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CORPUSCLES to ELECTRONS

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Introduction

On 30 April 1897 J. J. Thomson (Cavendish Professor of Physics at Cambridge) announced the results of his previous four months' experiments on cathode rays.[2] The rays, he suggested were negatively charged subatomic particles which were a universal constituent of matter, and whose arrangement determined the chemistry of the element. He called the particles 'corpuscle' but they became known as 'electrons' and Thomson has been hailed as their 'discoverer'.[3] I have argued elsewhere that the work was not the outcome of a concern with the nature of cathode rave, but of a much more general interest in the nature of gaseous conduction.[4] In this paper, I discuss the acceptance of Thomson's corpuscle theory.

In recent years an attributional account of discovery has become widespread. While my discussion may lend credence to such a model of the 'discovery' of the electron, it is distinct in at least two important ways.[5] First, it is agnostic as to whether there was actually a 'discovery' and of what that was constituted; it makes explicit that what we are considering is opinions. Second, it avoids some of the connotations of 'discovery' which seek to locate discovery in a specific place, time, and actor or team. Acceptance accommodates easily an episode that extends over several years, involves a variety of workers, and is a subject for debate.

Thomson later recalled that his *corpuscle* theory was not generally accepted until two years later when he spoke of it again at the British Association Meeting in 1899.[6] By 1900 also, the existence of *electrons* was becoming fairly widely accepted, end a whole new electromagnetic world view was being developed on this basis by H.A**.** Lorentz, J. Larmor, E. Wiechert, W. Kaufmann and others.[7] But were these 'electron' the same as Thomson's 'corpuscles' and how important were Thomson's experiments in establishing the existence of electrons?

In examining acceptance of a theory we need to look at the evidence other scientists considered important in its favour. I have chosen two accounts of the development of the electron hypothesis, one British, one German. First l look at their accounts of the development of the electron idea, up to the point at which they declare that the electron exists, then at their accounts of the acceptance of the electron idea and the role of the Cavendish experiments in this. The comparative approach I have chosen highlights clearly, but crudely, the complexity of what was going on in the 1890s. In particular it demonstrates how differing traditions led to different concepts of the electron and how identical experiments meant different things within these traditions.[8]

One account is Oliver Lodge's, *Electrons*, based on lectures given in 1902. but published in 1907.[9] Lodge was a leading British physicist, Professor at Liverpool and later Principle of Birmingham University. He was comparatively independent, owing no allegiance to Cambridge or the Cavendish, but it is worth noting that his bock is dedicated to Thomson. The other account is Walter

Kaufmann's, 'The Development of the Electron Idea', a lecture given to the 73rd Naturforscher Versammlung at Hamburg in 1901.[10] Kaufmann was at the time assistant at the Physics Institute at Göttingen and later became Director of the Physics Institute at Königsberg. He was to make his name by his accurate experiments on the mass of the electron. Since both men were experimentalists, rather than theoreticians, one might naively expect that, allowing for nationalistic bias and personal credit-seeking, their accounts would be broadly similar.

Neither account pinpoints a 'discovery' or 'discoverer' of the electron; both are reconstructions that attempt to trace how the Idea grew up and what evldence was important in its favour**.** Nevertheless**,** in both accounts there comes a point at which the author considers that the evidence is sufficient, that the electron has a real existence, and in this sense that it has been discovered. Their accounts might thus help resolve what seems the weak point In Achinstein's model of discovery, ie. how one defines when an actor knows enough to have 'discovered' an entity.[11]

The existence of electrons: Lodge's account

By 1902 the development of an electromagnetic view of nature was well under way, and this is the main thrust of Lodge's book. The first 90 pages, however, cover electron theory up to 1900. Figure 1 summarises Lodge's account of the discovery of the electron. He starts with the properties of a charged particle in motion, reviewing rapidly Heaviside's work on the state of the surrounding ether, Poynting's on the transmission of energy, and Larmor's on the radiated energy of such particles. This leads up to a chapter on J J Thomson's formulation of the concept of electromagnetic mass in 1881, 'one of the most remarkable physical memoirs of our time'.[12] This was the idea that a moving charged particle has extra inertia associated with it which depends on its velocity. It later proved fundamental to the electromagnetic world-view.

Evidence	**Worker**	
	Cavendish	**Elsewhere**
Theory of motion of charged particles		Heaviside Poynting Larmor
Electromagnetic mass	Thomson	
Faraday's laws imply a unit of electricity, the 'electron'		Stoney Loschmidt Kelvin
Cathode rays, attempts to explain		Crookes Goldstein Lenard Perrin
Mobility of carriers in gaseous conduction	Townsend	Schuster
1897, m/e for cathode rays, suggestion rays are 'corpuscles'	Thomson	

Electron/corpuscle exists

Figure 1. Lodge's account of the discovery of the electron

Lodge next turns to tracing the idea of an indivisible unit of electric charge, starting with Faraday's laws of electrolysis. He credits Johnstone Stoney naming this unit 'the electron' and derives the ratio of mass to charge for the hydrogen ion, citing experiments by Stoney, Loschmidt and Kelvin.[13] Here Lodge slips in, implicitly, the idea that the electron might be a particle rather than simply a set amount of charge.

Lodge then passes on to the problems of understanding the nature of cathode rays. The general belief was that they were negatively-charged particles. But particles of atomic dimensions would be too big to pass through thin metal foil, as cathode rays did, or to have the observed long mean free path in air. Moreover, Arthur Schuster, and later J S Townsend, had observed that the carriers of negative electricity in a discharge tube were highly mobile, implying a very small size.[14] Lodge suggests that they might be isolated charges or 'electrons'.

Lodge summarises, 'The magnitudes which need experimental determination in connection with cathode rays, in order to settle the question and determine their real nature, are the speed, the electric charge, and if possible the mass, of the flying particles'.[15] It is worth noting this evidence for Lodge's unquestioned adherence to the mechanical philosophy, the belief that all phenomena could be reduced to matter in motion and described by their mass and velocity. In this he was typical of most British physicists.

The scene was thus set for J J Thomson's experiments of April 1897 in which he measured the velocity and ratio of mass to charge for cathode rays by his first method. This involved combining the magnetic deflection of the rays with their heating effect on a thermocouple.[16] He found velocities of up to one tenth that of light, and mass to charge ratios only one thousandth that for the hydrogen ion. Furthermore, and very important, the mass to charge ratio proved independent of the nature of matter present (ie. of the gas in the discharge tube or the nature of the electrodes). It seemed likely, according to Lodge, that the mass associated with the cathode ray particle must be 1000 times smaller than the hydrogen atom, and the particles might be the 'detached and hitherto hypothetical individual electrons'.[17]

For Lodge, then, by the end of April 1897 the existence of the electron had been established through experiments on cathode rays. Note that this was *before* Thomson had found the charge to mass ratio by his classic method using electric and magnetic deflections.[18] Lodge's account, in increasingly abbreviated form, is that which has entered British textbooks ever since.[19]

The existence of electrons: Kaufmann's account

Let us now look at Kaufmann's account. It is summarised in Figure 2. We might be forgiven for thinking we were talking about a different entity.

Evidence	Worker	
	Cavendish	Elsewhere
Electric atom theory of electromagnetism		Weber
Optical dispersion by mechanical oscillators		Helmholtz
Optical dispersion by electric oscillators		Lorentz
Faraday's laws imply unit of electricity, the 'electron'		Helmholtz Stoney
Maxwell's continuum electromagnetic theory		Maxwell Hertz
Estimates of size of 'electron'		Richarz Ebert Stoney
Reconciliation of Maxwell's and atomic theories of electromagnetism		Helmholtz Lorentz
1896, magnetic splitting of spectral lines		Zeeman Lorentz
Electron exists		

Figure 2. Kaufmann's account of the discovery of the electron

Kaufmann starts with Weber's electromagnetic theory of the 1860s and 70s, of electric atoms acting at a distance. It had, Kaufmann said, described the electrodynamical phenomena known at the time. However Weber had made no attempt to calculate the size of the electrical atom. Then Faraday and Maxwell had suggested that a finite rate of propagation should replace Weber's action at a distance. Hertz's confirmation of Maxwell's theory in 1887 seemed to spell the end for Weber's views. Maxwell's formulae were wholly void of any atomistic conceptions, could explain fundamental phenomena as well as Weber's, and were the only way of representing Hertz's waves.[20]

However, judges Kaufmann, physicists were now in danger of throwing out the baby with the bath water. The success of Maxwell's theory in explaining Hertz waves blinded them to its inability to explain some optical phenomena, such as deviations in predicted refractive indices, and dependence of refractive index on colour.

Already Helmholtz had tried to explain these by a mechanical theory of dispersion, founded on the vibrations of material molecules. In 1880 H A Lorentz laid the foundations of an analogous electromagnetic theory of dispersion which regarded every molecule as containing material points charged with electricity, the origin of electric vibrations of a definite period.[21]

Like Lodge, Kaufmann stresses that Faraday's laws of electrolysis provided evidence for the existence of electric atoms. These, Kaufmann claims, must be the electric particles Lorentz postulated. Hertz's demonstration of electromagnetic waves in 1887 stimulated physicists to try to reconcile the two opposing theories of electromagnetism. Between 1890 and 1893 works by F Richarz, H Ebert and Johnstone Stoney attempted to determine the magnitude of the elementary quantity, which Stoney named 'electrons'. Most of these dealt with the emission mechanism of luminous vapours, and calculations were based on the kinetic theory of gases. Ebert showed that the size of the electron might be very small compared with the molecular diameter. The charge on an electron was determined by electrolysis.[22]

Kaufmann continues, 'The edifice of the electromagnetic theory of light', was completed in 1892 by Lorentz showing, 'how the assumption of vibrating charged particles in transparent bodies eliminates all the difficulties in the way of an adequate explanation of the propagation of light in moving bodies....'[23]

Then, 'In view of the facility with which Lorentz's theory explains the dispersion and observation phenomena, a direct proof of its truth was hardly required.'[24] But in 1896 Zeeman's discovery of the splitting of spectral lines in a magnetic field provided this proof. The effect was predicted by Lorentz's theory and allowed, for the first time, a determination of the size of the vibrating charges. The negative charges proved to have a mass to charge ratio about 2000 times smaller than the hydrogen ion, forcing the conclusion, Kaufmann said, that the vibration is that of the electron itself.

Thus, for Kaufmann, the electron was formulated theoretically and its existence was then established in the Zeeman effect in 1896.

Concepts of the electron

These accounts by Lodge and Kaufmann are so entirely dissimilar that we are left searching for explanations. We might expect that Kaufmann, as a German, might value German contributions more highly than Lodge did. We might also expect, that as a rival of Thomson's for credit for measuring m/e for cathode rays, he might downplay Thomson's contribution, as indeed he does, relegating him to the role of a mere experimenter. ('...an unobjectionable explanation of the numerical results [for gaseous conduction], especially as obtained by J J Thomson and his followers, is only possible on the assumption of wandering particles within the gas'[25] with no mention of Thomson as the author of this theory.)

What we would not expect, judging by traditional accounts of the discovery of the electron, is an entirely different conceptual build-up to the electron. Like Lodge, Kaufmann was an experimentalist, yet the development he concentrates on was theoretical and formulated to answer an entirely different set of questions from those posed by Lodge. The question arises, was the outcome of these two developments the same? Was the 'electron' whose existence Lorentz and Zeeman established in 1896, the same that Thomson demonstrated in 1897? The situation is further obscured by Lodge and Kaufmann's accounts which both talk of 'electrons', whereas in 1896 Lorentz termed his particles 'ions', while Thomson called his 'corpuscles'. Lorentz switched to 'electrons' in 1899 though, while Thomson clung to 'corpuscles' until 1911 or 1912.

Were either ions or corpuscles the same as the 'electron' we now deem to have been discovered in 1897? If different, what was the origin of the differences and how did the views become unified?

We must begin by considering the differing nature of German and British science, and looking at the work of a British physicist neglected in both accounts, Joseph Larmor.[26]

The essential difference between British and German world views, according to McCormach and Buchwald, was that the Germans held to a particulate world view.[27] They were concerned with material particles embedded in a stationary ether and, as Kaufmann points out, they had a tradition of atomistic theories of electricity. The problem of trying to reconcile these views, and the phenomena they explained, with the apparent success of Maxwell's continuum theory, loomed large. Lorentz succeeded in doing this in 1892 with his electric particles, which were material, charged, and embedded in a stationary ether.[28] These 'ions' were elastically bound within the molecules and mediated the interaction between ether and matter, but the coupling mechanism was not specified and neither was the structure of the ether. Nor did Lorentz's theory give any indication of the size of the ions, or a method of finding this. His terminology suggests that he thought them comparable to electrolytic ions. Following Zeeman's calculation of e/m for the ions, Lorentz briefly named his particles 'lightions', thus distinguishing them from the ions of electrolysis,[29] before switching to 'electrons' in 1899.[30]

In Britain similar problems with the inability of Maxwell's theory to explain some optical phenomena were occupying theoretical physicists. But they came from the opposite direction, that of continuum mechanics. At first reading their work often appears more atomistic than the German, and they seem preoccupied with reducing the world to matter in motion. But a second reading shows that, for them, matter is merely a structure of the ether, often a vortex ring or centre of strain.[31] By 1894 Joseph Larmor had independently arrived at a theory of electric particles which addressed the same problems as Lorentz's.[32] Following FitzGerald's suggestion, Larmor named his particles 'electrons', defining them as centres of radial strain in a rotationally elastic ether.[33] Larmor was the first to suggest that matter might be purely electromagnetic in origin, writing in the spring of 1895 that, 'material systems are built up solely out of singular points in the aether which we have called electrons and that atoms are simply very stable collocations of revolving electrons',[34] although he constantly hedged his bets on this subject.[35] He had previously shown that if the mass was purely electromagnetic, then electrons must be capable of moving near the speed of light and had noted their possible connection with cathode rays.[36] Until the discovery of the Zeeman effect, Larmor assumed that his electron was associated with a mass at least as massive as the hydrogen atom. In 1897 he revised this assumption and identified his electron with the small oscillating charges postulated by Zeeman and Lorentz.[37]

Thomson worked within the same theoretical framework as Larmor and was familiar with Larmor's work, which he refereed. Like Larmor, and Lorentz, he was deeply concerned about the interaction between the ether and matter, but his theory was formulated to answer a completely different set of questions from theirs. He was unique in seeing chemical effects as important and in seeking atomic models that would explain chemical, rather than optical or thermodynamic phenomena.[38] For the previous 15 years he had seen gaseous discharge (but not cathode rays in particular), as the experimental key to untangling the matter-ether relationship. Throughout, he relied on an analogy between gaseous discharge and electrolysis, which thus placed the problems and concerns of electrochemistry in a central position in his programme. By 1890, based on his discharge work, he had worked out qualitatively a view of discrete units of electricity, and by 1895 had a tentative explanation of how these interacted with matter. It is worth examining Thomson's views of 1890-1895 more closely, for they explain why he did not accept Larmor's theory, why he was in a unique position in 1897, and why his 'corpuscle' differed from contemporary 'electrons'.

Like Larmor, Thomson was trained in Maxwell's electrodynamics, and his early beliefs belong to this tradition. Maxwell relegated electric charge and electric current to the status of secondary phenomena – they were the by-product of processes in the field. The Maxwellian view of electricity was of a strain state of the ether. The ether was continuous and pervaded all matter. The strain state was also continuous throughout any medium, but there was a discontinuity at the boundary between media, with different ratios of conductivity to dielectric permeability. Electric charge was a manifestation of this discontinuity. It was smeared uniformly over the boundary and could not exist anywhere except at the boundary.[39]

Around 1890 Thomson felt forced by the evidence from electrolysis, which he believed analogous to discharge, to recognise that charge must be discrete rather than continuous. The Faraday tube theory that he devised reconciled the experimentally found discrete charges with Maxwell's theory.[40] Based mainly on Poynting's work on the energy of the electromagnetic field, Thomson suggested that electromagnetic effects were propagated by the motion of 'Faraday tubes', which carried electrostatic force. The tubes either formed closed loops or terminated on atoms. They were all of the same strength, corresponding to the charge of the electrolytic hydrogen ion. Thomson, himself, pictured these tubes as vortex filaments in the ether.

Faraday tubes were essentially discrete, and the electrification produced at the end of them was discrete also. Continuing the Maxwellian tradition, Thomson believed that a charge could exist only at the boundary of the dielectric and a conductor, ie. Faraday tubes could end only on matter. Blake and Sohncke's experiments had shown that molecules could not be charged, hence Thomson concluded that Faraday tubes could end only on atoms.[41] By 1895 he had developed this conclusion into a theory to account for the differing attractions which different chemical atoms had for electricity.[42] He suggested that the atom behaved as though it contained a large number of outward pointing 'gyrostats'. An incident ethereal vortex Faraday tube would modify the motion of the gyrostats depending on whether the tube and gyrostats were rotating the same, or opposite ways. In one case the energy of the atom would be lowered, in the other raised. Different atoms might have differently rotating gyrostats and thus have a preference for one particular type of vortex tube, or charge.

For our purposes the essential feature of this theory is that charge remained a boundary effect between matter and ether. Both chemical atom and vortex tube had to be present before a charge could exist. This may account for Thomson's remark that he did not find Larmor's (purely electromagnetic) theory very useful,[43] and certainly explains his emphatic statement in 1896 that, 'the idea of charge need not arise, in fact does not arise, as long as we deal with the ether alone'.[44] Furthermore, the particular structure and chemistry of atoms was implicated in the nature of electric charges.

This belief placed Thomson in a unique position among physicists. When he identified cathode rays as small, negatively charged 'corpuscles', he made their structural implications clear, citing Prout's and Lockyer's chemical ideas of divisible atoms as precedents, rather than Lorentz's or Larmor's electromagnetic theories (though he did point out that his result were in broad agreement with Zeeman's).[45] Two months later Thomson proposed an atomic structure based on the stable grouping of corpuscles in a uniform sphere of positive electrification.[46] Although he was not explicit about the nature of a corpuscle, he continued to treat it on occasion as the locus of interaction between the end of a vortex tube and some material part of the atom, which might have no more extension than a mathematical point. The whole entity, matter plus boundary plus vortex, however, was an essential part of the atoms.

Thus Lorentz's ion *was* different from Thomson's corpuscle, and was different again from Larmor's electron. Figure 3 summarises the characteristics of all three. The later idea of an electron took elements from all three theories.

Lorentz	Larmor	Thomson
Stationary ether	Stationary rotationally elastic ether	State of ether not mentioned
Material, particle electron	Ethereal, strain centre electron	Boundary effect between ether vortex and atom
Electron embedded in matter but separate from it	Electron provides ethereal origin of matter	Corpuscle a building block of chemical atoms

Figure 3. Summary of features of Lorentz's, Larmor's and Thomson's theories of 1897

Acceptance of the electron

Given these differences, how did Thomson's corpuscle theory become accepted and transmuted into the later electron?

If we return to our two accounts, there is more general agreement about the acceptance of electron theory than about its origin, but still some significant differences. Figures 4 and 5 summarise the accounts.

Evidence	**Worker**	
	Cavendish	**Elsewhere**
m/e for cathode rays		Lenard Kaufmann
m/e for Lenard rays		Lenard
Velocity of cathode rays		Wiechert
m/e for photoelectric carriers	Thomson	Lenard

Ionisation by incandescent metals	Thomson McClelland H A Wilson Richardson Owen	Branly Preece Fleming
Ions in flames	H A Wilson Gold	
No. ions in a conduction gas	Thomson Rutherford Zeleny McClelland McLennan Richardson H A Wilson Owen	Lenard Righi Beattie De Smolan
Mobilities of ions	Townsend Zeleny	
Measurement of e	Thomson H A Wilson	

Figure 4. Lodge's account of the acceptance of the electron

Evidence	**Worker**	
	Cavendish	**Elsewhere**
m/e for cathode rays	Thomson	Wiechert Aschkinass Kaufmann Lenard Des Coudres
Suggestion cathode rays are electrons		Wiechert
Metallic conduction		Riecke Drude
m/e for photoelectric carriers		Lenard
Gaseous conduction	Thomson et. al.	
Measurement of e	Thomson	
m/e for β rays		Becquerel Dorn Kaufmann
Electromagnetic view of nature	Thomson	Lorentz

	Wien

Figure 5. Kaufmann's account of the acceptance of the electron

We have two aspects of electron theory to consider. First, the electric particles of Lorentz and Larmor, whether ethereal or not, which explained optical phenomena, and second Thomson's corpuscle which also explained atomic structure.

Lorentz and Larmor both had theories of far reaching implication, but a dearth of definite experimental evidence to back them up. They had both already seized on Zeeman's results as support for their theory and were seeking further support.[47] Thomson's measurement of the mass to charge ratio for cathode rays provided this. George FitzGerald realised the implications for Larmor's theory immediately. Writing in the same issue of *The Electrician* in which Thomson's results were published, he suggested that Thomson's measurements be reinterpreted as showing that cathode rays were 'free electrons.[48]

Thus FitzGerald rejected the importance of corpuscles for atomic structure and shifted the context of Thomson's results to Larmor's electron theory. He ensured that the term 'electron' was associated with Thomson's experimental work several years before full assent to Thomson's theory. That 'electrons' were originally proposed as an alternative interpretation of the cathode ray results to 'corpuscles' was forgotten.

The Continental situation was similar, except that here Thomson was seen as just one of many who determined the mass to charge ratio for cathode rays, and not necessarily the most reliable. Kaufmann's measurements were generally deemed the most accurate.[49] Kaufmann credits Emil Wiechert with first suggesting that the cathode ray particles and Lorentz's ions were the same.[50] For Lorentz, the existence of a direct means of experimenting on ions was immensely significant, and he re-cast his whole theory in terms of individual particles, now called 'electrons', rather than averages over many ions.[51]

What both accounts show is that the ultimate success of Lorentz and Larmor's electron theories depended on their potential for unification. A wide variety of hither to unrelated experimental phenomena could be encompassed. And the suggestion that *all* matter might be electromagnetic in origin, first made by Larmor, promised fundamental advances in physics. Kaufmann stated, 'Although much may appear hypothetical, it is clear... that these electrons are one of the most important foundations of our whole world structure,' while Lodge, ever more florid in style, agrees that, 'We are now beginning to have some hope of obtaining unexpected answers to riddles – such as those concerning the fundamental properties of matter – which have proposed themselves for solution throughout the history of civilisation.'[52]

Both accounts suggest that Thomson played a major role in achieving this unification. Throughout the diverse branches of physics which were brought within the orbit of electron theory, Thomson's name crops up as having made significant contributions. Philip Lenard is the only other physicist whose name occurs so universally, and it is noteworthy that Lenard received his Nobel Prize in 1905 for his work on cathode rays the year *before* Thomson received his for his work on 'conductivity of gases'. Neither citation mentioned electrons.

The major difference between the two accounts is the importance they assign to other work on gaseous conductivity, largely done at the Cavendish. For Lodge, the idea of an electron had arisen from investigations of gaseous conduction. Electron theory and Thomson's conductivity theory were mutually self-supporting; the success of one depended critically on the success of the other. For Kaufmann gaseous conduction was merely another corroboration of a theory derived from, and supported by, advances in electrodynamics.

This difference shows most clearly in their attitude to Thomson's experiment of 1898 which measured the charge on a gaseous ion, and later a photoelectric particle, directly.[53] For the British, two lingering doubts had remained: for Thomson, that the small value of the mass to charge ratio might be due as much to a large charge as to a small mass;[54] for FitzGerald, Larmor and probably Lodge, that the corpuscle might not be the same as the electron.[55] When Thomson established for the first time the actual value of the charge, all doubts as to the smallness of the mass, and the equality of charge on corpuscle and electron, were removed. His results were later refined by his student H A Wilson.[56]

This experiment was, for the British, so fundamental that Lodge wrote, 'it seems to me one of the most brilliant things that has recently been done in experimental physics. Indeed I should not need much urging to cancel the 'recently' from this sentence....'[57]

Kaufmann, conversely, dismisses the experiment with a one-liner, 'J J Thomson has even succeeded by observation of conducting gases in measuring the absolute magnitude of the charge of a single ion, and found good agreement with the elementary quantity previously obtained.'[58] He added that Planck had also derived the charge from black-body radiation. Kaufmann evidently felt the value of the electronic charge sufficiently well established from electrolysis.

The experiment appears to have had significance only for the British. Ramsay was still stressing it in 1912, as was O W Richardson in 1916. For the Continentals, however, it seems unimportant. In his *Theory of Electrons* of 1909, Lorentz did not discuss it at all.[59]

Thus, the first aspect of Thomson's corpuscle, that it was a very small electrified particle, seems to have been accepted very readily, explicitly because it supported Lorentz and Larmor's theories. Disagreement continued over whether the particle was material or ethereal and how it was structured. This difference was brought into focus when Kaufmann actually attempted to discover whether the electron had purely electromagnetic inertia.[60] He measured the masses of beta rays travelling at various velocities approaching that of light and compared them with theoretical values for electromagnetic inertia developed by Thomson and O Heaviside. He initially used G Searle's model of the electron as a spherical shell over which charge is uniformly spread, and obtained the result that only ¼ to 1/3 of the mass was electromagnetic. Dissatisfied with this result Max Abraham revised Searle's analysis, on the assumption that the electron was a conducting sphere. Thomson, also, took up Kaufmann's results, but applied his own ideas, treating the particle as a mathematical point (the centre of the tubes of force). Both Abraham and Thomson found the entire mass to be electromagnetic. This result was physically preferable because, to quote Lodge, 'it enables us to progress and is definite',[61] and Kaufmann revised his analysis. Interestingly, Thomson's own ideas vacillated on this point, and by 1907, while agreeing that the corpuscle had purely electromagnetic mass, he emphatically refused to speculate about its ethereal structure or about the distinction between matter and non-matter.[62]

What of the second aspect of Thomson's corpuscle, that it was a building block of a divisible atom? This was much harder for physicists to entertain. It is not clear from Lodge's account at what point he, and the British, did accept it. It is evident, however, that initially they rejected it. A divisible atom smacked of alchemy. If corpuscles were a building block of a divisible atom, then their production involved disrupting or dissociating the atom, and it seemed that this should change the chemical nature of the atom and also allow the re-aggregation of corpuscles into new atoms. FitzGerald was clear that this was his objection to the corpuscle theory, writing that the free electron hypothesis 'is somewhat like Prof. J J Thomson's hypothesis, except that it does not assume the electron to be a constituent part of an atom, nor that we are dissociating atoms, nor consequently that we are on the track of the alchemists.'[63]

Thomson's experiments were sufficient to support electron theory, with which they intersected neatly, but not to establish corpuscle theory. An editorial in *The Electrician* on 2 July 1897 bears this out. It acknowledges the implications of corpuscle theory, but would 'wish to see the hypothesis verified at an early date by some crucial experiment.' Such an experiment was not forthcoming, at least from Thomson.

While the increasing power of electron theory added prestige to Thomson's experiments, physicists remained uncertain about the constituent role of corpuscles in atoms. Indeed Lodge in 1906 seems totally confused, writing, 'While the units of negative charge appear in some cases with a separate existence, - perhaps carrying with them part of the atom, in which case they might be called corpuscles, having a material nucleus; perhaps pure disembodied electricity, whatever that may be – an electrical charge detached from matter – a mere complexity in the ether, in which case they would correspond with those hypothetical entities familiar in theoretical and mathematical treatment as 'electrons'.'[64]

There are three things to note about this quotation. First that Lodge deems electrons 'familiar' while corpuscles were not. Second, and most significant here, that he still has not understood the distinction between Larmor's 'electrons' and Thomson's 'corpuscles', nor the constituent role of corpuscles. Despite his advocacy of the electronic theory of matter, he here divorces electrons from the matter of which Larmor claimed they were the origin. He speaks of negative charges 'carrying with them' some part of the atom, rather than actually of *being* an integral part of the atom as Thomson would have it. Third, Lodge was unable to make his attempted distinction stick, and failed to adhere to it through the rest of the book, betraying further confusion.

It appears that even in 1906 and in Britain, the corpuscle's constituent role was far from firmly established, and Thomson's theory might have disappeared into oblivion were it not for the discovery of radioactivity. Becquerel showed that beta rays could be deflected magnetically, and Dorn demonstrated their electric deflection. Becquerel, and then Kaufmann himself (not Thomson) showed that their mass to charge ratio was the same as for cathode rays, thus identifying them with electrons or corpuscles.[65]

Kaufmann's account suggests that this was a turning point.[66] Here was the crucial evidence that atoms might emit corpuscles without any external influence. Corpuscles were not an artefact of the interaction of atoms and the electric field, but must have been contained *within* the atom. Equally important, in 1903 Rutherford and Soddy argued that in radioactive decay atoms *did* change their chemical nature.[67] Physicists *were* on the track of the alchemists. Thus the corpuscle's constituent role was finally accepted, although by now it was almost universally known as an 'electron', and this terminology stuck. Indeed, Kaufmann's beta ray experiments gave additional momentum to electron theory, enabling his experiments on electromagnetic mass referred to earlier. These ensured the success of the electromagnetic view for several years to come.

Conclusion

To conclude, the story I have been telling traces two parallel and apparently quite similar theoretical developments by Lorentz and Larmor (although Larmor's is now largely submerged). Yet they were based on fundamentally different concepts of nature. Intertwined was a series of experiments which were ultimately successful largely because they go hijacked by both theoretical camps. The *existence* of the phenomena demonstrated by Thomson was sufficient evidence for Lorentz and, especially Larmor, but the *quality* of the experiments was not sufficient to establish Thomson's own corpuscular theory in opposition to the electron theories. The potential unifying power of the electromagnetic view of nature concentrated attention on the electron's charge and mass, and these became its defining characteristics.[68] The one respect in which Thomson does seem to have been before others is in deflecting cathode rays electrostatically. His crossed field e/m method, said to involve fewer assumptions than Wiechert's or Kaufmann's original measurements, came to exemplify the new physics.

In this process, a significant historical contingency is that Lodge's account, which set the tone for many later histories, was delivered to the Institution of Electrical Engineers. As Gooday points out, electrical engineers were a far larger community than academic physicists and were also intimately familiar with the history and potential of vacuum tube technology.[69] Lodge's decision to present the electron development through a familiar technology rather than a more abstruse theoretical path was well received and perpetuated by a wide audience. Thus, even 'acceptance' begins to look more complex than it at first seemed for, as well as the background concepts of the author, we have to take into account the potential influence of the intended audience.

Both accounts agree that cathode rays were particularly compelling evidence for the existence of electrons. Even Kaufmann, who placed the reality of electrons prior to 1897, considered that, 'We have in the cathode rays the electrons – which in optical phenomena lead a somewhat obscure existence – bodily before us so to speak.'[70] In Britain, the first to produce this evidence was Thomson, while in Germany Wiechert performed a similar role. That Wiechert is now largely forgotten while Thomson is remembered as 'the discoverer of the electron' is due to more than the contingency that Thomson had a large and increasingly powerful group of former research students who extolled his work. It is due in part to the nature of Thomson's corpuscle suggestion. In speculating about the role of the corpuscle in the structure of the chemical atoms, Thomson initiated a research programme in subatomic physics among these students which was to dominate British physics in the first half of the twentieth century. By the 1920s the ethereal concepts in which Thomson's work was founded were outmoded, yet his ideas underpinned subatomic physics, and his successors needed to justify their belief in them. His students, unable to accept his concepts, transformed his experiments into a paradigm of pure physics research. They thus used his cathode ray work to make their own enterprise acceptable (and fundable).[71]

Ultimately Thomson's corpuscle added an important property to electron theory, expanding its evidential context to the chemical atom.[72] But the accurate, precise, and sometimes crucial, experiments, were done by many different workers. The weight attached to these experiments depended on the differing metaphysical orientation of the physicists concerned and highlights the interplay of the differing traditions.

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[1] An earlier version of this paper has appeared in D Hoffmann, F Bevilacqua, R Stuewer (eds), *The emergence of modern physics,* Proceedings of a Conference, Berlin 22-24 March 1995, (1996) Pavia, pp217-232

[2] J J Thomson, 'Cathode-rays', *Electrician*, 39 (1897), 104-109

[3] This account has been much criticised in recent years, in particular in T Arabatzis, 'Rethinking the 'Discovery of the Electron' *Studies in the History of Modern,* 27 (1996), 405-435; N Robotti and F Pastorino, 'Zeeman's discovery and the mass of the electron', *Annals of Science,* 55 (1998), 161-183; and by Graeme Gooday (elsewhere n this volume)

[4] 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

[5] Note that, while I avoid the issue in this pacer, I am not advocating abandoning the attempt to define 'discovery'. The classic formulation of an attributional model of discovery is A Brannigan, *The social basis of scientific discoveries*, (1981) Cambridge; developed in S Schaffer, 'Scientific discoveries and the end of natural philosophy', *Social Studies of Science*, (1986), 367-420, who situates discovery accounts firmly in the 'local practices of contemporary research communities' (quote from abstract). This model is implicitly adopted by Gooday (elsewhere in this volume) who does, however, document one of the problems with it, the possibility that 'local practices' may further subdivide into individual practices, and the whole account become too messy to be useful, a danger indicated in S Shapin 'Discipline and bounding: The history and sociology of science as seen through the externalism-internalism debate', *History of Science*, 30 (1992, 333-369, esp. 353-354). Arabatzis has tried to avoid this problem by concentrating on the realism of the entity discovered rather than the actor making the discovery. In his account discovery Is still socially negotiated but 'an entity has been discovered only when consensus has been reached with respect to its reality' (op ct (3), on p406). While side-stepping some of the problem by providing a *terminus ad quem* for a discovery rather than attempting to pin-point a specific locus at which it was made, his approach seems to me still fraught with the difficulty of individualism when different scientists attach different concepts to the same word, eg 'electron'. Conversely, if the consensus of all is required, then significant differences in local practice may be lost

[6] J J Thomson, 'Or the masses of the Ions in gases at low pressures', *Philosophical Magazine*, 48 (1899), 547-S67, *Recollections and Reflections* (1936), London: Bell; for a fuller account of the proceedings at the British Association Meeting see 'Physics at the British Association', *Nature*, 60 (1899) 585-587; and G Gooday (elsewhere in this volume)

[7] J Buchwald, *From Maxwell to microphysics* (1985) Chicago: University of Chicago Press; R McCormmach, 'Lorentz and the electromagnetic view of nature', *Isis* 61 (1970), 459-497; 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 (1991), 29-91; Warwick distinguishes sharply between Lorentz's

electromagnetic view of nature and Larmor's electronic theory of matter, a distinction based on the foundational role Larmor assigned to the ether. Robotti implies that it was Thomson's measurement of the electronic charge independently of the mass that swayed the argument by 1900; N Robotti, 'J J Thomson at the Cavendish Laboratory: the history of an electric charge measurement', *Annals of Science* 52 (1995) 265-284

[8] The institutional basis of the German electrodynamic tradition is extensively described in C Jungnickel and R McCormmach, *Intellectual mastery of nature; theoretical physics from Ohm to Einstein,* 2 vols (1986) Chicago. The Maxwellian tradition in both Britain and Germany is described in Buchwald, op. cit (7), and within Britain in B Hunt, *The Maxwellians* (1991) Ithaca. Buchwald and Hon have both given detailed studies of episodes in the history of cathode rays or the electron which show how experiments may mean different things in different traditions: J Buchwald, *The creation of scientific effects* (1994) Chicago, especially chapter 10; 'Why Hertz was right about cathode rays,' in J Buchwald (ed), *Scientific Practice,* (1995), Chicago pp151-169; G I-Ion, 'Is the identification of experimental error contextually dependent? The case of Kaufmann's experiment and its varied reception', ibid, pp170-223; 'H. Hertz; "The electrostatic and electromagnetic properties of the cathode rays are either nil or very feeble" (1883): A case study at an experimental error', *Historical Studies in the Physical Sciences,* 18 (1967), 367-382

[9] 0 Lodge, *Electrons,* (1907) London: Bell

[10] W, Kaufmann, 'The development of the electron idea', *Electrician,* (8 Nov 1901), 95-97

[11] P Achinstein, 'Who really discovered the electron', (this volume). Achinstein suggests three components for discovery: the existence of that discovered; the knowledge that it exists, and priority. The problem is in defining who 'knows' that the entity exists

[12] Lodge, op cit (9), p17

[13] Discussed in J O'Hara, 'George Johnstone Stoney and the conceptual discovery of the electron', in *Stoney and the electron* (1993) Royal Dublin Society, 5-28

[14] This passage shows the reconstructive nature of Lodge's work. Although Lenard had stressed that cathode rays did pass through metal foil this was not clear to many other physicists, J,J. Thomson included. A large part of Thomson's April 1897 lecture (op. cit (2)) was devoted to showing that Lenard rays could be a secondary effect, and as late as 1900 he was surprised that beta particles could pass through matter: P. Lenard, 'Uber Kathodenstrahlen', *Verhandlunger der Gesel!schaft Deutsche Natforscher und Ärzte* 2 (1893), 36; 'Uber Kathodenstrahlen in Gaser von atmospharischern Druck und im aussersten Vacuum' *Annalen der Physik und Chemie* 51 (1894), 225; 'Uber die magnetische Ablenkung der Kathodenstrahlen', *Annalen der Physik und Chemie* 52 (1894), 23; Rayleigh, *The life of Sir J J Thomson* (1942) Cambridge, on p 133. Moreover, Townsend's experiments on the viscosity of ions post-dated Thomson's on the size of cathode rays: J S Townsend, 'The diffusion of ions into gases' *Philosophical Transactions of the Royal Society* 193 (1899), 259

[15] Lodge, op. cit. (9) on p39

[16] Thomson, op. cit. (2)

[17] Lodge, op. cit. (9) on p47

[18] J J Thomson, 'Cathode rays', *Philosophical Magazine* 44 (1897), 269-316v

[19] My account of Lodge's book differs from Graeme Gooday's (this volume). Gooday states that 'Lodge attached no special place to the contribution of J J Thomson... except in his prefatory dedication.' While agree that Lodge does not identify Thomson as the electron's discoverer I argue that he does give Thomson very considerable pride of place. While Schuster Is described as 'skilful', Kaufmann as having merely skill, and Lenard as 'indefatigable', Thomson's work is variously described as 'brilliant', 'epoch-making', and 'remarkable', Even more important in forming later accounts is the fact that Lodge describes the route to the electron via a cathode ray research programme *first;* Zeeman's work is relegated to later in the book and, in a remarkable piece of reconstruction to later in *time.* Lodge places Zeeman's 1896 calculation of e/m for the charged particle causing the Zeeman effect several months after Thomson's April 1897 determination of *elm* for cathode rays (p112). This is noteworthy because in 1897 Lodge himself was the chief publiciser of Zeeman's work in Britain, arranging for publication of Zeeman's papers in English, and had himself verified both the

effect and the e/m calculation, all *before* April 1897: *Nature,* 55 (1896), 192; P Zeeman, 'On the influence of magnetism on the nature of the light emitted by a substance' *Philosophical Magazine* 43 (1897), 226-239; 0 Lodge, 'The latest discovery in physics' *The Electrician,* 38 (1897), 566-570; 'The influence of a magnetic field on radiation frequency', *Proceedings of the Royal Society* 60 (189 7), 513-514; 'A few notes on Zeeman's discovery', *The Electrician,* 38 (1897), 643. For a discussion of this work see: Arabatzis, op cit (3), especially p423; and Robotti and Pastorino, op. cit. (3) especially pp172-175

[20] For a modern assessment of this period in German electrodynamics see O Darrigol, 'The electrodynamic revolution in Germany as documented by early German expositions of 'Maxwell's Theory', *Archive for History of Exact Sciences*, 45 (1993), 189-280; Buchwald op cit (7)

[21] Discussed in B Carazza and N Robotti, 'The first molecular models for an electromagnetic theory of dispersion and some aspects of physics at the end of the nineteenth century', *Annals of Science,* 53 (1996), 587-607; O Darrigol, 'The electron theories of Larmor and Lorentz: a comparative study', *Historical Studies in the Physical and Biological Sciences* 24 (1994) 265-336; Buchwald, op. cit. (7)

[22] See Carazza and Robotti op. cit. (21); Robotti arid Pastorino, op. cit. (3)

[23] Kaufmann, op. cit. (10), p96

[24] Kaufmann, op. cit. (10), p96. In this context it is worth noting Carazza and Robotti's observation that, 'none of the authors [of German dispersion theories] examined seems worried about a direct comparison of the dispersion formula obtained with specific experimental data… their prime interest is that of demonstrating the feasibility of a dispersion theory founded on electromagnetism, capable of acknowledging at first glance the qualitative aspects of the phenomenon,' (Carazza and Robotti, op. c:t. (21), p607)

[25] Kaufmann, op. cit. (10), p97

[26] Although Lodge does mention Larmor, his role is theoretical elucidation of experimental phenomena; his ideas are not presented as a coherent whole. The work of Lorentz and Larmor, and the influence of the traditions within which they were working, is compared very directly in Darrigol, op cit. (21)

[27] Buchwald, op. cit. (7); Darrigol, op. cit. (21); Jungnickel and McCormmach, op. cit. (8); McCormmach, op. cit. (7). A comparison of styles of materialism in Britain, France and Germany around the turn of the century may he found in H, Kragh, 'The new rays and the failed anti-materialistic revolution', in D Hoffmann, F Bevilacqua and B Stuewer, *The emergence of modern physics,* Proceedings of a conference, Berlin 22 March 1995 (1996) Pavia, p6l-77

[28] H A Lorentz. 'La théorie électromagnetique de Maxwell et son application au corps mouvants', *Archives Néerlandaises* 25 (1892), 383. Although, as Darrigol is at pains to point out. Lorentz was not German, and took elements from many different traditions, he did base his electrodynamics on Helmholtz's exposition of Maxwell, and this places him firmly in the German tradition in this respect: Darrigol, op cit (21). Helmholtz's interpretation of Maxwell and the influence this had on German electrodynamics are discussed in Buchwald, op. cit. (7), especially chapters 21, 22

[29] H A Lorentz, 'Optical phenomena connected with the charge and mass of the ions' (1898), in P Zeeman and A D Fokker (eds), *H A Lorentz Collected Papers,* 9 vols (1935-39), The Hague, vol 3, pp12-39

[30] McCormmach, op. cit. (7)

[31] Buchwald, op. cit. (7)

[32] J. Larmor, 'A dynamical theory of the electric and luminiferous medium', *Philosophical Transactions of the Royal Society* Part I 185 (1894) 719-822; Part II 186 (1895), 695-743; part III 190 (1897), 205-300; these papers are also contained in J Larmor, *Mathematical and Physical Papers,* 2 vols (1929) Cambridge, Part I (vol 1) 414-535; Part II (vol. 1) 543-597; Part III (vol 2) 11-132; references will be to *Mathematical and Physical Papers.* For detailed accounts of Larmor's work see: Buchwald, op. cit. (7); Darrigol op. cit. (21); Hunt, op. cit. (8); Robotti and Pastorino op. cit. (3); Warwick op. cit. (7). These accounts all point out the close relationship between Larmor and FitzGerald and, through him, with Stoney

[33] Buchwald, op. cit. (7) chapters 19, 20

[34] Larmor, 'Dynamical theory' Part II, op. cit. (32), p566

[35] eg '… the consideration of groups of electrons or permanent strain centres in the aether, which form a part of, or possibly the whole of, the constitution of the atoms of matter,…' (Dynamical theory' Part II, op cit (32) p543, (May 1895)); '… In this medium [the ether] unitary electric charges, or electrons, exist as point singularities, or centres of intrinsic strain, which can move about under their mutual actions; while atoms of matter are in whole or in part aggregations of electrons in stable orbital motion.' ('Dynamical theory' Part III, op. cit. (32) p12, (April 1897)); '… The quest ion is fundamental how far we can proceed in physical theory on the basis that the material molecule is made up of revolving electrons and of nothing else (J Larmor, 'The Influence of a magnetic field on radiation frequency', *Proceedings of the Royal Society* 60 (1897) 514 (February 1897); in *Mathematical and Physical Papers.* vol 2, op. cit. (32) 138-139). For Larmor's progress towards a completely electronic theory of matter see Warwick, op. cit. (7).

[36] Larmor, 'Dynamical theory' Part I. op. cit. (32), p523-24

[37] J Larmor, 'On the theory of the magneto influence on spectra and on the radiation from moving ions', *Philosophical Magazine* 44 (1897), 503-1.2; also *Mathematical and physical papers* vol 2 op. cit. (32) 140-149. For a detailed account of this episode, see Robotti and Pastorino, op. cit. (3). Here Larmor reserves 'electron' for charged particles with purely electromagnetic mass, using 'ion' for those that might be attached to inertial matter. This continues a distinction he had made n 1894 (Larmor 'Dynamical theory' Part I, op. cit. (32), p523)

[38] Thomson's interest in chemistry was first evident in his *Treatise on the motion of vortex rings* (Adams prize essay 1882, published London 1883). It continued in a series of papers on discharge (see Falconer, op. cit. (4) and references cited therein). A clear example of the different interests of Thomson and Larmor came in 1894. In elaborating his theory of vortex 'monads', the precursors of 'electrons', Larmor invoked Prout's hypothesis of primordial atoms**.** He noted as a difficulty, 'why the molecule say of hydrochloric acid is always H÷Cl-, and not sometimes H-Cl+', but dismissed it as not worthy of consideration; 'This difficulty would however seem to equally beset any dynamical theory whatever of chemical combination which makes the difference between a positive and a negative atomic charge representable wholly by a difference of algebraic sign (Larmor, 'Dynamical theory' Part I, op. cit. (32) p475). At the same time, Thomson was elaborating a theory of atoms and electric charge which had precisely this aim: to explain the different electric affinities of hydrogen and chlorine (J J Thomson, 'The relation between the atom and the charge of electricity carried by it', *Philosophical Magazine* 40 (1895) 511-544

[39] Buchwald, op. cit. (7)

[40] J J Thomson, 'On the illustration of the properties of the electric field by means of tubes of electrostatic induction', *Philosophical Magazine* 31 (1891), 150-171. The theoretical ramifications of this, and Thomson's associated theory of conduction, are discussed in Buchwald, op. cit. (7)

[41] L. Blake, 'Uber Eiectricitatsentwikelung bei der Verdampfung ...', *Annalen der Physik und Chemie,* 19 (1B83), 518, translation, *Philosophical Magazine* 16 211-224; L Sohncke, 'Beitrage zur Theorie der Luftelectricitat', *Annalen der Physik und Chemie,* 34 (1888), 925

[42] Thomson, op. cit. (38)

[43] J J Thomson, 'Referee's report on Larmor's paper', Royal Society MSS (6 February 1894) RR12.160; (9 June 1897) RR13.2O7

[44] J J Thomson, 'Notes for Princeton Lectures' (1896), Cambridge University Library MS ADD 7654 NB40

[45] Thomson, op. cit. (2)

[46] Thomson, op. cit. (18)

[47] Larmor, 'Influence of a magnetic field on radiation frequency', op. cit. (35); 'Magnetic influence on spectra', op. cit. (37); H A Lorentz, 'Uber den Einfluss magnetischer Krafte auf die Emission des Lichtes', *Annalen der Physik* 63 (1897) 278-284; Zeeman, op. cit. (19)

[48] G F FitzGerald, 'Dissociation of atoms', *Electrician* 39 (1897), 103-104

[49] W Kaufmann, 'Die magnetische Ablenkbarkeit der Kathodenstrahlen und ihre Abhangigkeit vom Entladungspotential', *Annalen der Physik und Chemie* 61 (1897), 544; 62 (1897), 596. Both the Curies, and Nicholas Oumoff (professor of physics at Moscow), recommended Kaufmann and Thomson jointly for the

Nobel Prize, extolling the quality of Kaufmann's experiments: 'Bien que les travaux de J.J. Thomson soient plus nombreux que ne le sont ceux de W. Kaufmann et embrassent un p!us grand nombre de phénomènes,… nos connaissances dans ce domaine de physique n'auraient pas atteint en ce moment leur niveau actuel sans les recherches de W. Kaufmann….' (Oumoff to the Nobel Foundation 31 January 1904, Nobel Foundation archives); 'Ces conceptions théorique on reçu diverses confirmations parmi lesquelles nous citerons les recherches récentes de Mr Kaufmann ' (P. and M. Curie to the Nobel Foundation 26 December 1904, Nobel Foundation archive)

[50] E Wiechert, 'Ergebniss einer Messung der Geschwindigkeit der Kathodenstrahlen', *Schriften der physikalischökonomisch Gesellschaft zu Königsberg* 38 (1897), 3. Wiechert was, at the time, Privatdozent at Königsberg, but moved in 1897 to the observatory at Göttingen, subsequently establishing a world famous school of geophysics there

[51] Lorentz, op. cit. (29); McCormmach, op. cit. (7)

[52] Kaufmann, op. cit. (10), p97; Lodge, op. cit. (9), pxv

[53] J J Thomson 'On the charge of electricity carried by the ions produced by Röntgen-rays', *Philosophical Magazine* 46 (1898), 528-545; 'On the masses of the ions in gases at low pressures', *Philosophical Magazine* 48 (1899), 547-567

[54] Thomson made this suggestion in his classic cathode ray paper of 1897, basing. it on evidence from the specific inductive capacity of gases, Thomson, op. cit. (18)

[55] Robotti, op. cit. (7)

[56] H A Wilson, 'A determination of the charge on the ions produced in air by Röntgen rays', *Philosophical Magazine* 5 (1903), 429-441

[57] Lodge, op. cit. (9), p79

[58] Kaufmann, op. cit. (10), p97

[59] H A Lorentz, *Theory of electrons,* (1909) Leipzig: Teubner**;** W Ramsay, *Elements and electrons,* (1912) London: Harper; O W Richardson, *The electron theory of matter* (2nd edn), (1916) Cambridge: Cambridge University Press

[60] W Kaufmann, 'Die magnetische und elektrische Ablenbarkeit der Becquerelstrahlen und die scheinbare Masse der Elektronen, *Göttingen Nachrichten,* (1901) 143-145. This whole episode, and the evidence it supplies for the interaction between theoretical and experimental interpretation is well discussed In Hon, op. cit. (8)

[61] Lodge, op. cit. (9), p96; M, Abraham, 'Prinzipein der Dynamik des Elektrons', *Annalen der Physik* 10 (1903), 105-179; A H Bucherer, 'Mathematische Einführung in die Elektronentheorie', (1904) Leipzig: Teubner

[62] J J Thomson, *The Corpuscular Theory of Matter,* (1907) London: Constable pp1-2, 28

[63] FitzGerald, op. cit. (48), p104

[64] Lodge, op. cit. (9), p69

[65] H Becquerel, 'Influence d'un champ magnetique sur les rayonnements des corps radio-actifs', *Comptes Rendus* 129 (1899) 996-1001; 'Contribution a l' étude du rayonnement du radium**',** *Comptes Rendus* 130 (1900) 206-211; E Dorn, *Abhandlungen der Naturforschenden Gesellschaft in Halle* 22 (1900), 47; *Physikalische Zeitschrift* 1 (1900) 337; Kaufmann, op. cit. (60)

[66] Kaufmann, op. cit. (10), p97

[67] E Rutherford and F Soddy, 'Radioactive Change', *Philosophical Magazine* 5 (1903), 576-591

[68] That these were not, of necessity, the cathode rays' defining characteristics can be seen by considering the amount of time Thomson devoted in his Royal Institution lecture to the chemical and thermoluminescent changes caused by cathode rays, and by Paul Villard's attempts to define them by their chemical reducing properties: B Lelong 'Paul Villard, J J Thomson, and the composition of cathode rays', paper given at the BSHS meeting 'The Electron: 100 years of physics and history', The Royal Society, 11-12 April 1997

[69] Gooday (this volume)

[70] Kaufmann, op, cit. (10), p97

[71] Alan Morton has shown the way Thomson's e/m tube was used to exemplify pure physics in the British Empire Exhibition of 1924, where it formed the centrepiece of the physics section. This section was put together by Blackett and Chadwick, two of Rutherford's closest students. A Morton, talk given at the Dibner Workshop 'The Birth of Microphysics', May 1997. Rutherford was, of course, Thomson's most powerful student, held to a particularly empirical philosophy, and seems influential in the transformation of accounts of Thomson's work.

[72] Mary Jo Nye suggests that Thomson's lecture*s* at Yale in 1903, in which he not only outlined his atomic model, but also discussed the role of Faraday tubes in chemical bonding, was influential in stimulating the adoption of the electron into chemistry: M J Nye: 'Remodeling a classic: The electron in organic chemistry, 1900-1940', (this volume)