HISTORIES OF THE ELECTRON

The Birth of Microphysics

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The MIT Press
Cambridge, Massachusetts
London, England

This book was set in Bembo by Graphic Composition, Inc. in Athens, Georgia with QuarkXPress and printed and bound by in the United States of America

Library of Congress Cataloging-in-Publication Data
Histories of the electron : the birth of microphysics / edited by Jed Z. Buchwald and Andrew Warwick.
p. cm. — (Dibner Institute studies in the history of science and technology)
Includes bibliographical references and index.
ISBN 0-262-02494-2 (hc. : alk. paper)
1. Electrons—History. 2. Thomson, J.J. (Joseph John), Sir, 1856–1940.
I. Buchwald, Jed Z. II. Warwick, Andrew. III. Series.

QC793.5.E62 H57 2001
539.7´2112´09—dc21
00-046580

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THE ZEEMAN EFFECT AND THE DISCOVERY OF THE ELECTRON

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The discovery of the electron is usually attributed to J. J. Thomson and assigned a specific date and location. On this widely accepted view, the electron was discovered by Thomson in 1897, while he was experimenting on cathode rays at the Cavendish Laboratory.[1] This attribution is problematic, both from a philosophical and a historiographical point of view. On the philosophical side, it presupposes a realist perspective toward unobservable entities and requires a theory of scientific discovery that would support such a perspective. As far as I can tell, no such adequate theory has been developed. On the historiographical side, this attribution downplays several British and continental developments that were quite decisive for the gradual acceptance of the electron as a universal, subatomic constituent of matter. In this chapter I want to examine one of those developments, an experimental discovery (the magnetic splitting of spectral lines) by the Dutch physicist Pieter Zeeman, and its effect on the main electromagnetic theories of the time by H. A. Lorentz and Joseph Larmor. As I will show, Zeeman's discovery was crucial for the initial articulation of the concept of the electron within the theoretical framework provided by Lorentz and Larmor and played a very important role in convincing physicists of the reality of the electron. Furthermore, I will address the question of whether Zeeman should also be considered as a discoverer of the electron.

ON SCIENTIFIC DISCOVERY

Before proceeding to the historical reconstruction, some methodological remarks about scientific discovery are in order. To talk about the discovery of an unobservable entity, like the electron, it is necessary to specify some criteria as to what constitutes a discovery of this kind. Antirealist philosophers would deny the possibility of finding such criteria, since from their point of view one has to be agnostic with respect to the existence of unobservable entities.[2] Realist philosophers, on the other hand, would have to suggest what constitutes an adequate demonstration for the existence of such entities. A realist would have to propose certain epistemological criteria whose satisfaction would provide adequate grounds for believing in the existence of an unobservable entity. Then he could reconstruct the discovery episode in question by showing how an individual or a group managed to meet the required criteria.

It is evident that the adequacy of the proposed way for deciding when something qualifies as a genuine discovery depends on the adequacy of the epistemological criteria for what constitutes unobservable reality. Any difficulties that might plague the latter would cast doubt on the adequacy of the former. Although this approach can be, in principle, realized, no adequate proposal of the kind outlined has been made so far. That is, no epistemological criteria have been formulated whose satisfaction would amount to an existence-proof of an unobservable entity.

Thus, the historical reconstruction of discovery episodes appears to require a resolution of one of the most intricate debates in philosophy of science. Rather than trying to resolve this debate, there is another way to approach discovery episodes that avoids philosophical pitfalls. One should simply try to adopt the perspective of the relevant historical actors, without worrying whether that perspective can be justified philosophically.[3] On this approach, the discovery of an entity amounts to the formation of consensus within the scientific community about its existence. Given the realist connotations of the term "discovery," one might even avoid using it when writing the history of a concept denoting an unobservable entity. In undertaking such a task, one would show how the given entity was introduced into the scientific literature and would reconstruct the experimental and theoretical arguments that were given in favor of its existence. The next step would be to trace the developmental process that followed the introduction of that entity and gradually transformed the concept associated with it. The evolution of any such concept resembles a process of gradual construction that takes place in several stages and, thus, can be periodized.[4] A realist might want to label the first stage of that process "the stage of discovery," but this would make no difference whatsoever with respect to the adequacy of the historical reconstruction.[5]

The main advantage of this approach is that it enables the reconstruction of past scientific episodes without presupposing the resolution of pressing philosophical issues. Since the debate on scientific realism goes on and has proved, so far, inconclusive, it is preferable to avoid historical narratives based, explicitly or implicitly, on realist premises. The intricacies of that debate suggest that an agnostic perspective is best suited for reconstructing the "discovery" of unobservable entities.

What I have said so far relies on the distinction between observable and unobservable entities, since my suggestion to avoid the category of discovery concerns unobservables. On the other hand, I do not wish to imply that the discovery of observable entities and phenomena should be treated in a similar agnostic fashion. In this case the category of discovery might be retained. It might be possible to specify when, say, a new species has been discovered, without relying on the notion of consensus within the relevant scientific community.

The question that immediately arises is why one should adopt different stances in the two cases. For two reasons, I think. First, because the realism debate has focused on the existence of unobservable entities, with both sides sharing a belief in the existence of observable objects and phenomena. Second, because to talk about the discovery of an unobservable entity one has to face a difficulty that does not appear in the case of observables. The discovery of an observable entity might simply involve its direct observation and does not require that all, or even most, of the discoverer's beliefs about it are true. For example, to discover "that there is a person in the ditch, . . . not every belief about that person needs to be true or known to be true."[6] This is not the case, however, when it comes to unobservable entities where direct physical access is, in principle, unattainable. The lack of independent access to such an entity makes problematic the claim that the discoverer's beliefs about it need not be true. If most, or even some, of those beliefs are not true it is not evident that the "discovered" entity is the same with its contemporary counterpart. It has to be shown, for instance, that Thomson's "corpuscles," which were conceived as classical particles and structures in the ether, can be identified with contemporary "electrons," which are endowed with quantum numbers, wave-particle duality, indeterminate position-momentum, etc. This would require, among other things, a philosophical theory of the meaning of scientific terms that would enable one to establish the referential stability of a term, despite a change of its meaning. In the philosophical literature there have been such proposals, most notably by Hilary Putnam, which are applicable to terms denoting observable objects. It is not clear, however, how these proposals would handle terms with unobservable referents.[7] Once more, one sees that an attempt to retain the category of scientific discovery with respect to unobservables leads us to philosophical deep water that a historian would rather avoid.

Let us now turn to Zeeman's discovery, which not only provided evidence for the existence of the electron but also led to a specification of two of its properties, its charge to mass ratio and the sign of its charge.

ZEEMAN'S DISCOVERY[8]

Pieter Zeeman (1856–1943) began to study magnetooptical phenomena in 1890, as Lorentz's assistant at the University of Leiden. The first phenomenon he investigated was the Kerr effect—the rotation of the plane of polarization of light upon reflection from a magnetized substance. The investigation of this phenomenon was also the subject of his doctoral dissertation, which he completed in 1893, under the supervision of Kamerlingh Onnes.[9] In the course of that research he made an unsuccessful attempt to detect the influence of a magnetic field on the sodium spectrum.[10] Several years later, inspired by reading "Maxwell's sketch of Faraday's life" and finding out that "Faraday thought of the possibility of the above mentioned relation [between magnetism and light]," he thought that "it might yet be worthwhile to try the experiment again with the excellent auxiliaries of the spectroscopy of the present time."[11] This time the experiment turned out to be a success.[12]

Zeeman placed the flame of a Bunsen burner between the poles of an electromagnet and held a piece of asbestos impregnated with common salt in the flame. After turning on the electromagnet, the two D-lines of the sodium spectrum, which had been previously narrow and sharply defined, were clearly widened. In shutting off the current the lines returned to their former condition. Zeeman then replaced the Bunsen burner with a flame of light gas fed with oxygen and repeated the experiment. The spectral lines were again clearly broadened. Replacing the sodium by lithium he observed the same phenomena.

Zeeman was not convinced that the observed widening was due to the action of the magnetic field directly upon the emitted light. The effect could be caused by an increase of the radiating substance's density and temperature. As noted by Zeeman, a similar phenomenon had been reported by Pringsheim in 1892.[13] Since the magnet caused an alteration of the flame's shape, a subsequent change of the flame's temperature and density was also possible. To exclude this possibility, Zeeman tried another more complicated experiment. He put a porcelain tube horizontally between the poles of the electromagnet, with the tube's axis perpendicular to the direction of the magnetic field (figure 5.1). Two transparent caps were attached to each terminal of the tube and a piece of sodium was introduced into the tube. Simultaneously the tube's temperature was raised by the Bunsen burner. At the same time the light of an electric lamp was guided by a metallic mirror to traverse the entire tube.

In the next stage of the experiment the sodium, under the action of the Bunsen flame, began to gasify. The absorption spectrum was obtained by means

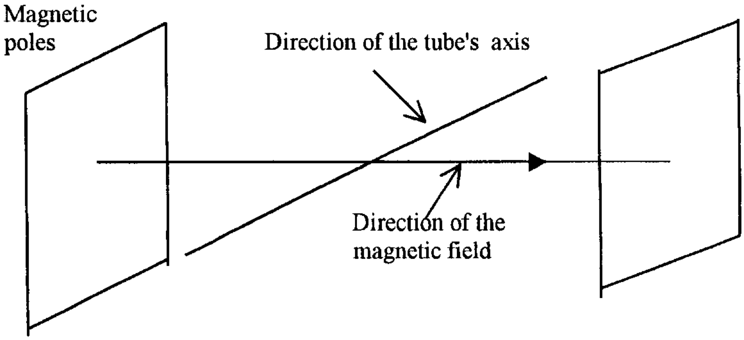

Figure 5.1

of the Rowland grating and finally the two sharp D-lines of sodium were observed. The heterogeneity of the density of the vapor at different heights of the tube produced a corresponding asymmetry in the lines' width, making them thicker at the top. By activating the electromagnet the lines became broader and darker. When it was turned off the lines recovered their initial form.

Zeeman, however, was still skeptical about whether the experiment's purpose, to demonstrate the direct effect of magnetism on light, had been accomplished. The temperature difference between the upper and lower parts of the tube was responsible for the heterogeneity of the vapor's density. The vapor was denser at the top of the tube and, since their width at a certain height depended on the number of incandescent particles at that height, the spectral lines were therefore thicker at the top. It was conceivable that the activation of the magnetic field could give rise to differences of pressure in the tube of the same order of magnitude and in the opposite direction to those produced by the differences of temperature. If this were the case, the action of magnetism would move the denser layers of vapor toward the bottom of the tube and would alter in this way the width of spectral lines without interacting directly with the light that generated the spectrum.

To exclude the possibility of these phenomena, which would undermine the experiment's aim, Zeeman performed a more refined experiment. He used a smaller tube and heated it with a blowpipe to eliminate disturbing temperature differences. Moreover, he rotated the tube around its axis and thus achieved equal densities of sodium vapor at all heights. The D-lines were now uniformly wide along their whole length. The subsequent activation of the electromagnet resulted in their uniform broadening.

Zeeman was by then nearly convinced that the outcome of his experiments was due to the influence of magnetism directly upon the light emitted or absorbed by sodium: "The different experiments . . . make it more and more probable, that the absorption—and hence also the emission—lines of an incandescent vapor, are widened by the action of magnetism."[14] The sentence immediately following is instructive with respect to the theoretical significance of Zeeman's experimentation: "Hence if this is really the case, then by the action of magnetism in addition to the free vibrations of the *atoms, which are the cause of the line spectrum,* other vibrations of changed period appear"[15] (emphasis added). It is evident that Zeeman identified the origin of spectral lines with the vibration of atoms. H. A. Lorentz, Zeeman's mentor and collaborator, had developed a theory of electromagnetic phenomena that accounted for the emission of light in this way. As the above excerpt indicates, Lorentz's theory could be used to provide a theoretical understanding of Zeeman's experimental discovery. As it turned out, that theory guided Zeeman's subsequent experimental researches and was, in turn, shaped by them. Let us examine more closely the state of Lorentz's theory at that time.

LORENTZ'S THEORY OF "IONS" AND ITS IMPACT ON ZEEMAN'S INVESTIGATIONS

In 1878 Lorentz had already suggested that the phenomenon of dispersion could be explained by assuming that molecules are composed of charged particles that may perform harmonic oscillations.[16] In 1892 he developed a unification of the continental and the British approaches to electrodynamics, which incorporated those particles. From the British approach he borrowed the notion that electromagnetic disturbances travel with the speed of light. That is, his theory was a field theory that dispensed with action-at-a-distance. From the continental approach he borrowed the conception of electric charges as ontologically distinct from the field. Whereas in Maxwell's theory charges were mere epiphenomena of the field, in Lorentz's theory they became the sources of the field.[17]

The aim of Lorentz's combined approach, in 1892, was to analyze electromagnetic phenomena in moving bodies. That analysis required a model of the interaction between matter and ether. The notion of "charged particles" provided him with a means of handling this problem.[18] The interaction in question could be understood if one reduced all "electrical phenomena to [. . . the] displacement of these particles."[19] The movement of a charged particle altered the state of the ether which, in turn, influenced the motion of other particles. Furthermore, macroscopic charges were "constituted by an excess of particles whose charges have a determined sign, [and] an electric current is a true stream of these corpuscles."[20] This proposal was similar to the familiar conception of the passage of electricity through electrolytic solutions and metals.

It is worth pointing out that in the last section of his 1892 paper Lorentz deduced a formula for the velocity of light in moving media that had been derived by Fresnel on the assumption that the ether was dragged by moving matter. Lorentz's derivation, however, discarded that assumption and capitalized on the influence of light on moving charged particles. The latter were forced to vibrate by the ethereal waves constituting light and gave rise to a complex interaction that produced the effect named after Fresnel. Lorentz's analysis enhanced considerably the credibility of his theory and facilitated the acceptance of his "charged particles" as real entities.[21]

In 1895 he explicitly associated those particles with the ions of electrolysis.[22] The transformation of "ions" to "electrons" took place as a result of Zeeman's experimental discovery, which after its initial stage was dominated by Lorentz's theory. To understand how this transformation took place it is necessary to examine Lorentz's theoretical analysis of Zeeman's initial results and its role in guiding further Zeeman's experimental research. The first form of that analysis is recorded in Zeeman's second paper on his celebrated discovery.[23] Zeeman initially thought that Lorentz's theory could provide an explanation of his experimental results. Thus, he asked Lorentz to provide a quantitative treatment of the influence of magnetism on light:

> Prof. Lorentz to whom I communicated these considerations, at once kindly informed me of the manner, in which according to his theory the motion of an ion in a magnetic field is to be calculated, and pointed out to me that, if the explanation following from his theory was true, the edges of the lines of the spectrum ought to be circularly polarized. The amount of widening might then be used to determine the ratio of charge and mass to be attributed in this theory to a particle giving out the vibrations of light.
>
> The above mentioned extremely remarkable conclusion of Prof. Lorentz relating to the state of polarization in the magnetically widened line, I have found to be fully confirmed by experiment.[24]

As I mentioned above, the emission of light, according to Lorentz, was a direct result of the vibrations of small electrically charged particles ("ions"), which exist in all material bodies. In the absence of a magnetic field an "ion" would oscillate about an equilibrium point under the action of an elastic force. The influence of a magnetic field would alter the mode of vibration of the "ion." Suppose that an "ion" is moving in the xy-plane under the action of a uniform magnetic field which is parallel to the z-axis. The equations of motion are:

$$m\frac{d^2x}{dt^2} = -k^2x + eH\frac{dy}{dt},$$

$$m\frac{d^2y}{dt^2} = -k^2y - eH\frac{dx}{dt},$$

where e and m are the charge and the mass of the "ion" respectively and H is the intensity of the magnetic field. The first term on the right side of the equations denotes the elastic force and the second term represents the force due to the magnetic field (the "Lorentz" force). Assuming that

$$x = ae^{st} \text{ and } y = \beta e^{st},$$

we get

$$ms^2a = -k^2a + eHs\beta,$$

$$ms^2\beta = -k^2\beta - eHsa.$$

In the absence of a magnetic field ($H = 0$), we can easily obtain the period of vibration of the ion:

$$s = i\frac{k}{\sqrt{m}} = i\frac{2\pi}{T} \Rightarrow T = \frac{2\pi\sqrt{m}}{k}.$$

When a magnetic field is present the period becomes

$$s \cong i\frac{k}{\sqrt{m}}\left(1 \pm \frac{eH}{2k\sqrt{m}}\right) \Rightarrow T' \cong \frac{2\pi\sqrt{m}}{k}\left(1 \pm \frac{eH}{2k\sqrt{m}}\right).$$

It follows that

$$\frac{T'-T}{T} = \frac{eH}{2k\sqrt{m}} = \frac{e}{m}\frac{HT}{4\pi}. \tag{1}$$

The physical implications of this analysis are as follows:[25] In the general case, the oscillation of the 'ion' has an arbitrary direction in space. In the absence of a magnetic field the motion of the 'ion' can be resolved into three components: a linear oscillation and two circular oscillations in a plane perpendicular to the first. All three oscillations have the same frequency, and the two circular ones have opposite directions. When a magnetic field is present, the oscillations along the direction of the field remain unaltered. But one of the circular components is accelerated, while the other is retarded. Thus, under the influence of magnetism the charged particle will yield three distinct frequencies. If the particle is observed along the direction of the field a doublet of lines will be seen. Each line represents circularly polarized light. If it is observed in a direction perpendicular to the field, a triplet of lines will be seen. The middle component represents plane-polarized light, its plane of polarization being parallel to the field. The two outer components also represent plane-polarized light, but their plane of polarization is perpendicular to the field.

All these theoretical expectations were subsequently confirmed by experiments designed specifically to detect them. In the same paper that contained Lorentz's analysis Zeeman confirmed that the polarization of the edges of the broadened lines followed the theoretical predictions. Lorentz considered the confirmation of his predictions as "direct proof for the existence of ions."[26] Furthermore, Zeeman estimated the order of magnitude of the ratio e/m. As we saw, the change in the period of vibration of an 'ion' due to the influence of a magnetic field depends on e/m (see equation 1 above). Thus, the widening of spectral lines, which is a reflection of the alteration in the mode of vibration of an 'ion,' is proportional to the 'ionic' charge to mass ratio. According to Zeeman's approximate measurements a magnetic field of 10000 Gauss produced a widening of the D-lines equal to 2.5 percent of their distance. From the observed widening of the spectral lines, Zeeman calculated (using equation 1) e/m, which turned out to be unexpectedly large (10^7 e.m.u.). As he recalled, when he announced the result of his calculation to Lorentz, the latter's response was: "That looks really bad; it does not agree at all with what is to be expected."[27]

It should be noted that this was the first estimate of the charge to mass ratio of the 'ions' that indicated that the 'ions' did not refer to the well-known ions of electrolysis, but corresponded instead to extremely minute subatomic particles. J. J. Thomson's measurement of the mass-to-charge ratio of the particles that constituted cathode rays was announced several months later and was in close agreement with Zeeman's result.[28] It is worth pointing out that the priority of Zeeman over Thomson was not always acknowledged. Oliver Lodge, for instance, claimed that Zeeman's results were obtained after Thomson's measurements.[29] Not surprisingly, Zeeman did not appreciate that remark. In a letter to Lodge, praising "your book on electrons" and thanking him for being "kind enough to send me a copy," he defended his priority over Thomson:

> May I make a remark concerning the history of the subject? On p. 112 of your book you mention that the small mass of the electron was deduced from the radiation phenomena in the magnetic field, the result "being in general conformity with J. J. Thomson's direct determination of the mass of an electron *some months previously*." I think, my determination of e/m

> being of order 10^7 has been previous to all others in this field. My paper appeared in the "Verslagen" of the Amsterdam Academy of October and November 1896. It was translated in the "Communications from the Leyden Laboratory" and then appeared in the Phil. Mag. for *March* 1897. Prof. Thomson's paper on cathode rays appeared in the Phil. Mag. for *October* 1897. [Emphasis in the original.][30]

Even though Zeeman neglected to mention that an early report of Thomson's measurements appeared in April 1897,[31] his complaint was justified. Thomson's supposed priority, however, continued to be promoted. In 1913, for instance, Norman Campbell erroneously suggested that Thomson's measurement of the charge to mass ratio of cathode ray particles preceded Zeeman's estimate of e/m.[32] Millikan also spread the same mistaken view.[33]

The splitting of lines was initially observed by Zeeman in 1897.[34] Instead of sodium he had used cadmium. Its indigo line was found to split into a doublet or triplet depending on whether the light was emitted in a direction parallel or perpendicular to the magnetic field. This stage of Zeeman's experimentation was dominated completely by the theoretical insight of Lorentz. Lorentz's theoretical anticipations led to new aspects of the novel phenomenon. The refinement of the experiment, however, soon led to theoretical advances. For instance, from the direction of polarization of the higher frequency component of the doublet Zeeman inferred that the charge of the 'ions' was negative.[35] Moreover, he gave a more accurate value of e/m and finally, by considering this unexpectedly large ratio, he was able to distinguish the 'ions' from the electrolytical ions.

As a result of Zeeman's discovery, the assumption that the radiating particles were as massive as hydrogen ions was abandoned and Lorentz's theory of ions was subsequently transformed into his theory of electrons. Zeeman's discovery had a similar effect on the transformation of the "ion's" British counterpart—the electron, as is testified to by Joseph Larmor's work.[36]

LARMOR'S "ELECTRON" AND ITS TRANSFORMATION BY ZEEMAN'S DISCOVERY

The name 'electron' was introduced by George Johnstone Stoney in 1891 to denote an elementary quantity of electricity.[37] At the Belfast meeting of the British Association in 1874 he had already suggested that "Nature presents us in the phenomenon of electrolysis, with a single definite quantity of electricity which is independent of the particular bodies acted on."[38] In 1891 he proposed that "it will be convenient to call [these elementary charges] *electrons*."[39] Stoney's electrons were permanently attached to atoms, that is, they could "not be removed from the atom," and each of them was "associated in the chemical atom with each bond." Furthermore, their oscillation within molecules gave rise to "electromagnetic stresses in the surrounding aether."[40]

In 1894 Stoney's electron was appropriated by Joseph Larmor, "at the suggestion of G. F. FitzGerald,"[41] to resolve a problem situation that had emerged in the context of the Maxwellian research tradition.[42] Larmor's adoption of the electron represented the culmination (and perhaps the abandonment) of that tradition. A central aspect of the research program initiated by Maxwell was that it avoided microscopic considerations altogether and focused instead on macroscopic variables (e.g., field intensities). This macroscopic approach ran into both conceptual and empirical problems. Its main conceptual shortcoming was that it proved unable to provide an understanding of electrical conduction. Its empirical defects were numerous: "It could not explain the low opacity of metal foils, or dispersion, or the partial dragging of light waves by moving media, or a number of puzzling magnetooptic effects."[43] It was in response to these problems that Larmor started to develop a theory whose aim was to explain the interaction between ether and matter.

The first stage in that development was completed with the publication of "A Dynamical Theory of the Electric and Luminiferous Medium. Part I" in August 1894.[44] Its initial version was submitted to the *Philosophical Transactions* on 15 November 1893 and was revised considerably in the months that preceded its publication under the critical guidance of FitzGerald. What is crucial here is that the published version concluded with a section, added on 13 August, titled "Introduction of Free Electrons."[45]

According to Larmor's representation of field processes, "the electric displacement in the medium is its absolute rotation . . . at the place, and the magnetic force is the velocity of its movement. . . ."[46] For a medium to be able to sustain electric displacement it must have rotational elasticity. In the original formulation of his theory conductors were conceived as regions in the ether with zero elasticity, since Larmor had "assumed that the electrostatic energy is null inside a conductor."[47] Conduction currents were regarded, in Maxwellian fashion, as mere epiphenomena of underlying field processes and were represented by the circulation of the magnetic field in the medium encompassing the conductor.

To explain electromagnetic induction, Larmor had to find a way in which a changing electric displacement would change that circulation. If conductors were totally inelastic, a changing displacement in their vicinity could not affect them.[48] Therefore, Larmor had to endow conductors with the following peculiar feature: they were supposed to contain elastic zones that were affected by displacement currents and were the vehicle of electromagnetic induction. This implied that in conductors the ether had to be ruptured, a consequence strongly disliked by Larmor. This problem could be circumvented, however, if one assumed that the process of conduction amounted to charge convection.[49] As he remarked,

> If you make up the world out of monads, electropositive and electronegative, you get rid of any need for such a barbarous makeshift as rupture of the aether . . . . A monad or an atom is what a geometer would call a "singular point" in my aether, i.e., it is a singularity naturally arising out of its constitution, and not something foreign to it from outside.[50]

There was another conceptual problem related to the phenomenon of electromagnetic induction. Larmor had initially appropriated William Thomson's conception of atoms as vortices in the ether, and he suggested that magnetism was due to closed currents within those atoms (already postulated by Ampère).[51] FitzGerald pointed out, however, that currents of this kind would not be affected by electromagnetic induction, since the ether could not get a hold on them. To solve this problem, Larmor suggested that the currents in question were unclosed. In connection with this issue FitzGerald sent a letter to Larmor which provided the inspiration for the introduction of the electron:[52]

> I don't see where you *require* a discrete structure except that you *say* that it is required in order to make the electric currents unclosed, yet I think that electrolytic and other phenomena prove that there is this discrete structure and you *do* require it, where you *don't* call attention to it, namely where you speak of a rotational strain near an atom. You *say* that electric currents are unclosed vortices but I can't see that this *necessitates* a *molecular* structure because in the matter the unclosedness might be a continuous peculiarity so far as I can see. That it is molecular is due to the molecular constitution of matter and not to any necessity in your theory of the ether.[53]

FitzGerald's point was that the discrete structure of electricity was an independently established fact that did not follow from Larmor's theory, but had to be added to it.

In a few months Larmor reconstructed his theory on the basis of FitzGerald's suggestion. Currents were now identified with the transfer of free charges ("monads"), which were also the cause of magnetic phenomena. Those charges had the ontological status of independent entities and ceased to be epiphenomena of the field. Furthermore, material atoms were represented as stable configurations of electrons. In Larmor's words,

> the core of the vortex ring [constituting an atom] . . . [is] made up of discrete electric nuclei or centres of radial twist in the medium. The circulation of these nuclei along the circuit of the core would constitute a vortex . . . its strength is now subject to variation owing to elastic action, so that the motion is no longer purely cyclic. A magnetic atom, constructed after this type, would behave like an ordinary electric current in a nondissipative circuit. It would, for instance, be subject to alteration of strength by induction when under the influence of other changing currents, and to recovery when that influence is removed.[54]

Thus, the problem that FitzGerald had brought up disappeared, since the ether could now get a hold on the core of the vortex ring and the atomic currents could be influenced by electromagnetic induction.

In July 1894 FitzGerald suggested the word "electron" to Larmor, as a substitute for the familiar "ion." In FitzGerald's words, Stoney "was rather horrified at calling these ionic charges 'ions.' He or somebody has called them 'electrons' and the ion is the atom not the electric charge."[55] This was the first hint of the need for a distinction between the entities introduced by Larmor and the well-known electrolytical ions. This distinction was obscured, however, by the fact that the effective mass of Larmor's electrons was of the same order of magnitude with the mass of the hydrogen ion. In this respect the subsequent discovery of the Zeeman effect was crucial, since it indicated that the electron's mass was three orders of magnitude smaller than the ionic mass (see below for details).

Larmor's "electrons" were conceived as permanent structures in the ether with the following characteristics:

> An electron has a vacuous core round which the radial twist is distributed. . . . It may be set in radial vibration, say pulsation, and this vibrational energy will be permanent, cannot possibly be radiated away. All electrons being alike have the same period: if the amplitudes and phases are also equal for all at any one instant, they must remain so . . . Thus an electron has the following properties, which are by their nature permanent
>
> (i) its strength [= electric charge]
> (ii) its amplitude of pulsation
> (iii) the phase of its pulsation.
>
> These are the same for all electrons. . . . The equality of (ii) and (iii) for all electrons may be part of the pre-established harmony which made them all alike at first,—or may, very possibly, be achieved in the lapse of aeons by the same kind of averaging as makes the equalities in the kinetic theory of gases.[56]

Furthermore, he suggested that they were universal constituents of matter. He had two arguments to that effect. First, spectroscopic observations in astronomy indicated that matter "is most probably always made up of the same limited number of elements."[57] This would receive a straightforward explanation if "the atoms of all the chemical elements [were] to be built up of combinations of a single type of primordial atom."[58] Second, the fact that the gravitational constant was the same in all interactions between the chemical elements indicated that "they have somehow a common underlying origin, and are not merely independent self-subsisting systems."[59]

Larmor's electronic theory of matter received strong support from experimental evidence. First, it could explain the Michelson-Morley experiment. Inspired by Lorentz, Larmor managed to derive the so-called FitzGerald contraction hypothesis, which had been put forward to accommodate the null result of that experiment.[60] As he mentioned in a letter to Lodge, "I have just found, developing a suggestion that I found in Lorentz, that if there is nothing else than electrons—i.e., pure singular points of simple definite type, the only one possible, in the aether—then movement of a body, *transparent* or *opaque,* through the aether *does actually* change its dimensions, just in such way as to verify Michelson's second order experiment."[61] Second, Fresnel had suggested that the ether was dragged by moving matter and had derived from this hypothesis a formula for the velocity of light in moving media. Larmor's theory was able to reproduce Fresnel's result: "The application [of electrons] to the optical properties of moving media leads to Fresnel's well known formula."[62]

The introduction of the electron initiated a revolution that resulted in the abandonment of central features of Maxwellian electrodynamics. Although in Larmor's theory, as in Maxwell's, the concept of charge was explicated in terms of the concept of the ether, there were significant differences between the two electromagnetic theories. In contrast to Maxwellian theory which did not attribute independent existence to charges, in Larmor's theory the electron acquired an independent reality. Furthermore, the macroscopic approach to electromagnetism was jettisoned and microphysics was launched. Conduction currents were represented as streams of electrons and dielectric polarization was attributed to the polarizing effect of an electric field on the constituents of molecules.

Larmor's "electron" was transformed as a result of Zeeman's discovery. Before that discovery, Larmor thought that a magnetic widening of spectral lines would be beyond experimental detection. The widening in question was proportional to the charge-to-mass ratio of the electron and, on the assumption that "electrons were of mass comparable to atoms," he was led to "the improbability of an observable effect."[63] Larmor's reaction to an announcement of Zeeman's discovery in *Nature*[64] shows that he immediately realized its far-reaching implications with respect to the characteristics of the electron. In a letter to Lodge, asking him to confirm Zeeman's results, he writes:

> There is an experiment of Zeeman's . . . which is fundamental + ought to be verified. . . . It demonstrates that a magnetic field can alter the free period of sodium vapor by a measurable amount. I have had the fact as I believe it is (on my views) before my mind for months . . . [but] it never occurred to me that it could be great enough to observe: and it needs a lot of proof that it is so.[65]

Several days later he was even more skeptical about the possibility of observing the effect: "I don't expect you will find the effect all the same. The only theory I have about it is that it must be extremely small."[66] Lodge managed to reproduce Zeeman's results and informed Larmor of his success several weeks after Larmor's initial request: "Did I tell you that I had verified Zeeman's result, to the extent of seeing the broadening of a Na line from a flame between magnetic poles. It is a *small* effect though."[67]

The implications of Zeeman's discovery were clear for Larmor:

> in an ideal simple molecule consisting of one positive and one negative electron revolving round each other, the inertia of the molecule would have to be considerably less than the chemical masses of ordinary molecules, in order to lead to an influence on the period, of the order observed by Dr. Zeeman.[68]

Furthermore, Zeeman's result and his subsequent estimate of e/m enabled Larmor and Lodge to determine a property of the electron that had been left unspecified in Larmor's theory, the electron's size. The value of e/m obtained by Zeeman together with the concept of electromagnetic mass made possible an estimate of the electron's size. The concept of electromagnetic mass was introduced by J. J. Thomson in 1881. A charged spherical body would possess, besides its material mass, an additional inertia due to its charge. The value of that inertia would depend on $\mu e^2/a$, where μ was the magnetic permeability of the ether and a the radius of the sphere.[69] Now assuming that the electron's mass was purely electromagnetic, one could calculate its size. Lodge performed the calculation and asked Larmor whether the result that he obtained was acceptable: "Zeeman's $e/m = 10^7$ means if $m = 2\mu e^2/3a$ that $a = 10^{-14}$ . . . is this too small for an electron?"[70]

Larmor's reply is very revealing with respect to the process that led to the construction of the concept of the electron:

> I don't profess to know à priori anything about the size or constitution of an electron except what the spectroscope may reveal. I do assert that a logical aether theory must drive you back on these electrons as the things whose mutual actions the aether transmits : but for that general purpose each of them is a point charge just as a planet is an attracting point in gravitational astronomy. But as regards their constitution am inclining to the view that an atom of 10^{-8} cm is a complicated sort of solar system of revolving electrons, so that the single electron is very much smaller, 10^{-14} would do very well—is in fact the sort of number I should have guessed.[71]

So, originally the concept of the electron was arrived in an a priori fashion, that is, as a solution to a theoretical problem. The remaining task was to construct its properties so as to accommodate the available empirical evidence. The size of the electron, for instance, was calculated by Lodge so as to "attain Zeeman's quantitative result."[72]

Larmor's detailed analysis of the Zeeman effect was completed by November 8, 1897.[73] Larmor considered "a single ion *e*, of effective mass M, describing an elliptic orbit under an attraction to a fixed centre proportional to the distance therefrom."[74] If a magnetic field was introduced, Larmor proved, by solving the corresponding equations of motion, that instead of the original frequency of vibration three distinct ones would appear: one of them would coincide with the original, whereas the other two would be shifted by an amount equal to $\pm eH/4\pi Mc^2$. A "striking feature" of Larmor's analysis was "that the modification thus produced is the same whatever be the orientation of the orbit with respect to the magnetic field."[75] This feature resulted from a general theorem that he had managed to prove a few weeks before he submitted his paper to the *Philosophical Magazine*. In his words,

> the following math prop is true:—Consider any system of (say) *negative* ions, with charges proportional to their effective masses, attracting each other according to some laws & attracted to fixed centres anywhere on the axes of the magnetic field: then their motion when the magnetic field is turned on relative to an observer fixed is the same as when it was off relative to an observer attached to a frame rotating round the axe of the field H with ang. velocity eH/Mc^2 where e/M is the constant charge/mass and *c* is the velocity of radiation.[76]

In this respect Larmor's analysis was superior to Lorentz's less general explanation of the results obtained by Zeeman. In other respects, such as the polarization of the emitted spectral lines, Larmor reached identical conclusions to those obtained by Lorentz (see above). Larmor's analysis, in conjunction with Zeeman's experiments, enabled the approximate estimate of *e*/M. As it turned out, "the effective mass of a revolving ion, supposed to have the full unitary charge or electron, is about 10^{-3} of the mass of the atom."[77]

As a result of Larmor's work and the support that it received by Zeeman's experiments, by 1898 the electron had become an essential ingredient of British scientific practice in the domain of electromagnetism.[78]

To summarize here, Zeeman's discovery was crucial with respect to the "discovery of the electron" in three respects. First, it provided direct empirical support for Lorentz's and Larmor's postulation of the ion-electron. As Zeeman remarked, it "furnishes, as it occurs to me, direct experimental evidence for the existence of electrified ponderable particles (electrons) in a flame."[79] Second, it led to an approximately correct value of a central property of the electron, namely its charge to mass ratio. The small value of that ratio indicated that Lorentz's "ions" were different from the ions of electrolysis and, thus, led to a revision of the taxonomy of the unobservable realm. Whereas before Zeeman's experiments the term "ions" denoted the ions of electrolysis as well as the entities producing electromagnetic phenomena, after those experiments the extension of the term was restricted to the ions of electrolysis. That is why Lorentz started using the expression "light-ions" to refer to the entities of his electromagnetic theory,[80] and later adopted the term "electrons."[81] Third, Zeeman's results in conjunction with Lorentz's analysis of optical dispersion led to an estimate of the light-ion's mass. In particular, using his equations for dispersion Lorentz expressed the light-ion's mass as a function of e/m. By substituting Zeeman's estimate of that ratio, he obtained a value of the mass in question that was approximately 350 times smaller than the mass of the hydrogen atom.[82]

The significant contributions of Zeeman, Lorentz, and Larmor to the acceptance of the electron as a subatomic constituent of matter might (mis)lead us to the opinion that they should be given credit for the "discovery" of the electron. In fact, some have adopted this view. As early as 1901, Walter Kaufmann suggested that the existence of the electron had been established by Zeeman's discovery.[83] More recently, according to "the opinion of Leiden physicists, as told to me by H. B. G. Casimir, . . . Lorentz was the "discoverer" of the electron."[84] This view is subject to all the historiographical and philosophical problems that I have pointed out elsewhere in connection with the attribution of the electron's discovery to J. J. Thomson.[85] To begin with, we have no adequate philosophical theory of scientific discovery that could be used to justify the attribution of the electron's discovery to Zeeman. Furthermore, and more importantly, from the point of view of the physics community at that time Zeeman's experimental discovery did not establish, beyond doubt, belief in the existence of electrons.[86]

It should be clear that the purpose of the preceding narrative was not to settle a priority question and suggest that it was Zeeman, as opposed to Thomson, who discovered the electron. On the contrary, this narrative in conjunction with narratives about Thomson can help us to reconsider the historiographical issues related to the "discovery of the electron." What these narratives tell depends on the philosophical perspective adopted with respect to scientific realism and scientific discovery. One thing is, however, clear. The electron was not discovered by any particular scientist. The concept of the electron was introduced in physics in the early 1890s and was gradually transformed as a result of various theoretical and experimental developments in the context of electromagnetic theory and in the study of the discharge of electricity in gases. Several physicists, theoreticians and experimentalists provided evidence that supported the electron hypothesis. The most that can be said about one of those, say Zeeman, is that his contribution to the acceptance of the electron hypothesis was significant.

ACKNOWLEDGMENTS

I wish to thank Jed Buchwald and Kostas Gavroglu for their helpful comments. In what appears here I have drawn on my previous publications: Arabatzis (1992, 1996). Excerpts from the Joseph Larmor letters are reproduced by kind permission of the President and Council of the Royal Society of London. I would also like to thank University College London Library for permission to quote from Oliver Lodge's correspondence.

NOTES

1. For an elaboration and criticism of this view see T. Arabatzis, "Rethinking the 'Discovery' of the Electron," *Studies in History and Philosophy of Modern Physics* 27 (1996): 405–435.

2. Cf. B. C. van Fraassen, *The Scientific Image* (New York: Oxford University Press, 1980).

3. This does not imply that no such philosophical justification is possible.

4. A similar approach, with respect to the construction of the concept of the electron, has been followed by O. Darrigol, "Aux Confins de l'Électrodynamique Maxwelliene: Ions et Électrons vers 1897," *Rev. Hist. Sci.* 51/1 (1998): 5–34.

5. Only in this weak sense can the term "discovery" be used with respect to unobservable entities. In its stronger form (i.e., as implying existence) two further conditions are required. First, the consensus with respect to the reality of the entity in question should be maintained to this very day. Second, one should propose some criteria that enable us to identify the original entity with its present counterpart; but more on this below.

6. P. Achinstein, "Who Really Discovered the Electron?" (this volume, 416). Nevertheless, *some* beliefs about the discovered entity have to be true. For example, in the case of the

discovery of a man in the ditch we have to know that what we have discovered is a person and not, say, a stone (or, in general, an I-know-not-what).

7. For a detailed argument, along with the relevant literature, see T. Arabatzis, *The Electron: A Biographical Sketch of a Theoretical Entity* (Princeton University: Ph.D. Dissertation, 1995).

8. For a more detailed account of Zeeman's path to his discovery see T. Arabatzis, "The Discovery of the Zeeman Effect: A Case Study of the Interplay Between Theory and Experiment," *Studies in History and Philosophy of Science* 23 (1992): 365–388. Cf. also J. B. Spencer, *An Historical Investigation of the Zeeman Effect (1896–1913)* (Ph.D. Dissertation, The University of Wisconsin, 1964). A more recent study, based on an examination of Zeeman's laboratory notebooks, is A. J. Kox, "The Discovery of the Electron: II. The Zeeman Effect," *European Journal of Physics* 18 (1997): 139–144.

9. See J. B. Spencer, "Zeeman, Pieter," in C. C. Gillispie (ed.), *Dictionary of Scientific Biography,* 16 vols. (New York: Charles Scribner's Sons, 1970–1980), vol. 14, 597–599.

10. See P. Zeeman, "On the Influence of Magnetism on the Nature of the Light Emitted by a Substance. (part I.)" *Communications from the Physical Laboratory at the University of Leiden* 33 (1896): 1–8, 1; translated from *Verslagen van de Afdeeling Natuurkunde der Kon. Akademie van Wetenschappen te Amsterdam,* 31 October 1896, 181. There is no unpublished record of this early attempt. See Kox, "The Discovery of the Electron," 139–140.

11. Zeeman, "On the Influence of Magnetism . . . (part I)," 3. Those "excellent auxiliaries" included a concave Rowland grating that had recently been acquired by the Physical Laboratory of the University of Leiden.

12. There is a record of this experiment in Zeeman's laboratory notebook, dated September 2, 1896. See Kox, "The Discovery of the Electron," 140. The description that follows is from Zeeman's published paper, which appeared on 31 October 1896. Kox's analysis of Zeeman's notebooks shows that Zeeman's publications gave a faithful account of his research.

13. See E. Pringsheim, "Kirchhoff'sches Gesetz und die Strahlung der Gase," *Wiedmannsche Annalen der Physik* 45 (1892): 428–459, 455–457.

14. Zeeman, "On the Influence of Magnetism . . . (part I)," 8.

15. Ibid.

16. See H. A. Lorentz, "Concerning the Relation Between the Velocity of Propagation of Light and the Density and Composition of Media," in P. Zeeman and A. D. Fokker (eds.), *H. A. Lorentz: Collected Papers,* 9 vols. (The Hague: Martinus Nijhoff, 1935–1939), vol. 2, 1–119.

17. H. A. Lorentz, "La Théorie Électromagnétique de Maxwell et son Application aux Corps Mouvants," in his *Collected Papers,* vol. 2, 164–343, esp. 229. Cf. R. McCormmach, "Einstein, Lorentz, and the Electron Theory," *Historical Studies in the Physical Sciences* 2 (1970): 41–87.

18. See T. Hirosige, "Origins of Lorentz' Theory of Electrons and the Concept of the Electromagnetic Field," *Historical Studies in the Physical Sciences* 1 (1969): 151–209,

178–179, 198; and N.J. Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Dordrecht: Martinus Nijhoff, 1984), 98.

19. "[L]es phénomènes électriques sont produits par le déplacement de ces particules." Lorentz, "La Théorie Électromagnétique de Maxwell," 228.

20. "[U]ne charge électrique est constituée par un excès de particules dont les charges ont un signe déterminé, un courant électrique est un véritable courant de ces corpuscules." Lorentz, "La Théorie Électromagnétique de Maxwell," 228–229. Cf. J. L. Heilbron, *A History of the Problem of Atomic Structure from the Discovery of the Electron to the Beginning of Quantum Mechanics* (University of California, Berkeley, Ph.D. dissertation, 1964), 98.

21. Cf. N.J. Nersessian, "Hendrik Antoon Lorentz," in *The Nobel Prize Winners: Physics* (Pasadena, CA: Salem Press, 1989), 35–42, esp. 39.

22. H. A. Lorentz, "Versuch einer Theorie der elektrischen und optischen Erscheinungen in bewegten Körpern," in his *Collected Papers,* vol. 5, 1–137, esp. 5.

23. See P. Zeeman, "On the Influence of Magnetism on the Nature of the Light Emitted by a Substance. (part II.)" *Communications from the Physical Laboratory at the University of Leiden* 33 (1896): 9–19; translated from *Verslagen van de Afdeeling Natuurkunde der Kon. Akademie van Wetenschappen te Amsterdam,* 28 November 1896, 242.

24. Ibid., 12.

25. Ibid., 16.

26. We know that from an entry in Zeeman's diary, dated 23 November 1896. See Kox, "The Discovery of the Electron," 142.

27. Cited in Zeeman, "Faraday's Researches on Magneto-Optics and their Development," *Nature* 128 (1931): 365–368, on 367. Even though Lorentz associated his 'ions' with the ions of electrolysis, he did not specify, to the best of my knowledge, their charge to mass ratio. Thus, there is no conclusive evidence that he assumed his 'ions' to be of an order of magnitude comparable with an atom's. Further indirect evidence is found in O. Lodge, "The History of Zeeman's Discovery and its Reception in England," *Nature* 109 (1922): 66–69, on 67. There we read that Larmor "*like everyone else at that time, . . .* considered that the radiating body must be an atom or part of an atom with an $e/m = 10^4$" (emphasis added). Moreover, Zeeman, while discussing various aspects of his discovery, remarked that "[t]he value found [for the charge to mass ratio of the 'ion'] is about 1500 times that of the corresponding value which can be derived for hydrogen from the phenomena of electrolysis. *This was something entirely new in 1896.*" (Emphasis added.) P. Zeeman, *Researches in Magneto-Optics: With Special Reference to the Magnetic Resolution of Spectrum Lines* (London: Macmillan, 1913), 39–40.

28. See J. J. Thomson, "Cathode Rays," *Proceedings of the Royal Institution* 15 (1897): 419–432.

29. O. Lodge, *Electrons, or the Nature and Properties of Negative Electricity* (London: George Bell and Sons, 1906), 112.

30. Zeeman to Lodge, 3 August 1907, University College Library, Lodge Collection, MS. Add 89/116.

31. Thomson, "Cathode Rays."

32. See N. R. Campbell, *Modern Electrical Theory,* 2nd edn (Cambridge: Cambridge University Press, 1913), 148–149.

33. See R. A. Millikan, *The Electron: Its Isolation and Measurement and the Determination of some of its Properties,* 2nd edn (Chicago: University of Chicago Press, 1924), 42–43.

34. P. Zeeman, "Doublets and Triplets in the Spectrum produced by External Magnetic Forces," *Philosophical Magazine,* 5th series, 44 (1897): 55–60, 255–259.

35. It should be noted that Zeeman initially reported that these polarization results led to the conclusion that the 'ions' were positively charged. See Zeeman, "On the Influence of Magnetism . . . (part II)," 18. He soon corrected his erroneous statement, however, in his following paper. See Zeeman, "Doublets and Triplets," 58. My attention was drawn to Zeeman's mistake by S. Endo and S. Saito, "Zeeman Effect and the Theory of Electron of H. A. Lorentz," *Japanese Studies in History of Science* 6 (1967): 1–18.

36. It is worth noting that Larmor acknowledged "Lorentz's priority about electrons which he introduced in 1892 very candidly." Larmor to Lodge, 7 February 1897, Univ. College London, Lodge Collection, MS. Add 89/65 (ii).

37. G. J. Stoney, "On the Cause of Double lines and of Equidistant Satellites in the Spectra of Gases," *The Scientific Transactions of the Royal Dublin Society,* 2nd series, 4 (1888–1892): 563–608, on 583. Cf. J. G. O'Hara, "George Johnstone Stoney, F.R.S., and the concept of the electron," *Notes and Records of the Royal Society of London* 29 (1975): 265–276; and J. G. O'Hara, "George Johnstone Stoney and the Conceptual Discovery of the Electron," *Stoney and the Electron–Papers from a Seminar held on November 20, 1991, to commemorate the centenary of the naming of the electron* (Dublin: Royal Dublin Society, 1993), 5–28.

38. Stoney's paper was first published in 1881. See G. J. Stoney, "On the Physical Units of Nature," *The Scientific Proceedings of the Royal Dublin Society,* new series, 3 (1881–1883): 51–60, on 54.

39. Stoney, "On the Cause of Double lines," 583.

40. Ibid.

41. J. Larmor, *Mathematical and Physical Papers,* 2 vols. (Cambridge: Cambridge University Press, 1929), vol. 1, 536. (Footnote added in the 1929 edition; not in the original paper.)

42. This tradition has been thoroughly studied by Jed Buchwald, Bruce Hunt, and Andrew Warwick. See J. Z. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago: University of Chicago Press, 1985); B. J. Hunt, *The Maxwellians* (Ithaca: Cornell University Press, 1991); and A. Warwick, 'On the Role of the FitzGerald-Lorentz Contraction Hypothesis in the Development of Joseph

Larmor's Electronic Theory of Matter', *Archive for History of Exact Sciences* 43:1 (1991): 29–91. For what follows, I am indebted to their analysis.

43. Hunt, *The Maxwellians,* 210.

44. J. Larmor, *Philosophical Transactions of the Royal Society,* vol. 185, 719–822; repr. in his *Math. and Phys. Papers,* vol. 1, 414–535.

45. Ibid., 514–535.

46. Ibid., 447.

47. Ibid., 448.

48. Ibid., 462.

49. Cf. Buchwald, *From Maxwell,* 161.

50. Larmor to Lodge, 30 April 1894, University College Library, Oliver Lodge Collection, MS. Add 89/65(i); also quoted in Buchwald, *From Maxwell,* 152–153.

51. See Larmor, *Math. and Phys. Papers,* vol. 1, 467. Cf. Hunt, *The Maxwellians,* 218.

52. Cf. Buchwald, *From Maxwell,* 163–164.

53. FitzGerald to Larmor, 30 March 1894, Royal Society Library, Joseph Larmor Collection, 448. This excerpt is also reproduced in Buchwald, *From Maxwell,* 166.

54. Larmor, *Math. and Phys. Papers,* vol. 1, 515.

55. FitzGerald to Larmor, 19 July 1894; quoted in Hunt, *The Maxwellians,* 220.

56. Larmor to Lodge, 29 May 1895, Univ. College London, Lodge Collection, MS. Add 89/65 (i). Larmor's suggestion of a pulsating electron appeared in print. See J. Larmor, "A Dynamical Theory of the Electric and Luminiferous Medium—Part III: Relations with Material Media," *Philosophical Transactions of the Royal Society,* Febr. 9, 1898, repr. in his *Math. and Phys. Papers,* vol. 2, 11–132, on 25.

57. Larmor, *Math. and Phys. Papers,* vol. 1, 475.

58. Ibid. Cf. O. Darrigol, "The Electron Theories of Larmor and Lorentz: A Comparative Study," *Historical Studies in the Physical and Biological Sciences* 24 (1994): 265–336, 312.

59. Ibid.

60. See Warwick, "On the Role of the FitzGerald-Lorentz Contraction."

61. Larmor to Lodge, 29 May 1895, Univ. College Library, Lodge Collection, MS. Add 89/65 (i). This excerpt from Larmor's letter is reproduced in Warwick, "On the Role of the FitzGerald-Lorentz Contraction," 56.

62. J. Larmor, "A Dynamical Theory of the Electric and Luminiferous Medium. Part II: Theory of Electrons," *Philosophical Transactions of the Royal Society of London A* 186 (1895): 695–743; repr. in his *Math. and Phys. Papers,* vol. 1, 543–597, the quote is from p. 544. Cf. Darrigol, "The electron theories of Larmor and Lorentz," 315–316.

63. Larmor, *Math. and Phys. Papers,* vol. 1, 622. (Footnote added in the 1929 edition; not in the original paper.) The accuracy of Larmor's retrospective remark is confirmed by contemporary evidence. See below.

64. *Nature* 55 (24 December 1896), 192; cf. N. Robotti and F. Pastorino, "Zeeman's Discovery and the Mass of the Electron," *Annals of Science* 55 (1998): 161–183, 172.

65. Larmor to Lodge, 28 December 1896, University College Library, Oliver Lodge Collection, MS. Add 89/65 (ii).

66. Larmor to Lodge, 6 January 1897, *ibid.*

67. Lodge to Larmor, 6 February 1897, Royal Society Library, Larmor Collection, 1244. The repetition of Zeeman's experiment was reported by Lodge on 11 February 1897 in the *Proceedings of the Royal Society.* See O. Lodge, "The Influence of a Magnetic Field on Radiation Frequency," *Proceedings of the Royal Society of London,* 60 (1897): 513–514.

68. J. Larmor, "The Influence of a Magnetic Field on Radiation Frequency," *Proceedings of the Royal Society of London,* 60 (1897): 514–515, on 515.

69. See J. J. Thomson, "On the Electric and Magnetic Effects produced by the Motion of Electrified Bodies," *Philosophical Magazine,* 5th series 11 (1881): 229–249, on 234.

70. Lodge to Larmor, 8 March 1897, Royal Society Library, Larmor Collection, 1247. Lodge's calculation was probably prompted by a letter that he had received from FitzGerald (FitzGerald to Lodge, 6 March 1897, Univ. College London, Lodge Collection, MS. Add 89/35 (iii)). This calculation appeared a few days later in *The Electrician.* See O. Lodge, "A few notes on Zeeman's discovery," *The Electrician,* 12 March 1897: 643–644, on 644.

71. Larmor to Lodge, 8 May 1897, Univ. College London, Lodge Collection, MS. Add 89/65 (ii). Well before he wrote this letter, Larmor had found out that Lodge "had verified Zeeman's result." Lodge to Larmor, 6 February 1897, Royal Society Library, Larmor Collection, 1244. Thus, he had no reason to doubt the validity of that result. Furthermore, he had realized early on its implications with respect to the magnitude of the electron's mass. Therefore, his allusion to Lodge's estimate of the size of the electron as "the sort of number I should have guessed" is not surprising and does not contradict my previous claim that, *prior to Zeeman's discovery,* Larmor had attributed to the electron a mass comparable to the mass of the hydrogen ion.

72. Lodge, "A Few Notes," 644.

73. J. Larmor, "On the Theory of the Magnetic Influence on Spectra; and on the Radiation from moving Ions," *Philosophical Magazine,* 5th series, 44 (1897): 503–512.

74. Ibid., 503.

75. Ibid., 504.

76. Larmor to Lodge, 12 October 1897, University College Library, Lodge Collection, MS. Add. 89/65 (ii).

77. Larmor, "On the Theory of the Magnetic Influence on Spectra," 506.

78. Cf. Buchwald, *From Maxwell,* 172; and Hunt, *The Maxwellians,* 220–221.

79. Zeeman to Lodge, 24 January 1897, University College Library, Lodge Collection, MS. Add. 89/116. In a subsequent letter he clarified his previous remark: "I have called electrons ponderable particles; I wished to express that they must possess inertia." Zeeman to Lodge, 28 January 1897, *ibid.*

80. See, for instance, H. A. Lorentz (1898), "Optical Phenomena Connected with the Charge and Mass of the Ions (I and II)," in his *Collected Papers,* vol. 3, 17–39, on 24.

81. He began using this term in 1899. See C. Jungnickel and R. McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein,* 2 vols. (Chicago: The University of Chicago Press, 1986), vol. 2, 233.

82. Lorentz, *Collected Papers,* vol. 3, 24–25. Cf. C. L. Maier, *The Role of Spectroscopy in the Acceptance of an Internally Structured Atom, 1860–1920* (University of Wisconsin, Ph.D. thesis, 1964), 298.

83. See Isobel Falconer, "Corpuscles to Electrons," (this volume, 82).

84. Nersessian, "'Why Wasn't Lorentz Einstein?,' An Examination of the Scientific Method of H. A. Lorentz," *Centaurus* 29 (1986): 205–242, 209. Cf. also A. Romer, "Zeeman's Discovery of the Electron," *American Journal of Physics* 16 (1948): 216–223.

85. See Arabatzis, "Rethinking the 'Discovery' of the Electron."

86. Ibid.