Conceptions of ether

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'Subtler forms of matter' in the period following Maxwell

HOWARD STEIN
Department of Philosophy, University of Chicago, Chicago, Illinois 60637 USA

This investigation is put forward as a confirmation of Professor Maxwell's electromagnetic theory of light, in which, though there are some points requiring further investigation, nevertheless the foundation has certainly been laid of a very great addition to our knowledge, and if it induced us to emancipate our minds from the thraldom of a material ether might possibly lead to most important results in the theoretic explanation of nature.
George Francis Fitzgerald (1878)

Luminiferous ether must be a substance of most extreme simplicity.
Lord Kelvin (1884; 1904)

Alas! Have all the barbers lived in vain
That not one curl in nature has survived?
Wallace Stevens (1923)

It will be useful to begin with two general remarks about the period to be considered in the present chapter. First, when Maxwell's phrase 'the subtler forms of matter' is brought to bear upon our period, a multiple pun can be seen: (1) 'Ether', conceived originally as 'subtle' in the sense *finely divided,* proves to require, for the understanding of its own attributes and its relationships to 'ponderable' matter, a more refined ('subtler') *system of concepts;* (2) the more finely divided forms (thus, in the original sense, 'subtler forms') of *ordinary* matter – molecules, ions, and eventually subatomic particles – come, during this period, to play an increasing role in physical theory (and in a manner that is closely linked with the theory of ether); (3) developments initiated in this period have led in the end to an unprecedentedly radical transformation and conceptual 'subtilisation' of the notion of 'ordinary matter' itself – so that in a very important sense *all* matter turns out to be 'subtler' than anyone could previously have imagined. Second, although the triumph of Maxwell's theory led ultimately to so basic a conceptual transformation that physicists now tend to regard the electromagnetic theory as having supplanted the 'mechanical' theory of light – as having in effect superseded attempts to construct a theory of the luminiferous (and now also electromagnetic) ether as a species of matter subject to the laws of elasticity and the laws of motion – this is a badly distorted view: There is in point of fact an enormous literature devoted to such attempts, to what may be called the 'classical ether-problem for the electromagnetic theory'. To survey this literature with any pretension to adequacy within the limits here available is out of the question. The primary aim of this chapter, therefore, is to sketch what I see as the most salient features of the developments referred to under the first head above – more particularly under (1), with some attention to (2) – with (it is hoped) historical responsibility, but with no claim at all to historical completeness.[1]

The MacCullagh–Fitzgerald dynamical ether

For an appreciation of the state of affairs in the early part of our period, it is important to remember what sort of theory it was that Maxwell himself left us. He outlined its general character with great clarity in article (3) of his decisive paper 'A dynamical theory of the electromagnetic field':

> The theory I propose may . . . be called a theory of the *Electromagnetic Field,* because it has to do with the space in the neighbourhood of the electric or magnetic bodies, and it may be called a *Dynamical* Theory, because it assumes that in that space there is matter in motion, by which the observed electromagnetic phenomena are produced.[2]

As to the detailed constitution, in mechanical terms – the actual motions and interconnections – of that matter 'by which the electromagnetic phenomena are produced', however, Maxwell's theory (in this paper and in the later *Treatise*) is deliberately noncommittal; except that in the *Treatise* he does distinctly embrace the view that the energy of the magnetic field is the kinetic energy of the medium, the energy of the electric field its potential energy.[3]

Twenty-three years after Maxwell's 'Dynamical theory', and fifteen years after his *Treatise,* Poincaré lectured at the Sorbonne on Maxwell's theory; these lectures were published as volume 1 of his *Electricité et optique.*[4] At the time of this course, Maxwell's theory was still widely regarded as a dark mystery (Hertz, for instance, declared: 'Many a man has thrown himself with zeal into the study of Maxwell's work, and . . . been compelled to abandon the hope of forming . . . an altogether consistent conception of Maxwell's ideas. I have fared no better myself'),[5] and Poincaré took it as his express aim to clarify the structure and content of the theory (for 'un lecteur français'). The course could not have been given at a more propitious time: at the end of his introduction, Poincaré remarks: 'Science has advanced with a rapidity that nothing could have allowed one to foresee at the moment I began this course. Since that time, the theory of Maxwell has received, in a brilliant manner, the experimental confirmation it had lacked'. Poincaré's next course on electrodynamics, given two years later and published as volume 2 of the same work, was devoted to the critical discussion of the epoch-making experiments of Hertz on electromagnetic waves.

The nub of Poincaré's diagnosis of the basic difficulty faced by Maxwell's readers, and of the clarification he offers, is expressed by him as follows:

> On opening Maxwell, a Frenchman expects to find a theoretical whole as logical and as precise as the physical Optics founded on the hypothesis of the ether; he thus prepares himself for a deception which I should wish to obviate for the reader . . .
>
> *Maxwell does not give a mechanical explanation of electricity and of magnetism; he restricts himself to demonstrating that that explanation is possible.*[6]

Poincaré goes on to make entirely plain what he means by a 'mechanical explanation', and what by a 'demonstration that mechanical explanation is possible'; the latter demonstration consists in the introduction of a kinetic and a potential energy function, in such a way that the resulting Lagrangian 'generalised dynamical equations' can be shown to be satisfied by the system in question; and Poincaré proves that when these conditions are met, it is always possible – and in infinitely many ways – to construct, in his sense, a 'mechanical explanation' of the processes of the system.[7] (Although this is no place for a full discussion, it should not go unremarked that this celebrated result is seriously defective: Poincaré's theorem is perfectly correct, but the sense he allows in it to the notion of 'mechanical explanation' is far too wide from the point of view of physical theory.) In short, the point made central by Poincaré is this: Maxwell demonstrated the possibility of a 'mechanical explanation' of electromagnetic phenomena, by subsuming the laws of these phenomena under the generalised mechanics of Lagrange.

Now, such a subsumption under the principles of generalised mechanics is certainly what Maxwell appears to aim at in part 4, chapters 5 and following, of the *Treatise;* it is therefore rather striking to find, on more careful examination, that neither Maxwell in the *Treatise* nor Poincaré in the *Electricité et optique* in actual fact applies Lagrangian dynamics to the electromagnetic field. Both, in effect, apply Lagrangian mechanics only to the case in which the sole electric currents of which account need to be taken are closed currents in well-defined curvilinear conducting circuits; in which in particular, then, the Maxwellian 'displacement currents' can be neglected. It is thus precisely to the case *not* characteristic of what we think of as 'Maxwell's theory' – the case, be it noted, that was not the subject of the 'brilliant experimental confirmation' achieved by Hertz – that both Maxwell and Poincaré demonstrate the applicability of generalised dynamics. In the *Treatise* this application occurs in chapters 6–8 of part 4; chapter 9 simply generalises the results obtained to the unrestricted case. (One is tempted to regard this step as a straightforward inference from the hypothesis that displacement currents obey all the same laws as ordinary currents; but this interpretation fails, because the preceding dynamical analysis will not go through for ordinary currents either, when the conductors cannot be regarded as one dimensional.) The situation is described by Lorentz (with his characteristic judiciousness and simplicity), in his pathbreaking paper of 1892 on the electrodynamics of moving bodies: 'The equations that determine the motion of electricity in bodies of three dimensions do not result, in the book of Maxwell, from a direct application of the laws of mechanics; they rest upon the results previously obtained for linear circuits'.[8]

Lorentz himself repairs the indicated defect; and we shall return to this point. But he was anticipated in this (although, as we shall later see, only partially); credit for the first demonstration that Maxwell's general field laws can indeed be brought within the scope of dynamical principles belongs to G. F. Fitzgerald, who accomplished this in 1878, in a work devoted to the electromagnetic theory of reflection and refraction.[9] This subject had not been pursued by Maxwell, either in 'A dynamical theory' or in the *Treatise;* he had contented himself with discussing the propagation of electromagnetic waves in homogeneous (although not necessarily isotropic) media. The first published intimation of an electromagnetic theory of the reflection and refraction of light (and of its advantages over the standard elastic-ether theories) was made in 1870 – thus three years before the publication of Maxwell's *Treatise* – by Helmholtz, in a few lines, in a footnote to the introductory section of the first of his remarkable series of comparative discussions of electrodynamic theories;[10] and Helmholtz's suggestion was worked out in detail by Lorentz in his doctoral thesis.[11] Fitzgerald's treatment was evidently elaborated in ignorance both of Lorentz's work and of Helmholtz's remarks.[12] His method, which is what chiefly concerns us here, is sketched at the beginning of the abstract of his investigation:

> In the first part of the paper the media are not assumed to be isotropic as regards electrostatic inductive capacity, so that the results are generally applicable to reflection and refraction at the surfaces of crystals. I use the expressions given by Professor J. Clerk Maxwell in his 'Electricity and Magnetism', vol. ii, part 4, chap 11, for the electrostatic and electrokinetic energy of such media. By assuming three quantities, ξ, η, ζ, such that, t representing time, $d\xi/dt$, $d\eta/dt$, and $d\zeta/dt$ are the components of the magnetic force at any point, I have thrown these expressions for the electrostatic and electrokinetic energy of a medium into the same forms as M'Cullagh assumed to represent the potential and kinetic energy of the ether, in 'An Essay towards a Dynamical Theory of Crystalline Reflection and Refraction', published in vol. xxi of the 'Transactions of the Royal Irish Academy'. Following a slightly different line from his, I obtain . . . the same results as to wave propagation, reflection, and refraction, as those obtained by M'Cullagh . . . Of course, the resulting laws of wave propagation agree with those obtained by Professor Maxwell from the same equations by a somewhat different method. For isotropic media, the ordinary laws of reflection and refraction are obtained, and the well-known expressions for the amplitudes of the reflected and refracted rays.[13]

Fitzgerald, therefore, reduces the theory of electromagnetic propagation in dielectric media to the dynamical theory of ether that had been developed forty years earlier by James MacCullagh.[14] The vector with components ξ, η, ζ, referred to by Fitzgerald (let us henceforth call it $\mathbf{q}$), is in MacCullagh's theory the displacement vector, at each given point and time, of ether – that is, the displacement from the given point of the ether particle whose 'normal' location is at that point; thus, Fitzgerald identifies the magnetic field intensity at any point with (roughly) the velocity $\dot{\mathbf{q}}$ of MacCullagh's ether at the same point (in conformity with the assumption that magnetic energy is kinetic). MacCullagh's basic dynamical assumption was that the potential energy of the optical medium is a quadratic form in the components of the vector field curl $\mathbf{q}$. From this assumption there follows, as MacCullagh succeeded in showing, by purely dynamical reasoning (but under an approximating condition, often unremarked, that we shall have occasion to consider later), a system of laws of the propagation of ethereal disturbance in homogeneous media, and of boundary relations on opposite sides of a surface of discontinuity separating two such media, that agrees fully with the experimentally established facts of optics. Fitzgerald points out that if one puts the vector of magnetic field intensity proportional to $\dot{\mathbf{q}}$ and the vector of dielectric displacement proportional to curl $\mathbf{q}$, then MacCullagh's expressions for the kinetic and potential energies coincide with Maxwell's, *and moreover* the fundamental relations of Maxwell's theory for regions free of charge and of conduction current all follow.[15] The success of MacCullagh's analysis of optical phenomena is thus fully transferred to the theory of Maxwell (in which, furthermore, the approximating condition just referred to is rigourously assumed); and, as it were incidentally, we have the subsumption of the general equations of the field (for charge- and current-free regions) under dynamical principles.

It should, perhaps, be noted that Whittaker (1951:144, n. 1) attributes this interpretation of MacCullagh's theory in electromagnetic terms to Heaviside (in a paper of 1891). It is strange that he gives the priority to Heaviside, for on the same page he refers to Fitzgerald as the first to have properly appreciated MacCullagh's work, and later (p. 286) he describes Fitzgerald's interpretation explicitly. (The careful reader should be warned that in Whittaker's footnote on p. 144 there is a substantive mistake, evidently a typographical error: In the notation there used, not **e**, as is written, but $\dot{\mathbf{e}}$ 'corresponds to the magnetic force'.)

For a correct understanding of the significance of Fitzgerald's result, it is important to make clear some peculiar circumstances attaching to the work of MacCullagh. In the first place, that work had gained little attention and no acceptance. Larmor, for example, in a historical note added, in his collected papers, to the first of his series of articles 'A dynamical theory of the electric and luminiferous medium' (1893–7), states:

> When these papers were written the work of MacCullagh had fallen into almost complete discredit. In this country it had suffered under the destructive criticism of Stokes: while abroad it was assumed to be merely a belated version of the theory of crystalline optics of elastic-solid type, developed by F. E. Neumann. At present it has been restored in some degree to its proper position in the historical development of physical optics: see, for example, Rayleigh's obituary notice of Stokes, *Roy. Soc. Proc.* 1903, or F. Klein, *Entwicklung der Mathematik im 19 Jahrhundert* (1926).[16]

One receives a more poignant impression from the brief (unsigned) biography of MacCullagh in as late an edition of the *Encyclopaedia Britannica* as the eleventh (1911), where one reads: 'Overwork, mainly on subjects beyond the natural range of his powers, induced mental disease; and he died by his own hand in October 1847'; and: 'His methods . . . were altogether inadequate to the solution of the more profound physical problems to which his attention was mainly devoted, such as the theory of double refraction, &c. See G. G. Stokes's "Report on Double Refraction" (*B. A. Report*, 1862)'. (The same edition of the *Britannica* contains statements by Larmor and by Lorentz, placing a far higher value upon MacCullagh's work.)[17]

What Stokes had pointed out was that MacCullagh's potential-energy function is physically paradoxical: MacCullagh's medium would, on the one hand, offer no resistance whatever to irrotational distortion (which is strange, even if not strictly paradoxical); whereas, on the other hand, it would resist nondistorting rotation, with a quasi-elastic 'restoring torque' proportional (for 'small' displacements) to the total angle through which any infinitesimal element had been rotated from its 'normal' orientation in absolute space. (Note that it is not a question here of relative rotations of the parts of the medium with respect to one another, but of absolute rotations, as just specified.) It is quite clear that such a law not merely is different from any found to characterise 'ordinary' bodies, but is in conflict with the basic assumption of rotational invariance for the laws of nature. As is well known, violation of rotational invariance is associated with violation of the principle of the conservation of angular momentum. The objections to MacCullagh's theory were therefore by no means frivolous.

But none of this affects the standing of Fitzgerald's result. He was not in fact propounding a theory of the constitution of ether; in particular, he did not presume that the vector here called **q** represents ethereal displacement, but only that $\dot{\mathbf{q}}$ and curl **q** give the magnetic intensity and dielectric displacement, respectively. The existence of such a **q**, under the indicated conditions, is implied by Maxwell's theory, as are the expressions for magnetic and electric energy. And MacCullagh's analysis had shown in complete rigour that the application of the principles of dynamics as if these were the kinetic and potential energies, respectively, leads to the ascertained optical laws. (No comparably complete success had been achieved by any of the more orthodox constitutive theories of the optical medium.)

In sum, MacCullagh had discovered a system of assumptions from which the laws of optics follow by dynamical reasoning; but these assumptions appeared (*malgré* the theorem of Poincaré) to be unrealisable by any material dynamical system. Fitzgerald, however, discovered that the electromagnetic field of Maxwell (more exactly, the 'free' field) is a system that satisfies exactly the assumptions of MacCullagh.

Fitzgerald's conditions for a material ether

The direct concern of Fitzgerald's investigation was not with questions about the dynamics of ether, such considerations having entered rather as ancillary to a method of attack upon his central problem; but his paper concludes with the striking passage quoted in the epigraph to the present chapter. The passage is undoubtedly pregnant, and in a measure prophetic; but some caution is required in its interpretation. When Fitzgerald here speaks of 'emancipating our minds from the thraldom of a material ether', what are we to take him to mean? The ensuing history (and Fitzgerald's part in it) can help to enlighten us.

A similar question can be raised about the statement of Lord Kelvin (or Sir William Thomson, as he then was), also quoted in my epigraph: What sort of thing did Kelvin mean by 'a substance of most extreme simplicity'? Here the clarification is immediate, for he tells us:

> We might imagine it to be a material whose ultimate property is to be incompressible; to have a definite rigidity for vibrations in times less than a certain limit, and yet to have the absolutely yielding character that we recognize in wax-like bodies when the force is continued for a sufficient time.[18]

And the passage immediately following makes it plain that the 'simplicity' that distinguishes, in Kelvin's opinion, ether from 'ponderable' matter is connected with its extreme fine-grainedness (its 'subtlety'):

> It seems probable that the molecular theory of matter may be so far advanced sometime or other that we can understand an excessively fine-grained structure and understand the luminiferous ether as differing from glass and water and metals in being very much more finely grained in its structure. We must not attempt, however, to jump too far in the inquiry, but take it as it is, and take the great facts of the wave theory of light as giving us strong foundations for our convictions as to the luminiferous ether.

In short, pending that eventual advance in the molecular theory, ether is to be treated as a continuous rather than a 'structured' medium, using the simplest applicable concepts of the standard theory of continuous elastic solid media, the whole account to be founded upon that 'natural history of the luminiferous ether' – 'an infinitely simpler subject than the natural history of any other body' – which is comprised in 'the great facts of the wave theory of light'.[19]

The general view of ether here advocated by Kelvin, and extensively developed in his *Baltimore lectures,* was first suggested by Stokes as a way to reconcile the demands of the wave theory of light with the fact of the ordinary unresisted motion of ordinary bodies (Whittaker, 1951:128). It is a view against which Fitzgerald argues, forcefully and repeatedly. Thus he writes in 1885:

> All theories of the ether that suppose it to be simply a jelly with matter spread through it, like grapes in a jelly, hardly seem to attribute sufficient importance to the difficulty of explaining upon any such simple hypothesis such phenomena as electricity and magnetism; and although the equations of motion of the jelly may fairly well represent the equations of motion of the ether, as regards its propagation of light, yet the properties of a jelly prevent our supposing continuous rotation of its elements, which seems almost necessary in order that the same quantities which represent small motions in the light-propagation may represent known phenomena in electricity and magnetism.
>
> Although Professor Stokes seems to think that there is no contradiction in supposing the ether to be a jelly, and at the same time sufficiently little rigid to permit the free motion of matter through it, nevertheless, there is no doubt that this is a serious stumbling-block in the way of a general acceptance of the hypothesis that the ether is, in all respects, like a thin jelly, and I hardly think the difficulty diminished when its strains, as a rigid body, are required to be capable of producing permanent electrical forces.[20]

In one of his last published pieces – a review of Larmor's book *Aether and matter* (1900) – Fitzgerald again puts forward this last consideration (with a pointed humour characteristic of him):

> In discussing the result of Michelson and Morley's experiments, from which they concluded that the ether is carried along by the Earth in its motion, Mr. Larmor shows that such a hypothesis is quite inconsistent with the fact of aberration and with the tenability of Sir George Stokes's suggestion that ether is like a very soft jelly. How such a soft material could be the means by which tramcars are driven by shearing stresses seems an additional difficulty in the way of this suggestion.[21]

Returning to 1885, we have a brief communication to *Nature* on the subject of Kelvin's *Baltimore lectures* (the stenographic records of which had been made available).[22] In this note, Fitzgerald comments instructively on the differences between Kelvin's and Maxwell's views (with generosity towards both, but without concealing his own predilection for Maxwell). The note ends with the following paragraph:

> I cannot conclude without protesting strongly against Sir William Thomson's speaking of the ether as *like* a jelly. It is in some respects *analogous* to one, but we certainly know a great deal too little to say that it is *like* one. May be Maxwell's conceptions as to its structure are not very definite, but neither are anybody's as to the actual structure of a jelly, and there is no real difficulty in supposing a medium whose condition is represented by symbols that obey the laws that Maxwell has shown should be the laws of symbols representing the condition of a medium that would explain electric and magnetic phenomena . . . It seems . . . likely that what

> he [Maxwell] called 'electric displacements' are changes in structure of the elements of the ether, and not actual displacements of the elements . . . so that I think the word 'displacement' was unfortunately chosen. I also think that Sir William Thomson, notwithstanding his guarded statements on the subject, is lending his overwhelming authority to a view of the ether which is not justified by our present knowledge, and which may lead to the same unfortunate results in delaying the progress of science as arose from Sir Isaac Newton's equally guarded advocacy of the corpuscular theory of optics.

In the light of these passages, it is clear that a part (at least) of the 'thraldom to a material ether' from which Fitzgerald wished our minds emancipated is just this doctrine of ether as a 'jelly'; it is moreover plain that he – in contrast to Kelvin – would have us include the relations of electromagnetism, alongside 'the great facts of the wave theory of light', within the 'natural history' of ether. But what sort of positive theory of ether might he have supposed would eventuate? Do his words of 1878 imply, in particular, that this ether might be in some sense *non*material? No conclusive evidence is at hand concerning Fitzgerald's more detailed intentions at the earlier date; but his subsequent remarks on the subject quite fail to support such a notion. In the paper of 1885 cited previously, after registering his objection to the view that 'the ether is, in all respects, like a thin jelly,' he proceeds as follows:

> There are, of course, many ways in which matter may move through the ether besides by displacing it; as, for instance, in the way in which a volume of liquid water might pass through ice, namely by dissolving in front, and by freezing as fast behind, and such hypotheses do not require any limit to be assigned to the rigidity of the ether. In all these cases it is, of course, evident, that when once it is shown that the energy of the medium depends on quantities which obey the laws of Maxwell's electric and magnetic induction and displacements, it follows that the forces on the places that represent the electrified and magnetized bodies must be the known electric and magnetic attractions and repulsions; and one great difficulty in framing hypotheses as to the connexion of the ether and matter is in explaining how the matter moves through the ether.[23]

Relating this suggestion – that ordinary matter might be fruitfully considered as, in effect, a state propagated through, rather than a substance interacting with and moving through, ether – to the 'vortex atom' hypothesis that had been advanced by Kelvin and explored by both Kelvin and J. J. Thomson, Fitzgerald observes that 'there seems no doubt that the simplest theory as to the constitution of the ether is that it is a perfect liquid'; but, he continues,

> it seems almost impossible to explain electric and magnetic phenomena without some further hypothesis . . . Now, it seems certain that the only way in which a perfect liquid can become everywhere endowed with properties analogous to rigidity is by being everywhere in motion. The most general supposition of this kind would be, that it was what Sir William Thomson has called a vortex-sponge, *i.e.* everywhere endowed with vortex motion, but with this motion so mixed up as to have within any sensible volume an equal amount of vortex motion in all directions. There are many ways in which this supposition seems to be in accordance with what we know of the properties of the ether.[24]

Three points emerge rather clearly: (1) Fitzgerald has here no thought of ether as in any sense nonmaterial; (2) he *does* think it likely – or more than likely – that the constitution of the ether, and the physical properties derived by ordinary mechanical principles from that constitution, are very different from those of 'ordinary' matter; (3) a possibility to be reckoned with – although, for Fitzgerald, far more speculative than the preceding – is that the fundamental system of things, the *natura rerum*, may simply *be* ether, and what we perceive as the properties and interactions of 'ordinary bodies' may be the macroscopic aspect of what most basically are just processes within the ether (cf. the statement of Larmor: 'Matter may be and likely is a structure in the aether, but certainly aether is not a structure made of matter').[25]

The suggestion that ether may be a perfect liquid is less central to Fitzgerald's view; but his further suggestion that it may have the character of a 'vortex sponge' in the sense of Kelvin is based upon a more general consideration that is of great importance. Why does Fitzgerald reiterate (as he does) the need for physicists to search for 'such a mode of motion in space as will confer upon it the properties required'[26] to support electromagnetic phenomena? (Fitzgerald shows in general no especial predilection for the Cartesian principle that all material distinctions are to be grounded in motion.) Although he does not say so, the answer seems to be this: that by positing a medium that is a repository of fine-scale internal motion – and more particularly, of *angular momentum* distributed throughout its volume – Stokes's objection to MacCullagh's ether can be overcome. For if ether were full of microscopic rotational motion (tiny gyroscopes, or fluid vortices), the torque required to maintain what seems in the large like a constant deviation in the orientation of a portion of the medium might be seen as giving rise on the fine scale, through precessions of the rotating elements, to a continual transfer of angular momentum to that portion; and thus the conservation principle could be saved.

Kelvin's quasi-rigid ether

The vortex-sponge theory involved the notoriously difficult problem of the turbulent motion of a liquid, and was never developed beyond a rudimentary stage (for some details, and reference to further literature, Whittaker, 1951:295–301, may be cited); but the suggestion that quasi-elasticity based upon motion might serve the needs of the case was fully confirmed, some four years after Fitzgerald's proposal, by Kelvin, who devised a kinetic model (using cells in which gyroscopes were mounted) of what came to be called the 'quasi-rigid', or 'rotationally elastic', or 'gyrostatically loaded' ether.[27]

It is well known that Kelvin's references to Maxwell's theory are of a puzzling character. A brief sketch is given by Whittaker (1951:266–7) of the record of his fluctuating, ambivalent, at times downright hostile comments. The strongest favourable reference Whittaker cites is in Kelvin's preface to the English translation of Hertz's papers on electromagnetic wave propagation; there, Whittaker says, Kelvin 'appeared to accept "magnetic waves" '. (Yet if Kelvin, all things considered, seems odd on this subject, it must be said that Whittaker also seems a little strange; the phrase just quoted is a curious one to use of a passage in which Kelvin – referring to what he calls Hertz's 'experimental demonstration of magnetic waves' – says that 'for electricity and magnetism Faraday's anticipations and Clerk-Maxwell's splendidly developed theory have been established on the sure basis of experiment by Hertz's work'.)[28] It appears, in fact, that the favourable remarks in the Hertz preface signify a good deal more than (what one might suspect) an effort got up for the sort of ceremonial occasion on which 'a man is not upon oath': In the period from shortly before to a few years after 1890, stimulated presumably by a conjunction of the great results of Hertz, the influence of his compatriots Fitzgerald, Lodge, and Heaviside, and his own partial successes with the theory, Kelvin seems to have experienced a surge of enthusiasm for Maxwell's theory. Here, for instance, is what he told the Institution of Electrical Engineers, in his presidential address to that body in January 1889 (after referring to 'the velocity which is the conductance in electrostatic measure, and the resistance in electromagnetic measure of one and the same conductor', and stating that this velocity is 'not very different from that of light'):

> But its relationship to the velocity of light was brought out in a manner by Maxwell to make it really a part of theory which it never was before. Maxwell pointed out its application to the possible or probable explanation of electric effects by the influence of a medium, and showed that that medium – the medium whose motions constitute light – must be ether. [*Note:* This is correctly transcribed from the source cited; but it seems to be a transposition – the intended sense, surely, is, 'that medium must be ether – the medium whose motions constitute light.'] Maxwell's 'electro-magnetic theory of light' marks a stage of enormous importance in electro-magnetic doctrine, and I cannot doubt but that in electro-magnetic practice we shall derive great benefit from a pursuing of the theoretical ideas suggested by such considerations.[29]

(A reference to 'Heaviside's way of looking at the submarine cable' follows as an example of such benefit.)

The connexion with Kelvin's own work on the theory of the quasi-rigid ether is manifest in his basic paper on that subject, although Witte (1906:47) is a little misleading when he describes this paper as containing an attempted mechanical explanation of the totality of electrical phenomena. Here is Kelvin's own summary of what he has and has not accomplished:

> We thus have simply *the undulatory theory of light,* as an inevitable consequence of believing that the displacement of an elastic solid by which, in my old paper [of 1847], I gave merely a 'representation' of the electric currents and the corresponding magnetic forces, is a reality. But to give anything like a satisfactory material realisation of Maxwell's electro-magnetic theory of light, it is necessary to show *electro-static force* in relation to the forcive (X,Y,Z) of my formulas; to explain the generation of heat according to Ohm's law in virtue of the action of this forcive when it causes an electric current to flow through a conductor; and to show how it is that the velocity of light *in ether* is equal to, or perhaps we should rather say, *is,* the number of electro-static units in the electro-magnetic unit of electric quantity. All this essentially involves the consideration of ponderable matter permeated by, or imbedded in ether, and a *tertium quid* which we may call electricity, a fluid go-between, serving to transmit force between ponderable matter and ether . . . I see no way of suggesting properties of matter, of electricity, or of ether, by which all this, or any more than a very slight approach to it, can be done, and I think we must feel at present that the triple alliance, ether, electricity, and ponderable matter is rather a result of our want of knowledge, and of capacity to imagine beyond the limited present horizon of physical science, than a reality of nature.[30]

This is a remarkable and trenchant statement. Much of our remaining discussion is concerned with a quite different line of research that succeeded in advancing knowledge of the matters pointed out by Kelvin as crucially problematic. But first let us consider some work in the immediate line of descent from Fitzgerald and Kelvin. Later I shall deal briefly with two other approaches to a theory of a materially constituted ether (one closely related to those previously discussed, the other not), and shall have especially to examine the nature of the difficulties that ultimately proved insuperable for all attempts to solve the 'classical ether-problem for the electromagnetic theory' (and proved to require, in Kelvin's words, a 'capacity to imagine beyond the limited present horizon of physical science' – farther beyond that horizon than Kelvin was ready to follow).

The problem of discrete electric charges: Larmor's electrons

Fitzgerald, we have seen, established the applicability of MacCullagh's dynamical analysis to Maxwell's electromagnetic field in uncharged dielectric media. The restriction is essential, for the identification of the dielectric displacement with curl **q** implies that this field is source-free. It is also, clearly, a serious restriction; from it, for example, it follows that the processes referred to by Kelvin as problematic – electrostatic forces, energy transformation in electric currents, and in fact most of the interactions by which energy is transferred between the electromagnetic field and ordinary matter – are outside the scope of this mechanical analysis.

Another consequence of the restriction is an inevitable ambiguity about how the translation between mechanical and electromagnetic terms ought really to be made. For in the charge-free and current-free case, there is complete symmetry in the field laws between electric fields and magnetic fields. Fitzgerald, following Maxwell, took magnetic energy to be kinetic, and accordingly interpreted the electric field as related to the twist of the medium; Kelvin, on the other hand, pursuing an analogy he had described as long before as 1847, preferred to associate the magnetic field with the twist: Therefore in his version it is the electric field that corresponds to the velocity of the medium.[31] The prospect thus arises that among the alternatives that stand on a par for free fields, one or another may offer an advantage for the extension to the interaction with charges and currents.

As to Kelvin's interpretation, a difficulty looms at once: Since electric charges are sources or sinks of the electric field, a theory identifying this field with the velocity of a material medium implies continual creation and destruction of the matter of this medium at the locations of charges (creation at the charges of one sign, destruction at those of the other). Perhaps this objection should not be regarded as necessarily fatal – perhaps, even (although this is very doubtful), Kelvin himself had this in mind when he spoke of a 'capacity to imagine beyond the limited present horizon of physical science' – but it would certainly be a serious departure from the notion of a 'classical' ether.

If we turn instead to the Fitzgerald version, the situation looks more encouraging (as an impressive array of investigators concluded: This mode of representation was pursued by Heaviside,[32] Sommerfeld,[33] Reiff,[34] and – to most notable effect – Larmor);[35] for the field of magnetic induction (assuming no 'true magnetic poles') is source-free, and can therefore be represented as the velocity field of an incompressible conserved substance. The crux of the problem becomes how to modify Fitzgerald's identification of the dielectric displacement **D** with curl **q**, and the associated identification of the electric field intensity **E** with the quasi-elastic restoring torque of the rotated element of the medium. Larmor proposed – first in an addendum, entitled 'Introduction of free electrons', to the initial paper of the series of three cited in n. 35 – to modify the theory of the quasi-rigid ether by postulating the presence of permanent (but mobile) *'centres of rotational strain'* in the medium, giving rise to uneliminable distributions of torque (upon which the additional torque proportional to additional twist is superposed).[36] These nuclei of 'intrinsic radial twist' would amount to elementary electric charges. Larmor names them *electrons* ('at the suggestion of G. F. Fitzgerald after G. J. Stoney's term'),[37] and goes on to sketch a 'purely electric theory of matter', based on the supposition that these electrons have no 'intrinsic inertia': that the laws of their motion are simply those of the dynamics of ether (in other words, that electrons have only what later came to be called 'electromagnetic mass'). He remarks that according to the theory such 'free electrons' could easily acquire velocities 'a considerable fraction of that of radiation', and suggests (this is in 1894) a possible connexion with the 'negative rays in vacuum tubes', remarking that he is informed by J. J. Thomson that these rays have been determined to have velocities of about 2×10^7 cm/sec.[38]

What we see emerge, as Larmor elaborates this theory, is a version of what has come to be, for us, the 'classical theory of electrons' – here developed out of speculation upon a constitution of ether that could provide a place in the very structure of this medium for what Kelvin calls the *tertium quid,* electricity. But the manner in which electricity serves for the transmission of force between 'ether' and 'ponderable matter', as it is envisaged in this theory, is not that of a go-between; instead there is projected an account of matter as in effect constituted out of electricity, which in its turn is nothing more than a particular (mobile) condition of a region of ether. This clearly entails exactly that view of the way matter can move through ether adumbrated by

Fitzgerald in his 1885 paper (quoted in at n. 23 of this chapter); and in his review of Larmor's book *Aether and matter* (1900), Fitzgerald repeats his own analogy: 'His theory practically assumes that matter can move through the ether much in the way that a drop of liquid water can move through a lump of ice, namely, by melting in front and freezing up again behind'.[39] Thus the framework appears to be present of a truly grand synthesis: a unified theory of matter, light, electricity, and magnetism, as manifestations of that single 'substance of most extreme simplicity', luminiferous ether.

Lorentz's particulate ether

Writing in 1901, in an obituary notice of Fitzgerald in the *Physical Review*, Larmor spoke of this work of his own in the following terms:

> In 1893 the formal development of electrodynamic theory from this point of view, inspired at the start by Fitzgerald's electric interpretation of MacCullagh's optical analysis, and by mechanical models based on Lord Kelvin's construction for a rotationally elastic aether, and resting directly on the single broad dynamical basis of the principle of Least Action . . . was initiated in Great Britain; it was practically complete early in 1897, running parallel in the main results, though not in method or mode of development, with Lorentz's final presentation in his tract of 1895.[40]

If, today, it is preeminently Lorentz, not Larmor, who is remembered as the great exponent of the theory of electrons, this has to be regarded as at least to some degree related to the difference in 'method or mode of development' that Larmor refers to – and to the fact that Lorentz's mode of thought proved in the end the more tenable.

The contrast may roughly be put thus: The centre of attention in the work we have been considering was upon the 'mechanical constitution' of the light-bearing medium, and the speculations of Fitzgerald, Kelvin, and Larmor were concerned especially with the fine-scale structure (or lack of structure) of ether; the centre of attention in the work of Lorentz was upon the electrical constitution of transparent media, and the speculations of Lorentz were concerned with the fine-scale structure of ordinary bodies.

We have already seen that Lorentz, like Fitzgerald, early addressed himself to the electromagnetic theory of reflection and refraction. In doing so, he was expressly following the lead that had been given by Helmholtz; and he employed Helmholtz's formulation of electrodynamic theory as the basis of his own investigation. Now, Helmholtz's procedure was the following: Starting from a theory – or, rather, a parametric family of theories – of charges and currents interacting at a distance, he investigated the laws that would result for the propagation of electromagnetic effects in a polarisable dielectric medium.[41] Helmholtz found that such a medium would, in general, carry both longitudinal and transverse electric oscillations, with different velocities (each characteristic of the medium). In the limiting case of what might be called 'complete polarisability', the velocity of the longitudinal wave becomes infinite, leaving in effect only transverse oscillations; and in this limit, Helmholtz's theory approaches that of Maxwell. (There are difficulties in the way of conceiving this limit as actually attained, so that it is only in a somewhat stretched sense that Helmholtz's account can be said to include Maxwell's 'as a special case'.) Electromagnetic waves, then, are represented in this picture as actual oscillations of 'electricity' in a polarisable medium; and Lorentz, pursuing within this general framework the question of the optical properties of bodies, came very naturally to deal with bodies through hypotheses about their electrical structure. In the early stages of this work, no special consideration of ether was required: It was simply a dielectric medium, comparable to air or glass.

Lorentz's thesis ends with a brief résumé of what has been treated and a review of problems that present interesting and promising subjects of further investigation.[42] Among these are 'the phenomenon of dispersion, the rotation of the plane of polarisation, and the way in which these phenomena are connected to molecular structure; then the mechanical forces that perhaps play a certain role in optical phenomena'; among them, too, is 'the influence exercised upon light by . . . the motion of the medium'.

The phenomenon of dispersion was treated by Lorentz in an early work: a paper published (in Dutch) in 1878 and in abridged form in German in 1880.[43] The most fundamental conclusion of this investigation is expressed by Lorentz in these words (my italics):

> If we accept the electromagnetic theory of light, there is nothing left, in my opinion, but to look for the cause of dispersion *in the molecules of the medium themselves*. And we can indeed obtain formulae from which a dispersion follows if we adopt the supposition that, in such a molecule, *as soon as an electric moment is excited, a certain mass is at the same time brought into motion*.[44]

This investigation of Lorentz's appears to have gone largely unnoticed for a considerable time. It is hard to understand why; but Gibbs, for example, in a series of papers dealing with related subjects (and with dispersion in particular), from 1882 to 1889, refers repeatedly to the German précis of Lorentz's thesis, but not once to Lorentz's theory of dispersion;[45] and a paper presented to the Berlin Academy in 1892 by Helmholtz seems to show that the latter remained, at that date, unaware of Lorentz's work on the subject. Helmholtz's paper opens with the remark that in his opinion a satisfying explanation of dispersion on the basis of the electromagnetic theory of light has not yet been given; he proceeds to explain the grounds of his dissatisfaction, and to develop the hypotheses on which a cogent account can be based[46] – which hypotheses turn out to be just those proposed fourteen years earlier by Lorentz.

The argument offered by Helmholtz is interesting. It was already understood from the elastic-ether theory of dispersion (to which Helmholtz had himself contributed) that a satisfactory account of this phenomenon seems to require the presence, in dispersive media, of particles capable of resonating to the optical oscillations at certain characteristic frequencies. In this paper, Helmholtz remarks that in Maxwell's theory of the forces exerted by the electromagnetic field upon neutral bodies, these forces are unchanged if the fields are reversed; these forces 'would therefore attain their greatest and smallest values each twice in each [optical] period of oscillation, so that they could not as a rule produce or sustain [mechanical] oscillations of the length of a simple period'. From this he concludes:

> Only if the ponderable particles contain charges of true electricity can the periodic alternations of the electric moments in the ether educe ponderomotive forces of the same period. The analogous assumption, that embedded atoms contain isolated northern or southern magnetism, I pass over as too improbable. On the other hand, the electrolytic phenomena, especially Faraday's law of electrolytic equivalents, have long since led to the assumption that electric charges of determinate magnitude attach to the valence positions of chemically bound ions – charges that can be now positive, now negative, but which must everywhere, for every valence position of every atom, have the same absolute magnitude.

This line of thought, therefore, leads to a break with the conception of electricity as a 'weightless fluid', and to its amalgamation instead with 'ponderable' matter – and, further, particulate matter. The working out of the theory demands a set of principles concerning the coupling of charged particles to the ether – that is, to the electromagnetic field – and this is not quite so straightforward a matter as it might be thought. So far as the electric field is concerned, to be sure, there can be no doubt: The charges of charged particles will be sources of the field of dielectric displacement, and they will subject the particles they belong to to the 'ponderomotive' force exerted, in an electric field, on charged bodies. But when Lorentz published his work of 1878, it was far less a matter of course to assume that a moving charged particle is to be regarded, in its relations to the magnetic field, as tantamount to a current. Lorentz explicitly introduces this as an assumption, with the remark: 'The experiments announced by Helmholtz as made by Rowland support this'.[47] More precisely, what Lorentz assumes is that a moving charged particle 'produces the same effects' as a current element: that is, that it is to be reckoned into the 'total current' term in Maxwell's equation connecting the current with the curl of the magnetic field. As to the 'ponderomotive' force on a moving charge, Lorentz has no occasion in this paper to consider anything but the electric contribution to that force; for this optical problem, the magnetic ponderomotive forces are negligible, and Lorentz does not even mention them.[48]

In his celebrated work of 1892 (cited in n. 8), Lorentz sets himself more far-reaching objectives. The problem of the electrodynamics of bodies in motion had just been treated by Hertz, on the tentative working assumption that ether within a moving body completely shares the motion of the body;[49] and Lorentz includes a chapter on Hertz's hypothesis. But Lorentz himself inclines to the extreme opposite view: that the motion of a body is to be regarded (at least in first approximation) as without any influence upon the state of motion of the ether through which it moves. This poses a very serious problem at the outset, for Maxwell's theory offers no clear guidance about the laws governing the connexion, or interaction, of the electromagnetic field with a body in motion through the ether; it was just this circumstance that led Hertz to adopt the working hypothesis he did: not because it was plausible (he points out that it is not), but because it was manageable.

Lorentz's solution of this difficulty is based upon two strategic moves. In the first place, he makes a general constitutive assumption about bodies: that they may be regarded as systems of charged particles, whose only interaction with ether is the interaction of charges (at rest or in motion) with the electromagnetic field. This means that the theory requires only, on the one hand, suitable assumptions about the binding of these charges into the system that is an ordinary body, and, on the other hand, the electrodynamic laws governing the relation to the field of a single charged particle – for it is presupposed that direct interaction with the field occurs independently for the individual charges of the system. Lorentz puts the matter thus:

> It has seemed to me useful to develop a theory of electromagnetic phenomena based on the idea of a ponderable matter perfectly permeable to the ether and able to move without communicating to the latter the least motion. Certain facts of optics may be invoked in support of this hypothesis and, although doubt is still permitted, it is certainly important to examine all the consequences of this view. Unfortunately, a quite serious difficulty presents itself from the beginning. How, in fact, is one to form a precise idea of a body

> which, moving in the bosom of the ether and consequently traversed by this medium, is at the same time the seat of an electric current or a dielectric phenomenon? To surmount this difficulty, so far as I could, I have sought to reduce all the phenomena to a single one, the simplest of all, which is nothing else than the motion of an electrified body. One will see that, without any deeper study of the relation between ponderable matter and the ether, one can establish a system of equations suited to describe what passes in a system of such bodies. These equations accommodate very varied applications, which will be the object of the subsequent chapters.[50]

There remains the crucial problem of finding the laws of that 'simplest phenomenon of all,' the interaction of a single moving charge with ether; and to accomplish this, Lorentz makes his other strategic move. He alludes to this in the introduction to his paper,[51] where he contrasts his own procedure with that of Hertz. The latter had abstained, says Lorentz, from any 'mechanical' account of electromagnetic actions, and 'contented himself with a clear and succinct description, independent of any preconceived idea about what happens in the electromagnetic field' – a procedure (he says) that has its advantages (and, indeed, that Lorentz himself tends to follow in his later writings –after his own 'clear and succinct description' has been once attained). Nevertheless, he goes on, one is always tempted to return to mechanical explanations, and he will therefore make use of a generalisation of Maxwell's own method (in the *Treatise*) Then he adds:

> I have yet another motive to undertake these inquiries. In the memoir in which M. Hertz treats bodies in motion, he admits that the ether they contain moves with them. Now, optical phenomena have long since demonstrated that it is not always so. I therefore wished to know the laws that govern the electrical motions in bodies that traverse the ether without entraining it; and it seemed to me difficult to attain this end without having as guide a theoretical idea. The views of Maxwell [namely, on the application of mechanical principles to the field and charges as a 'connected mechanical system'] can serve as foundation of the desired theory.

The generalised dynamical assumptions Lorentz makes are (1) that the *configuration* – in the technical dynamical sense – of an electrodynamic system is specified jointly by the positions of the charged particles and the distribution of the electric field, and (2) that the potential and kinetic energies are as Maxwell had long since postulated. As what might be called 'constitutive conditions' or 'constraints' of the connected system, he assumes both the divergence equations of Maxwell (charges are the sources of the electric field; the magnetic field is source-free) and Maxwell's equation connecting the curl of the magnetic field with the total current (where the latter is now understood to be the sum of the free-ethereal 'displacement current' of Maxwell and the 'convection current' constituted by motion of charges). From these assumptions Lorentz deduces, by standard variational methods of dynamics, first, Maxwell's equation for the curl of the electric field – which, of course, is old, but whose derivation constitutes the sought-for 'mechanical explanation' of the general laws of the field – and, second, the total 'ponderomotive' force exerted 'by the ether on a charged particle', a result of fundamental importance for Lorentz's theory and for all subsequent physics.[52]

Other ether models, *c.* 1900

From the point of view of one demanding a mechanically cogent theory, neither Lorentz's theory nor Larmor's was altogether immune to criticism, as we shall presently see. Two other hypotheses, still actively explored in the 1890s and 1900s, may be here briefly mentioned. One is an alternative conception of ethereal elasticity, put forward by Kelvin in 1888 (shortly before the 'quasi-rigid' ether), reviving a nearly fifty-year-old suggestion of Cauchy (cf. Whittaker, 1951:146 ff.), according to which ether has the character not of a 'jelly' but of a 'foam': 'a solid of such negative compressibility as should make the velocity of the condensational-rarefactional wave, zero or small'.[53] This theory (called that of the 'quasi-labile ether', 'contractile ether', or 'foam ether') has a remarkable relation to the theory of the quasi-rigid ether: In the foam ether, under given conditions of displacement of its particles (but assuming that the displacement vanishes outside a bounded region), the distribution of elastic forces will be the same as for the quasi-rigid ether; but there will be no volume distribution of (nonvanishing) torque. As Witte (1906:120) points out, the two theories ascribe to the strained ether stress tensors which differ, that of the foam ether being symmetric and that of the quasi-rigid ether not so, but which agree in the volume forces they determine. It follows that the actual motions in the foam ether will be the same (under the same initial conditions, and with the boundary condition mentioned previously) as in the quasi-rigid ether, so that the difficulties to be noted for the latter affect the former as well.

It is, perhaps, the eventual failure of these ether schemes that had looked so promising for electrodynamics that best explains the curious statement by Kelvin in his preface (1904) to the published version of his *Baltimore lectures*, cited by Whittaker (1951:267) as giving Kelvin's 'final judgment' on Maxwell's theory: 'The so-called "electro-magnetic theory of light" has not helped us hitherto'. (A further essential gloss upon this is another passage, near in date, in the same work – from appendix A, dated 1900: 'The so-called "electro-magnetic theory of light" does not cut away this foundation [namely, the elastic solid ether] from the old undulatory theory of light. It adds to that primary theory an enormous province of transcendent interest and importance; it demands of us not merely an explanation of all the phenomena of light and radiant heat by transverse vibrations of an elastic solid called ether, but also the inclusion of electric currents, of the permanent magnetism of steel and lodestone, of magnetic force, and of electrostatic force, in a comprehensive etherial dynamics'.)[54] At any rate, the theory that Kelvin in fact prefers, in the parts of the *Baltimore lectures* written last (and apparently until his death in 1907),[55] is a strange one indeed. In it (as for Lorentz) ether and 'ponderable matter' are perfectly permeable to one another; but the particles of matter and of ether act upon one another at a distance. Free ether he takes to be nearly incompressible (so that longitudinal waves in it have nearly infinite velocity); but the equilibrium of the ether in regions within 'ponderable matter', rearranged under the influence of the forces of attraction and repulsion there operating, he takes to be of the 'quasi-labile' kind, with nearly zero velocity for longitudinal waves. With this curious assortment of penetrable matters, actions at a distance, and both incompressible and 'contractile' elasticities,[56] Kelvin shows, in the parts of the *Baltimore lectures* written last, that he can account for much – but not quite all – of optics. He says in the preface: 'My object in undertaking the Baltimore Lectures was to find how much of the phenomena of light can be explained without going beyond the elastic-solid theory. We have now our answer: *every thing non-magnetic; nothing magnetic*'. (In fact this is slightly too optimistic: See his appendix B, on the 'two clouds' – neither of them essentially 'magnetic'.)

The second, and altogether different, view of ether referred to at the opening of this section has its roots partly in work of Kelvin's of around 1870, but especially in the investigations of C. A. Bjerknes (see Whittaker, 1951:284–6). It envisaged the ether as a perfect incompressible liquid, and was based upon the demonstrations by Kelvin and Bjerknes that in such a medium forces of attraction and repulsion would occur between immersed solid bodies in suitable vibratory states. Whittaker cites a work of Arthur Korn as containing a theory of gravitation based upon this general conception; but he fails to remark that the same work actually contains a development of the entire electromagnetic theory of Maxwell upon this foundation![57] But how is this possible? He who is curious will have to look into it; here it must suffice to say that Korn obtains a stock of parameters to manipulate by introducing special kinds of solid bodies, characterised by special intrinsic modes of vibration and by nonstandard boundary conditions for their surfaces of contact with the fluid ether. (As late as 1917–18, Korn was pursuing an analogous approach to an alternative to Einstein's general relativistic gravitational theory.)[58]

Another figure active in cultivating the point of view of Bjerknes was the latter's son, Vilhelm F. K. Bjerknes, himself a physicist of some distinction,[59] whose book *Fields of force*, devoted to the exposition of the analogies between hydrodynamics and electromagnetism, although without significant influence upon the latter theory, appears to have been of considerable importance for the former – and especially for meteorology.[60] It is a striking fact that this monograph appeared as publication number one of the Ernest Kempton Adams Fund for Physical Research, as the record of a course of lectures delivered in 1905 at Columbia University, and that publication number two of the same fund, containing lectures delivered at Columbia in 1906, was the first edition (1909) of Lorentz's famous book *The theory of electrons*.

The problem of ethereal motion

The difficulties alluded to at the beginning of the preceding section all have to do, although in somewhat different ways, with the idea of *ethereal motion*. We have already seen the problem that was presented for Kelvin's version of his quasi-rigid ether by the motion required in it in the simple case of electrostatic fields; and we have seen how the Fitzgerald–Larmor alternative seemed to promise a coherent solution. But it is necessary now to revert to a fine point, earlier slurred over. I said in the first section of the chapter that Fitzgerald identified the magnetic field intensity at any point 'with (roughly) the velocity $\dot{\mathbf{q}}$ of MacCullagh's ether at the same point'. The import of the qualification is this: The value at a given point (and time) of the vector field $\mathbf{q}$ is supposed to represent the displacement (then) of the ether particle 'belonging' there; but this particle is not in fact (then) at that point (except when $\mathbf{q}$ happens to vanish there); and the velocity $\dot{\mathbf{q}}$ is therefore, quite strictly, that of the ether, not at the position in question, but at the *displaced* position. This is an entirely familiar situation in the theory of elastic vibrations, where it is understood that the distinction just pointed to can be safely ignored, so long as the oscillations of the medium are 'small'. Analogously, one is able on similar grounds to disregard the difference between the rate of change (with time) of a quantity at a fixed location and the corresponding rate of change as one follows the motion of a single particle. Now, for the arguments establishing the agreement of Maxwell's electrodynamics with the properties of the quasi-rigid ether, it is necessary to neglect these differences; that is to say, for the theory as sketched to be tenable, it is necessary that the differences be negligible – that, in effect, the actual velocities and the actual displacements in the ether be 'small'. ('Smallness' of these quantities is actually required yet again, in this theory, for the cogency of the basic definition of 'rotational elasticity' itself; for it is only – in a sense that can be made precise – in the 'infinitesimal limit' that curl **q** can be said to represent the 'rotation' or 'twist' of the medium; for sizable values of **q**, a restoring torque proportional to curl **q** makes no physical sense.)

This, however, leads to almost immediate disaster. Once more it is the *non*optical case, the simple case of steady fields, that offers the obvious stumbling block; for if the intensity of the magnetic field represents the velocity $\dot{\mathbf{q}}$ of ether, then in a steady magnetic field of sufficiently long duration, large displacements of the ether particles must ensue.

It would be misleading to suggest that this simple remark suffices to end the matter. Remedial measures might be sought, and were sought, for instance, by Larmor; and more elaborate discussion than is possible here would be required to do justice to the issue. But an accurate general statement can be made, and it is this: The 'rotational' theory of Larmor has as a consequence that in certain situations – those in which either high velocities or sizable displacements of ether particles occur – new observable effects, deviations from the orthodox theories of optics or electromagnetism, are to be expected. Such effects were sought for; but they were never found. One example at least is worth citing: Having modified his theory to take account of the difficulty mentioned, concerning curl **q** when **q** is of noticeable size, Larmor inferred from his revised dynamics of ether that in a steady magnetic field in a region initially neutral electrically, there will eventually be evolved a distribution of electric charge throughout the region.[61] (It is of some interest from a methodological point of view to note that these predictions do not constitute a point of 'falsifiability' for Larmor's theory, since the theory leaves unspecified the magnitude of the constants of proportionality relating electric and magnetic quantities to displacements and velocities in the ether: In any given case, a negative result can be explained away on the grounds that the effect was still too small to be observed. On the other hand, a positive outcome of the search for such an effect as the evolution of charge by a magnetic field would undoubtedly have meant a triumph for Larmor's theory comparable to the triumph supplied for Maxwell's by Hertz's production and detection of electromagnetic waves.)

In commenting, at the end of the first of his series of papers 'A propos de la théorie de M. Larmor' (see n. 36), upon an attempt by Lodge to detect one of the effects of ether motion suggested by Larmor's theory (namely, an alteration of the velocity of light by a strong magnetic field), Poincaré expresses himself as follows:

> Thus this motion [of the ether] was so slow that the experiments of M. Lodge, although very precise, were yet not precise enough to detect it. To say all that I think, I believe that if these experiments had been a hundred or a thousand times more precise, the result would still have been negative.
>
> In support of this opinion I have nothing to offer but a subjective conviction [*des raisons de sentiment*]; if the result had been positive, one would have been able to measure the density of the ether, and – if the reader will forgive me the vulgarity of this expression – it is repugnant to me to think that the ether is *si arrivé que cela*.[62]

(I have left the last phrase untranslated, for its full colloquial savour; a rough equivalent would be 'that big a success'.)

In the second and third papers of this series, Poincaré turns his attention to the problem of the electrodynamics of moving bodies (which Larmor himself, in the preliminary article that had occasioned Poincaré's reflections, had not yet discussed); and, reviewing the salient evidence and the major theories thus far proposed, he concludes that of all these theories Lorentz's is 'the least defective'.[63] Yet Lorentz's theory does have, in his view, a major defect, which makes it impossible for one to accept it as definitive: In this theory, the principle of the equality of action and reaction is violated.

The nub of the difficulty can be seen in the formula for, or indeed in the very concept of, the 'Lorentz force'; for this, the only 'ponderomotive' force in Lorentz's electrodynamics, is exerted only upon charges, and only by ether. A comparison with the more orthodox Maxwellian tradition is instructive: There the 'displacement current' of Maxwell is regarded as subject to the exercise of magnetic force, just as is the ordinary current in a conductor; and this means that 'ponderomotive' forces will be exercised upon ether itself, as a seat of displacement currents.[64] (Thus we note, incidentally, an important break, in the theory of Lorentz, with Maxwell's principle of the 'displacement current': The latter is *not* in all electrodynamic respects 'equivalent to a current'; it is so in its relation to the distribution of fields, but not as regards the determination of moving force; and just this break leads to the failure of the principle of reaction. It is also clear, of course, that Lorentz has departed quite fundamentally from his earlier adherence to Helmholtz's theory of the 'electrically polarisable ether': Of all the investigators, Lorentz is the one most radically 'emancipated from the thraldom of a material ether'.)

In his estimate of the theoretical situation, Poincaré goes further than to register his unhappiness at the violation of the principle of reaction in Lorentz's theory; he also deprecates the notion that forces upon ether should be required in order to maintain that principle, saying: 'It seems to me very hard to admit that the principle of reaction is violated, even in appearance, and that it is no longer true if one envisages solely the actions suffered by ponderable matter and leaves aside the reaction of this matter upon the ether'.[65] He then cites the fact that a series of experiments have strongly suggested the impossibility of detecting the relative motion of 'ponderable matter' with respect to ether (Lodge's experiments are one example; 'a recent experiment of M. Michelson' is another), connecting this with his remark about action and reaction – and, by implication, with the 'subjective conviction' stated in his comment on the experiments of Lodge – and expressing the hope that the same repair of theory that succeeds in explaining why motion with respect to ether can in principle not be detected will also repair the breach of the principle of reaction, restoring this principle for ponderable matter considered alone and apart from ether. One might fairly sum up Poincaré's conviction as that of the impotence of ether.

One consideration would seem to make this view puzzling. Maxwell had shown that on his electromagnetic theory of light a reflecting or absorbing surface is subject to *pressure* from the light that falls on it;[66] quite elementary arguments then show that if the principle of reaction – and therefore that of conservation of momentum – is to be maintained, a beam of light must contain, and transport, momentum; and this *must* be reckoned into the balance if conservation is not to be 'violated in appearance'. How, then, could Poincaré have held the hope and expectation that he did?

The answer seems to be simply this: that for Poincaré in 1895, Maxwell's light pressure was a theoretical notion of not significantly higher standing than ether winds or the influence of magnetic fields on the velocity of light. He does not say this; but it is extremely hard to make sense of his position otherwise. And indeed the pressure of radiation *was* at the time a dubious matter; it was first detected experimentally by Lebedev in 1899, despite attempts dating back to 1873 (Whittaker, 1951:267). It is true that Fitzgerald suggested in 1882 that the pressure of sunlight might be responsible for comets' tails;[67] but this was very speculative. It is also true that pressure of radiation had played a crucial part in the theoretical investigations by Boltzmann (1884) and Wien (1893) of black-body radiation (Whittaker, 1951:374, 379–80); but these, too, were speculative and, at the time, somewhat out-of-the-way pieces of work (although within another few years they had proved to have played a role in transforming the foundations of physics). The clearest possible indication of how precarious a status belonged to light pressure is provided by this passage from a paper of Larmor in 1892:

> A formula has been given by Maxwell for the intensity of the pressural force produced by electric undulations in the aether striking against a plate of conducting matter, a force which has apparently not been detected for the case of light-waves. If the notions here suggested have any basis, this force may likely be non-existent.[68]

To this there is a note, added in the edition of 1929: 'The pressure of radiation is now abundantly verified, and has become a keystone of theory'. (Note in passing that anyone concerned to form a just estimate of the stance of Kelvin, and impressed by Lodge's 1898 statement that 'Kelvin doesn't even believe in Maxwell's light pressure' [Whittaker, 1951:267] must take into consideration this passage from Larmor.)

Now, once one accepts the pressure of radiation as genuine, any theory of radiation as consisting in the transmission of motions through a system of interacting masses has to attribute to this underlying material carrier of the radiation not merely fine-scale oscillations, but net streaming motions. And this means that the difficulties that were obvious for the Kelvin and Larmor theories in the case of steady fields are really present in the 'purely optical' case as well.

What, then, in this situation, was the position of Lorentz? First, Lorentz's theory definitely implies that light exerts a pressure: yet another manifestation of the breakdown, in this theory, of momentum conservation 'if one envisages solely . . . ponderable matter'. Second, Lorentz accepts the evidence suggesting the undetectability of 'streamings' of ether, and systematically abstains from any attempt to characterise ether as a system of masses capable of motions and interacting by forces. As we have seen, this by no means prevents him from applying, as Fitzgerald had done, to ether – and further, as Fitzgerald had not done, to ether and charged particles (in arbitrary motion) as a single system – the principles of generalised dynamics.

An account of the developments that clearly established Lorentz's point of view as the most fruitful – and, at the same time, did indeed solve the problems of momentum conservation and of undetectability of motion with respect to ether, although not in the way anticipated by Poincaré – would require a treatment of the history of the special theory of relativity, a subject that would demand another chapter. Limitations of space compel the present discussion to end with no more than the briefest suggestion of the shape of the results. These may be described as a striking confirmation of half of what Poincaré expected, and an equally striking disconfirmation of the other half. So far as the question of the velocity of ether is concerned, Poincaré's 'principle of impotency' is true: In the definitive form of the theory, the concept 'state of motion of the ether at a given point' does not occur at all. So far as the momentum of ether is concerned, Poincaré's 'principle of impotency' is false: None of the fundamental conserved quantities of classical physics – momentum, angular momentum, energy – is in fact conserved, unless the part associated with 'ether', that is, with the electromagnetic field, is reckoned into the balance. That such dynamical quantities as momentum and angular momentum are to be ascribed to the field, without definite masses moving with definite velocities as their bearers, is a very basic result, comparable in significance to the earlier extension of the concept of energy beyond the kinematically grounded *vis viva* of the older mechanics.

When this conceptual transformation has been effected, it is very striking how some older notions and principles remain, shining through the new forms, whereas others simply vanish. A striking example has already been given: The ether has momentum; but it has no velocity. Another notable instance is the tensor of stress in the field that had been introduced by Maxwell, but seemed to have fallen out of the Lorentz theory (since Lorentz's force was not derived from the Maxwell stress; had it been so derived, it would have had to satisfy the principle of reaction). And lo! the clarifying explanation: The Maxwell stresses indeed have a role in the theory, intimately associated with the conservation principle, for they define the *flow of momentum;* but since momentum is no longer entirely 'kinetic', only a part of this momentum flow is through 'ponderomotive forces' that transfer momentum to moving bodies. On the other hand, the angular momenta posited by the various 'gyroscopic' or 'vortex-sponge' theories do *not* survive this transformation, for contrary to these theories, there is not a store of angular momentum in a pure magnetic field (or in a pure electric field); again, contrary to all the 'classical' ether theories, the momentum vanishes both for pure electric and for pure magnetic fields.

It might be supposed that this attribution of dynamical quantities like momentum and angular momentum to fields is merely a kind of 'tailoring' of a theory to make it shapely, or (*vide* Stevens) a kind of 'barbering' of natural appearances to make them comely. Perhaps the most convincing demonstration that it is not so would be a discussion of the role these notions have played in the development of the quantum theory (a discussion, however, that would demand, not another chapter, but another volume). Let one example suffice. Arnold Sommerfeld, in his very important book *Atomic theory and spectral lines*, discusses the Zeeman effect, an effect of magnetism upon the emission of light, for the discovery and – partial – explanation of which he and Lorentz shared a Nobel Prize in 1902 (cf. Kelvin's phrase of 1904, 'nothing magnetic', and his associated comment on the unhelpfulness of 'the so-called "electro-magnetic theory of light" '). In the course of his discussion,

Sommerfeld remarks that a previous attempt to develop a quantum-theoretic account of this effect had had to end with the statement that 'Bohr's energy equation . . . can never account for the polarisations'; but we now see, he tells us, that it is only necessary to add the equation of angular momentum balance between atom and radiation to obtain a satisfactory treatment.[69]

The example may also remind us that the same Sommerfeld worked on ether models in the 1890s and on quantum-theoretic models of atoms and molecules in the 1910s and 1920s. The two inquiries were comparably well motivated, and were executed with comparable skill; the former has left no trace upon the physics of our own time, the latter has been profoundly important. Some 'curls' survive; which do is determined by something other than barbering.

Notes

1 No historical account known to me approaches adequacy. The later chapters of Whittaker (1951) provide a useful point of departure; but they are deficient both in comprehensiveness and (as we shall have occasion to see) in reliability. Another secondary source of considerable value is Witte (1906); this monograph, which originated as a doctoral dissertation under the sponsorship of Planck, is conceived as a critical discussion of the existing state of theory rather than as a historical account, but it contains a great deal of historical information and much valuable elucidation of the relationships among theories.

2 J. C. Maxwell, *The scientific papers of James Clerk Maxwell*, 2 vols. (Cambridge, 1890), 1:527.

3 J. C. Maxwell, *A treatise on electricity and magnetism*, 3rd ed., 2 vols. (Oxford, 1892), 2:276 (art. 638).

4 H. Poincaré, *Electricité et optique*, vol. 1, *Les théories de Maxwell et la théorie électromagnétique de la lumière* (Paris, 1890). (The lectures were delivered in the spring of 1888, not of 1889 as the title page indicates: See vol. 2 of the same work, *Les théories de Helmholtz et les expériences de Hertz* [Paris, 1891)], vii–viii.)

5 H. Hertz, *Electric waves*, trans. D. E. Jones (London, 1893), 20.

6 *Électricité et optique*, 1:vii. See also H. Poincaré, *Science and hypothesis* (reprinted, New York, 1952), 214–15.

7 *Électricité et optique*, 1:ix–xiv.

8 H. A. Lorentz, 'La théorie électromagnétique de Maxwell et son application aux corps mouvants', *Archives Néerlandaises 25* (1892), §3; reprinted in H. A. Lorentz, *Collected papers*, 9 vols. (The Hague, 1934–9), 2:168.

9 H. A. Lorentz, two papers with the same title, 'On the electromagnetic theory of the reflection and refraction of light', the first an abstract of the second, in *Proceedings of the Royal Society* (1879), and *Philosophical Transactions of the Royal Society* (1880), respectively; G. F. Fitzgerald, *The scientific writings of the late George Francis Fitzgerald*, ed. Joseph Larmor (Dublin and London, 1902), 41–4, 45–73.

10 H. Helmholtz, 'Ueber die Theorie der Elektrodynamik: erste Abhandlung: ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper', *Journal für die Reine und Angewandte Mathematik 72* (1870); reprinted in H. Helmholtz, *Wissenschaftliche Abhandlungen von Hermann Helmholtz*, 2 vols. (Leipzig, 1882–3), 1:558–9, n. 1.

11 H. A. Lorentz, 'Over de Theorie der Terugkaatsing en Breking van het Licht', Leiden, 1875; reprinted in Lorentz, *Collected papers,* 1:1–192 (French translation, ibid., 193–383). A précis was published in German, in three parts: 'Ueber die Theorie der Reflexion und Refraction des Lichtes', *Zeitschrift für Mathematik und Physik 22* (1877), 1–30, 205–19, and *23* (1878), 197–210.

12 Cf. Larmor's statement to this effect in *Scientific writings of Fitzgerald,* xlii.

13 *Scientific writings of Fitzgerald,* 41–2.

14 *Transactions of the Royal Irish Academy 21* (1846); MacCullagh's paper is dated 9 Dec. 1839. (Several writers refer to this volume as issued in 1848; but the copy I have consulted in the library of Columbia University bears the date MDCCCXLVI.)

15 *Scientific writings of Fitzgerald,* 45–9.

16 J. Larmor, *Mathematical and physical papers,* 2 vols. (Cambridge, 1929), 1:415.

17 'MacCullagh, James (1809–1847)', *Encyclopaedia Britannica,* 11th ed. (1911), 17:207 'Aether', ibid., 1:296 (article signed by Sir James [*sic*] Larmor; through some inadvertence, MacCullagh's first initial is here given as 'T'.); 'Light', ibid., 16:622 (subsection 'Nature of Light' signed by Hendrik Antoon Lorentz).

18 W. Thomson, *Baltimore lectures on molecular dynamics and the wave theory of light* ('founded on Mr. A. S. Hathaway's stenographic report of twenty lectures delivered in Johns Hopkins University, Baltimore, in October 1884') (London, 1904), 12.

19 Ibid., for all quoted phrases in this paragraph.

20 G. F. Fitzgerald, 'On a model illustrating some properties of the ether', *Scientific Proceedings of the Royal Dublin Society* (read 19 Jan. 1885); reprinted in Fitzgerald, *Scientific writings of Fitzgerald,* 153.

21 G. F. Fitzgerald, 'The relations between ether and matter', *Nature* (19 July 1900); reprinted in Fitzgerald, *Scientific writings of Fitzgerald,* 508.

22 G. F. Fitzgerald, 'Sir W. Thomson and Maxwell's electromagnetic theory of light', *Nature* (7 May 1885); reprinted in Fitzgerald, *Scientific writings of Fitzgerald,* 170–3.

23 *Scientific writings of Fitzgerald,* 155–6.

24 Ibid., 154.

25 J. Larmor, *Aether and matter* (Cambridge, 1900), vi n.

26 *Scientific writings of Fitzgerald,* 162.

27 W. Thomson, 'Motion of a viscous liquid; equilibrium or motion of an elastic solid; equilibrium or motion of an ideal substance called for brevity *ether;* mechanical representation of magnetic force', in W. Thomson, *Mathematical and physical papers,* 6 vols. (Cambridge, 1882–1911), 3:436 ff.

28 Hertz, *Electric waves,* Kelvin's preface to the English ed., xiii.

29 Thomson, *Papers,* 3:490.

30 Ibid., 465.

31 For a very interesting discussion of alternative electromagnetic interpretations of a 'rotationally elastic ether' see O. Heaviside, *Electromagnetic theory,* 3 vols. (reissued, London, 1922), 1:243–56.

32 Ibid., 1:127–31, 243–56. See also O. Heaviside, *Electrical papers,* 2 vols. (London, 1892), 1:467.

33 A. Sommerfeld, 'Mechanische Darstellung der elektromagnetischen Erscheinungen in ruhenden Körpern', *Annalen der Physik und Chemie, n.s. 46* (1892), 139–51.

34 R. Reiff, *Elasticität und elektricität* (Freiburg, 1893).

35 J. Larmor, in the three-part series of papers 'A dynamical theory of the electric and luminiferous medium', *Philosophical Transactions of the Royal Society 185* (1894), 719–822, *186* (1895), 695–743, and *190* (1897), 205–300; reprinted, with amplifying commentary, in Larmor, *Papers,* 1:414–535, 543–97, and 2:11–132. Extensive abstracts of these papers, from the *Proceedings of the Royal Society,* are also given in Larmor's *Papers:* 1:389–413, 536–42, 625–39. See also Larmor, *Aether and matter.*

36 Larmor's style is not of the most transparent; in one of his reviews of *Aether and matter,* Fitzgerald comments that it is 'in many places so condensed and general in its language as to be very difficult to follow'. *Scientific writings,* 515. Helpful accounts are given by Witte (1906), 150 ff., and by Poincaré, 'A propos de la théorie de M. Larmor', *L'Eclairage Electrique 3* (1895), 5–13, 289–95, and *5* (1895), 5–14, 385–92; reprinted in H. Poincaré *Oeuvres de Henri Poincaré,* 11 vols. (Paris, 1936–56), 9:369–426; also reissued, slightly revised and with an addendum – most useful for our present concern – not included in the *Oeuvres,* in the 2nd ed. (Paris, 1901) of *Électricité et optique* (for the addendum, 'Forme définitive de la théorie de Larmor', see 627–32).

37 *Papers,* 1:536, historical fn. (For Larmor's first introduction of the term, see ibid., 514, 516.)

38 Ibid., 521–4. The assumption of 'no intrinsic inertia' appears on 522, the phrase 'a purely electric theory of matter' in a marginal note on 523, and the comparison with cathode rays on 524, n. 1.

39 *Scientific writings of Fitzgerald,* 513.

40 Reprinted in ibid., liii–liv.

41 The paper of Helmholtz cited in n. 10 and the sequels to that paper merit more attention than they seem to have received. Helpful accounts are given in Poincaré, *Électricité et optique,* vol. 2; and in Emil Wiechert, *Grundlagen der Elektrodynamik,* issued as pt. 2 of the *Festschrift zur Feier der Enthüllung des Gauss–Weber–Denkmals in Göttingen* (Leipzig, 1899), 64–70.

42 *Collected papers,* 1:382–3.

43 H. A. Lorentz, 'Concerning the relation between the velocity of propagation of light and the density and composition of media', in Lorentz, *Collected papers,* 2:1–119 (this is an English translation of the full original work: 'Ueber die Beziehung zwischen der Fortpflanzung des Lichtes und der Körperdichte', *Annalen der Physik und Chemie, n.s. 9* [1880], 641–65).

44 *Collected papers,* 2:79–80.

45 For the papers in question, see J. W. Gibbs, *The collected works of J. Willard Gibbs* 2 vols. (New York, 1928), 2:pt. 2, 182–252; references to Lorentz occur on 220, 221, 238, 252.

46 H. Helmholtz, 'Elektromagnetische Theorie der Farbenzerstreuung', *Sitzungsberichte der Berliner Akademie* (Dec. 1892), 1093–1109; reprinted in *Annalen der Physik und Chemie, n.s. 48* (1893), 389–405; and in Helmholtz, *Wissenschaftliche Abhandlungen,* vol. 3 (Leipzig, 1895), 505–25. Whittaker (1951), although he refers in passing to Lorentz's memoir of 1878 (392, n. 6), gives the erroneous impression that it was only in 1892 that a theory of dispersion was developed by Lorentz.

47 *Collected papers,* 2:5.

48 See ibid., 81, equations (19).

49 See Hertz, *Electric waves,* 241 ff. (note in particular Hertz's expression of scepticism towards his own working hypothesis, 242–3).

50 'La théorie électromagnétique de Maxwell et son application aux corps mouvants', beginning of chap. 4; reprinted in Lorentz, *Collected papers,* 2:228.

51 Ibid., §3; reprinted in Lorentz, *Collected papers,* 2:168.

52 Lorentz's fundamental hypotheses and derivations are to be found in ibid., §§75–80; reprinted in Lorentz, *Collected papers,* 2:230–8. The formula for the force on a moving charge was obtained by similar reasoning in a paper of Heaviside in 1889: see his *Electrical papers,* 2:505–6. To see in Lorentz's derivation a strong influence

of the distant-action theory of Clausius, as Whittaker (1951), 393 ff., esp. 395, does, appears rather farfetched.

53 Thomson, *Baltimore lectures*, 351 (where the paper of 1888 is quoted – this lecture having itself been 'written afresh 1902' – see 324).

54 Ibid., 468 n.

55 See W. Thomson, 'On the motions of ether produced by collisions of atoms or molecules, containing or not containing electrions' (1907), in Thomson, *Mathematical and physical papers*, 6:235–43.

56 The basic assumptions of this theory are stated in the rewritten lecture 19 (1903) of the *Baltimore lectures*, 411–14.

57 A. Korn, *Eine Theorie der Gravitation und der elektrischen Erscheinungen auf Grundlage der Hydrodynamik*, 2nd ed. (Berlin, 1898).

58 A. Korn, 'Mechanische Theorie des elektromagnetischen Feldes', a series of papers in the *Physikalische Zeitschrift 18, 19,* and *20* (1917–19).

59 Cf. Hertz, *Electric waves*, 17.

60 See *Encyclopaedia Britannica*, 11th ed., 18:286.

61 'A dynamical theory of the electric and luminiferous medium', pt. 3, §17; reprinted in Larmor, *Papers*, 2:42–4.

62 *Oeuvres*, 9:381–2.

63 Ibid., 409.

64 Cf., e.g., Heaviside, *Electromagnetic theory*, 1:108.

65 *Oeuvres*, 9:412.

66 *Treatise*, 2:440–1(arts. 792–3).

67 *Scientific writings of Fitzgerald*, 108–9.

68 *Papers*, 1:286.

69 A. Sommerfeld, *Atomic structure and spectral lines*, trans. H. L. Brose (New York, 1923), 304.

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