Pergamon

Stud. Hist. Phil. Mod. Phys., Vol. 27, No. 4, pp. 405–435, 1996

1355–2198/96 $15.00+0.00

Rethinking the 'Discovery' of the Electron

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1. Several Approaches to the Discovery of Unobservable Entities: A Taxonomy and Critique

In order to identify an event or a process as the discovery of an unobservable entity, one needs a criterion (or a set of criteria) that would enable one to say that such a discovery has taken place. Several possible stances to the problem of what constitutes a discovery can be adopted. The first two possibilities depend on the position that one favours *vis-à-vis* the debate on scientific realism, a salient aspect of which concerns the grounds that we have for believing in the reality of the unobservable entities postulated by science (electrons, protons, fields, atoms, etc.). First, one might favour an antirealist perspective, i.e. deny that we have any good reasons for believing in the existence of unobservable entities. From such a point of view discoveries of unobservables never take place. To quote from an eminent contemporary representative of this approach, 'scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable'.[1] This view of scientific activity is compatible with (but not necessary for) the approach to the issue of scientific discovery that I favour; but more on this below.

On the second stance, one might propose certain epistemological criteria whose satisfaction would provide adequate grounds for believing in the existence of a particular entity. From this point of view a discovery takes place when an individual or a group has managed to meet the required criteria. As an example consider Ian Hacking's proposal that a belief in the reality of an, in principle, unobservable entity is justified to the extent that the entity in question can be manipulated.[2] It follows then that an unobservable entity has been discovered only if a scientist has found a way to manipulate this entity. The adequacy of the proposed recipe for deciding when something qualifies as a

(*Received 9 October 1995; revised 3 September 1996*)
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[1]B. C. van Fraassen, *The Scientific Image* (New York: Oxford University Press, 1980), p. 5.
[2]See I. Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983), esp. pp. 262–266.

PII: S1355-2198(96)00019-6

genuine discovery depends on the adequacy of the epistemological criterion for what constitutes unobservable reality. Any difficulties that might plague the latter would cast doubt on the adequacy of the former. While, in principle, I do not find any difficulty with this approach to the issue of scientific discovery, this specific manifestation, i.e. Hacking's proposal, leaves much to be desired. The merits and limitations of Hacking's view *vis-à-vis* the 'discovery' of the electron will be discussed below.

If these two possibilities capture the philosopher's main stances towards the issue of scientific discovery, a third possibility captures the scientist's perspective. This perspective usually characterizes scientists who write retrospective accounts of scientific discoveries. The recipe in this case involves two steps. First, one identifies the most central aspects of the modern concept associated with the particular entity in question. Second, one looks at the historical record and tries to identify the scientist who first articulated those salient aspects and who, furthermore, provided an experimental demonstration of the validity of her conception. This person (or persons) would then qualify as the discoverer of the given entity. There is a serious difficulty, however, that undermines this approach. Any entity that forms part of the accepted ontology of contemporary science is endowed with several properties. The electron, for instance, has a given mass, a certain charge, an intrinsic magnetic disposition (spin), a dual nature (particle vs wave), and many other features. The question then arises how many properties must one have discovered in order to be granted the status of the discoverer of the entity in question. Until the proponents of this approach provide the necessary selection principle, a principle flexible enough to be applicable to a wide variety of discoveries, this approach will remain indefensible. I will discuss below the concrete manifestations of this approach as revealed in some of the historiography of the discovery of the electron.

A final approach—and the one I favour—takes as central to the account the perspectives of the relevant historical actors and tries to remain as agnostic as possible *vis-à-vis* the realism debate. The criterion that this approach recommends is the following: since it is the scientific community (or its most eminent representatives) which adjudicates discovery claims, an entity has been discovered only when consensus has been reached with respect to its reality. The main advantage of this criterion is that it enables the reconstruction of past scientific episodes without presupposing the resolution of pressing philosophical issues. For historical purposes, one does not have to decide whether the consensus reached by the scientific community is justifiable from a philosophical point of view. Furthermore, one need not worry whether the entity that was discovered (in the above weak sense) can be identified with its present counterpart.

This historicist approach is by no means novel and many, perhaps most, historians of science would subscribe to it. As a result of considerable discussion in science studies about 'scientific discovery' as a historiographical category,[3] it is widely accepted that scientific discoveries do not transcend 'the local practices of contemporary research communities'.[4] However, the scope of the approach outlined above is more restricted. Whereas the intricacies of the philosophical debate on scientific realism suggest that an agnostic perspective is best suited for reconstructing the 'discovery' of unobservable entities, it is not thereby implied that the discovery of observable entities and phenomena is also reducible to the local practices of particular scientific groups and should be treated in a similar agnostic fashion.

Furthermore, the approach advocated here does not run a risk associated with more sweeping historicist claims. A reconstruction of past scientific developments in terms of criteria borrowed by the historical actors themselves turns out to be problematic when the criteria in question are not universally shared.[5] If, for instance, the actors involved in an ontological debate, where the existence of an entity is contended, accept different 'existence proofs' then it is impossible to offer an actor-oriented reconstruction of that debate 'without making some actors' voices inaudible'.[6] This difficulty does not plague the approach I recommend, which regards consensus formation as an essential aspect of scientific discovery. No episode where controversy persists can be interpreted as constituting a discovery.

The following sections will provide concrete illustrations of the theoretical issues discussed so far and will attempt a revisionist account of the 'discovery' of the electron.

2. How Not to Talk About the Discovery of the Electron

One reads repeatedly in the historical and philosophical literature that the electron was discovered by J. J. Thomson in 1897 over the course of his experiments on cathode rays.[7] However, the information contained in some of

[3]The discussion started with Kuhn's seminal essays and continued with more recent, sociologically oriented treatments. Kuhn's views are expounded in *The Structure of Scientific Revolutions* 2nd edn (University of Chicago Press, 1970), pp. 52–65; and *The Essential Tension* (University of Chicago Press, 1977), pp. 165–177. The sociological perspective is developed in A. Brannigan, *The Social Basis of Scientific Discoveries* (New York: Cambridge University Press, 1981). See also S. Schaffer, 'Scientific Discoveries and the End of Natural Philosophy', *Social Studies of Science* **16** (1986), 387–420; and the literature cited therein.

[4]Schaffer, *op. cit.*, note 3. The quote is from the abstract.

[5]This difficulty has been pointed out by Steven Shapin. See his 'Discipline and Bounding: The History and Sociology of Science as seen through the Externalism–Internalism Debate', *History of Science* **30** (1992), 333–369, esp. pp. 353–354. Despite his warnings against an over-enthusiastic historicism, however, Shapin espouses a moderately historicist outlook.

[6]*Ibid.*, p. 353.

[7]See, for instance, P. Achinstein, *Particles and Waves* (New York: Oxford University Press, 1991), pp. 286–287, 299; M. Chayut, 'J. J. Thomson: The Discovery of the Electron and the Chemists', *Annals of Science* **48** (1991), 527–544, see p. 531; P. Galison, *How Experiments End* (University of Chicago Press, 1987), p. 22; R. Harré, *Great Scientific Experiments* (New York: Oxford University Press, 1983), pp. 157–165; J. Heilbron, *Historical Studies in the Theory of Atomic*

footnote continued on p. 408

that literature suggests that the extended process that led to the acceptance of the electron hypothesis was neither exclusively linked with J. J. Thomson nor confined to 1897.

In most of those works the discovery of the electron is credited to J. J. Thomson quite casually and no attempt is made to support this claim through a specific historiographical model of scientific discovery. A notable exception is Isobel Falconer, who recently argued that the attribution of the electron's discovery to J. J. Thomson can be justified if one adopts Hacking's criterion of what constitutes unobservable reality.[8] A discussion of her proposal will illuminate not only Thomson's achievement but will also reveal the limitations of Hacking's criterion as a historiographical tool.

Falconer challenged traditional interpretations of Thomson's discovery which portrayed

> this discovery [... as] the outcome of a concern with the nature of cathode rays which had occupied Thomson since 1881 and had shaped the course of his experiments during the period 1881–1897.[9]

Instead she argued that '[a]n examination of his work shows that he paid scant attention to cathode rays until late 1896'.[10] Furthermore,

> [t]he cathode ray experiments in 1897 were not the origin of the corpuscle [which has been re-named electron] hypothesis; instead they acted as a focus around which Thomson synthesized ideas he had previously developed.[11]

However, she did not deny a central presupposition of the traditional view, namely that the discovery of the electron was a temporally non-extended event which can be credited to a single individual. Even though the 'corpuscle *hypothesis*' did not originate with Thomson's experiments with cathode rays,

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Structure (New York: Arno Press, 1981), pp. 1, 14; E. N. Hiebert, 'The State of Physics at the Turn of the Century', in M. Bunge and W. R. Shea (eds), *Rutherford and Physics at the Turn of the Century* (New York: Dawson and Science History Publications, 1979), pp. 3–22, see p. 7; M. Jammer, *The Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill, 1966), p. 121; H. Kragh, 'Concept and Controversy: Jean Becquerel and the Positive Electron', *Centaurus* **32** (1989), 203–240, see p. 205; W. McGucken, *Nineteenth-Century Spectroscopy: Development of the Understanding of Spectra 1802–1897* (Baltimore: The Johns Hopkins Press, 1969), pp. xi, 209; A. I. Miller, 'Have Incommensurability and Causal Theory of Reference Anything to do with Actual Science?—Incommensurability, No; Causal Theory, Yes', *International Studies in the Philosophy of Science* **5**:**2** (1991), 97–108, see p. 102; A. Pais, *Niels Bohr's Times, In Physics, Philosophy, and Polity* (New York: Oxford University Press, 1991), pp. 105–106; M. Paty, 'The Scientific Reception of Relativity in France', in T. F. Glick (ed.), *The Comparative Reception of Relativity, Boston Studies in the Philosophy of Science* **103** (Dordrecht: Reidel, 1987), pp. 113–167, see p. 125; L. Pyenson, 'The Relativity Revolution in Germany', *ibid.*, pp. 59–111, see p. 71; A. N. Stranges, *Electrons and Valence: Development of the Theory, 1900–1925* (Texas A&M University Press, 1982), pp. xi, 32. This list is by no means exhaustive.

[8]See I. Falconer, 'Corpuscles, Electrons and Cathode Rays: J. J. Thomson and the "Discovery of the Electron" ', *British Journal for the History of Science* **20** (1987), 241–276.

[9]*Ibid.*, p. 241.

[10]*Ibid.*

[11]*Ibid.*, p. 255.

the *discovery* of corpuscles (i.e. the experimental demonstration of their existence) was the outcome of these experiments.

> Arriving at the theoretical concept of the electron was not much of a problem in 1897. Numerous such ideas were 'in the air'. What Thomson achieved was to demonstrate their validity experimentally. Regardless of his own commitments and intentions, it was Thomson who began to make the electron 'real' in Hacking's sense of the word [...] He pinpointed an experimental phenomenon in which electrons could be identified and methods by which they could be isolated, measured and manipulated. This was immensely significant for the development of the electron theory which hitherto had been an abstract mathematical hypothesis but now became an empirical reality.[12]

Falconer attempts to reduce the discovery process to the precise moment when experimental verification took place, thus equating discovery with the ability to isolate, measure and manipulate. However, there are several problems with her attribution of the discovery of the electron to J. J. Thomson. First, he was not the only one who provided an experimental demonstration of all those 'ideas [...] "in the air" '. Several months before Thomson, Pieter Zeeman had done the same *vis-à-vis* Lorentz's theory of 'ions' and Larmor's theory of 'electrons'.[13] Second, in what sense did Thomson pinpoint 'an experimental phenomenon in which electrons could be *identified* and methods by which they could be *isolated, measured* and *manipulated*'? (emphasis added) The actual isolation of the electron was accomplished several years later by Millikan and even then there were grave doubts that the electron had, in fact, been isolated.[14] Moreover, Thomson's measurement of the electron was, by no means, his exclusive achievement. Zeeman, several months before Thomson, had made an estimate of e/m (the charge to mass ratio of an 'ion') and E. Wiechert, as well as W. Kaufmann, had also measured e/m, independently of and simultaneously with Thomson. Finally, Thomson was not the only one who could manipulate electrons. All those who experimented with cathode rays were able to manipulate them in various ways. For example, they could deflect them by means of magnetic fields. That is, from our perspective, given that they manipulated cathode rays and that cathode rays are streams of electrons, it follows that they manipulated electrons. And this brings me back to Hacking's criterion of what constitutes unobservable reality.

It is evident that Falconer has employed Hacking's criterion as a means for justifying attributing the discovery of the electron to J. J. Thomson. The validity of this attribution depends, therefore, on whether the manipulation of unobservable entities in the laboratory provides sufficient grounds for believing

[12] *Ibid.*, p. 276.

[13] These theories, along with Zeeman's work, will be discussed below.

[14] See G. Holton, 'Subelectrons, Presuppositions and the Millikan–Ehrenhaft Dispute', in his *Scientific Imagination: Case Studies* (Cambridge University Press, 1978), pp. 25–83. Cf. A. Franklin, 'Millikan's Published and Unpublished Data on Oil Drops', *Historical Studies in the Physical Sciences* **11** (1981), 185–201.

in their existence. To see the limitations of Hacking's criterion consider Thomson's experiments with cathode rays. Since one could describe these experiments in terms of cathode rays as opposed to electrons, the act of manipulation could be described without even mentioning the entities that, according to present-day physics, were manipulated. Moreover, an antirealist could give an even less theory-laden description, by avoiding the term 'cathode rays' and using instead the phenomenological expression 'spot on a phosphorescent screen'. The only thing that we know, the antirealist would argue, is that by activating an electromagnet Thomson could move a spot on a phosphorescent screen. Since an act of manipulation can be described without mentioning the unobservable entity that is (supposedly) manipulated, this act does not, by itself, imply the existence of the entity in question. Thus, given that experiments can be (re)described in phenomenological terms, manipulability cannot be employed, to the satisfaction of an antirealist, for existential inferences. Whereas for Hacking manipulability justifies existence claims, for the antirealist it is the other way around: it is the belief in the existence of, e.g. electrons, prior to the act of manipulation, that allows us to interpret that act as a manipulation of electrons (as opposed to something else).[15]

Since Hacking's criterion does not provide adequate grounds for a realist position *vis-à-vis* unobservable entities, it cannot be employed to justify discovery claims. Thus, Falconer's claim that the discovery of the electron was Thomson's exclusive experimental achievement is seriously undermined. In other respects, however, her article is an excellent reconstruction of Thomson's theoretical and experimental contribution to the process that culminated to the consolidation of the belief that 'electrons' denote real entities. In reconstructing that process below I will draw on her analysis.

Abraham Pais has also argued that 'Thomson should be considered the sole discoverer of the first particle [the electron]'.[16] His approach exemplifies what I called in the previous section the scientist's perspective on the issue of scientific discovery. A discussion of his claim, which he backs by a considerable amount of historical information, reveals in a concrete manner the limitations of this approach.

Even though he attributes the discovery of the electron to J. J. Thomson, he disputes the traditional claim that this discovery took place in 1897:

> It is true that in that year Thomson made a good determination of e/m for cathode rays, an indispensable step toward the identification of the electron, but he was not the only one to do so. Simultaneously Walter Kaufmann had obtained the same result. It is also true that in 1897 Thomson, less restrained than Zeeman, Lorentz, and (as we shall see) Kaufmann, correctly conjectured that the large value for e/m he had

[15]This is just one problematic aspect of Hacking's entity realism. For further criticism of his view see T. Arabatzis, *The Electron: A Biographical Sketch of a Theoretical Entity* (Princeton University, Ph.D. Thesis, 1995).

[16]A. Pais, *Inward Bound* (New York: Oxford University Press, 1988), p. 78.

> measured indicated the existence of a new particle with a very small mass on the atomic scale. However, he was not the first to make that guess. Earlier in that year, Emil Wiechert had done likewise, on sound experimental grounds, even before Thomson and Kaufmann had reported their respective results. Nevertheless, it is true that Thomson should be considered the sole discoverer of the first particle, since he was the first to measure not only *e/m* but also (within 50 per cent of the correct answer) the value of *e*, thereby eliminating all conjectural elements—but that was in 1899.[17]

According to Pais, then, the most central features of the modern concept of the electron are the values of its charge and its mass. An important step in measuring these two values was the measurement of the charge to mass ratio, achieved independently by Zeeman, Kaufmann, Thomson and Wiechert. This step, according to Pais, was not sufficient for establishing the existence of the electron. One had also to conjecture that the unexpectedly large value of *e/m* was due to the very small mass of a new sub-atomic particle and, more importantly, to confirm experimentally that conjecture. Even though Wiechert had also made that conjecture, Thomson was the only one who both made it and demonstrated it experimentally. By measuring the charge of the electron in 1899 he was able to deduce from that measurement and the already known charge to mass ratio the mass of the electron.

As I have argued in the previous section, there is a serious difficulty that undermines Pais's approach. The charge and mass of the electron are only two of the several properties associated with the electron of present-day physics. It is not at all clear why the theoretical and experimental detection of just two of these properties constitutes the discovery of the electron. On the one hand, one could claim that the measurement of the charge to mass ratio was by itself the key aspect of that discovery. Stuart Feffer, for instance, has suggested that '[t]he finding that this ratio was much larger than anticipated was the key experimental ingredient in the discovery of the electron'.[18] On the other hand, one could argue that Thomson's measurement of the electron's charge and mass is not sufficient for earning him the status of the 'discoverer of the electron'. On such an account, one should also have detected experimentally, say, the intrinsic magnetic disposition of the electron (spin) in order to be considered its discoverer.

Besides this methodological difficulty, there are further historiographical and philosophical problems that undermine Pais' claim. To begin with, it is not true that Thomson's measurement of the charge of the electron eliminated 'all

[17] *Ibid.* Some historians have also advocated a similar view. Barbara Turpin, for instance, has suggested that 'it would be more correct to set the date of the discovery of the electron as 1899 rather than 1897, the date most scholars have adopted'. See B. M. Turpin, *The Discovery of the Electron: The Evolution of a Scientific Concept, 1800–1899* (University of Notre Dame, Ph.D. Thesis, 1980), p. 202.

[18] S. M. Feffer, 'Arthur Schuster, J. J. Thomson, and the Discovery of the Electron', *Historical Studies in the Physical and Biological Sciences* **20** (1989), 33–61, on p. 33.

conjectural elements'. If that measurement had led the scientific community to a consensus on the reality of electrons, it would have simultaneously resolved the atomism debate. Someone who believes in the existence of sub-atomic constituents of all atoms is forced to believe in the reality of atoms. The atomism debate, however, remained open until the early 1910s;[19] a fact that clearly contradicts the view that Thomson had already proved beyond doubt in 1899 the existence of electrons. Furthermore, there is specific evidence that Thomson's measurements had not convinced everybody of the reality of electrons. Max Planck, for instance, 'confessed that as late as 1900 [one year after Thomson's "discovery" of the electron] he did not fully believe in the electron *hypothesis*' (emphasis added).[20]

Thus, from the perspective of the historical actors Thomson's measurements did not eliminate 'all conjectural elements'. However, one might want to disregard that perspective and argue that any doubts *vis-à-vis* the existence of electrons were unwarranted after Thomson had performed his measurements. From a philosophical point of view, I do not see how such an ahistorical judgement could be justified. Even Hacking, who is one of the most eminent advocates for entity realism, has admitted that '[o]nce upon a time it made good sense to doubt that there are electrons. *Even after Thomson had measured the mass of his corpuscles, and Millikan their charge, doubt could have made sense*' (emphasis added).[21]

Having argued against two main stances towards the 'discovery of the electron', it is time to propose my own account of that episode, an account that embodies the historiographical approach that I sketched above.

3. The Birth of the Electron

The name 'electron' was introduced by George Johnstone Stoney in 1891 to denote an elementary quantity of electricity.[22] At the Belfast meeting of the British Association in 1874 Stoney 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'.[23] In 1891 he proposed that 'it will be convenient to call [these elementary charges] *electrons*'.[24] Stoney's electrons were permanently attached to atoms, i.e. they could 'not be removed from the atom', and each of them was 'associated in the

[19]See M. J. Nye, *Molecular Reality* (New York: Elsevier, 1972).

[20]Holton, *The Scientific Imagination, op. cit.*, note 14, p. 42.

[21]*Op. cit.*, note 2, p. 271.

[22]See 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, see p. 583.

[23]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 p. 54.

[24]*Op. cit.*, note 22, p. 583.

chemical atom with each bond'. Furthermore, their oscillation within molecules gave rise to 'electro-magnetic stresses in the surrounding aether'.[25]

Even though Stoney coined the term 'electron', key aspects of the concept associated with that term, most notably the notion of the atomicity of charge, preceded considerably his proposal. In the period between 1838 and 1851 a British natural philosopher, Richard Laming, conjectured 'the existence of sub-atomic, unit-charged particles and pictured the atom as made up of a material core surrounded by an "electrosphere" of concentric shells of electrical particles'.[26] On the continent several physicists had made similar suggestions. Those physicists attempted to explain electromagnetic phenomena by action-at-a-distance forces between electrical particles. As an example of the continental approach to electrodynamics consider Wilhelm Weber's electrical theory of matter and ether.[27] Weber's theory originated in 1846 and continued to evolve until the time of his death (1891). According to the initial version of that theory, electricity consisted of two electrical fluids (positive and negative). The interactions of these fluids were governed by inverse square forces which were functions of their relative velocity and their relative acceleration. In the 1870s he attempted to construct an electrical theory of matter, based on discrete electrical particles, suggesting

> that each of the identical ponderable atoms combining to form chemical elements was a neutral system consisting of a negative, but highly massive, central particle with a positive satellite of much smaller mass.[28]

Thus, Weber's 'electrical particles' shared some aspects in common with the subsequently introduced 'electrons': both were particles of electricity and sub-atomic constituents of all atoms.

Another central aspect of the modern concept of the electron, the notion that electricity (like matter) has an atomic structure, emerged as a result of Faraday's experiments on electrolysis and his enunciation of the 'second law of electrochemistry'.[29] This law stated that when the same quantity of electricity passed through different electrolytic solutions the amounts of the decomposed substances were proportional to their chemical equivalents. What Faraday meant by 'chemical equivalents' is revealed by the following passage from his *Experimental Researches in Electricity*:

[25] *Ibid.*

[26] Kragh, 'Concept and Controversy', *op. cit.*, note 7, p. 205.

[27] For an extended discussion of Weber's programme see M. N. Wise, 'German Concepts of Force, Energy, and the Electromagnetic Ether: 1845-1880', in G. N. Cantor and M. J. S. Hodge (eds), *Conceptions of Ether: Studies in the History of Ether Theories 1740–1900* (Cambridge: Cambridge University Press, 1981), pp. 269–307, esp. pp. 276–283.

[28] *Ibid.*, p. 282. Cf. R. McCormmach, 'H. A. Lorentz and the Electromagnetic View of Nature', *Isis* **61** (1970), 459–497, p. 472.

[29] See L. P. Williams, *Michael Faraday: A Biography* (New York: Da Capo Press, 1987), pp. 256–257.

[T]he equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which *determines* the equivalent number, *because* it determines the combining force. Or if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom*; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature [...][30]

For someone like Faraday who was 'jealous of the term *atom*' the regularities in the phenomena of electrolysis need not have implied the atomic character of electricity. The inference from 'Faraday's law' to the atomicity of charge required a prior conviction in the atomic nature of matter and was clearly drawn by Helmholtz in 1881. In his Faraday Lecture to the Fellows of the Chemical Society Helmholtz argued that

the most startling result of Faraday's law is perhaps this. If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity.[31]

As I mentioned above, Helmholtz's conclusion had already been drawn by Stoney in 1874. Its significance is shown by the fact that Stoney was eager to reaffirm his priority over Helmholtz when he discovered that it was (inadvertently) challenged. In a letter to the editors of the *Philosophical Magazine* on 4 September 1894 he disputed a claim that had appeared in the same month's issue, according to which

[v]on Helmholtz, on the basis of Faraday's Law of Electrolysis, *was the first* to show in the case of electrolytes that each valency must be considered charged with a minimum quantity of electricity, [...] which like an electrical atom is no longer divisible.[32]

In rebutting this claim Stoney emphasized that

I had already twice pointed out this remarkable fact: first, at the Belfast meeting of the British Association in August 1874, in a paper 'On the Physical Units of Nature', in which I called attention to this minimum quantity of electricity as one of three [...] physical units, the absolute amounts of which are furnished to us by Nature [...] This same paper was again read before the Royal Dublin Society on the 16th of February, 1881, and is printed both in the Proceedings of that meeting and in the Phil. Mag. of the following May [...] In this paper an estimate was made of the actual amount of this

[30]M. Faraday, *Experimental Researches in Electricity*, 3 vols (London, 1839–1855), pars 868–869; quoted in Williams, *Michael Faraday, op. cit.*, note 29, p. 257.

[31]H. von Helmholtz, 'On the Modern Development of Faraday's Conception of Electricity', *Journal of the Chemical Society* **39** (1881), 277–304, on p. 290.

[32]Quoted in G. J. Stoney, 'Of the "Electron", or Atom of Electricity', *Philosophical Magazine*, 5th series **38** (1894), 418–420, on p. 418.

most remarkable fundamental unit of electricity, for which I have since ventured the name *electron*.[33]

Even though Stoney's reaffirmation of his priority over Helmholtz betrays the importance of the proposal of the atomicity of electricity, it lacks, like most questions of priority, any further historiographical significance. What is important is that this proposal and its reformulation by Stoney in terms of the electron hypothesis played a significant role in a different context, that of electromagnetic theory.

4. Larmor's 'electron'

In 1894 Stoney's electron was appropriated by Joseph Larmor, 'at the suggestion of G. F. Fitzgerald',[34] to resolve a problem situation that had emerged in the context of the Maxwellian research tradition.[35] Larmor's adoption of the electron represented the culmination (and perhaps the abandonment) of that tradition. A central aspect of the research programme 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 magneto-optic effects'.[36] 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.[37] 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 for my purposes is that the published version concluded with a section, added on 13 August 1894, titled 'Introduction of Free Electrons'.[38]

[33]*Ibid.*, pp. 418–419.

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

[35]This tradition has been thoroughly studied by Jed Buchwald and Bruce Hunt. 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); and B. J. Hunt, *The Maxwellians* (Ithaca: Cornell University Press, 1991). For what follows, I am indebted to their analysis.

[36]Hunt, *The Maxwellians, op. cit.*, note 35, p. 210.

[37]J. Larmor, *Philosophical Transactions of the Royal Society* **185**, 719–822; repr. in his *Mathematical and Physical Papers, op. cit.*, note 34, Vol. 1, pp. 414–535.

[38]*Ibid.*, pp. 514–535.

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 [...]'[39] In order 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'.[40] 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.[41] 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.[42] 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.[43]

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 suggested that magnetism was due to closed currents within those atoms (already postulated by Ampère).[44] 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: [45]

[39] *Ibid.*, p. 447.
[40] *Ibid.*, p. 448.
[41] *Ibid.*, p. 462.
[42] Cf. Buchwald, *From Maxwell, op. cit.*, note 35, p. 161.
[43] Larmor to Lodge, 30 April 1894, University College Library, Oliver Lodge Collection, MS. Add 89/65(i); also quoted in Buchwald, *From Maxwell, op. cit.*, note 35, pp. 152–153.
[44] See Larmor, *Mathematical and Physical Papers, op. cit.*, note 34, Vol. 1, p. 467. Cf. Hunt, *The Maxwellians, op. cit.*, note 35, p. 218.
[45] Cf. Buchwald, *From Maxwell, op. cit.*, note 35, pp. 163–164.

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.[46]

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 non-dissipative 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.[47]

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'.[48] 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. His initial reaction to Zeeman's discovery of a magnetic widening of spectral lines testifies to that fact. Before that discovery, Larmor thought that such an effect would be beyond experimental detection. The widening in question was proportional to the

[46]Fitzgerald to Larmor, 30 March 1894, Royal Society Library, Joseph Larmor Collection, 448. This excerpt is also reproduced in Buchwald, *From Maxwell, op. cit.*, note 35, p. 166, with a couple of minor mistakes.

[47]Larmor, *Mathematical and Physical Papers, op. cit.*, note 34, Vol. 1, p. 515.

[48]Fitzgerald to Larmor, 19 July 1894; quoted in Hunt, *The Maxwellians, op. cit.*, note 35, p. 220.

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'.[49] 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.[50]

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'.[51] 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'.[52] 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'.[53]

[49]Larmor, *Mathematical and Physical Papers*, *op. cit.*, note 34, Vol. 1, p. 622. (Footnote added in the 1929 edition; not in the original paper.) The accuracy of Larmor's retrospective remark is confirmed by contemporary evidence. As we read in a letter that he sent to Lodge, '[t]here 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 vapour 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'. Larmor to Lodge, 28 December 1896, University College Library, Oliver Lodge Collection, MS. Add 89/65 (ii). Several days later Larmor 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'. Larmor to Lodge, 6 January 1897, *ibid.*

[50]Larmor to Lodge, 29 May 1895, University College London, Lodge Collection, MS. Add 89/65 (i). To the best of my knowledge, Larmor's suggestion of a pulsating electron was not published. Moreover, his use of terms like 'monad' and 'pre-established harmony' indicates a Leibnizian element in his thought that has not been explored.

[51]Larmor, *Mathematical and Physical Papers*, *op. cit.*, note 34, Vol. 1, p. 475.

[52]*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, p. 312.

[53]*Ibid.*

Other characteristics of the electron were initially left unspecified in Larmor's theory. For instance, nothing was said about its size. This parameter would be determined when it was deemed relevant to the accommodation of new experimental results. An opportunity for this determination arose when Zeeman translated the magnetic widening of spectral lines to a value of the charge to mass ratio of the electron.[54] That value together with the concept of electromagnetic mass enabled one to estimate 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.[55] 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?'[56]

Larmor's reply is very revealing *vis-à-vis* the process that led to the construction of the electron's representation.

> 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.[57]

So, originally the concept of the electron was arrived in an *a priori* fashion, i.e. as a solution to a theoretical problem. The remaining task was to construct its properties so as to accommodate the available empirical evidence. The resulting representation had also to be coherent. For instance, the specification of, say, the electron's size from spectroscopic evidence had to be compatible with other indications about that size from the domain of atomic structure.

[54]See my '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.

[55]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 p. 234.

[56]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, University College London, Lodge Collection, MS. Add 89/35 (iii)).

[57]Larmor to Lodge, 8 May 1897, University 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 above claim that, *prior to Zeeman's discovery*, Larmor had attributed to the electron a mass comparable to the mass of the hydrogen ion.

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.[58] 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'.[59] 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'.[60]

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. As a result of Larmor's work and the support that it received by Zeeman's and Thomson's experiments, by 1898 the electron had become an essential ingredient of British scientific practice in the domain of electromagnetism.[61] This had already long been the case with the electron's continental counterpart—the ion, as the example of H. A. Lorentz testifies.[62]

[58]See 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.

[59]Larmor to Lodge, 29 May 1895, University 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', *op. cit.*, note 58, p. 56, with a few minor mistakes.

[60]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 *Mathematical and Physical Papers, op. cit.*, note 34, Vol. 1, pp. 543–597, the quote is from p. 544. Cf. Darrigol, 'The Electron Theories of Larmor and Lorentz', *op. cit.*, note 52, pp. 315–316.

[61]Cf. Buchwald, *From Maxwell, op. cit.*, note 35, p. 172; and Hunt, *The Maxwellians, op. cit.*, note 35, pp. 220–221.

[62]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, University College London, Lodge Collection, MS. Add 89/65 (ii).

5. Lorentz's Ion: A Somewhat Startling Hypothesis[63]

In 1878 Lorentz had already suggested that the phenomenon of dispersion could be explained by assuming that molecules are composed by charged particles which may perform harmonic oscillations.[64] 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.[65]

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.[66] The interaction in question could be understood if one reduced all 'electrical phenomena to [... the] displacement of these particles'.[67] The movement of a charged particle altered the state of the ether, which, in turn, influenced the motion of other particles.[68] Furthermore, charged particles could be employed to represent other observable entities. 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'.[69] This proposal was similar to the familiar

[63]In a letter to Rayleigh on 18 August 1892 Lorentz characterized the 'ions' as 'a supposition which may appear somewhat startling but which may, as I think, serve as a working hypothesis'. Quoted in N. J. Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Dordrecht: Martinus Nijhoff, 1984), p. 108.

[64]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, pp. 1–119. Cf. Nersessian, *Faraday to Einstein, op. cit.*, note 63, p. 100; and N. J. Nersessian, ' "Why wasn't Lorentz Einstein?" An Examination of the Scientific Method of H. A. Lorentz', *Centaurus* **29** (1986), 205–242, esp. p. 212.

[65]H. A. Lorentz, 'La théorie électromagnétique de Maxwell et son application aux corps mouvants', in his *Collected Papers, op. cit.*, note 64, Vol. 2, pp. 164–343, esp. p. 229. Cf. R. McCormmach, 'Einstein, Lorentz, and the Electron Theory', *Historical Studies in the Physical Sciences* **2** (1970), 41–87; and C. Jungnickel and R. McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, 2 vols (Chicago: University of Chicago Press, 1986), Vol. 2, p. 233.

[66]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, esp. pp. 178–179, 198; and Nersessian, *Faraday to Einstein, op. cit.*, note 63, p. 98.

[67]'[L]es phénomènes électriques sont produits par le déplacement de ces particules'. Lorentz, *Collected Papers, op. cit.*, note 64, Vol. 2, on p. 228.

[68]Cf. Nersessian, *Faraday to Einstein, op. cit.*, note 63, p. 103.

[69]'[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, *Collected Papers, op. cit.*, note 64, Vol. 2, on pp. 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. Thesis, 1964), p. 98.

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 Fresnel's formula (see p. 420), without, however, assuming that the ether was dragged by moving matter. Instead, his derivation 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, in turn, to secondary waves which interfered with the original ones. This complex interaction produced the effect named after Fresnel. The above analysis enhanced considerably the credibility of Lorentz's theory and facilitated the acceptance of his 'ions' as real entities.[70]

In 1895 he explicitly associated those particles with the ions of electrolysis.[71] Furthermore, he assumed that they were rigid entities with constant mass. By 1899 he had abandoned the latter assumption, because the ions' mass, calculated by means of the transformations that he developed to connect systems with different states of motion, turned out to depend on their velocity. The former assumption was also jettisoned in 1904, when he suggested that the dimensions of electrons were affected by their motion through the ether.[72]

The transformation of Lorentz's 'ions' to 'electrons' took place as a result of an experimental discovery by Pieter Zeeman. Zeeman observed that a magnetic field altered the spectrum of a radiating substance. In particular, the emitted spectral lines were widened under the influence of an electromagnet. I have provided elsewhere a detailed reconstruction of Zeeman's path to his discovery.[73] For the purposes of the present paper, it is sufficient to give a brief account of the role of Zeeman's experiments in transforming Lorentz's 'ions' into 'electrons'.

As I mentioned above, the emission of light, according to Lorentz, was a direct result of the vibrations of small electrically charged particles ('ions'). I have discussed elsewhere the details of his theoretical analysis of Zeeman's discovery.[74] Suffice it to say here that the results of Zeeman's experiments were employed to specify certain parameters of Lorentz's theory which at that time were not explicitly specified. From the observed widening of spectral lines Zeeman made an estimate of the charge to mass ratio of the 'ion', an estimate which turned out contrary to Lorentz's (and Larmor's) expectations. That

[70]Cf. Nersessian, *Faraday to Einstein, op. cit.*, note 63, p. 107; and Nersessian, 'Hendrik Antoon Lorentz', in *The Nobel Prize Winners: Physics* (Pasadena, CA: Salem Press, 1989), pp. 35–42, esp. p. 39.

[71]H. A. Lorentz, 'Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern', in his *Collected Papers, op. cit.*, note 64, Vol. 5, pp. 1–137, esp. p. 5. Cf. Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, p. 99.

[72]Cf. Nersessian, *Faraday to Einstein, op. cit.*, note 63, p. 112.

[73]See Arabatzis, 'The Discovery of the Zeeman Effect', *op. cit.*, note 54.

[74]*Ibid.*

estimate indicated that the 'ions' were three orders of magnitude (a thousand times) smaller than Lorentz had supposed.[75]

In a subsequent paper (1897) Zeeman, by considering the unexpectedly large charge to mass ratio, explicitly distinguished Lorentz's ions from the electrolytical ions. I should emphasize 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 sub-atomic particles. Moreover, from the observation of polarization effects he inferred that the charge of the ions was negative. Thus, he employed his experimental results to construct some of the qualitative and quantitative features of the ions.

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.[76] Not surprisingly, Zeeman did not appreciate that remark. In a letter to Lodge, praising '[y]our 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].[77]

Even though Zeeman neglected to mention that an early report of Thomson's measurements appeared in April 1897 (see below), his complaint was justified. However, Thomson's supposed priority 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*.[78] Millikan also spread the same mistaken view.[79]

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

[75]See P. Zeeman, 'Experimentelle Untersuchungen über Teile, welche kleiner als Atome sind', *Physikalische Zeitschrift* 1 (1900), 562–565, 575–578, on p. 578.

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

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

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

[79]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), pp. 42–43.

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'.[80] Second, it led to an approximately correct value of a central property of the electron, namely its charge to mass ratio. The large 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,[81] and later adopted the term 'electrons'.[82] 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.[83]

The significant contributions of Lorentz and Zeeman to the acceptance of the electron as a sub-atomic 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. 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, of course, subject to all the historiographical and philosophical problems that I discussed above in connection with the attribution of the electron's discovery to J. J. Thomson. I will now proceed to a brief examination of Thomson's achievement.

6. Thomson's Corpuscle: 'A By No Means Impossible Hypothesis'[85]

Thomson's contribution to the acceptance of the electron hypothesis was closely tied with the experimental and theoretical investigation of cathode rays. These rays were detected in the discharge of electricity through gases at very low pressure. Their main observable manifestation was that they gave rise to a

[80]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.*

[81]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, op. cit.*, note 64, Vol. 3, pp. 17–39, on p. 24.

[82]He began using this term in 1899. See Jungnickel and McCormmach, *Intellectual Mastery of Nature, op. cit.*, note 65, Vol. 2, p. 233.

[83]Lorentz, *Collected Papers, op. cit.*, note 64, Vol. 3, pp. 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), p. 298.

[84]Nersessian, 'Why wasn't Lorentz Einstein?' *op. cit.*, note 64, p. 209.

[85]This characterization of Thomson's proposal comes from Fitzgerald. See his 'Dissociation of Atoms', *The Electrician* **39** (1897), 103–104, on p. 104.

fluorescent spot when they collided with certain substances. There were two conflicting interpretations of the nature of cathode rays. According to the first view, mainly endorsed by British physicists, they were beams of charged particles. William Crookes, for instance, suggested in 1879 that they consisted of charged molecules. His proposal was based on two aspects of their behaviour, namely that they were emitted in a direction perpendicular to the cathode and that their trajectory was bent by a magnetic field.

The alternative view, favoured by German physicists, was that cathode rays were another species of waves in the ether. Eugen Goldstein, for instance, gave the following argument to that effect. It was known that they travelled in straight lines and that their impact caused fluorescence. Since ultra-violet rays, which were represented as waves in the ether, had similar characteristics, this suggested by analogy that cathode rays too were waves in that medium. There were other arguments in favour of this representation. In 1883 Hertz had attempted to deflect them by an electric field, but failed to do so. If they were charged particles, their path should have been affected by the electric field.[86]

Experimental research revealed some further characteristics of cathode rays. It turned out that they could not penetrate certain materials that did not block the course of ultra-violet radiation. This weighed heavily against their being waves in the ether. Furthermore, Hertz established in 1892 that they could pass through thin layers of metals. This result could also be explained by those who conceived cathode rays as charged particles. J. J. Thomson, for instance, argued in 1893 that the material bombarded by cathode rays turned into a source of cathode rays itself.[87] Finally, in 1895 Perrin showed that they were carriers of negative charge.[88]

Thus, there were conflicting pieces of evidence about the nature of cathode rays. On the one hand, the fact that they could not be deflected by an electric field and that they could pass through metals that were impenetrable to particles of atomic size suggested that they were waves in the ether. On the other hand, the fact that their trajectory was influenced by a magnetic field and that they were carriers of electricity supported their representation as charged particles.

This was the state of knowledge *vis-à-vis* cathode rays when Thomson put forward his corpuscle hypothesis in a lecture to the Royal Institution on 30 April 1897.[89] According to that hypothesis, cathode rays consisted of

[86]See D. L. Anderson, *The Discovery of the Electron: The Development of the Atomic Concept of Electricity*, repr. edn (New York: Arno Press, 1981), pp. 27–30; Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, p. 244; and Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, pp. 61–62.

[87]See Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, p. 65.

[88]See Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, p. 244.

[89]J. J. Thomson, 'Cathode Rays', *Proceedings of the Royal Institution* **15** (1897), 419–432. It is worth mentioning that no unpublished material documenting Thomson's path to that hypothesis still exists. Cf. Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, p. 267.

extremely small, sub-atomic particles. He inferred their small size from their ability to pass through thin sheets of metals. Moreover, the unexpectedly small value of their mass to charge ratio supported further that inference. By that time he had not yet succeeded in detecting the influence of an electric field on cathode rays. However, he explained away his failure to do so, by assuming that the gas in the cathode ray tube was ionized by the rays and, thus, screened off the external electric field.[90]

In October 1897 Thomson published a more elaborate report of his experiments with cathode rays and the conclusions that he drew from them.[91] By that time he had managed to deflect the rays by means of an electric field, but 'only when the vacuum was a good one'.[92] Furthermore, by deflecting the rays by a magnetic field he observed that 'the path of the rays is independent of the nature of the gas'.[93] Those results threw light on the question of the nature of cathode rays. In Thomson's words,

> [a]s the cathode rays carry a charge of negative electricity, are deflected by an electrostatic force as if they were negatively electrified, and are acted on by a magnetic force in just the way in which this force would act on a negatively electrified body moving along the path of these rays, I can see no escape from the conclusion that they are charges of negative electricity carried by particles of matter.[94]

However, as Fitzgerald had argued, there was 'escape' from that conclusion. Thomson's observations were consistent with the hypothesis that cathode rays were free electrons, i.e. disembodied charges; but more on this below.

Having established that cathode rays were charged material particles, he then inquired further into their nature: 'The question next arises, What are these particles? are they atoms, or molecules, or matter in a still finer state of subdivision?'[95] His mass to charge measurements aimed at elucidating this question.[96]

Let us see how he constructed this quantitative attribute of cathode ray particles (m/e) from the experimental data at his disposal. First, the charge carried by cathode rays was $Q=ne$, where n was the number of particles. This charge could be measured by collecting 'the cathode rays in the inside of a vessel connected with an electrometer'.[97] Second, their kinetic energy was $W=(1/2)nmv^2$. This energy could be measured by 'the increase in the temperature of

[90]Cf. Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, p. 266.
[91]J. J. Thomson, 'Cathode Rays', *Philosophical Magazine*, 5th series **44** (1897), 293–316.
[92]*Ibid.*, p. 297.
[93]*Ibid.*, p. 301.
[94]*Ibid.*, p. 302.
[95]*Ibid.*
[96]As Heilbron has noted, in his October paper Thomson presented the arguments for his corpuscle hypothesis in the reverse order from that of his April account. That is, he introduced his measurements of *m/e* as the main argument for the smallness of corpuscles, and then presented the ability of cathode rays to penetrate thin sheets of metal as secondary confirmation. See Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, p. 81.
[97]Thomson, 'Cathode Rays', *Philosophical Magazine, op. cit.*, note 91, p. 302.

a body of known thermal capacity caused by the impact of these rays'.[98] Third, the effect of a magnetic field on the trajectory of the rays was given by the following formula: $(mv)/e = H\rho = I$, where ρ was 'the radius of curvature of the path of these rays'.[99] From these observable magnitudes he could deduce m/e:

$$\frac{1}{2}\frac{m}{e}v^2 = \frac{W}{Q},$$

$$v = \frac{2W}{QI},$$

$$\frac{m}{e} = \frac{I^2 Q}{2W}$$

Thomson came up with a different method for measuring m/e that capitalized on the deflection of the cathode rays by an electric field. When the rays entered the region where the electric field was active, they would be accelerated in the direction of the field. The magnitude of that acceleration would be Fe/m, where F was the intensity of the field. Assuming that the length of the region under the influence of the field was l, the time taken by the cathode rays to traverse that region would be l/v. Thus, when they exited that region their direction would be deflected by an angle $\theta = Fe/m\ l/v^2$.[100] By a similar reasoning, Thomson calculated that angle when the rays were deflected by a magnetic field. The value that he obtained was $\varphi = He/m\ l/v$. From these two formulas, he derived the mass to charge ratio: $m/e = H^2\theta l/F\varphi^2$. By subjecting the cathode rays simultaneously to the action of an electric and a magnetic field and by adjusting the intensity of the former so that $\varphi = \theta$, he obtained the value of $m/e = H^2 l/F\theta$.[101]

The results that he obtained with both of the above methods indicated 'that the value of m/e is independent of the nature of the gas'.[102] The order of magnitude of that value (10^{-7}) was a thousand times smaller than 'the smallest value of this quantity previously known, and which is the value for the hydrogen ion in electrolysis'.[103] It should be emphasized that in 1897 Thomson did not guess correctly one of the key properties of the electron, the value of its charge: 'The smallness of m/e may be due to the smallness of m or the largeness of e, or to a combination of these two'.[104] He preferred the last option. There

[98]*Ibid.*
[99]*Ibid.*
[100]More precisely, this would be the value of tanθ, but Thomson did not mention that.
[101]See Thomson, 'Cathode Rays', *Philosophical Magazine, op. cit.*, note 91, pp. 307–308.
[102]*Ibid.*, p. 310.
[103]*Ibid.*
[104]*Ibid.*

was independent warrant for the conjecture that the size of cathode ray particles was very small. Lenard had investigated 'the rate at which the brightness of the phosphorescence produced by these rays diminishes with the length of path travelled by the ray'.[105] His results were incompatible with the view that cathode rays consisted of particles of atomic dimensions, since particles of this kind would have been slowed down by collisions with the molecules of the surrounding gas much more rapidly than indicated by observation. Furthermore, Thomson suggested that there was 'some evidence that the charges carried by the corpuscles in the atom are large compared with those carried by the ions of an electrolyte'.[106]

He also conjectured that the corpuscle was a universal constituent of all matter.[107] He had several arguments to that effect: the mass to charge ratio of cathode rays depended neither on the chemical composition of the gas within the cathode ray tube, nor on the material of the tube's electrodes. Furthermore, as Lenard had demonstrated in 1895, their absorption by a substance depended only on its density and not on its chemical composition. If matter was composed of corpuscles, Lenard's observations would be readily explained.[108] Finally, by means of the corpuscle hypothesis one could explain away the measurements of atomic weights that were not exact multiples of the weight of the hydrogen atom.[109]

However, the seemingly 'alchemical' connotations of Thomson's suggestion generated a great deal of resistance towards his corpuscle theory. Fitzgerald, for instance, interpreted Thomson's theory as implying the 'presence of a possible method of transmutation of matter'.[110] Fitzgerald's aversion to alchemy led him to deny that the cathode ray particles were material constituents of atoms. He offered, instead, an alternative interpretation of them as free charges ('electrons') that did not require the divisibility of atoms and kept him away from 'the track of the alchemists'.[111]

[105] *Ibid.*

[106] *Ibid.*, p. 312.

[107] Thomson's conjecture might seem incompatible with his suggestion that the charge of the corpuscle was larger than the elementary unit of electric charge, the unit carried by a hydrogen ion in electrolysis. An electrolytic ion was also supposed to consist of corpuscles and it is not clear how it could carry a charge smaller than the charge carried by its constituents. For Thomson, however, this was not a problem. Considering the molecules of HCl as an example, he represented the 'components of the hydrogen atom as held together by a great number of tubes of electrostatic force; the components of the chlorine atom are similarly held together, while only one stray tube binds the hydrogen atom to the chlorine atom' (*ibid.*). Thus, the above difficulty did not arise, since the unit of charge, associated with each electrostatic tube, was portrayed as one 'stray' component of the charge associated with the unit of matter.

[108] For details see Thomson, 'Cathode Rays', *Philosophical Magazine, op. cit.*, note 69, pp. 311–312.

[109] Cf. Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, pp. 82–83.

[110] Fitzgerald, 'Dissociation of Atoms', *op. cit.*, note 85, p. 104.

[111] *Ibid.* Cf. Achinstein, *Particles and Waves, op. cit.*, note 7, p. 288; and Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, p. 271.

William Sutherland made a similar proposal in 1899.[112] Thomson's reaction was negative. When he found out about Sutherland's suggestion he prepared a rebuttal, where he pointed out its shortcomings:

> [T]he view that in the cathode rays we have electric charges without matter Electrons [...] I took at the beginning of my experiments, but exactly in the form (which I gather Mr Sutherland does not adopt) that the atoms are themselves a collection of electrons [...], in which form it only differs verbally from the form I used. replacing corpuscles by electrons, seems to be wanting in precision & definiteness as compared with the other view and beset with difficulties from which the other was free [...] the advantages of the hypothesis as far as I can see [...] are only that it does not involve the necessity of the atom being split up.[113]

The main drawback of the hypothesis of free electrons, on the other hand, was that 'it supposes that a charge of electricity can exist apart from matter of which there is as little direct evidence as of the divisibility of the atom'.[114] As he had mentioned a few years earlier, '*an electric charge* is always found associated with matter'.[115]

Thomson's objection to Sutherland's view was not applicable to Larmor's version of the electron hypothesis. According to Larmor, electricity did not exist apart from matter because it constituted matter. Electrons were material particles, in the sense that their material aspect (inertia) was an epiphenomenon of their electric charge. Furthermore, material atoms themselves were composed of (positive and negative) electrons in dynamical equilibrium. That is why Thomson referred to Larmor's view as only 'verbally' different from his own.[116]

Nevertheless, Thomson wanted to differentiate his 'corpuscle' from the 'electron' (even in Larmor's sense). His resistance to the latter can be explained by three factors. First, in 1897 he still subscribed to a Maxwellian conception of charge, i.e. he considered charge an epiphenomenon of the interaction between ether and matter.[117] Since charges could not exist independently of

[112]W. Sutherland, 'Cathode, Lenard and Röntgen Rays', *Philosophical Magazine*, 5th series **47** (1899), 269–284. Cf. Feffer, 'Arthur Schuster, J. J. Thomson', *op. cit.*, note 18, p. 59.

[113]Cambridge University Library, Add 7654/NB 44. This notebook is not dated, but it must have been written sometime in early March 1899, since Sutherland's paper appeared in the March issue of the *Philosophical Magazine* and Thomson's published response was dated 11 March 1899. See J. J. Thomson, 'Note on Mr. Sutherland's Paper on the Cathode Rays', *Philosophical Magazine*, 5th series **47** (1899), pp. 415–416.

[114]Cambridge University Library, Add 7654/NB 44.

[115]Cambridge University Library, Add 7654/NB 40. The contents of this notebook, which is undated, 'were probably written in August or September 1896'. Falconer ('Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, p. 258).

[116]It is worth noting that Larmor had identified cathode rays with free electric charges as early as 1894: 'What strikes me also is the fact that free electric charges of ionic or some such character can flash about space with velocity comparable with radiation, provided they are not bothered by inertia other than that of the medium around them: cf. discharge in vacuum tubes'. Larmor to Fitzgerald, 14 June 1894, quoted in Buchwald, *From Maxwell to Microphysics, op. cit.*, note 35, p. 167.

[117]Cf. Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, pp. 261–262; and Feffer, 'Arthur Schuster, J. J. Thomson', *op. cit.*, note 18, pp. 60–61.

matter, the 'corpuscle' had to be a material particle that carried charge. Thus, one of the reasons he avoided the term 'electron' was that it denoted a disembodied charge, whereas his 'corpuscle' was a material particle.[118] Second, the term 'electron' denoted both negative and positive elementary charges. Whereas the former, according to Thomson, were real entities, the reality of the latter had not been established (as late as 1907). Since he did not want to inadvertently confer reality to the positive electron, he did not use that term.[119] These were not very significant differences, however, and eventually negative 'electrons' and 'corpuscles' came to be identified, even by Thomson himself.[120]

The acceptance of Thomson's proposal was, however, gradual. Whereas his success in deflecting cathode rays by means of an electric field established that they were charged particles, his suggestion that they were universal, sub-atomic constituents of matter was not accepted until, at least, 1899. In 1897 he had not shown that those entities were present in other phenomena, besides the discharge of electricity through gases. Furthermore, he did not measure separately the charge and mass of the corpuscle, and, thus, the smallness of m was not sufficiently established.[121]

By 1899 these difficulties had been alleviated. In 1898 Thomson devised a method for measuring the charge of ions in gases that had been ionized by X-rays. The results of his measurements agreed with those for the charge carried by electrolytic ions. In 1899 he published measurements of the charge to mass ratio of the particles produced in the photoelectric effect as well as by thermionic emission. That ratio agreed with the corresponding ratio of cathode ray particles. Furthermore, he measured the charge of those particles by the new method that he had come up with, and he found that it coincided with the charge carried by hydrogen ions. This result along with the large charge to mass ratio implied the smallness of m. Finally, the reception of Thomson's proposal was facilitated by the theoretical and experimental developments that we examined in the previous two sections, namely the construction of Lorentz's and Larmor's theories and the discovery of the Zeeman effect.[122]

[118]See also J. J. Thomson, 'Carriers of Negative Electricity', Nobel Lecture, 11 December 1906, in *Nobel Lectures: Physics, 1901–1921* (Amsterdam: Elsevier, 1967), pp. 145–153, esp. p. 149.

[119]See Kragh, 'Concept and Controversy', *op. cit.*, note 7, p. 210.

[120]In 1923, for instance, he published a book titled *The Electron in Chemistry* (Philadelphia).

[121]Cf. Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, p. 271; and Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, p. 84.

[122]J. J. Thomson, 'On the Masses of the Ions in Gases at Low Pressures', *Philosophical Magazine* **48** (1899), 547–567. Thomson's research between 1897 and 1899 is discussed in N. Robotti, 'J. J. Thomson at the Cavendish Laboratory: The History of an Electric Charge Measurement', *Annals of Science* **52** (1995), 265–284. Cf. also Falconer, 'Corpuscles, Electrons and Cathode Rays', *op. cit.*, note 8, pp. 272–273; and Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, pp. 84–85. Heilbron's thesis continues to be the most comprehensive discussion of the various developments that led to the acceptance of the electron hypothesis. However, in some of his statements he attributes the electron's discovery exclusively to Thomson, an attribution that is at odds with the rest of his analysis.

7. Concluding Remarks

By the turn of the century, the constituents of cathode rays had been identified with Lorentz's ions,[123] and both had been already associated with electrons by Larmor himself.[124] This identification strengthened the case for the existence of the electron, since it unified the theoretical and experimental evidence on which the claim for its existence rested. If one insists on using the expression 'the discovery of the electron', one would have to use it as meaning the complex process that led to the consolidation of the belief that 'electrons' denote real entities. In that sense, was the electron discovered by Thomson? The answer must be negative. His significant contribution to the acceptance of the belief in the reality of the electron was not sufficient for establishing that belief. Furthermore, most of his theoretical suggestions had been previously put forward by others, and many of the experimental results that he obtained were also independently produced by others.

One could suggest that Thomson should be considered as the electron's discoverer because his 'corpuscle' is closer to our electron than any of the entities proposed by his contemporaries. On such a view, it was Thomson who discovered the electron; not the 'electron' of the late 1890s which was 'erroneously' thought to be a disembodied charge, but the subsequent electron, a material particle and the carrier of the unit of electric charge. The corpuscle's description (material particle, carrier of the unit of negative charge) suffices to identify it with our electron, whereas the description of the late 19th century 'electron' (disembodied charge, either positive or negative) is incompatible with our use of that term.

This suggestion also leaves much to be desired. Consider, first, the material origin of the corpuscle's mass. Even though the mass of Larmor's electrons was purely electromagnetic, they did not cease to be material particles. Their material aspect was inextricably tied with their electrical character. In Larmor's scheme matter (inertia) was an epiphenomenon of electricity. More importantly, a few years later, as a result of Kaufmann's experiments on the velocity dependence of the electron's mass, Thomson was converted to the view 'that this mass arises entirely from the charge of electricity on the corpuscle'.[125] His conversion would be an embarrassing incident if we attributed the discovery of the electron to him because of the materiality of his corpuscle.

[123]See, for instance, Zeeman, 'Experimentelle Untersuchungen', *op. cit.*, note 75, pp. 577–578.

[124]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, esp. p. 506. Cf. Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, pp. 103–104. I should add that this identification was by no means universally accepted circa 1900. The most notable exception was, of course, Thomson.

[125]J. J. Thomson, *The Corpuscular Theory of Matter* (London: Charles Scribner's Sons, 1907), p. 28. Cf. Feffer, 'Arthur Schuster, J. J. Thomson', *op. cit.*, note 18, p. 60; and Heilbron, *A History of the Problem of Atomic Structure, op. cit.*, note 69, p. 98.

Second, consider the negative sign of the corpuscle's charge. Can one attribute the electron's discovery to Thomson because 'these physicists [Stoney, Larmor and Lorentz] were referring to particles or charges or both that could be positive as well as negative, and not to the negative particles that comprise cathode rays'?[126] Again, the answer must be negative. Zeeman's experiments had also indicated that the charge of the ion-cum-electron was negative.

Finally, even if the 'corpuscle' resembled more our 'electron' than, say, Lorentz's 'ions', this resemblance would still be slight, to say the least. First, the corpuscle was conceived as a structure in the ether, which has disappeared from the ontology of physics. Second, all of the bizarre properties now associated with electrons (quantum numbers, wave-particle duality, indeterminate position–momentum, etc.) were literally inconceivable in Thomson's time.

It should be evident that there is something fundamentally wrong, both historiographically and philosophically, about viewing the introduction and gradual consolidation of theoretical entities through the distorting lens of 'discovery'. The electron was not the product of a sudden discovery. Rather, it emerged out of several problem situations in the study of chemical phenomena (electrolysis), in the context of electromagnetic theory, and in the study of the discharge of electricity in gases. By 1900 those diverse situations had found a single solution in the form of the electron qua sub-atomic, charged particle. Several historical actors provided the theoretical reasons and the experimental evidence which persuaded the physics community about its reality. However, none of those people discovered the electron. The most that we can say is that one of those, say Thomson, contributed significantly to the acceptance of the belief that 'electrons' denote real entities.

It is worth pointing out that the perspective of contemporary scientists, as opposed to later scientists writing surveys of the history of that period, provides further support for my view that the electron was not 'discovered', but was gradually constructed and legitimized as an element of the ontology of physics and chemistry. Several contemporary scientists, who were involved in the development of the theory of electrons, did not think of Thomson as the 'discoverer' of the electron. Lorentz, for instance, in a laudatory letter to Thomson that was meant to thank him for 'contributing to the collection of physical papers that has been offered me on the occasion of the 25th anniversary of my graduation', characterized his achievement as having 'taken so prominent a part in the investigation of the subject [of the theory of electrons]'.[127] Thus, Lorentz regarded Thomson as a significant contributor to the development of the theory of electrons and not as the discoverer of these

[126]Achinstein, *Particles and Waves, op. cit.*, note 7, p. 286.
[127]H. A. Lorentz to J. J. Thomson, 1 February 1901, Cambridge University Library, Add 7654/L61.

entities. Furthermore, in his Nobel lecture Lorentz mentioned Thomson's name only once, in connection with the latter's secondary confirmation that the mass of the electron was very small compared to the mass of the hydrogen atom. This result had been previously obtained by Lorentz, who combined Zeeman's measurements of *e/m* with an analysis of the phenomenon of dispersion (see p. 424).[128]

Millikan also did not think that Thomson's experiments were decisive *vis-à-vis* the electron's discovery. 'J. J. Thomson and after him other experimenters [... provided] an indication of an affirmative answer to the [...] question above [is there a primordial subatom out of which atoms are made?]—an indication which was strengthened by Zeeman's discovery in 1897'.[129] Neither Thomson nor Zeeman discovered the electron; rather their work provided an *indication* of its reality. Some doubts remained with respect to its existence, doubts that were removed by Millikan's own experimental work: 'The most direct and unambiguous proof of the existence of the electron will probably be generally admitted to be found in an experiment which for convenience I will call the oil-drop experiment'.[130] The self-serving character of this assertion notwithstanding, it points the way to an adequate understanding of the process that established the reality of the electron. That process was an extended one, stretching from Faraday to Millikan, and culminating in the oil-drop experiments.

Owen Richardson, who had been Thomson's student and devoted his 'entire scientific career [... to] the electron',[131] shared a similar perspective: 'The electron theory may now be said to have developed far beyond the region of hypothesis. Discovery after discovery *during the last fifteen years* has established indubitably the existence of a negative electron whose properties are independent of the matter from which it originates' (emphasis added).[132] So it was a 15 year process which established the existence of the electron, and not any isolated experiment or measurement.

Rutherford, in a review of the evidence that supported 'the existence of bodies smaller than atoms', did not attribute the electron's discovery to Thomson. Rather his view was that

> during the last few years considerable evidence has been obtained of the production, under various conditions, of bodies which behave as if their mass was only a small fraction of the mass of the chemical atom of hydrogen. As far as we know at present,

[128]See H. A. Lorentz, 'The Theory of Electrons and the Propagation of Light', Nobel lecture, 11 December 1902, in *Nobel Lectures: Physics, 1901–1921* (Amsterdam: Elsevier, 1967), pp. 14–29, esp. p. 24.

[129]Millikan, *The Electron, op. cit.*, note 79, pp. 42–43.

[130]R. A. Millikan, 'The Electron and the Light-Quant from the Experimental Point of View', Nobel Lecture, 23 May 1924, in *Nobel Lectures: Physics, 1922–1941* (Amsterdam: Elsevier, 1965), pp. 54–66, on p. 55.

[131]Galison, *How Experiments End, op. cit.*, note 7, p. 32.

[132]O. W. Richardson, *The Electron Theory of Matter* (Cambridge University Press, 1914), p. 3.

these minute particles are always associated with a negative electric charge. For this reason they have been termed 'electrons'.[133]

Thus, according to Rutherford, the belief in the existence of the electron was the outcome of a gradual process of accumulating evidence, as opposed to an event. Even though Thomson contributed significantly to that process, he did not produce all that evidence by himself. More importantly, as late as 1904 the existence of the electron was not universally accepted: 'The physical existence of electrons is now accepted by many [but not all] scientific men'.[134]

Finally, the belief in the existence of the electron *qua* sub-atomic particle presupposed a conviction in the existence of atoms. Therefore, the issue of the reality of atoms and the issue of the existence of electrons were closely tied. The latter could not be resolved without a prior resolution of the former. One could suggest that the prior belief in the existence of atoms was not necessary for resolving the issue of the existence of electrons qua atoms of electricity. But, as we have seen expressed in Helmholtz's insightful remark, the belief in the atomicity of matter was necessary for the hypothesis of the atomic structure of electricity to get off the ground.

A different connection between the two issues was perceived by Lorentz, who suggested that the grounds for skepticism towards atoms could also throw doubt on the existence of electrons:

the theory of electrons is to be regarded as an extension to the domain of electricity of the molecular and atomistic theories that have proved of so much use in many branches of physics and chemistry. Like these, it is apt to be viewed unfavourably by some physicists, who prefer to push their way into new and unexplored regions by following those great highways of science which we possess in the laws of thermodynamics, or who arrive at important and beautiful results, simply by describing the phenomena and their mutual relations by means of a system of suitable equations.[135]

The opponents of the atomic hypothesis were likely to be unfavourably disposed towards the electron. Duhem, for instance, detested atoms and electrons alike.[136] What remains to be done is to examine the attitudes of the anti-atomic opposition towards the electron. However, this task is beyond the scope of this paper.

Regardless of the beliefs of that opposition, by the end of the first decade of the 20th century the electron had become indispensable for the practice of physicists concerned with electromagnetic theory. As we read in a textbook from that period, a 'significant sign of its [the electron theory's] acceptance is

[133] E. Rutherford, 'The Existence of Bodies Smaller than Atoms', *Transactions of the Royal Society of Canada* 2nd Series **8** (1902), 79–86; repr. in *The Collected Papers of Lord Rutherford of Nelson, Vol. 1* (London: George Allen & Unwin Ltd, 1962), pp. 403–409, on p. 403.

[134] *Ibid.*, p. 409.

[135] H. A. Lorentz, *The Theory of Electrons*, 2nd edn (Leipzig: Teubner, 1916), p. 10.

[136] See P. Duhem, *The Aim and Structure of Physical Theory* (Princeton University Press, 1991), p. 304.

the almost complete absence of attempts to formulate electrical theories not based upon electrons'.[137] In that sense, it would remain a permanent part of the ontology of physics.

Acknowledgements—I am indebted to Kostas Gavroglu, Nancy Nersessian, Bas van Fraassen, Norton Wise and two anonymous referees for discussion and comments. I am grateful to the Syndics of the Cambridge University Library for permission to reproduce excerpts from the J. J. Thomson collection. Excerpts from the Joseph Larmor Letters are reproduced by kind permission of the President and Council of the Royal Society of London. Finally, I would like to thank University College London Library for permission to quote from Oliver Lodge's correspondence.

[137]E. E. Fournier D'Albe, *The Electron Theory: A Popular Introduction to the New Theory of Electricity and Magnetism*, 2nd edn (London: Longmans, Green & Co., 1907), p. xxi (from the author's preface to the second edition).