate the possibility of a mechanical explanation of electricity, we do not have to worry to find this explanation itself; it is enough for us to know the expression of the two functions T and U that are the two parts of the energy, to form the Lagrange equations with these two functions and to compare these equations with the experimental laws. [...] As soon as the functions $U(q_k)$ and $T(q'_k, q_k)$ exist, one can find an infinity of mechanical explanations of the phenomenon" (Poincaré 1901*a*, p. viii).[30]

Thus it is not wholly inaccurate to say that the ether in 1864, though still quite new to electrodynamics, was already being cast by Maxwell for the Cheshire cat part it later played so skillfully under Lorentz.[31] There was, however, one significant philosophical reason for Maxwell's retention of the ether hypothesis. He expected to account for electric currents and electrostatic charge distributions as epiphenomena of ether dynamics. This would spare physicists the need to postulate one or two special electric fluids (as they did in the 18th century) or to acknowledge electric charge as a primitive

to represent the motions of the planets about the sun by means of clockwork" (*Oxford English Dictionary*).

28 Following this method, one constructs a Lagrangian function L, whose value at each instant represents the difference between the kinetic energy T and the potential energy U stored at that instant in the physical system under consideration, and one assumes Hamilton's principle, according to which the temporal evolution of the system is always such that the integral Ldt takes an extremal value, i.e. such that $\delta\ Ldt = 0$. Since the value of this integral is conventionally known as 'action', Hamilton's principle is also referred to, with mild impropriety, as *the Principle of Least Action.*

29 Or, in Poincaré's lucid words: "*Maxwell ne donne pas une explication mécanique de l'électricité et du magnétisme; il se borne à démontrer que cette explication est possible*" (1901*a*, p. iv). Lorentz's words point in the same direction: "Maxwell fait voir comment les principes de la mécanique peuvent servir à élucider les questions d'électrodynamique et la théorie des courants induits, *sans qu'il soit nécessaire de pénétrer le secret du mécanisme qui produit les phénomènes*" (1892*a*, in Lorentz 1935-39, 1: 164; my italics).

30 Maxwell was well aware of this. He writes near the end of his *Treatise* (1891, 2: 470): "The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions. Of these, some may be more clumsy or some more complex than others, but all must satisfy the conditions of mechanism in general", that is , the conditions entailed by Hamilton's principle, or by the Euler-Lagrange differential equations mentioned by Poincaré, which are necessary and sufficient for $\delta\ Ldt$ to be zero. Cf. also Maxwell's classical description of a belfry as a mechanical system (1890, 2: 783-784; quoted *in extenso* in Buchwald 1985, p. 21).

31 I owe the comparison between Maxwell's ether and Carroll's cat to Tricker (1966, p. 109); but it may well have been intended by Carroll himself (just as his Humpty Dumpty mimics the speech of mathematicians).

property of matter (as they have been doing since the 1890s). Maxwell's ether eludes our senses and was endowed by him and other researchers with either a far-fetched or an altogether unperspicuous mechanical structure, but it was assigned only such properties or relations as could be conceived in classical mechanical terms. However, in the last quarter of the 19th century, the discovery of new effects (Kerr 1877, Hall 1879) and the progress of experimental research on the conduction of electricity in electrolytes and gases (see Darrigol 2000, ch. 7) led Lorentz (1892*a*) and Larmor (1894, 1895) to assume the existence of electrically charged particles of ordinary matter, which were the sources of the force fields located in the ether and also the objects of their accelerative action.[32] This important step received decisive and apparently irreversible experimental support when J.J. Thomson "discovered the electron" (1897),[33] i.e. when he successfully identified cathode rays with a spurt or stream of negatively charged elementary particles. After that, one could no longer expect to understand electric charges and currents as mechanical effects in the ether.

In the highly acclaimed electrodynamic theory —or ought we to say *theories*?— of Lorentz, the ether is completely motionless and its mechanical structure and behavior —if it has any— is of no concern at all. As Whittaker lucidly wrote: "Such an aether is simply space endowed with certain dynamical properties" (1951/53, 1:393). It is therefore no wonder that, despite Lorentz's explicit warning to the contrary,[34] the general public, including most philosophers and even some physicists, assumed that his ether was at rest in Newton's absolute space, and even identified the latter with it.[35] Still, the idea that this elusive form of matter was part of the furniture of the universe had become deeply entrenched during the 19th century, and nobody seemed willing to dismiss it. Surely it is not easy for a highly respected profession to admit that one of its

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[32] This transition "from Maxwell to microphysics" was persuasively described by Buchwald (1985).

[33] The word 'electron' was introduced by Stoney (1881) as a term of art for the quantity of electricity, "the same in all cases", which traverses an electrolyte "for each chemical bond which is ruptured within" it, in other words, the electrolytic quantum. Larmor (1894) designated with it his new elementary charged particles (§§114-125: "Introduction of Free Electrons"). Lorentz began using the word five years later (1899, p. 507; French translation in Lorentz 1935-39, 5: 139); in his German book of 1895 he spoke of "Ionen".

[34] "When I say for brevity's sake that the ether is at rest, I mean only that no part of this medium is displaced with respect to its other parts and that all perceptible motions of the heavenly bodies are motion relative to the ether" (Lorentz 1895, §1, in Lorentz 1935-1939, 5: 4). I gather from this that one should regard Lorentz's ether as a body at rest in the so-called firmament, i.e., the inertial frame of the fixed stars constructed by astronomers.

[35] This identification caused SR to appear much more un-Newtonian than it is. If the spacetime structure underlying Newtonian mechanics had been duly acknowledged before 1900, the continuity with SR would have been obvious. In either system any congruence of parallel timelike geodesics determined a privileged frame of reference, equivalent to every other such frame; though, of course, the respective spacetime geometries have different symmetry groups, namely. the Poincaré-Lorentz group in SR, and the so-called Galilei group in Newtonian mechanics. But again this points to a continuity, inasmuch as the latter may be regarded as a degenerate limiting case of the former.

time-honored terms of art is a noun without a referent.[36] Even Poincaré, who in 1889 had predicted that "the day will doubtless come when the ether will be rejected as useless",[37] at the Paris Congress of Physics of 1901 argued thus for believing in it:

> We know where our belief in ether comes from. If we receive light from a distant star, for several years that light is no longer at the star and is not yet on the Earth. It must therefore be somewhere, sustained, so to speak, by some material support. The same idea can be expressed in a more mathematical and more abstract way. What we record are the changes suffered by material molecules; we see, for example, that our photographic film displays the consequences of phenomena staged many years earlier in the incandescent mass of the star. Now, in ordinary mechanics, the state of the system under study depends only on its state in an immediately preceding state; the system therefore satisfies differential equations. But if we did not believe in the ether, the state of the material universe would depend not only on the immediately preceding state (*l'état immédiatement antérieur*), but on much older states; the system would satisfy finite difference equations. To avoid this derogation of the general laws of mechanics we have invented the ether.[38]

Poincaré went on to say that Fizeau's experiment makes you feel you are touching the ether with your finger ("on croit toucher l'éther du doigt"—Poincaré 1968, p. 181). I need not describe this experiment here.[39] It was designed to test Fresnel's ether drag formula. According to Fresnel, the ether is partially dragged by the bodies it permeates. Any body in motion carries with it precisely the amount of ether it contains in excess of what would be found inside the same volume of otherwise empty space. If n is the refractive index of a transparent material m, then, according to Fresnel (and Young), $n^2 = \rho_m/\rho$, where ρ_m is the density of ether inside a body made of m and ρ is the density of ether outside all bodies. The density of the ether dragged by this body is $(\rho_m - \rho) = (n^2 - 1)\rho$, while a quantity of ether of density ρ remains at rest. Let c be the speed of light in interstellar space. The speed of light inside the said body is then cn^{-1}. Let the body travel in interstellar space with constant speed v in the same direction as

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36 We see even today that some scientists and philosophers who share the common prejudices concerning the epistemic status of physics will not easily acknowledge that the ether, whose dynamics and molecular structure absorbed the attention of so many great minds, simply does not exist. They feel perhaps that this would be like saying that Cuvier or Darwin spent years studying the physiology and histology of the unicorn.

37 "Peu nous importe que l'éther existe réellement, c'est l'affaire des métaphysiciens; l'essentiel pour nous c'est que tout se passe comme s'il existait et que cette hypothèse est commode pour l'explication des phénomènes. Après tout, avons-nous d'autre raison de croire à l'existence des objets matériels? Ce n'est là aussi qu'une hypothèse commode; seulement elle ne cessera jamais de l'être, tandis qu'un jour viendra sans doute où l'éther sera rejeté comme inutile" (Poincaré 1889, préface; quoted from Poincaré 1968, p. 215).

38 Poincaré (1901*b*); translated by me from Poincaré (1968), pp. 180-181. I cannot repress the feeling that Poincaré the mathematician must have known (i) that if a given physical state depends on another in accordance with a system of differential equations, none of the two states can *immediately* precede (or follow) the other one, and (ii) that time dependent vector fields can be defined on space without assuming a material support for them to sit on. I suppose (ii) is the reason why he talks of *inventing* the ether at the end of this tirade about *believing* in it.

39 Fizeau (1851). The experiment is described in Darrigol (2000), pp. 315-316; also in Janssen and Stachel (forthcoming), p. 13. Both this paper and Stachel (2005) contain important remarks about the significance of Fizeau's experiment.

light is sent through the body. Then, the center of gravity of the ether contained in the body moves in this direction with constant speed equal to $(n^2 - 1)n^{-2}v$. Let c_v denote the speed —relative to the interstellar sea of ether— of light propagating inside a transparent body of refractive index n in the direction in which the body travels with speed v. Then, under Fresnel's assumption and the classical rule for the composition of velocities,

$$c_v = \frac{c}{n} + \frac{n^2 - 1}{n^2}v = cn^{-1} + \left(1 - \frac{1}{n^2}\right)v \qquad (1)$$

Fizeau (1851) verified formula (1) by letting light travel along two parallel tubes in which water flowed in opposite directions. Fizeau's experiment, which was repeated with greater accuracy and the same positive result by Michelson and Morley (1886), is a true treasure-trove for philosophers, for it can be read in at least three very different ways, as confirming either (i) Fresnel's ether drag hypothesis, as explained above; or (ii) Lorentz's theory of the motionless ether, which also entails the so-called *drag factor* $\left(1 - \frac{1}{n^2}\right)$, but does not attribute it to a partial ether drag; or (iii) Special Relativity itself. Indeed, under the SR rule for the composition of velocities, Fresnel's purported ether drag turns out to be a purely kinematic effect, a consequence of substituting the moving body's reference frame for that of the fixed stars, when both frames are related by a Lorentz transformation. (To be precise, the relativistic formula differs from Fresnel's by a term of the second order in v/c.) Indeed, Mascart already noted in 1872 that Fizeau's experiment merely confirms Fresnel's formula (9), but "one can replace Fresnel's hypothesis by any other hypothesis that will finally lead to the same formula, or a slightly different one" (Stachel 2005, p. 7). Further remarks by Mascart's contemporaries Ketteler, Veltmann and Potier give substance to Stachel's claim that "the challenge presented by Fresnel's formula was the first indication of the breakdown of classical (Galilei-Newtonian) kinematics, and could have led directly to the search for a new kinematics" (Stachel 2005, p. 1). It seems to me, however, that someone less adventurous than Einstein would not have dared to substitute reading (iii) for reading (ii); certainly not a mere HPS research worker.[40] Indeed, there are good reasons for thinking that the scientific establishment would have continued to believe that Fizeau's experiment allowed one to touch the Lorentz ether with one's fingers, if Michelson and Morley's failed attempt to ascertain the motion of the Earth in the ether had not belied, already in 1887, the success of their improved Fizeau experiment of 1886.[41] Lorentz (1892*b*) sought to explain these experimental results by means of

[40] Not even Poincaré dared to do so (at least not openly), although in June 1905 he was in possession of all the mathematical and physical ingredients of SR. Giannetto (1998) provides sufficient evidence of this (though not enough, in my view, to establish his claim that "Poincaré must be considered the actual creator of special relativity").

[41] Michelson and Morley's experiment with the Michelson interferometer is described in practically every textbook on SR, usually from a relativistic standpoint. To get the perspective from which the experi-

the contraction hypothesis that had been earlier proposed by FitzGerald (1889): because every macroscopic solid body is held together by presumably electric intermolecular forces, a metal rod traveling across the electromagnetic ether with constant speed v will contract in the direction of motion by the exact amount necessary to make its motion impossible to detect through observations accurate to the second order in v/c. This hypothesis is not so implausible as some philosophers have later said.[42] However, as Lorentz (1895, 1899, 1904) gradually realized, to properly do its job the contraction hypothesis had to be supplemented with the introduction of "local time", which Lorentz conceived as a mere mathematical aid to calculation, but which will make very little sense unless real clocks in moving labs actually agree with it.[43] As is well known, Lorentz's mature theory of 1904, complete with rod contraction and clock retardation, is Lorentz-invariant[44] and, despite profound conceptual differences,

ment was designed, I recommend reading Michelson (1881). From the negative result of his experiment, Michelson drew *neither* (i) the relativistic conclusion customarily associated with it today, *nor* (ii) the geostatic conclusion that Tycho Brahe or Roberto Bellarmino could have based on it. Instead, Michelson concluded that Gabriel Stokes (1845, 1846*a*, 1846*b*) was right in assuming that the local ether is dragged completely by the solid Earth, and in decreasing proportions by the atmosphere that surrounds it. Michelson therefore eventually had his experiment repeated on a mountain top. Had he lived much longer, he might have persuaded NASA to put an interferometer aboard a space shuttle. However, Lorentz (1886) had proved that Stokes conception of the total ether drag is untenable: the flow of an incompressible fluid around a moving solid sphere cannot be irrotational and yet adhere to the sphere (Darrigol 2000, p. 317); the lemma on which this proof rests, namely. that Stokes theory must assume a velocity potential, was later also proved by Lorentz (1892*c*).

42 They objected that the Lorentz-FitzGerald contraction hypothesis is "ad hoc", as if one could ever design a good scientific hypothesis without having specifically in mind the phenomena it is meant to explain and the problems it is intended to solve. As Janssen (1995, pp. 160, 183) aptly notes, this unfair and misguided criticism of Lorentz and Fitzgerald has precious little to do with Einstein's complaint (1907, pp. 412-413) that the contraction hypothesis was ad hoc in the sense that it was expressly devised to save the superfluous ether hypothesis. For a spirited and skillful defense of the Lorentz-FitzGerald approach see Brown (2005), ch. 4.

43 Lorentz (1916, p. 321, n. 72*) acknowledged this and gave Einstein credit for it. However, he could have learned it before 1904 from Poincaré. On this issue see the illuminating and well documented exposition by Michel Janssen (1995), §3.5.4, pp. 244-248.

44 Eqns. (4) and (5) in Lorentz 1904, §4 (1935-39, 5: 175), include as a factor a function of velocity $l(v)$, which is equal to 1 for $v = 0$ and "for small values of v, differs from unity no more than by a quantity of the second order". This entails that the theory is only approximately Lorentz-invariant, and that an experiment with a higher order of accuracy could one day disclose the motion of the Earth in the ether. However, a few pages later Lorentz shows, by a tortuous physical argument, that $l(v)$ must be constant and therefore equal to 1 (1935-39, 5: 187-188). On this condition, the transformation proposed by Lorentz becomes what we now call a Lorentz transformation and the *exact* Lorentz-invariance of the theory is secured. However, Lorentz's argument for $l(v)$ = const. failed to convince Poincaré, who proved instead that the set of all transformations that satisfy Lorentz's eqns. (4) and (5) *form a group* if and only if $l(v) = 1$ (Poincaré 1906, pp. 144-146). This in turn implies that the inverse of a given transformation is on a par with it, so that no privilege can be claimed for one of the coordinate systems mutually related by the transformation. Interestingly, Lorentz (1914), commenting on Poincaré (1906), asserted that his theory of 1904 was not exactly Lorentz-invariant. "My formulas —he says— remain loaded with certain terms that ought to have disappeared. These terms were too small to exercise a perceptible influence on the phenomena and I could thus explain the independ-

agrees exactly in every prediction with SR. This means that, although it postulates an ether, it does not afford any conceivable way of experimentally detecting its presence.[45] At this point, the time was ripe for someone to call the bluff, as in Andersen's tale.[46]

Few will deny that, if the ether does not exist, the dismissal of the ether hypothesis was a major improvement of physics. It seems to me that, in principle, a critical thinker imbued in Newton's methods (or Fourier's!) and acquainted with the discoveries and ideas of Faraday could have perceived already *ca.* 1855 that the hypothesis was both groundless and superfluous. However, William Thomson, Lord Kelvin, who certainly was such a thinker, remained during the next half-century ether's most adamant advocate. Considering this and other aspects of the story summarized above, as well as the deference that is usually —and sensibly— shown by philosophers and historians of science towards scientific authorities of Kelvin's stature, I do not believe that a greater presence of HPS in 19th century academic life could have significantly speeded the abolition of ether. On the other hand, most historians agree that the ether hypothesis exercised a fruitful influence on research. From this point of view, it was perhaps just right that it survived as long as it did.

Summing up: There was no dearth of mathematical resources for representing the electric and magnetic forces without having to assume an underlying substance that supports them. But this was not an intellectually attractive alternative for 19th-century scientists and philosophers steeped in the *mentalité chosiste* of our flint carving forefathers. Although an HPS worker could, in principle, have reasonably abolished ether in the 1850s, it is unlikely that this move would have found acceptance in the philosophical or the scientific community.

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ence from the motion of the Earth that observations had disclosed; but I have not established the principle of relativity as rigorously and universally true." (Lorentz 1935/39, 7: 263). In a lecture delivered in Leiden, Paul Ehrenfest expressed a different view: "The etherless theory of Einstein demands exactly the same as the ether theory of Lorentz. […] As a matter of principle (*ganz principiell*), there is no experimentum crucis between these two theories" (1913, pp. 17-18). Lorentz attended this lecture and voiced no objections. See also Ehrenfest (1912), a discussion note concerning Einstein (1909), in which Ehrenfest, who apparently sympathized at that time with the etherless emission theory of light propounded by Ritz (1908), stresses that "the partisans of *Einstein's theory of relativity* must wish that [in a practically viable experimentum crucis between the theories of Ritz and Lorentz] the partisans of the *ether hypothesis* should prevail over the partisans of the proper emission hypothesis" (p. 319).

[45] In his lectures concerning "Old and new problems of physics" (1910), Lorentz himself said, after describing the situation in electrodynamics that immediately preceded Einstein's SR: "Hat es dann überhaupt einen Sinn vom Äther zu reden? Schliesslich ist ihm nur noch soviel Substantialität geblieben, dass man durch ihn ein Koordinatensystem festliegen kann" (Lorentz 1935-1939, 7: 210). And yet even this alleged possibility is unclear, given that there is no way of telling relative to which coordinate system the ether is at rest.

[46] Shortly before Einstein (1905), Alfred Bucherer (1903, 1904) and Emil Cohn (1904) put forward etherless theories of electromagnetic phenomena. However, they were not predictively equivalent to Lorentz's mature theory and were therefore experimentally unsuccessful. See Darrigol (2000), pp. 366-372.

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