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J. J. Thomson and the Electron, 1897–1899

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What, precisely, did J. J. Thomson contribute to the discovery of the electron?

Because the electron was "discovered" in 1897, one naturally takes this to be a question about what Thomson claimed pertaining to the electron during 1897, and hence a question about his April 30 Friday Evening Discourse on cathode rays at the Royal Institution,[1] in which he first put the subatomic proposal forward, and his subsequent classic paper "Cathode Rays" in the October issue of *Philosophical Magazine*.[2] Restricting the question to 1897, however, gives one a seriously incomplete and consequently misleading answer to the question of what Thomson contributed. Further, it gives a picture of what he and his research students at the Cavendish Laboratory were up to at the time that they would have had trouble recognizing. Thomson's contribution to the discovery of the electron stretched over the next two years as well. His 1897 paper is the first in a sequence of three equally classic *Philosophical Magazine* papers presenting fundamental experimental results on the electron: the second, "On the Charge of Electricity carried by the Ions produced by Röntgen Rays," appeared in December 1898,[3] and the third, "On the Masses of the Ions in Gases at Low Pressures," in December 1899.[4] The last five pages of this 1899 paper put forward a new account of ionization and electrical conduction in gases. These five pages culminated Thomson's efforts on the electron. The purpose of the present chapter is to answer the question of what Thomson contributed by considering these three papers together, taking them as presenting consecutive results of a research effort on "the connexion between ordinary matter and the electrical charges on the atom"[5] that began taking shape in 1896.

The key experiments in the 1897 *Philosophical Magazine* paper proceeded from the working hypothesis that cathode rays consist of negatively charged particles to two complementary measures of the mass-to-charge ratio, m/e, of these particles. Thomson's data, however, were less than perfect, with more than a factor of 4 variation in the m/e values he obtained. Moreover, he was not alone in publishing m/e values for cathode rays in 1897. Emil

Wiechert had announced more or less the same value on 7 January 1897 in a talk in Königsberg,[6] weeks before Thomson, and Walter Kaufmann published a value that proved, in hindsight, to be more accurate than Thomson's.[7] The only way in which Thomson's experiments might be said to have accomplished more than Wiechert's and Kaufmann's lay in his offering two complementary measures of *m/e* for cathode rays and in his confirming so extensively that this quantity does not vary with the cathode material or the residual gas in the tube.

Thomson also differed from Wiechert and Kaufmann in the emphasis he put on the proposal that cathode rays consist of particles.[8] Indeed, Thomson's 1897 paper and his earlier talk both give the impression that his primary aim was to settle a dispute over whether cathode rays are particles, the view favored in Britain, or some sort of etherial process, the view favored on the Continent. The paper did achieve this aim, for within months opposition to the particle view died. In point of fact, however, the issue over cathode rays was not drawing much attention at the time, and Thomson himself had not done much with cathode rays before late 1896 and did little with them after 1897.[9] To single out Thomson over Wiechert and Kaufmann for championing the particle view of cathode rays is to attach more importance to this issue than it probably deserves.

The second announced aim of Thomson's 1897 paper was to answer the question, "What are these particles?" The increasing importance of this question to Thomson when writing the paper becomes clear from comparing it with the text of his April talk. George FitzGerald's commentary on this talk had focused almost exclusively on the proposal that these particles are subatomic.[10] Partly in response to this commentary, the paper advanced considerably more evidence than the talk in support of subatomic "corpuscles." Thomson was unique in drawing this conclusion from the 1897 *m/e* values for cathode rays. Nevertheless, in contrast to the rapid acceptance of the particle view of cathode rays, the subatomic claim, while attracting a great deal of attention, was not accepted until after his December 1899 paper. Perhaps Thomson receives more credit than he deserves for putting this proposal forward in 1897. We need to ask, what exactly did the October 1897 paper show about the particles forming cathode rays, and what remained to be shown to provide compelling grounds that they are subatomic?

Thomson's 1897 paper ends with conjectures on the structure of atoms and the relationship between his subatomic corpuscles and the periodic table. As is widely known, over the next decade Thomson attempted to develop a "plum-pudding" model of the atom in which the negatively charged corpuscles are at rest in a configuration of static equilibrium within a positively charged matrix. The resulting widely held picture of Thomson's 1897 achievement is that he discovered the electron and then went off on a garden path on the structure of the atom, leaving to Rutherford in 1911 and Bohr in 1913, not to mention Millikan, the task of completing the project he had begun.

Taking Thomson's 1897 paper together with those from 1898 and 1899 gives a very different picture of what he accomplished. As noted, his central concern at the time was with "the connexion between ordinary matter and the electrical charges on the atom."[11] Electrical phenomena in gases provided his experimental means for getting at this connection. His 1897 paper gave a rough *m/e* value for cathode rays that was independent of the residual gas in the tube and the material of the cathode; this result pointed to a single carrier of negative charge that might well be ubiquitous. His December 1898 paper gave a rough value for the charge on individual ions in gases ionized by x-rays, concluding that it may well be the same as the charge per hydrogen atom in electrolysis. His December 1899 paper reported that the *m/e* of both the electrical discharge in the photoelectric effect and the electrical discharge from incandescent filaments is the same as the *m/e* of cathode rays he had obtained in 1897, and the *e* in the photoelectric effect is the same as the charge per ion in gases ionized by x-rays he had obtained in 1898. From these results, joined with those his research students had obtained on the migration of ions in gases, Thomson concluded that there is no positively charged counterpart to his corpuscle entering into electrical phenomena in gases. The 1899 paper ends by putting forward a new "working hypothesis" for electrical phenomena in gases in which the negatively charged corpuscle is universal and fundamental, ionization results from the dissociation of a corpuscle from an atom, and electrical currents in gases at low pressures consist primarily of the migration of corpuscles.

The three Thomson papers thus form a unit. The sequence of novel experiments reported in them replaced conjecture about the microstructural mechanisms involved in the electrification of gases with a new, empirically driven picture of these mechanisms. At the heart of this picture is an asymmetry of charge in the mechanism of electrification. This asymmetry, which stood in direct opposition to almost all theoretical work preceding it, was Thomson's most important and unique contribution to the discovery of the electron. Commentators have often pointed out that he received the Nobel Prize in 1906 not for the discovery of the electron but for his research on electricity in gases. Drawing a contrast between these two in this way misses the point made in the first two sentences of the preface to the first (1903) edition of his *Conduction of Electricity through Gases:*

> I have endeavoured in this work to develop the view that the conduction of electricity through gases is due to the presence in the gas of small particles charged with electricity, called ions, which under the influence of electric forces move from one part of the gas to another. My object has been to show how the various phenomena exhibited when electricity passes through gases can be coordinated by this conception rather than to attempt to give a complete account of the very numerous investigations which have been made on the electrical properties of gases.[12]

The work for which Thomson received the Nobel Prize was a direct extension from and elaboration of the "working hypothesis" he put forward at the end of the December 1899 paper. The central element of this working hypothesis, established experimentally through the efforts from 1897 to 1899, is the subatomic electron and its asymmetric activity.

There are four reasons why this picture of Thomson's efforts on the electron is of more than passing importance for both historians and philosophers of science. First, this episode is a striking example of research in which experiment took the lead and theory at best lagged behind and at worst acted as an impediment. The key experiments reported in Thomson's three papers and the many supporting experiments of his research students were not done for the philosophically standard purpose of testing theoretical claims. The aim of virtually every one of these experiments was to measure some quantity or other, generally a microphysical quantity. The goal of the experiments taken together was to develop enough data about what was happening microphysically to allow sense to be made of the large array of experimental phenomena involving electricity in gases that had been accumulating for over half a century. Theory offered no way of getting at many of the discoveries that came out of these experiments. In particular, the asymmetry in the action of charge at the microphysical level could not have been discovered except through experiment. The two pertinent theories at the time—Lorentz's theory of the electrodynamics of point charges and Larmour's theory of the aetherial electron—both assumed fully symmetrical activity of positive and negative charges. Episodes like this in which experiment is forced to take the lead have not received the attention they deserve, especially among philosophers.

Second, even setting aside the dominant role of experiment, this episode is an example of a kind of science that has not received enough attention, namely research in which an evolving working hypothesis substitutes for established theory. The fundamental problem in doing science is turning data and observations into evidence. High quality evidence is difficult to extract from data in the absence of established theory, for data rarely carry their evidential import on their surface, and the intervening steps in reasoning from them to evidential conclusions threaten to be too tenuous when not mediated by independently supported theory. This poses an obvious challenge for research in the early stages of theory construction in any domain. A common way of trying to surmount this challenge is to ask a working hypothesis to serve in place of theory in mediating steps in evidential reasoning, hoping to extend and develop the initial working hypothesis step by step in a bootstrap fashion into a reasonably rich fragment of a theory.

While Thomson drew heavily from both classic electromagnetic theory and the kinetic theory of gases in this research, the then available conjectural theories of the microphysics of electricity were failing to open the way for effective experimental investigations. The series of experiments that he and his research students carried out from 1896 to 1899 allowed him to develop his initially limited working hypothesis that cathode rays consist of negatively charged particles into the working hypothesis presented in the final pages of the December 1899 paper. One thing that makes this episode an especially instructive example of research predicated on an evolving working hypothesis is that so much was accomplished before the theory that was ideally needed began to emerge some fourteen years later, with the Bohr model.

Looking on Thomson's efforts on the electron during these years as science built off a working hypothesis carries with it a corollary on his research style in these efforts. The experiments he and his research students carried out had a "rough draft" character. The measured values obtained from them were at best approximate, usually indicating only the order of magnitude of the quantity under investigation. The key experiments were remarkably complex, requiring several separate measurements—each with their own problems—to be combined to obtain the targeted quantity. Admittedly, these experiments were groundbreaking not just in their gaining experimental access to the microphysics of electricity for the first time, but also in their adding a good deal of new experimental technology to laboratory practice. Even so, the variances in his results are large enough to prompt questions about whether Thomson should not have done more to perfect the experiments before publishing and moving on. As we shall see below, such questions reflect a lack of appreciation for the kind of science Thomson was engaged in. The experimental style he adopted in these efforts is entirely appropriate when the goal is one of further elaborating a working hypothesis. Trying to perfect experiments prematurely will more often than not be a waste of time; everyone will be in a much better position to refine them once more of a theory is in place. Just the opposite of being open to criticism for not doing more to perfect his experiments and leaving too much for others to clean up, Thomson should be praised for the judgment he showed in developing the experiments only to the point where they gave him what he needed to carry on.

The third reason why this episode is important for historians and philosophers of science is that the contrast between it and Thomson's efforts on the plum-pudding model of the atom underscores a crucial requirement in this kind of science: the empirical world has to cooperate for the research to get anywhere. It is sometimes suggested that Thomson's efforts on his plum-pudding model show him in decline as a scientist. To the contrary, he was engaged in exactly the same kind of science in his efforts on atomic structure, groping for a working hypothesis that would provide the logical basis for extracting conclusions from experimental results that could extend and refine this hypothesis. None of the variants of his plum-pudding model enabled such a bootstrap process to get off the ground. But then too he had tried several dead-end working hypotheses on the electrification of gases before cathode rays gave him one that turned out to be amenable to systematic experimental development. Criticizing Thomson for being unable to intuit the planetary structure that the subsequent experiments by Rutherford, Marsden, and Geiger revealed makes sense only if one thinks that the difference between great and mediocre scientists is some sort of clairvoyance. Perhaps instead we should praise Thomson, as we praise Rutherford and Bohr, for insight in recognizing the faint possibility that the empirical world might cooperate with a certain line of thought and for his ingenuity and diligence in marshalling experimental results in then developing this line of thought.

Fourth, considering Thomson's paper on cathode rays as just the first in a sequence of three seminal papers clarifies the way in which this paper marks a watershed in the history of science. Surely, the 1897 paper was a watershed, for it was the first time experimental access was expressly gained to a subatomic particle. When viewed from the perspective of twentieth century atomic physics, however, Thomson's cathode ray paper appears at most a minor initial breakthrough, of no more importance than the breakthroughs of Becquerel, the Curies, and Rutherford during the next few years. Modern atomic physics appears to derive far more from Rutherford's 1911 and Bohr's 1913 papers than from Thomson's 1897 paper. This is true. What made Thomson's paper a watershed is not that it initiated modern atomic and elementary particle physics. It was a watershed because, together with the papers of the next two years, it freed the investigation of phenomena of electrical conduction, in metals and liquids as well as in gases, from aether theory and questions about the fundamental character of electricity. As such, it marked the end of the period Jed Buchwald describes in *From Maxwell to Microphysics*[13] and the start of a new era in electrical science.

Because Thomson himself was a central figure in the electrical science in which ether theory and questions about the fundamental character of electricity were at the forefront, he had to go through a personal version of the transition that his papers effected. For this reason, an examination of Thomson's three seminal papers needs to start a little before 1897.

Some Historical Background

One tends to forget how much clearer the fundamental importance of cathode rays is in retrospect than it was during the six decades of research on electrical discharges at reduced pressures prior to the last years of the nineteenth century. In contrast to the often spectacular displays elsewhere in evacuated tubes, cathode rays themselves are invisible. They were discovered by Julius Plücker only in 1859, after Heinrich Geissler's invention of the mercury vapor pump allowed a degree of rarifaction at which the fluorescence they produce stood out. This was a century and a half after Hauksbee had called attention to visible electrical phenomena in gases at reduced pressure and two decades after Faraday had carried out his experimental investigations of these phenomena. Cathode rays were in turn experimentally characterized in the late 1860s and the 1870s, first by J. W. Hittorf and then by Eugen Goldstein and William Crookes. None of their findings, however, linked the cathode rays with the visible discharge, which tends to disappear at rarifactions suitable for investigating the rays. It was thus easy in the early 1890s to regard cathode rays as a separate discharge phenomenon unto themselves, occurring in the special circumstance of extreme rarifaction.

The six decades of research on electrical discharges at reduced pressures had revealed a wide array of phenomena by 1890, but scarcely anything of value for theory construction—not even well-behaved regularities among measurable quantities of the sort that had been established for electricity in solids and liquids. Nevertheless, interest remained high. This was not merely because the microstructure of gases was better understood than that of liquids and solids. A further key reason was stated by J. J. Thomson in his *Notes on Recent Researches in Electricity and Magnetism* of 1893:

> The phenomena attending the electric discharge through gases are so beautiful and varied that they have attracted the attention of numerous observers. The attention given to these phenomena is not, however, due so much to the beauty of the experiments, as to the widespread conviction that there is perhaps no other branch of physics which affords us so promising an opportunity of penetrating the secret of electricity; for while the passage of this agent through a metal or an electrolyte is invisible, that

> through a gas is accompanied by the most brilliantly luminous effects, which in many cases are so much influenced by changes in the conditions of the discharge as to give us many opportunities of testing any view we may take of the nature of electricity, of the electric discharge, and of the relation between electricity and matter.[14]

As will be pointed out, Thomson was not speaking of cathode rays in this passage.

In his President's Address to the Royal Society at the end of 1893, Lord Kelvin attached comparable importance to research on electrical discharges in gases, though for a reason that puts a little more emphasis on cathode rays.[15] Kelvin turned to the subject of electricity in gases by raising the question of the difference between positive and negative electricity:

> Fifty years ago it became strongly impressed on my mind that the difference of quality between vitreous and resinous electricity, conventionally called positive and negative, essentially ignored as it is in the mathematical theories of electricity and magnetism with which I was then much occupied (and in the whole science of magnetic waves as we have it now), must be studied if we are to learn anything of the nature of electricity and its place among the properties of matter.[16]

Calling attention to the great difference in the behavior of the positive and negative electrodes in gaseous discharges led him into a brief history of cathode ray research, with primary emphasis on Crookes's electrical and other experiments at extremely high rarifaction. Whether in Crookes's experiments or those of Arthur Schuster and J. J. Thomson on the passage of electricity through gases, he went on to say, molecules are essential, while "ether seems to have nothing to do except the humble function of showing to our eyes something of what the atoms and molecules are doing." He then concluded:

> It seems certainly true that without the molecules there would be no current, and that without the molecules electricity has no meaning. But in obedience to logic I must withdraw one expression I have used. We must not imagine that "presence of molecules is *the* essential." It is certainly *an* essential. Ether also is certainly *an* essential, and certainly has more to do than merely telegraph to our eyes to tell us of what the molecules and atoms are about. If a first step towards understanding the relations between ether and ponderable matter is to be made, it seems to me that the most hopeful foundation for it is knowledge derived from experiment on electricity at high vacuum.[17]

On the question of whether cathode rays consist of negatively charged molecules, as Crookes had proposed, or some sort of wave-like disturbance of the ether, Kelvin in his presidential address considered the issue settled: "This explanation has been repeatedly and strenuously attacked by many other able investigators, but Crookes has defended it, and thoroughly established it by what I believe is irrefragable evidence."[18] Crookes had published his proposal in 1879,[19] and Goldstein had attacked it in 1880, raising a series of objections, including mean-free-path worries.[20] The case against the particle view was reinforced by Heinrich Hertz in 1883.[21] In one set of experiments designed for the purpose, Hertz was unable to detect any sign of the cathode discharge being discontinuous. When he moved the anode out of the direct stream of the cathode rays in a second set of experiments, he found that the current departed from the rays, leading him to conclude that the rays do not carry an electric charge. In a third set of experiments he was unable to deflect cathode rays electrostatically, from which he concluded that the only way there could be streams of charged particles was for their velocity "to exceed eleven earth-quadrants per second—a speed which will scarcely be regarded as probable."[22]

The Continental objections did not deter Schuster from putting forward a different version of the charged particle hypothesis in his Bakerian Lecture of 1884.[23] Schuster's experiments had persuaded him that intact molecules cannot receive a charge from contact with the cathode. He proposed instead that the emanations from the cathode consist of negatively charged atoms generated at it when molecules are torn apart by the fields produced by the interaction between it and positive ions migrating to it. He proceeded to formulate an algebraic relationship between the m/e and the velocity of these atoms implied by their curved trajectory in a magnetic field, arguing that measurements of this trajectory would allow a determination of the magnitude of m/e sufficient to corroborate his claim. In 1890 he used this relationship and such measurements, supplemented by assumptions giving estimates of the velocity, to calculate upper and lower bounds for this m/e.[24]

Kelvin's outspokenness notwithstanding, the issue of whether cathode rays consist of negatively charged particles or are a disturbance of the ether had, of course, not really been settled by the end of 1893, for figures on both sides were still advancing new evidence against the other. Hertz had augmented his argument against the particle hypothesis in 1892 when he found that cathode rays appear to pass through thin films of gold leaf.[25] In a footnote added in press to *Recent Researches,* Thomson had dismissed this finding, arguing that the cathode rays striking the film had turned it into a cathode with new rays generated from it.[26] Hertz's protégé, Phillip Lenard, then carried out extensive investigations of the rays external to the tube—which the British came to call "Lenard rays"—publishing the results in 1894.[27] In addition to showing that these rays do not propagate perpendicularly from the thin film in the way cathode rays propagate from electrodes, he added to the mean-free-path objection by determining the depth to which the rays outside the tube penetrate various gases at different densities.

During these same years Thomson advanced a similarly confuting line of argument against the aetherial-disturbance hypothesis, contending that the propagation velocity of cathode rays is orders of magnitude less than that of electromagnetic waves. The first version of this argument appeared in *Recent Researches.* Deriving basically the same relationship between *m/e,* velocity, and the curved trajectory of cathode rays in a magnetic field as Schuster, and adopting for *e* the value for hydrogen from electrolysis, Thomson concluded from Hittorf's published values for the curvature that the corresponding velocity of the cathode rays is no greater than "six times the velocity of sound."[28] The trouble with this argument was that it rather begged the question by assuming atomic values for *m.* Thomson published a second, seemingly more forceful version of this line of argument in 1894, obtaining comparably low values of velocity more directly from experiments using rotating mirrors.[29] This is the one set of experiments that Thomson himself conducted on cathode rays before 1896. His concern at the time appears to have been not so much with the properties of cathode rays as with the complications to ether theory that would be entailed by the magnetic deflection of these rays if they were some sort of electromagnetic waves.[30]

Thomson had succeeded Lord Rayleigh as the third Cavendish Professor and head of the Cavendish Laboratory in 1884, at the age of twenty-eight. After training first in engineering and then in physics and mathematics at Owens College in Manchester, where Schuster was one of his teachers, he matriculated at Cambridge, graduating in 1880. Although he was not a student of Maxwell's, his research between 1880 and 1896 was in the tradition of Maxwell's work in electricity and magnetism. The title page of *Recent Researches* includes as subtitle, "Intended as a Sequel to Professor Clerk-Maxwell's Treatise on Electricity and Magnetism." In surveying progress made in the field in the twenty years after Maxwell's *Treatise,* Thomson's book was no less committed than Maxwell's to combining abstract mathematical theory and experiment with concrete physical models of mechanisms and processes underlying electric and magnetic phenomena.[31] The physical model dominating Thomson's book is not the ether as such, but the Faraday tube[32]—"tubes of electric force, or rather of electrostatic induction, . . . stretching from positive to negative electricity."[33] Thomson introduces unit tubes all of the same strength, saying "we shall see reasons for believing that this strength is such that when they terminate on a conductor there is at the end of the tube a charge of negative electricity equal to that which in the theory of electrolysis we associate with an atom of a monovalent element such as chlorine."[34]

Thomson's introductory chapter on Faraday tubes ends with a proposed approach to the conduction of electricity generally in which a view of electrolysis takes the lead. The troubling issue of the interaction between electricity and matter that Maxwell's equations had left open included questions about electrical conduction and the contrasting conductivities of different substances.[35]

Chapter II of *Recent Researches* presents 154 pages covering research on "the passage of electricity through gases," including his own investigations on electrodeless tubes. The chapter surveys the full range of experiments on electricity in gases: circumstances in which gases can and cannot be electrified at normal pressures, the spark discharge, electrical discharges at reduced pressures, first in electrodeless tubes, then in tubes with electrodes, and the arc discharge; it ends with a 19 page section entitled "Theory of the Electric Discharge." The chapter is thus ideally suited for comparison with the first (1903) edition of *Conduction of Electricity Through Gases* to see just what difference the three seminal papers of 1897 to 1899 made.

Thomson reviews too many experiments in the chapter to cover them all here. Let me merely highlight some main points. The chapter calls attention to numerous asymmetries between electrical phenomena in gases at negatively and positively charged surfaces. It concludes early on, in keeping with Schuster, that molecules do not become charged, so that electrification of gases involves chemical dissociation:

> When electricity passes through a gas otherwise than by convection [i.e. such as by electrified dust particles], free atoms, or something chemically equivalent to them, must be present. It should be noticed that on this view the molecules even of a hot gas do not get charged, it is the *atoms* and not the molecules which are instrumental in carrying the discharge.[36]

Thomson cites Schuster in concluding that cathode rays—or "negative rays" as he here called them—consist of negatively charged, dissociated atoms; he responds to mean-free-path worries by suggesting that the charged atoms form "something analogous to the 'electrical wind.'"[37] Although he reviews cathode ray results thoroughly, he dismisses them as of secondary importance: "Strikingly beautiful as the phenomena connected with these 'negative rays' are, it seems most probable that the rays are merely a local effect, and play but a small part in carrying the current through the gas."[38] He lists a number of reasons for holding this, the key being the low velocity inferred from their magnetic curvature. The primary phenomenon is instead the striated positive column, the luminosity of which he concludes travels from the anode toward the cathode at a velocity of the same order of magnitude as that of light, with the striae forming a sequence of separate discharges.

The section on theory, which opens with the remarks on why electricity in gases is important quoted above, offers not a detailed theory but a "working hypothesis by which they [the phenomena] can be coordinated . . . to a very considerable extent."[39] Not surprisingly, this working hypothesis focuses predominately on the visible "positive discharge." It proceeds from two basic tenets:

> That the passage of electricity through a gas as well as through an electrolyte, and as we hold through a metal as well, is accompanied and effected by chemical changes; also that 'chemical decomposition is not to be considered merely as an accidental attendant on the electrical discharge, but as an essential feature of the discharge without which it could not occur'. (*Phil. Mag.* [5], 15, p. 432, 1883)[40]

The electric field between the anode and cathode, Thomson goes on to argue, is not sufficient to break up molecules, nor can the convection of dissociated charged atoms produce the great velocity of the discharge from the anode. Instead, the electric field polarizes the molecules spatially in the manner shown on top in figure 1.1, allowing them to form chains of the sort Grotthus had proposed for electrolysis. So aligned, interaction with the Faraday tube extending from anode to cathode is sufficient to dissociate the end atom, allowing it to combine with a charged atom at the anode, in the process contracting the Faraday tube and reinitiating the sequence. "The shortening of a tube of electrostatic induction is equivalent to the passage of electricity through the conductor."[41] The individual striae are bundles of such chains in parallel, so that the scale of electrical action in gases is not the mean-free-path from kinetic theory but the length of these chains, as dictated by conditions in the gas.

As Thomson indicates, this is a working hypothesis in the broad sense, a coordinated way of conceptualizing electrical phenomena in gases. In contrast to working hypotheses in the more narrow sense emphasized later, it does not enter constitutively into either the design or the formulation of the results of any of the experiments discussed in this chapter. As such, it is more a strategic approach for constructing a detailed theory than it is an initial fragment of such a theory that further experiments can extend and enrich. Thomson appears perfectly aware of this. At several points he tries to develop specific relationships out of his working hypothesis of a kind that might be

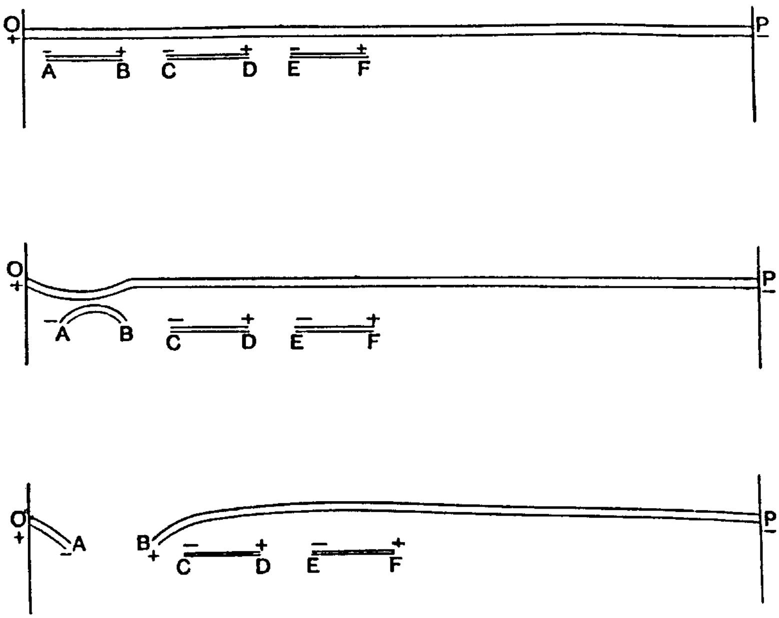

Figure 1.1
A schematic representation of Thomson's view of electrical conduction in gases (and liquids and solids) in *Recent Researches*. The electric field aligns the molecules AB, CD, and EF in a chain-like pattern. The interaction of the Faraday tubes OP and AB causes the molecule AB to dissociate, with the atom A combining with O, thereby shortening the Faraday tube OP to BP, reinitiating the sequence.

systematically tied to experimentally observable quantities, but he never sees a way of integrating any of these relationships into experiments. This is not to say that Thomson did not believe the working hypothesis he put forward. Rather, the question whether he believed it or not is largely beside the point so long as his goal was to formulate a comprehensive, detailed theory thoroughly tied to experiment and the hypothesis was unmistakably not yet enabling him to achieve progress toward that goal.

In presenting this working hypothesis—as well as earlier in this chapter and in the discussion of conduction at the end of the preceding chapter—Thomson puts special emphasis on "a remarkable investigation made more than thirty years by Adolphe Perrot, which does not seem to have attracted the attention it merits, and which would well repay repetition."[42] Perrot's experiments had shown that chemical equivalents of hydrogen and oxygen are released respectively at the cathode and anode when electricity passes through steam, just as in electrolysis. Thomson viewed these experiments as coming closer than any others to exhibiting a phenomenon whose interpretation is as "unequivocal as some in electrolysis."[43] He repeated and extended these experiments himself, publishing a paper on them in 1893[44] and including an appendix to *Recent Researches* devoted to them. A sign of how radically his view of electricity in gases changed with the three papers of 1897 to 1899 is that no mention whatever of Perrot or his experiments occurs in any of the editions of *Conduction of Electricity Through Gases*.

One shortcoming of Thomson's working hypothesis, which he noted near the end of the chapter, was that it offered nothing toward accounting for the various asymmetries of electricity in gases, in particular "the difference between the appearances presented by the discharge at the cathode and anode of a vacuum tube." Thomson's long theoretical paper of December 1895, "The Relation between the Atom and the Charge of Electricity carried by it," took a step in this direction.[45] Here too he emphasized the conjectural character of his proposals:

> The connexion between ordinary matter and the electrical charges on the atom is evidently a matter of fundamental importance, and one which must be closely related to a good many of the most important chemical as well as electrical phenomena. In fact, a complete explanation of this connexion would probably go a long way towards establishing a theory of the constitution of matter as well as of the mechanism of the electric field. It seems therefore to be of interest to look on this question from as many points of view as possible, and to consider the consequences which might be expected to follow from any method of explaining, or rather illustrating, the preference which some elements show for one kind of electricity rather than the other.[46]

In Thomson's view at the time, a molecule of hydrogen, for example, had to consist of a positively charged and a negatively charged atom of hydrogen, with a Faraday tube between them. Yet no hydrogen at all is released at the anode during electrolysis, implying that somehow all the atoms of hydrogen take on a positive charge in the process. The body of the December 1895 paper extends the working hypothesis of *Recent Researches,* taking Faraday tubes to consist of vortex filaments in the aether and trying gyroscope-like analogies, with their directional asymmetries, to account for the difference between electropositive atoms like hydrogen and electronegative atoms like chlorine and oxygen.

December 1895 was more notable for the publication of Wilhelm Röntgen's paper announcing the discovery of x-rays.[47] Since Röntgen's rays were generated by cathode ray tubes, his paper stimulated new interest in and experimentation with these tubes. Of more initial importance to Thomson was an effect of x-rays: "The facility with which a gas, by the application and removal of Röntgen rays, can be changed from a conductor to an insulator makes the use of these rays a valuable means of studying the conduction of electricity through gases."[48] 1895 was also the year in which Cambridge University first began admitting graduates of other universities as "research students."[49] Ernest Rutherford from New Zealand and John Townsend and J. A. McClelland from Ireland became research students at Cavendish at the end of 1895, joining C. T. R. Wilson, a Cambridge graduate, who had already begun his research on the condensation of moist air, having started at Cavendish early in the year. Thomson and McClelland carried out a series of investigations of x-rays and their effects in early 1896, immediately following Röntgen's announcement.[50] Thomson and Rutherford worked together on a series of experiments on gases electrified by x-rays during the first half of 1896 and Rutherford continued this effort into 1897.[51]

Sometime late in 1896 Thomson, without involving any of the research students, began experiments on cathode rays. Nothing indicates why he decided to do this, although several factors may have contributed. The efficacy of x-rays in ionizing gases implied energy levels for them, and hence for the cathode rays that generated them, that may have raised some doubts about the values for the velocity of the rays that he had published. Lenard's paper of 1894 had not changed Thomson's mind about the thin-film acting as a secondary cathode source, but the results it presented on the penetration and absorption of the rays external to the evacuated tube may have given him occasion to reconsider the mean-free-path worries. Recall that he had appealed to an "electric wind" to duck these worries in *Recent Researches*. Another factor that surely made a difference was a paper published by Jean Perrin in late 1895 reporting an experiment in which, contrary to Hertz, the negative electric charge does accompany the cathode rays.[52] Specifically, Perrin had measured an accumulation of negative charge as cathode rays strike a collector. Thomson was fully aware of the relationship between the m/e and the velocity implied by the curved trajectory of cathode rays under a magnetic field, for he had used it together with assumptions about the value of m/e in his 1893 estimates of the velocity and he knew of Schuster's similarly using it together with assumptions about the velocity in his 1890 estimates of m/e. The problem in both cases was that the magnetically curved trajectory provides a single experimental relationship between two unknowns. Perhaps what most of all got Thomson going on his cathode ray experiments in late 1896 was his seeing the possibility of Perrin's experiment yielding a second relationship between these two unknowns.

J. J. Thomson on Cathode Rays—1897

The first public indication that Thomson was doing experiments on cathode rays was in a February 8 talk he gave to the Cambridge Philosophical Society, reported a month later in *Nature*.[53] There, Thomson presented his results from experiments on the magnetic deflection of cathode rays and a refined version of Perrin's experiment from 1895. He appears to have made no mention of the subatomic. The occasion for his April 30 talk was a Friday Evening Discourse at the Royal Institution in London. Most of this lecture-with-demonstrations was again devoted to these experiments, but what made news was the subatomic hypothesis he placed before his distinguished audience at the end. The tenor of the reaction can be seen in an editorial remark in *The Electrician* three months later: "Prof. J. J. Thomson's explanation of certain cathode ray phenomena by the assumption of the divisibility of the chemical atom leads to so many transcendentally important and interesting conclusions that one cannot but wish to see the hypothesis verified at an early date by some crucial experiment."[54]

The text of the April 30 talk appeared in the May 21 issue of *The Electrician,* immediately following FitzGerald's commentary on it. After a brief review of the history of cathode rays, Thomson presented some experiments displaying the deflection of the rays in magnetic fields, in the process providing visible evidence that their trajectory in a uniform field is circular. He then demonstrated his version of Perrin's experiment and described some related experiments showing that cathode rays carry a charge. Along the way he pointed out that cathode rays turn the residual gas in the tube into a conductor, and he appealed to this to explain Hertz's failure to deflect the rays electrostatically. Finally, he demonstrated Lenard's result of rays outside the tube and reviewed Lenard's absorption data, agreeing that these data show that the distance the rays travel depends only on the density of the medium. This led him to the question of "the size of the carriers of the electric charge. . . . Are they or are they not of the dimensions of ordinary matter?" A mean-free-path argument gave him the answer: "they must be small compared with the dimensions of ordinary atoms or molecules."[55]

Thomson adopted a cautious tone in putting the "somewhat startling" subatomic hypothesis forward in the talk. It doubtlessly would have been passed over as nothing more than an interesting conjecture were it not for his having given an experimentally determined value of *m/e* for the cathode ray particles at the end of the talk. The single value he gave, 1.6×10^{-7} (in electromagnetic units), was inferred by combining the accumulation of charge and heat at the collector in a further variant of Perrin's experiment with the product ρH, where ρ is the radius of curvature of the rays deflected by a magnetic field of strength H. Not much could be made of the precise magnitude of this single value. (In fact, it falls entirely outside the range of values Thomson gives in his subsequent paper.) The point Thomson stressed was that this value is three orders of magnitude less than the m/e inferred for hydrogen from electrolysis and this favors "the hypothesis that the carriers of the charges are smaller than the atoms of hydrogen."[56] He closed his talk by noting that his m/e agrees in order of magnitude with the m/e Pieter Zeeman had inferred for charged particles within the atom in a recent paper on the magnetic splitting of lines in the absorption spectrum of sodium.[57]

As the title, "Dissociation of Atoms," suggests, FitzGerald's comments focused entirely on the subatomic proposal, ignoring the first three-quarters of Thomson's talk. It would be wrong to say that FitzGerald's response was dismissive. His concluding paragraph underscored the potential importance of Thomson's proposal:

> In conclusion, I may express a hope that Prof. J. J. Thomson is quite right in his by no means impossible hypothesis. It would be the beginning of great advances in science, and the results it would be likely to lead to in the near future might easily eclipse most of the other great discoveries of the nineteenth century, and be a magnificent scientific contribution to this Jubilee year.[58]

The stance FitzGerald adopted was that the potential importance of the proposal demanded that alternative interpretations of Thomson's experimental evidence be considered. The state of the field—FitzGerald expressly noted how little was known "about the inner nature of conduction and the transference of electricity from one atom of matter to another"—made other interpretations not hard to find. The alternative line of interpretation FitzGerald developed was that cathode rays consist of aetherial "free electrons" and the mass in Thomson's m/e measurement was entirely "effective" or quasi-mass from the electromagnetic inertia exhibited by a moving charge.[59]

Something needs to be said here about the word "electron." Thomson eschewed the term even as late as the second edition of his *Conduction of Electricity Through Gases* in 1906, when virtually everyone else was using it to refer to his corpuscles. Thomson chose "corpuscle" to refer to the material carrier of negative electric charge constituting cathode rays. G. Johnstone Stoney had introduced "electron" two decades earlier to refer to a putative physically fundamental unit of charge, positive and negative. He did this as part of a general argument that physically constituted units are preferable to arbitrary ones, proposing in the case of charge that the laws of electrolysis pointed to a fundamental unit, which at the time he calculated to be 10^{-20} electromagnetic units.[60] In the early 1890s Joseph Larmor, of Cambridge, had adopted the term at FitzGerald's instigation for the unit "twists" of ether comprising the atom in his theory of atomic structure.[61] (Larmor's proposal was that the quasi-mass of positive and negative electrons formed the mass of the atom; his original value for the electron quasi-mass corresponded to the mass of the hydrogen ion, but he reduced this in response to Zeeman's result.) Lorentz, who in 1892 had developed his version of Maxwell's equations, allowing for charged particles, did not adopt "electron" until 1899. Zeeman, who had turned to Lorentz, his former teacher, for the calculation of *m/e,* also did not use "electron." FitzGerald's "free electron" was adapted from Larmor. It refers to an aetherial unit charge, positive or negative, liberated from the atom, and was thus expressly intended to contrast with Thomson's "corpuscle." A compelling empirical basis for identifying Thomson's corpuscle with Stoney's unit charge emerged only with Thomson's December 1899 paper.

The influence of FitzGerald's commentary on Thomson is evident in the respects in which his October 1897 paper extends beyond his April 30 talk. In the results in the paper Thomson uses more than one material for the cathode, just as FitzGerald had suggested. The *m/e* experiment is repeated several times in different configurations, offering some response to FitzGerald's worries about the measurement of charge and heat accumulation. More importantly, a second way of determining *m/e* is added in which the charge and heat measurement is replaced by electrostatic deflection of the cathode rays. Thomson and his assistant encountered a good deal of difficulty in achieving stable electrostatic deflections of cathode rays.[62] Because the rays liberated gas from the walls of the tube, the rays had to be run in the tube and the tube then reevacuated several times to eliminate sufficiently the nullifying effects of ions in the residual gas.

Thomson submitted his paper on 7 August 1897, three months after his first going public with the subatomic hypothesis. The paper has three principal parts. After posing the particle versus ether-disturbance issue, the first part presents results of qualitative experiments supporting the particle hypothesis, including electrostatic deflection. The carefully phrased transition from the first to the second part is worth quoting:

> As 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 neg-

> atively 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. The question next arises, What are these particles? are they atoms, or molecules, or matter in a still finer state of subdivision? To throw some light on this point, I have made a series of measurements of the ratio of the mass of these particles to the charge carried by it.[63]

The second part presents the results of the two ways of determining *m/e.* The third part opens by laying out the subatomic hypothesis, stated finally as:

> Thus on this view we have in the cathode rays matter in a new state in which the subdivision of matter is carried very much further than in the ordinary gaseous state: a state in which all matter—that is, matter derived from different sources such as hydrogen, oxygen, &c.—is of one and the same kind; this matter being the substance from which all the chemical elements are built up.[64]

The remainder of the third part offers conjectures about atomic structure and the periodic table. The paper ends with brief remarks on the difference in the announced cathode ray velocities between this paper and the paper of 1894 and on effects observed with different cathode materials.

Thomson's opening sentence announces that "the experiments discussed in this paper were undertaken in the hope of gaining some information as to the nature of the Cathode Rays." If the paper is read in isolation from its historical context, the rhetorical flourish with which the charged particle versus aetherial-disturbance issue is laid out in the remainder of the first paragraph gives the impression that the question Thomson was most seeking to answer was whether cathode rays are particles. Given the view of this question at the time among his primary British audience, however, a more historically plausible reading of this first sentence is that the information he most hoped to gain bore on the questions posed at the outset of the second part of the paper quoted above: "What are these particles?" and so forth. The qualitative experiments discussed in the first part have a more important role than merely providing evidence that cathode rays are particles, the presupposition of these questions. They clear the way for using charge accumulation, electrostatic deflection, and magnetic curvature to obtain experimental values of *m/e* and *v.* They do this by obviating worries about whether the accumulation of charge being measured is that of the cathode rays, whether the failure to obtain electrostatic deflection at anything but extraordinary levels of evacuation is truly because the rays ionize the residual gas, and whether the specifically observed curvature of the trajectory is that

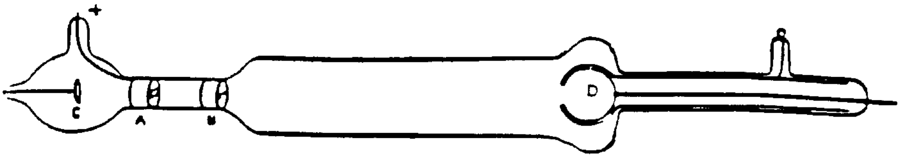

Figure 1.2
A schematic of one of the three kinds of tubes Thomson used in his first approach to measuring *m/e* for cathode rays (based on the description given in his October 1897 paper).

of the rays, in contrast to some secondary luminosity in the gas. (This fits the suggestion that what prompted Thomson to begin his experiments on cathode rays in late 1896 was the prospect of a fully experimental determination of *m/e* and *v*; for, to this end, he would have first needed to gain mastery of the basic experiments and safeguard against the possibility that measurements made in them are misleading artifacts.)

Only the experiments for *m/e* in the second part of the paper merit much comment here. Figure 1.2 shows a schematic of one of the three types of tubes Thomson used with the first method. A narrow cathode ray beam passes through slits in the anode A and the plug B, striking the collector D unless it is magnetically deflected as a consequence of current flowing through a coil magnet located along the middle of the tube. From the expressions given in the paper for the charge Q accumulated at the collector, the kinetic energy W of the particles striking it, and the radius of curvature ρ of the beam under a uniform magnetic field H, Thomson obtains the following expressions for the *m/e* and the velocity v of the particles:

$$\frac{m}{e} = \frac{H^2\rho^2 Q}{2W}, \qquad v = \frac{2W}{QH\rho}.$$

An electrometer was used to measure Q, W was inferred from the temperature rise at the collector (measured by a thermocouple), H was inferred by measuring the current in the coils, and ρ was inferred from the length of the magnetic field and the displaced location of the point of fluorescence on the glass tube. The design of the experiment is thus opening the way to obtaining values of microphysical quantities from macrophysical measurements.

In the second method, shown schematically in figure 2 of Thomson's paper,[65] electrostatic deflection of the beam replaces the accumulation of charge and heat at the collector. Thomson derives expressions for the angle θ to which the beam is deflected as it leaves the uniform electric field of strength F between plates of length l, and the angle θ to which it is deflected by the magnetic field H of the same length. In the version of the experiment reported in the paper, the magnetic field was superimposed on the electric field, and its strength H was varied until the electrostatically displaced spot was restored to its original location. In this case:

$$\frac{m}{e} = \frac{H^2 l}{F\theta}, \qquad v = \frac{F}{H},$$

where θ was inferred from the displaced location of the fluorescent spot when only the electric field was present and F was inferred from the voltage drop applied to the plates. This method also thus involves only macrophysical measurements.

Thomson's presentation proceeds so smoothly, and the crossed-field approach with cathode rays has become so familiar, that readers can easily fail to notice the complexity of the logic lying behind these *m/e* experiments. The derivations of the two expressions giving *m/e*, along with the instruments used to obtain values of the parameters in them, presuppose a number of laws from physics; many of these had been discovered within the living memory of some of Thomson's colleagues and hence were less firmly entrenched in 1897 than they are now. The derivations also presuppose a variety of further assumptions. Some of these serve only to simplify the math. For example, in deriving the angular displacement of the beam in a magnetic field in the crossed-field experiment, Thomson implicitly assumes that the velocity of the beam is great enough that he can treat the magnetic force as unidirectional, just like the electrostatic force. He could easily have derived a more complicated expression, taking into account that the magnetic force is always normal to the direction of the beam. Similar to this are some assumptions in which he idealizes the experimental setup. He assumes, for example, that the collector is perfectly insulated thermally so that no heat leaks from it, and he assumes that the magnetic and electric fields extend only across the length l, ignoring the small field effects extending beyond the edges of the plates and the coils. He could easily have introduced corrections for these effects, complicating the math a little.[66]

Beyond these are such assumptions as the particles all have the same *m/e* and, in any one experiment, the same constant velocity both across the length of the magnetic and electric fields and downstream at the collector. These assumptions have a more wishful character. Because they concern the unknown quantities that are being measured, they are not so readily amenable to corrections. The main safeguard against being misled by them lies in the quality of the data. The falsity of any of them should show up in the form of poorly behaved data when the experiments are repeated with different field strengths, anode-to-cathode voltage drops, and tube configurations.

Some difficulties in executing the experiments complicated matters still further. Because the cathode rays ionized the residual gas in the tube, the leak of charge from the collector became increasingly significant as the total charge accumulated. As a consequence, the charge accumulation experiment had to be run over short time durations, entailing small temperature rises and hence greater sensitivity to small inaccuracies in measurement. Far worse was the so-called "magnetic spectrum." Birkeland had called attention to the fact that the fluorescent spot spreads out when displaced magnetically, generally forming a sequence of spots with darker regions between them. Thomson found the same thing with electrostatic deflection. The magnetic spectrum was prima facie evidence against all the particles having the same *m/e*. In the April 30 talk Thomson suggested that this effect might be from two or more corpuscles clumping together. In the October paper, however, he makes no mention of this possibility. Instead, the magnetic and electric displacements are identified with the brightest spot in the spectrum, if there is one, and with their middle, if there is not.

The magnetic and electrostatic "spectra" were in fact experimental artifacts, caused by different velocities among the particles resulting from Thomson's use of an induction coil to produce the anode-to-cathode voltage drops instead of a continuous source, such as a stack of batteries. This was established roughly a year later by Lord Rayleigh's son, R. J. Strutt, while still an undergraduate at Trinity College, Cambridge, and it was announced in a paper in the November 1899 issue of *Philosophical Magazine*.[67] No one at Cavendish appears to have repeated the cathode ray *m/e* measurements when this discovery was made.

The pivotal assumption underlying the *m/e* experiments is that cathode rays are streams of particles. One can think of this as a working hypothesis, with the results of the qualitative experiments presented in the first part of Thomson's paper providing the justification for predicating further research on it. A failure to come up with well-behaved results for *m/e* in the experiments would be evidence against it. Conversely, evidence would accrue to it from the experiments presupposing it to the extent that (1) the value of *m/e* obtained from each method remains stable as the field strengths, the anode-to-cathode voltage, and other things are varied and (2) the values obtained from the two methods are convergent with one another. This is typical of the way in which successful theory-mediated measurements of fundamental quantities have always provided supporting evidence for the theory presupposed in them.

How stable and convergent were Thomson's results? Here the logic becomes subtle. On the one hand, the data fall far short of yielding a precise value for *m/e*. His values for *m/e* from the first method range from a low of

0.31×10^{-7} to a high of 1.0×10^{-7}, and from the second method, from a low of 1.1×10^{-7} to a high of 1.5×10^{-7}.[68] Looking at his *m/e* numbers by themselves, therefore, one can legitimately question whether the results were all that stable or convergent. On the other hand, the *m/e* values are all three orders of magnitude less than the smallest theretofore known value, the *m/e* of the hydrogen ion. When viewed in this light, the results at the very least provided strong additional evidence for predicating further research on the hypothesis that cathode rays consist of negatively charged particles.

Because of the "rough draft" character of the *m/e* experiments, as well as the confounding factor of Birkeland's spectrum, Thomson's 1897 paper did not settle the question of whether all the particles forming cathode rays have the same *m/e.* The one feature of the data supporting a single, universal particle was the absence of systematic variation in *m/e* with the gas in the tube and the material of the cathode. This was enough for Thomson to proceed further under the extended working hypothesis that all cathode rays consist of corpuscles with a mass-to-charge ratio around 10^{-7} emu—presumably subatomic corpuscles of a single, universal type. He set the question whether there is a single value of *m/e* for cathode rays and, if so, what precisely it is to one side, turning instead to other questions raised by the paper.[69] The paper announces two questions: (1) is the very small *m/e* a consequence of a small *m,* a large *e,* or a combination of the two?; and (2) how many corpuscles are there in an atom, and how do they fit into it? Judging from his research over the next two years, however, the question most on his mind was, (3) how do the cathode ray corpuscles enter into other electrical phenomena?

Three final points need to be made about the 1897 paper. First, the experiments reported in it do not in themselves refute the view that cathode rays are wave-like. The velocities Thomson obtained varied with the cathode-to-anode voltage, ranging from a low of 2.2×10^{9} to a high of 1.3×10^{10} cm/sec—that is, from roughly 7 to 43 percent of the speed of light.[70] This difference from the speed of light was enough to accomplish Thomson's 1894 objective of refuting the proposal that cathode rays are a type of electromagnetic wave propagation, but not enough to show that they are not waves. The only way of proceeding from Thomson's results to the conclusion that cathode rays have no wave-like character is via the tacit premise that anything consisting of particles cannot have a wave-like character. However much this premise was an ingrained article of belief at the time, it was not presupposed by the experiments themselves. Consequently, nothing in the experiments of the 1897 paper, or subsequent refined versions of them, required any correction or adjustment when the wave-like character of electrons was established three decades later.[71]

Second, the premise that cathode rays consist of charged particles—or at least constituents that are sufficiently particle-like for laws governing charged particles to hold—is presupposed by the experiments. It is a constitutive element in the experiments and hence a working hypothesis in the narrow sense to which I alluded in the preceding section: a proposition of conjectural status that enters indispensably into a train of evidential reasoning leading from observations to the statement of the results of an experiment. Consider what the two *m/e* experiments would amount to without this premise. Ignoring the unlikelihood that someone would still have pursued the investigation, each would have shown only that a certain algebraic relationship among some macroscopic variables retains more or less the same numerical value when conditions involving cathode ray tubes are varied. Worse, without it the only reason to have taken the two algebraic relationships to perhaps be representing the same thing would have been the degree to which their roughly invariant values matched one another, which in fact was not all that great. The charged-particle working hypothesis, joined with the relevant laws from prior science and the various simplifying assumptions, put Thomson in a position where the empirical world could provide answers to such questions about the nature of cathode rays as, what is the mass-to-charge ratio of their constituents?, How, if at all, does this ratio vary with the gas in the tube, the electrode material, and the voltage drop from cathode to anode?, and how does it compare with other known values of *m/e*? Evidence—or at least grounds for predicating further research on it—accrued to the particle hypothesis from the extent to which these answers were well-behaved, allowing experiment to replace conjecture in extending it.

Third, as indicated earlier, Thomson was not the only one measuring *m/e* for cathode rays at the time. Both Emil Wiechert[72] and Walter Kaufmann[73] in Germany were independently obtaining more or less the same *m/e* values as Thomson by combining magnetic deflection with eV, the upper bound for the kinetic energy particles of charge e would acquire in falling through a potential difference V between the cathode and anode. Wiechert, in particular, had announced his results on 7 January 1897 in a talk in Königsberg, stating that the mass of the particle is between 2000 and 4000 times smaller than that of a hydrogen atom, having first assumed that the charge is one "electron"—that is, the charge per hydrogen atom in electrolysis, inferred from existing estimates of Avogadro's number. Thomson's 1897 work was nonetheless distinctive in three respects. First, he went beyond the others in the extent to which he determined that *m/e* is independent of the gas in the tube and the material of the cathode. Second, he was alone in devising two complementary measures, thereby adding a good deal of support for the underlying working hypothesis that the constituents of cathode rays are particle-like. Third, he alone immediately proposed that the charged particles in question are dissociated constituents of atoms.

J. J. Thomson on the Charge of Ions—1898

The results of several experiments supporting Thomson's *m/e* results for cathode rays, including more refined experiments by Kaufmann and by Lenard, were published in 1897 and 1898. In 1898 Lenard also announced that the *m/e* for the rays outside the cathode tube that were being named after him is the same as for cathode rays.[74] In 1886 Goldstein had noted faint rays passing through holes in the cathode into the space on the opposite side of it from the anode, seemingly symmetric counterparts of cathode rays. Wilhelm Wien used magnetic and electric deflection to determine that these rays, called "Canalstrahlen," were positively charged with a mass-to-charge ratio around three orders of magnitude greater than that of cathode rays; he announced the distinctive contrast between these and cathode rays in 1898.[75] By contrast, while others were pursuing refined measures of *m/e* for cathode and related rays, Thomson, though noting their results,[76] shifted the focus of his research away from these rays.

Thomson published two papers in *Philosophical Magazine* in 1898. The first, "A Theory of the Connexion between Cathode and Röntgen Rays," appeared in February.[77] In it Thomson derives theoretical expressions for the magnetic force and electric intensity that propagate when a moving electrified particle is stopped suddenly—more specifically, a particle moving at a velocity high enough that the square of the ratio of it to the speed of light can no longer be neglected. At the end of the paper he calls attention to the high velocity he had obtained for the negatively charged particles forming cathode rays, concluding that Röntgen rays are most likely impulses generated by the sudden stoppage of these particles, and not waves of very short wave-length.

The second paper, "On the Charge of Electricity carried by the Ions produced by Röntgen Rays," appeared in December 1898.[78] It reports the results of an elaborate experiment for determining the charge *e* of the negative ions produced when x-rays pass through a gas. The relationship between these negative ions and Thomson's corpuscle is left an entirely open question throughout this paper. The basic idea behind the experiment is to infer the charge per ion from the amount of electricity (per unit area per unit time) passing through the ionized gas under an electromotive force. Assuming all ions have the same magnitude of charge, *e*, this quantity of electricity is simply *neu*, where *n* is the number of ions per unit volume and *u* is the mean velocity of the positive and negative ions under the electromotive force. The charge per ion can be thus be inferred from a determination of *n* and *u*.

Three separate results published by Thomson's research students during 1897 opened the way to determining *n* and *u*. First, Rutherford's research on the conduction of electricity in gases ionized by x-rays had culminated in a paper published in *Philosophical Magazine* in November 1897, entitled "The Velocity and Rate of Recombination of the Ions of Gases exposed to Röntgen Radiation."[79] In an experiment that was fairly elaborate in its own right, Rutherford had determined ion velocities for a number of gases. In particular, the velocity of both the negative and the positive ions that he found in the case of atmospheric air was around 1.6 cm/sec per volt/cm potential gradient (i.e. 480 cm/sec per unit potential gradient in the esu units Thomson chose to use at the time); and the velocity he found in the case of hydrogen was around three times greater than this. Thomson assumed these values in his experiment.

Second, Wilson had established that, when x-rays pass through dust-free, saturated damp air and the air is then suddenly expanded, a cloud is produced by a degree of adiabatic expansion that produces no cloud when the air has not been subjected to x-rays.[80] The presumption was that the ions act as nuclei around which droplets of water form. Wilson had devised means for determining, through calculation, the total volume of water formed, so that the number of droplets—and hence the number of ions—per unit volume could be inferred if the radius of the presumably spherical droplets could be determined. The one tricky element, which Wilson had also found a way of handling, was to gain some assurance that a droplet forms on every available ion.

The remaining problem was to determine the radius of the droplets. For this Thomson ended up adopting an approach Townsend had devised in determining an approximate value for the charge on positive and negative ions of oxygen released in electrolysis.[81] Townsend too had relied on the formation of water droplets, in his case droplets that formed after the gases given off in electrolysis were bubbled through water. To determine the size of the droplets, he had measured their velocity in fall under their own weight and had then inferred their radius from Stokes's theoretical law for the purely viscous resistance force acting on small moving spheres.

As should be evident by this point, the logic underlying Thomson's method for measuring the charge of the ions is even more complicated than the logic underlying his methods for measuring *m/e* for cathode rays. Some of the assumptions entering into the method are not stated in his paper but are instead buried in the papers of his research students. On top of this, the experiment itself is complicated, involving three distinct parts: an irradiation part in which a quantity of gas is subjected to x-rays of an appropriate intensity; an electrical part in which the amount of electricity passing through the ionized gas under an electromotive force is determined; and a gaseous-expansion part in which the velocity of the water droplets is measured and the total amount of water is inferred from a measurement of temperature change.

Not surprisingly, the apparatus for the experiment (shown schematically in figure 1.3) has a distinctly Rube Goldberg character. The ionized gas is contained in the vessel A, which is covered by a grounded aluminum plate and

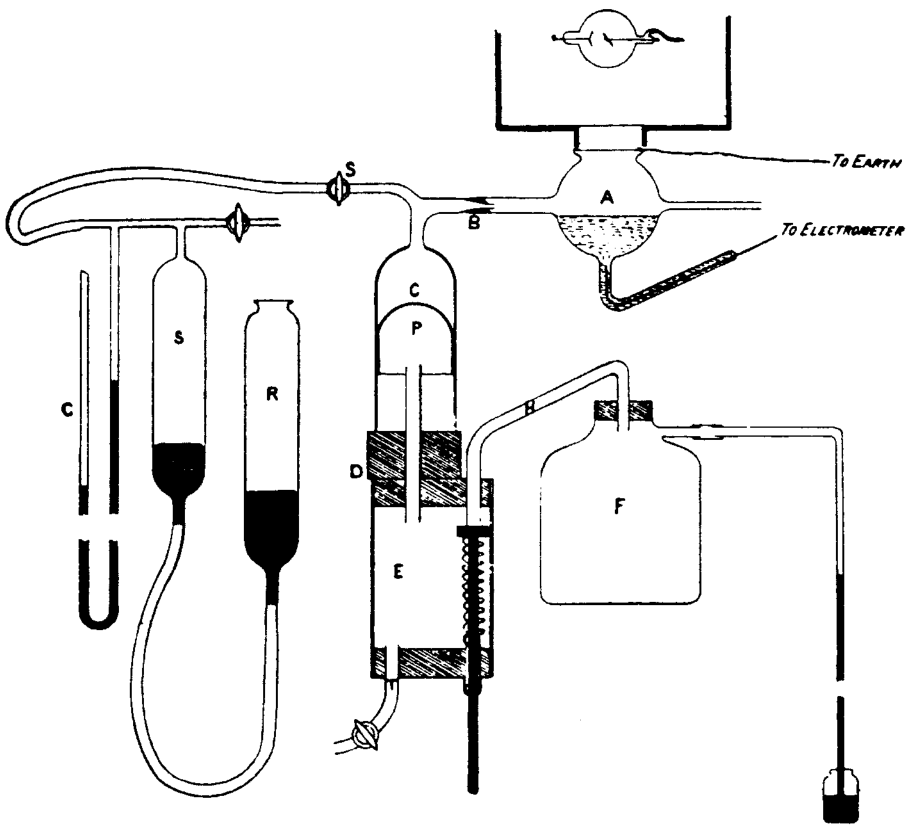

Figure 1.3
Thomson's schematic of his experiment to measure the charge per ion in gas ionized by x-rays. The gas to be ionized is contained in vessel A, below the cathode ray tube used to generate the x-rays. Most of the rest of the apparatus serves to effect the controlled expansion required for droplets to form on individual ions in a well-behaved fashion.

contains a pool of water electrically charged by a battery. The aluminum plate serves to limit the intensity of the x-rays reaching the gas. The expansion of the gas is effected by the piston P; all the paraphernalia attached to it, as well as the tubes R and S, serve to control the expansion. One pair of quadrants of an electrometer are connected to the tank and the aluminum plate, and the other pair are connected to the water. The tank, the aluminum plate, the water, the electrometer, and the wires connecting them form a system with an electric capacity that can be measured. Given this capacity, the amount of electricity passing through the ionized gas is determined by measuring the rate of charge leaking from the electrometer when the gas is irradiated.

Thomson's paper falls into six parts. The first presents the basic ideas underlying the experiment. The second describes precautions taken to assure that the level of radiation and the amount of expansion were appropriate. The third describes the apparatus and the method used for measuring the amount of electricity passing through the gas—that is, CV, where C is the measured electric capacity of the system and V the voltage change observed for it with the electrometer. The fourth part goes through the process of calculating, in sequence, the total amount of water q, the droplet radius a, the number of droplets n, and finally the charge-per-ion e from measured values for one trial of the experiment. The fifth part presents the results for e obtained from several trials for air and then for hydrogen. The last part offers concluding remarks, first in defense of an assumption and then on comparisons between the value obtained for e, the value of unit charge inferred from electrolysis, and the value Lorentz had recently inferred from the splitting of spectral lines.

The entire approach presupposes that there is some definite charge per ion when a gas is ionized by x-rays. Because so little was known about gaseous ions, the only way of defending this assumption was to appeal to regularities observed in electrolysis, the microphysical basis for which was still largely a matter of conjecture. This assumption accordingly fell mostly into the category of wishful thinking. It is akin to what is called "taking a position" in the card game contract bridge: if the only way to make a contract is for a particular card to be in a particular hand, then the best approach is to postulate that the card is in that hand and draw further inferences under this assumption, taken as a working hypothesis. If the only prospect for coming up with a telling experiment is to assume that nature is simple in some specific way, then the best approach may be to make this assumption and see what comes out of the experiment. This is especially true in the early stages of scientific research into a domain that cannot be observed comparatively directly. Thomson could have adopted a weaker assumption in this experiment: there is a consistent *average* charge per ion when a gas is ionized by x-rays. But if one is going to engage in wishful thinking, why adopt a less desirable line until the data give one reason to?

As with the m/e experiments, the most immediate safeguard against being misled by an experiment predicated on a tenuous assumption lies in the quality of the data obtained as the experiment is repeated in varying conditions. Thomson found it necessary to introduce two corrections to his raw data. The first correction, applied to the value of e obtained in each trial, served to compensate for the fact that some droplets form even in gas not radiated by x-rays.[82] (Cosmic rays, which were discovered in 1911, were causing some ionization, confounding the experiment.) The second correction, applied to the mean value of e obtained over the series of trials, compensated for electric conduction in the film of moisture coating the walls of the vessel. Neither of these corrections appears to have been introduced solely to make the data appear better behaved.

The values Thomson reports for e in air have a range about their mean of roughly ±16 percent. His corrected mean value for air is 6.5×10^{-10} electrostatic units, around 35 percent above the current value for the electron charge. The measurements with hydrogen involved greater uncertainty so that Thomson does not bother to carry through the corrections to the raw data. The range of the raw data is nevertheless about the same as in air. Thomson concludes that "the experiments seem to show that the charge on the ion in hydrogen is the same as in air. This result has very evident bearings on the theory of the ionization of gases produced by Röntgen rays."[83] The thrust of this last remark is that a single fundamental quantity of electricity per ion is involved when gases are ionized by x-rays, regardless of the chemical composition of the gas. (The comparison between the results for air and hydrogen might be more accurately summarized by saying that the experiments do not show that the charge on the ion in hydrogen is not the same as in air. The element of wishful thinking is carrying over into the extended working hypothesis that Thomson is extracting from the results of this experiment.)

The element of wishful thinking is also evident when he compares his 6.5×10^{-10} with the value of e inferred from the total quantity of electricity in electrolysis, using Avogadro's number—or, as Thomson preferred, the number of molecules per cubic centimeter at standard conditions. Thomson's value of charge, together with the total electricity per cubic centimeter of hydrogen released in electrolysis, gives a value of 20×10^{18} molecules per cubic centimeter. He compares this with the value of 21×10^{18} obtained from experiments on the viscosity of air. (Our modern value is 27×10^{18}.) The values at the time ranged far more widely than Thomson's comparison would suggest. A prominent 1899 textbook in kinetic theory, for example, gave 60×10^{18} as the value.[84] The conclusion Thomson draws from his comparison is suitably qualified: the agreement "is consistent with the value we have found for *e* being equal to, or at any rate of the same order as, the charge carried by the hydrogen ion in electrolysis."[85]

Thomson's experiments for determining *e* in ionization by x-rays were logically independent of his experiments for determining *m/e* for cathode rays. Even so, these 1898 experiments, more complicated though they were, evince the same research style as the 1897 experiments. The hypothesis that there is some characteristic value of ion-charge when a gas is ionized by x-rays is a constitutive element in the experiments, presupposed in inferring the value for *e* from the measured current *neu*. This working hypothesis, joined with experimental techniques and results from his research students, relevant laws from prior science, and some simplifying assumptions, allowed Thomson to design experiments in which the empirical world could give answers not only to the question of the magnitude of this *e* but also to whether it varies with the conditions under which a given gas is ionized by x-rays, whether it varies from one gas to another, and how it compares with the *e* of electrolysis.

Finally, just as with his *m/e* experiments, the achievement of Thomson's *e* experiment was not so much to establish a definite value for *e* as it was to license a working hypothesis for ongoing research: the same fundamental quantity of electricity is involved in both electrolysis and the ionization of gases by x-rays, and this quantity is of the order of magnitude of 6.5×10^{-10} esu. Thomson was struggling to find experiments involving macrophysical measurements that would yield some reasonably dependable conclusions about microphysical processes. In this early stage of research, working hypotheses had to stand in for established theory in the logical design of experiments. The results of his *e* experiment, in principle, could have provided good reasons for abandoning the wishful thought that nature is simple in the way the working hypothesis says it is. They did not. Instead, in spite of their roughness and uncertainty, his results showed the working hypothesis to have sufficient promise to warrant predicating further research on it. To see the role it ended up playing in this further research, we need to turn to his December 1899 paper.

The Electron and Ionization—1899

Again in 1899 Thomson published two papers in *Philosophical Magazine:* "On the Theory of the Conduction of Electricity through Gases by Charged

Ions" in March,[86] and "On the Masses of Ions in Gases at Low Pressures" in December.[87] The first of these takes off from results obtained by Thomson's research students on the velocities of ions: by Rutherford and John Zeleny for gases exposed to x-rays; by Rutherford for gases exposed to uranium radiation and to the photoelectric discharge produced by ultraviolet light;[88] by McClelland and Harold Wilson for the ions in flames; and by McClelland for the ions in gases near incandescent metals and gases exposed to arc discharges.

> A remarkable result of the determination of the velocities acquired by the ions under the electric field is that the velocity acquired by the negative ion under a given potential gradient is greater than (except in a few exceptional cases when it is equal to) the velocity acquired by the positive ion. Greatly as the velocities of the ions produced in different ways differ from each other, yet they all show this peculiarity.[89]

Under the assumption that current in gases consists of migrating ions that have not yet recombined to form an electrically neutral molecule, Thomson derives a differential equation relating ion velocity to current. He is able to integrate this equation only under a simplifying assumption. He nevertheless proceeds in this way to develop an expression for the flow of electricity in gases of the form, $V = Ai^2 + Bi$, where V is the potential difference across a pair of plates, i is the current, and expressions for A and B are formulated in terms of properties of the ions, including their charge. The paper ends by considering various asymmetries between negative and positive electricity in the light of Thomson's mathematical theory and the observed asymmetry in ion velocities.

The paper immediately following Thomson's in the March issue of *Philosophical Magazine* is by William Sutherland, entitled "Cathode, Lenard, and Röntgen Rays."[90] This entire paper is in response to Thomson's subatomic proposal: "Before a theory of such momentous importance should be entertained, it is necessary to examine whether the facts to be explained by it are not better accounted for by the logical development of established or widely accepted principles of electrical science."[91] The principles Sutherland has in mind are those of ether theory and Larmor's etherial electron. He summarizes his alternative theory in two propositions: "The cathode and Lenard rays are streams, not of ions, but of free negative electrons. The Röntgen rays are caused by the internal vibrations of free electrons."[92] Negatively charged free electrons are generated when an immaterial "neutron" consisting of a positively and negatively charged pair becomes dissociated.

In a curt reply published in the following month's issue,[93] Thomson points to questions about whether an impacting quasi-mass is sufficient to produce x-rays and to questions about how aetherial electricity can be distributed within the atom, invoking the Zeeman effect to suggest that "the electron thus appears to act as a satellite to the atom." Thomson summarizes the situation from his point of view:

> As far as I can see the only advantage of the electron view is that it avoids the necessity of supposing the atoms to be split up: it has the disadvantage that to explain any property of the cathode rays such as Lenard's law of absorption, which follows directly from the other view, hypothesis after hypothesis has to be made: it supposes that a charge of electricity can exist apart from matter, of which there is as little evidence as of the divisibility of the atom: and it leads to the view that cathode rays can be produced without the interposition of matter at all by splitting up neutrons into electrons.[94]

Thomson's other 1899 *Philosophical Magazine* paper was originally presented at a meeting of the British Association a few months earlier. The published version, the next to last paper in the December issue, would have been a fitting final word of the nineteenth century from this journal. The paper consists of five parts. The first summarizes the findings of the paper, concluding, "we have clear proof that the ions have a very much smaller mass than ordinary atoms; so that in the convection of negative electricity at low pressures we have something smaller even than the atom, something which involves the splitting up of the atom, inasmuch as we have taken from it a part, though only a small one, of its mass."[95] The second part presents a novel method for measuring e/m of the electric discharge in the photoelectric effect, the results from which indicate that this discharge has the same m/e as Thomson's cathode ray corpuscles. The third part uses essentially the same method to determine the e/m of the electrical discharge from incandescent filaments, showing this too is the same. The fourth part uses the method of the December 1898 paper to obtain the charge e of the ions discharged in the photoelectric effect, concluding it agrees with the value obtained in that paper. The final part first draws conclusions about the fundamental character of this quantity of electricity and about the mass of the particle in cathode rays and these discharges (holding open the question whether it is quasi-mass); it then draws on the findings of this and related papers to elaborate a new "working hypothesis" about the microphysical mechanisms underlying not only electrical phenomena in gases but also electrolysis and ionic bonding.

Because the photoelectric and incandescent-filament discharges could not readily be collimated into beams that fluoresce glass, neither of the methods Thomson had used to determine m/e for cathode rays was applicable to

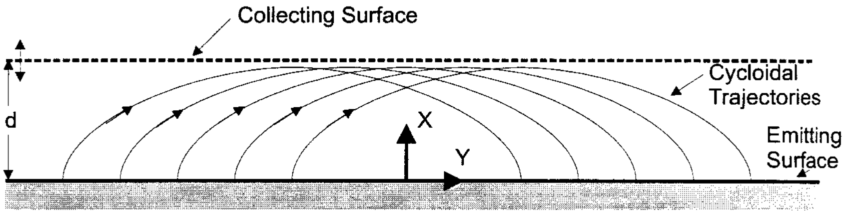

Figure 1.4
The cycloidal path of the photoelectric discharge under the action of an electric force parallel to the x-axis and a magnetic force parallel to the z-axis. For an appropriate combination of electric and magnetic force, the particles will cease reaching the collecting plate at a distance *d* from the emitting surfact. (The same approach was used in measuring the *e/m* of the incandescent discharge.)

them. His new method employs crossed magnetic and electric fields to a different effect. Let the *x*-axis be normal to the surface producing the discharge, and let the electric force be parallel to the *x*-axis and the magnetic force be parallel to the *z*-axis (figure 1.4). Thomson shows that the trajectory of a negatively charged particle starting at rest on the emitting surface will then be a cycloid. Let a plate be located parallel to the emitting surface a short distance away from it. So long as the electric force is great enough, all the emitted charged particles will reach the plate. As the electric force is reduced, however, a value will be reached where the number of charged particles reaching the plate will abruptly diminish. If V is the voltage between the emitting surface and the plate at which the amount of charge reaching the plate drops, H is the magnetic field, and d is the distance between the emitting surface and the plate, then:

$$\frac{e}{m} = \frac{2V}{d^2H^2}.$$

According to this theory, there should be a sharp cutoff point where the charges cease to reach the plate. In practice Thomson found this not to be the case. He consequently modified the approach a little. He still varied the voltage, but he now compared the amount of charge reaching the plate with and without the magnet on, searching for the voltage where this comparison would first show a difference. The formula for *e/m* remained the same.[96]

In the case of the photoelectric discharge, the paper gives the results of seven trials of the experiment with different distances *d*. With the exception of one slight outlier, the values obtained for *e/m* show relatively little variation. Inverted to ease comparison with the *m/e* values obtained for cathode rays, these values all lie between 1.17×10^{-7} and 1.43×10^{-7} except for one at 1.74×10^{-7}. Save for this exception, then, the range of these values falls within the range of the cathode ray *m/e* values Thomson had reported for the cross-field method. The same is true of the five *e/m* values obtained in the case of the incandescent filament discharge. Again inverted for ease of comparison, they all lie between 1.04×10^{-7} and 1.36×10^{-7} except for one at 0.88×10^{-7}.[97] Thomson concludes "that the particles which carry the negative electrification in this case are of the same nature as those which carry it in the cathode rays and in the electrification arising from the action of ultraviolet light."[98]

The experiments for measuring *e/m* of the incandescent filament discharge had initially been confounded by positively charged ions of gas released from the filament. These positively charged particles behaved quite differently from the negatively charged discharge, giving Thomson occasion to mention Wien's results for Canalstrahlen in reaching a further conclusion: "the carriers of positive electricity at low pressures seem to be ordinary molecules, while the carriers of negative electricity are very much smaller."[99]

Two results by Thomson's research students lay behind his determining the charge *e* of the photoelectric discharge. First, C. T. R. Wilson had shown that this discharge produces cloud formation once an electric field is applied to the discharge so that it moves away from the emitting surface.[100] Second, as noted earlier, Rutherford had measured the velocity of the discharge particles per unit electromotive force, thereby giving the value *u* needed in order to infer *e* from *neu*.[101] In developing the technique for cloud formation with the photoelectric discharge, Wilson had found that, just as with x-rays, the determination of the number of droplets *n* was best done with ultraviolet light of limited intensity. This, together with the relatively long times of ultraviolet irradiation required for measuring *e*, made the measurement sensitive to nonuniformities in the ultraviolet intensity. Thomson blames this for the larger variation in the values of *e* obtained here than in those in his 1898 paper.

Still, the variation in Thomson's results for the photoelectric *e* is not all that large, and more importantly their mean, 6.8×10^{-10}, is close to the 6.5×10^{-10} he had obtained for the ions produced by x-rays. A series of no less complex experiments on the diffusion of ions in gases that were being carried out at Cavendish by Townsend had in the meantime provided stronger evidence than Thomson had given at the end of the 1898 paper that the charge on the ions produced by x-rays is the same as the charge on an atom of hydrogen in electrolysis.[102] Thomson concludes from these results "that the charge on the ion produced by ultraviolet light is the same as that on the hydrogen ion in ordinary electrolysis."[103]

Thomson then joins the *e/m* and *e* results presented in this paper with the *m/e* results for cathode rays of the October 1897 paper to draw two major conclusions:

> In gases at low pressures negative electrification, though it may be produced by very different means, is made up of units each having a charge of electricity of definite size; the magnitude of this negative charge is about 6×10^{-10} electrostatic units, and is equal to the positive charge carried by the hydrogen atom in the electrolysis of solutions.
>
> In gases at low pressures these units of negative electric charge are always associated with carriers of a definite mass. This mass is exceedingly small, being only about 1.4×10^{-3} of that of the hydrogen ion, the smallest mass hitherto recognized as capable of a separate existence. The production of negative electrification thus involves the splitting up of an atom, as from a collection of atoms something is detached whose mass is less than that of a single atom.[104]

In a very real sense, then, the experimental results of this paper complete the line of argument that Thomson had first laid out tentatively in the 30 April 1897 talk before the Royal Institution.

A brief pause is required here to consider the logic of this line of argument—more especially, the way in which the conclusions Thomson reached in the October 1897 and December 1898 paper are entering into the reasoning. I have called these conclusions "extended working hypotheses" because each extended the basic working hypothesis underlying the key experiments presented in the paper by appending a value, admittedly rough, to it: the first, a value of *m/e* for the particles forming cathode rays, and the second, a value of *e* for the distinctive quantity of electricity involved in the ionization of gases by x-rays. My further point in calling them extended working hypotheses was that, while Thomson had not established their truth, he had provided strong grounds for predicating ongoing research on them. We can now see the way in which they entered his ongoing research. They did not play the role of assumptions in the experiments presented in the December 1899 paper. Rather, they functioned as premises in the evidential reasoning yielding the conclusions quoted above. Further research was predicated on them in the sense that they made a line of evidential reasoning possible that would have had the character of pure conjecture without them. In effect, Thomson is invoking a version of one of Newton's four rules for inductive reasoning in science, same effect, same cause. The version here is, same distinctive value for a characteristic property of two things, two things of a single kind—or, more precisely, same distinctive order of magnitude for the value of a characteristic property of two things, two things of a single kind.[105] Because the values Thomson is invoking are precise at best only to their order of magnitude, his evidential argument does not establish once and for all either of the conclusions quoted above. Nevertheless, it does provide compelling grounds for accepting them provisionally for purposes of continuing research.

The next sentence in the second of the paragraphs quoted above is, "We have not yet data for determining whether the mass of the negative atom is entirely due to its charge."[106] Thomson is backing off his earlier insistence that the mass is not quasi-mass, most likely because the magnitude of mass he has now obtained would entail, if taken to be quasi-mass, a radius of the corpuscle of the order of 10^{-13} cm, a not altogether implausible value. Typical of the style he has evidenced throughout the three papers included here, he is prepared to leave the question of mass versus quasi-mass for subsequent experimental investigation, suggesting one possible line of experiment himself.

The transition to the final segment of the paper, which considers the electrification of gases generally and not just at low pressure, is effected by Thomson's noting the three different kinds of carriers of charge in gases that experiments have revealed: a carrier of negative charge, with mass three orders of magnitude less than that of the hydrogen atom; carriers of positive charge with masses equal to or greater than that of the hydrogen atom; and carriers of negative charge with masses equal to or greater than that of the hydrogen atom. The first of these dominates electrical conduction in gases at low pressures, and the other two dominate it at higher pressures. Glaringly absent is a carrier of positive charge with small mass, a counterpart to Thomson's corpuscle. This gives his corpuscle a special status which, when joined with the fact that its charge is the characteristic charge of the more massive carriers of both kinds, leads him to the following proposal:

> These results, taken in conjunction with the measurements of the negative ion, suggest that the ionization of a gas consists in the detachment from the atom of a negative ion; this negative ion being the same for all gases, while the mass of the ion is only a small fraction of the mass of an atom of hydrogen.
>
> From what we have seen, this negative ion must be a quantity of fundamental importance in any theory of electrical action; indeed, it seems not improbable that it is the fundamental quantity in terms of which all electrical processes can be expressed. For, as we have seen, its mass and its charge are invariable, independent both of the processes by which the electrification is produced and of the gas from which the ions are set free. It thus possesses the characteristics of being a fundamental conception of

> electricity; and it seems desirable to adopt some view of electrical action which brings this conception into prominence.[107]

Thomson is still resisting the term "electron," doubtlessly because of Larmor's use of the word to cover both positive and negative immaterial centers of charge. Nonetheless, the conclusion of this paper is that the negative ion Thomson is here referring to fulfills the requirements of Stoney's electron, so that the shift to this term had clearly become appropriate at this point.

The second of the paragraphs just quoted ends with the sentence, "These considerations have led me to take as a working hypothesis the following method of regarding the electrification of a gas, or indeed matter in any state." The three pages that follow are richer in detail than the listing of main points I offer here can indicate:

1. All atoms contain negatively charged corpuscles "equal to each other," with a mass around 3×10^{-27} grams, a very small fraction of the mass of any atom.[108] These corpuscles are somehow neutralized in the normal atom.
2. Electrification of a gas involves the detachment of a corpuscle from some of the atoms, turning these atoms into positive ions; negative ions result from a free corpuscle attaching to an atom.
3. In the release of anions and cations at the electrodes during electrolysis of solutions, "the ion with the positive charge is neutralized by a corpuscle moving from the electrode to the ion, while the ion with the negative charge is neutralized by a corpuscle passing from the ion to the electrode. The corpuscles are the vehicles by which electricity is carried from one atom to another."[109]
4. Assuming the hydrogen atom has the positive and the chlorine atom the negative charge in a molecule of HCl, the mass of the hydrogen atom in this molecule is less and the mass of the chlorine atom is greater than their nominal values. The extent to which the mass of an atom can vary from association and dissociation of corpuscles in known processes is proportional to the valence of the atom.
5. In the ionization of gases by x-rays and uranium rays, it appears that no more than one corpuscle can be detached. But the many lines of the spectrum in the Zeeman effect are evidence that atoms generally contain more than one corpuscle, raising the possibility that a process with sufficient energy can tear more than one corpuscle from an atom.

Needless to say, Thomson is calling this a "working hypothesis" not in the narrow sense that I have been using, but in the customary broad sense of a manner of conceptualizing the phenomena in question by which, in the phrasing of *Recent Researches,* "they can be coordinated." Even so, this working hypothesis differs radically in logical status as well as in substance from the one in *Recent Researches*. It is not just a conjecture that can be made qualitatively consistent with known experimental results. It is anchored to a core that has grown from the two premises on which the *m/e* and *e* measurements were predicated: (1) cathode rays and other negative discharges consist of charged particles with a distinct mass-to-charge ratio, and (2) the ionized atoms in an electrified gas have a characteristic magnitude of charge. The results of these *m/e* and *e* measurements, supplemented by the measurements Thomson and his research students carried out on velocities of ions in electrified gases, had yielded experimentally dictated extensions and refinements of these two initially narrow premises. Moreover, the experimental techniques and laboratory technology employed in these measurements were opening the way to further empirically driven extensions and refinements of this core. The extent to which the working hypotheses—in my narrow sense—forming its core had been fleshed out by experiments designed to answer specific questions was the most compelling reason to think that Thomson's new working hypothesis was on the right track.

Four other points about the new working hypothesis should be noted. First, even though the available evidence was indicating that all ionization involves liberation or attachment of a single corpuscle, the magnetic splitting of lines in the spectrum was indicating more than one corpuscle per atom. Thomson leaves the question of the number of corpuscles per atom open for subsequent investigation. Indeed, the new working hypothesis leaves all questions about atomic structure open.

Second, even though Thomson extends his working hypothesis beyond gases to the electrolysis of liquids and ionic bonding, and he says at the outset that it holds for electrification of matter generally, he does not here expressly extend it to conduction in metals. A few months later, at an international conference in Paris, he did propose a free-electron-based account of electrical conduction in metals along the lines that came to be called the Drude theory.[110] By the time he delivered the lectures at the Royal Institution in 1906 that became *The Corpuscular Theory of Matter,* however, he had backed off this view. The problem of the conduction of electricity in metals involved special phenomena, like the Hall effect, that the electron by itself did not shed much immediate light on.[111]

Third, a more conspicuous element missing from the new working hypothesis is any mention of the electrical phenomena in gases on which *Recent Researches* had placed primary emphasis, namely electrical breakdown and the spark discharge at normal pressures and the visible discharge, especially the striated positive column, at reduced pressures. Thomson rectified this by extending his working hypothesis in a paper read to the Cambridge Philosophical Society in February 1900 and published that September in *Philosophical Magazine* under the title, "On the Genesis of the Ions in the Discharge of Electricity through Gases."[112] The central idea of this paper is that corpuscles, when sufficiently accelerated by an electric field, produce further corpuscles either directly when they strike molecules or indirectly from the x-rays then generated. Electric breakdown and the spark discharge occur when corpuscles are liberated in a cascading fashion at high voltages—a proposal Thomson shows is consistent with observed trends, like the electrical force required for breakdown being roughly proportional to the density of the gas. In the case of evacuated tubes, experiments at Cavendish reported in Thomson's paper of March 1899 had led to "the conclusion that there is one centre of ionization close to the cathode, and another in the negative glow."[113] Corpuscles accelerated away from the cathode produce ionization in the negative glow, and corpuscles liberated in it produce the striated positive column. The luminous striae are regions where corpuscles have reached accelerations sufficient to produce ionization, which then reduces the electric force locally, slowing their acceleration; in the dark regions the energy reached by the accelerating corpuscles is below that required for ionization. The asymmetry between phenomena at the anode and cathode result from corpuscles being so much more effective than positive ions in producing ionization.[114]

Fourth, one should note the absence of the ether—more precisely, the ether continuum—in the working hypothesis elaborated in the three pages. The negatively charged electron, not some state or process in the ether, is doing the work. Needless to say, Thomson's experiments had not shown anything about the constitution of electricity in its own right. This is why Thomson speaks carefully of the "carriers of charge." Rather, what the working hypothesis was implying was that a theory covering a wide array of electrical phenomena could be developed without having to address the question of the ultimate constitution of electricity at all. The ether had ceased having a role to play in ongoing research in the areas Thomson was concerned with.

Earlier I remarked that his December 1899 paper would have been a fitting final word of the nineteenth century for *Philosophical Magazine*. The experiments reported in the three papers examined above are very much a product of nineteenth century science. The scientific laws underlying them and the instruments used in them, as well as the various phenomena they exploit and the laboratory practices followed in dealing with these phenomena, are almost entirely products of the nineteenth century where science had reached a position that allowed Thomson, with the help of two working hypotheses, to penetrate experimentally into the microphysics of electrical phenomena.

Aftermath—The Next Decade

The working hypothesis Thomson elaborates at the end of his December 1899 paper comprised only an initial fragment of a theory. A huge amount of experimental work remained to flesh this fragment out in detail, to pin points down, and to revise and refine it where needed. Thomson's order-of-magnitude numbers had generated promissory notes that would remain outstanding until precise values for *m/e*, *e*, and *m* had been determined. Only then would his insistence on their uniqueness be fully justified. Several advances were made in the immediately following years on *m/e*. In 1900 Henri Becquerel used crossed magnetic and electric fields to determine that the *m/e* of the uranium discharge is around 10^{-7}. The velocity he found in the experiments exceeded 60 percent of the speed of light. This led Kaufmann to develop much more precise measures of *m/e* of these particles in 1901–02, correcting for the theoretical change of mass with velocity implied by the Lorentz-FitzGerald equations. The value of *e/m* he zeroed in on was 1.77×10^7 or, inverted, an *m/e* of 0.565×10^{-7}. By the end of the decade values were being given to as many as four significant figures.[115]

Progress on *e* came more slowly. Thomson and his cadre at Cavendish recognized the uncertainties in their 1898 and 1899 results better than anyone, including uncertainties beyond those noted in the papers and above, such as the possible confounding effects of droplet evaporation. C. T. R. Wilson continued to refine techniques in using cloud formation, among other things determining an expansion ratio for which droplets would form almost exclusively on negatively charged ions. Thomson redid the 1898 measurement taking advantage of these advances and using uranium instead of x-rays as the radiation source to achieve a more uniform intensity of irradiation. These results, which he published in 1903 dropped his value of *e* from 6.5×10^{-10} to 3.4×10^{-10}. In the same year Harold Wilson added the further refinement of an electric field aimed vertically upward, counteracting the effects of gravity on the droplets. The values he published ranged from 2×10^{-10} to 4.4×10^{-10}, with a mean of 3.1×10^{-10}.

R. A. Millikan picked up from where Wilson left off, first with water drops, then a single water drop, and finally switching to oil drops to eliminate worries about evaporation. His single-water-drop experiments, published in 1909, gave comparatively stable values clustering around 4.6×10^{-10}. With the oil-drop experiments, which he initiated in 1909, he zeroed in on the tight value of 4.774×10^{-10}, published in 1913 and tightened further in 1917. Even though this value had to be refined two decades later to eliminate a systematic error arising from an inaccuracy in the viscosity for air, the tightness of Millikan's results rightly settled almost all questions about, in his words, "the atomicity of electricity."[116]

Some may want to accuse Thomson of having overreached the earlier data in saying that his corpuscles all have the same *m/e* and *e*. One thing that can be said in reply is that his taking *m/e* and *e* to be uniquely valued, rather than merely having characteristic orders of magnitude, involved little risk. Neither the results of his experiments nor the evidential reasoning on electricity in gases issuing from these results would have been undercut if electrons had later turned out to have several different values of *m/e* and *e*, all of the same order of magnitude.

Moreover, Thomson's stance can be defended as a sound approach to empirical research, reminiscent of Newton's first rule of inductive reasoning: No more causes of natural thing should be admitted than are both true and sufficient to explain their phenomena, restated for the case at hand as, No more complexity or degrees of freedom should be granted inferred entities than is dictated by the phenomena from which they are being inferred. What lies behind this dictum is more than just a blind faith in the simplicity of nature. The simpler a domain of nature is, the easier it is not only to develop a theory of it, but also to marshal high quality evidence bearing on the theory. Where nature is not simple, the best hope for developing a theory and marshalling evidence may be to proceed by successive approximations, starting with the most simple construal of the domain that shows promise of allowing experimental results to extend and refine it in a step by step fashion. Introducing more degrees of freedom in the early stages of theory construction than are absolutely needed runs the risk of having misleading ways of accommodating further experimental findings, heading the theory development process off on a garden path. It is safer to insist that further degrees of freedom and other complexities be added only when clearly forced by experimental results. Something of this general sort happened historically when electron spin proved necessary for the free-electron theory of conduction in metals.[117] No experimental results on conduction in gases and liquids had given reason to grant corpuscles spin, and the subsequent addition of spin in no way undercut any of the evidential reasoning that had issued from these results.

Thomson published the first edition of *Conduction of Electricity Through Gases* in 1903, well before Millikan's results. With the exception of a section on radioactivity, this book amounts to a rewrite of the long chapter on the subject in *Recent Researches* from ten years earlier, but now reflecting the new working hypothesis from December 1899 and the huge body of experimental research attendant to it. The second edition of the book appeared three years later. Even though it dropped the section on radioactivity, leaving that subject to Rutherford's *Radioactivity,* published a year earlier, more recent research expanded the new edition to 670 pages. Remarkably much of this second edition went over almost intact into the third edition two decades later, which Thomson authored jointly with his son. The Bohr model, quantum theory, and the wave character of the electron necessitated less revision of the account of electric conduction in gases than one might think, though they added immensely to it, expanding the work to two volumes and 1,100 pages. In the same year that the second edition was published, 1906, J. J. Thomson received the Nobel Prize for his research on electricity in gases.

That year also marked the first full year of his experimental research on Canalstrahlen or, as he renamed them, rays of positive electricity. He used strong crossed electric and magnetic fields to measure e/m, initially managing to get clean results only for hydrogen and helium, which he published in a *Philosophical Magazine* paper in 1907. He continued to develop the techniques involved in these experiments, joined in the effort by his new experimental assistant, F. W. Aston, in 1910. By 1913, the year in which Thomson's *Rays of Positive Electricity* appeared, they had established two distinct values of e/m for neon, corresponding to atomic weights of 20 and 22, though at that time the interpretation of these results was still very much up in the air. Aston continued this work after WWI, developing the mass spectrograph, which enabled him first to make a decisive case that these were two distinct isotopes of neon and then to distinguish isotopes of a great number of other nonradioactive elements.

Thomson had begun research on rays of positive electricity at the end of 1905 to obtain additional experimental basis for elaborating his "plum-pudding" model of the atom. Much of his effort in the first decade of the twentieth century went into this model. He published two books in which the subject of atomic structure is central during these years, both initially series of lectures, *Electricity and Matter* at Yale in 1903 and *The Corpuscular Theory of Matter* at the Royal Institution in 1906.[118] Both of these books hark back to the hope expressed in the passage from his 1895 paper "The Relation between the Atom and the Charge of Electricity carried by it" quoted earlier: an explanation of the connection between ordinary matter and the electrical charges on the atom should go a long way toward establishing a theory of the constitution of matter. Both books hark back to his earlier work in other ways, too, including the role played by Faraday tubes, especially prominent in the first. For Thomson the plum-pudding model was more than just a hypothesis about atomic structure; it was an attempt at a grand synthesis of his life's work.

When read today, both of these books on atomic structure have far more the flavor of unfettered conjecture than do the three seminal papers of 1897–99, even after adjustments are made for our awareness that the plum-pudding model led nowhere. This gives an impression that Thomson somehow became less a scientist in the years immediately following these papers. This is wrong. No less than before, Thomson was trying to open a pathway that would enable experimental research to develop a detailed theory:

> From the point of view of the physicist, a theory of matter is a policy rather than a creed; its object is to connect or coordinate apparently diverse phenomena, and above all to suggest, stimulate, and direct experiment. It ought to furnish a compass which, if followed, will lead the observer further and further into previously unexplored regions. Whether these regions will be barren or fertile experience alone will decide; but, at any rate, one who is guided in this way will travel onward in a definite direction, and will not wander aimlessly to and fro.[119]

The difference in the case of atomic structure lies in Thomson's failure to find even a fragment of a theory that lent itself to continuing elaboration and refinement through experimental research. This was accomplished by the Danish physicist Niels Bohr, who worked briefly with Thomson in Cambridge before going on to Manchester to work with Rutherford. Manchester provided an atmosphere conducive to Bohr's theoretical approach, and it was there in 1913 that he developed his model of the atom. The most telling piece of evidence Bohr offers for his model in his 1913 *Philosophical Magazine* paper is his purely theoretical calculation of the Rydberg constant:

$$\frac{2\pi^2 me^4}{h^3} = \frac{2\pi^2 e^5}{h^3}\left(\frac{m}{e}\right) = 3.1 \times 10^{15}.$$

Bohr used 4.7×10^{-10} (esu) for e and 1.77×10^{7} (emu) for e/m in this calculation, obtaining a value within 6 percent of the observed value.[120]

Thomson contributed to the Bohr model in one other respect, albeit indirect. Starting while he was Thomson's research student at Cavendish, C. G. Barkla carried out extensive investigations of x-ray scattering during the decade, establishing a wide range of results, including that these rays are transverse electromagnetic waves. Thomson had published a theoretical formula for x-ray scattering in the first edition of *Conduction of Electricity through Gases,* adapting Larmor's old theory of radiation from an accelerated electron. In 1904 Barkla used this formula to infer from scattering results that the number of corpuscles per molecule of air is between 100 and 200. In 1906 Thomson published a paper, "On the Number of Corpuscles in an Atom," in which he concludes on the basis of a refined version of Barkla's result and two other methods that this number is the same as the atomic weight.[121] Looked at carefully, the most that can be said for Thomson's reasoning here is that the number implied by scattering, using then available values of the relevant quantities, was closer to the atomic weight than to any other salient number. While his conclusion misled Thomson in one respect in his work on the atom, it did not in another, for it showed that almost all the mass of the atom is due to something other than corpuscles. Barkla corrected the situation in 1911: "Using the more recently determined values of *e/m*, *e*, and *n* (the number of molecules per cubic centimetre of gas), the calculation gives the number of scattering electrons per atom as about half the atomic weight of the element."[122] Bohr cites Barkla on this in 1913.[123]

Thomson's Contribution to the Electron

The lesser part of Thomson's contribution to the discovery of the electron was his order-of-magnitude measurement of *m/e* for cathode rays and the proposal that the particle in these rays is subatomic. The major part of his contribution was his characterization of this particle as the asymmetrically acting, fundamental factor in ionization and electrical discharges. This part of the contribution, which dates from 1899 and culminates the effort on ionized gases begun by Thomson and his research students early in 1896, had the consequence of redirecting research on electrical conduction and related phenomena by indicating that a detailed theory of these phenomena could likely be developed without having to address questions about the fundamental character of electricity. From the point of view of the history of research into atomic structure, what Thomson's December 1899 paper contributed was primarily an experimentally determined order-of-magnitude for the electron mass, adding support for the subatomic thesis. This explains why most discussions of the discovery of the electron put comparatively little emphasis on this paper, for, viewed from that standpoint, it appears not much more than an addendum to the 1897 paper. From the point of view of the history of research on electrical conduction and the electrification of gases, however, the 1899 paper is most important. Only with it did it become clear that the electron is fundamental to ionization and a variety of electrical discharges and that no positively charged counterpart to it enters into any of these phenomena.

In a sense of the term that has not received the attention it deserves, the December 1899 paper established these claims about the electron. Of course, given the limited extent and quality of Thomson's data, this paper did not establish them once and for all. But it did provide decisive grounds for accepting them as an initial fragment of a theory and, pending evidence to the contrary, taking them for granted in further research aimed at extending this fragment. The success of the continuing further research—both before, but even more so after Bohr added his corresponding initial fragment of a theory of atomic structure—resulted en passant in the increasingly deep entrenchment of Thomson's claims. Nothing has been more central to twentieth century science than the electron. Thomson's 1899 paper has strong claim to being the point of departure for most strands of this effort.

Neither of the limited working hypotheses from which Thomson started—that cathode rays consist of charged particles and that ionization involves a characteristic magnitude of charge—originated with him. Nor did the idea that ions form when a unit charge becomes dissociated from atoms or molecules. What was original in Thomson's contribution was the design of a series of complex experiments predicated on these working hypotheses, enabling order-of-magnitude values of microphysical quantities to be inferred from macrophysical measurements. These values provided the basis for the claims made in the 1899 paper about the fundamental, asymmetric action of the electron. Save perhaps for the subatomic thesis, Thomson's work during this period is not marked by bold proposals. Even the extraordinary conclusion about the asymmetric role of the electron was less a bold proposal than it was a straightforward inference from experimental results. Thomson's contribution in these years thus lies not so much in the conceptual history of science as in the history of evidence. With the work at Cavendish from 1896 to 1899, effective empirical access was gained for the first time to the microphysics of electricity.

Society's predilections in judging the importance of advances in science incline one to underestimate Thomson's achievement with the electron. He put forth no mathematical theory, nor even any lasting laws. His discovery of the asymmetry of charge required no deep insight, and, anyway, this asymmetry is so second nature to us now that we have trouble appreciating how contrary to expectation it was. The experimental evidence Thomson and his research students produced has long since been supplanted by a vast array of higher quality, more definitive evidence, leaving no reason to appeal to it. Indeed, the only one of his experiments from the 1897 to 1899 period that still gets mentioned in physics textbooks is the cross-field experiment on cathode rays, usually with the misleading implication that the modern technology of cathode ray tubes dates from this experiment; in fact, Ferdinand Braun had published his paper describing the cathode-ray oscilloscope, from which CRT technology grew, on 15 February 1897, months before Thomson's experiment.[124]

What considerations like these overlook is how difficult and, even more so, how important to the history of science it is to get a sustained, experimentally driven process of theory elaboration off the ground. This is what Thomson accomplished. The crucial respect in which he went beyond Wiechert, Kaufmann, and others at the time was his successful pursuit of further experiments in 1898 and 1899 to answer questions about the role the electron plays in electrical phenomena.

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Notes

1. The text of this lecture appeared under the title "Cathode Rays" three weeks later in the May 21, 1897 issue of *The Electrician* 39 (1897): 104–109.

2. J. J. Thomson, "Cathode Rays," *Philosophical Magazine* 44 (1897): 293–316.

3. J. J. Thomson, "On the Charge carried by the Ions produced by Röntgen Rays," *Philosophical Magazine* 46 (1898): 528–545.

4. J. J. Thomson, "On the Masses of the Ions in Gases at Low Pressures," *Philosophical Magazine* 48 (1899): 547–567. This paper had been presented a few months earlier at the Dover meeting of the British Association. These three papers, the text of the April talk, and FitzGerald's commentary on it have been reissued, with a historical introduction from which the present paper is adapted, in *The Chemical Educator* 2, n. 6 (1997), i.p. S1430–4171(97)06149–4, avail. url:http//journals.Springer-NY.com/chedr.

5. J. J. Thomson, "The Relation between the Atom and the Charge of Electricity carried by it," *Philosophical Magazine* 40 (December 1895): 512.

6. Emil Wiechert, "Ergebniss einer Messung der Geschwindigkeit der Kathodenstrahlen," *Schriften der physikalischökonomisch Gesellschaft zu Königsberg* 38 (1897): 3–16.

7. Walter Kaufmann, "Die magnetische Ablenkbarkeit der Kathodenstrahlen und ihre Abhängigkeit vom Entladungspotential," *Annalen der Physik und Chemie* 61 (1897): 544–552.

8. Wiechert thought he was measuring the *m/e* of immaterial electrons, and Kaufmann expressly concluded that "the hypothesis which assumes the cathode rays to be charged particles shot from the cathode is insufficient."

9. Isobel Falconer has made a convincing case that the issue over cathode rays was not drawing much attention at the time of Thomson's talk and that Thomson's interest in cath-

ode rays stemmed from other concerns. See her "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. I have been helped by this paper in several places, as well as by John Heilbron's unpublished doctoral dissertation, *A History of the Problem of Atomic Structure from the Discovery of the Electron to the Beginning of Quantum Mechanics,* University of California, Berkeley, 1964 (available from University Microfilms, Inc. Ann Arbor, Michigan). I should also mention David L. Anderson's *The Discovery of the Electron* (Princeton: Van Nostrand, 1964), which first drew my attention to this episode in the history of science and is unfortunately now out of print, and Steven Weinberg's *The Discovery of Subatomic Particles* (New York: Freeman, 1983). The main difference between all of these and the present paper is that they treat Thomson's work primarily from the perspective of the problem of atomic structure, not from that of the problems of the electrification of gases and the conduction of electricity. Finally, I should acknowledge philosophic insights I gained from reading Peter Achinstein's chapters on cathode rays and the electron in his *Particles and Waves: Historical Essays in the Philosophy of Science* (New York: Oxford University Press, 1991), 279–333.

10. George F. FitzGerald, "Dissociation of Atoms," *The Electrician* 39 (21 May 1897): 103–104.

11. J. J. Thomson, "The Relation between the Atom and the Charge of Electricity carried by it,"cited in n. 5, 512.

12. J. J. Thomson, *Conduction of Electricity Through Gases* (Cambridge: Cambridge University Press, 1903), v. (Thomson uses the term "ion" in all three editions of this book to refer to electrons as well as to charged atoms and molecules.) This preface was retained in the second edition of 1906 but dropped from the third edition of 1928 and 1933. Its second paragraph nicely summarizes Thomson's conception of what had been achieved by 1903:

The study of the electrical properties of gases seems to offer the most promising field for investigating the Nature of Electricity and the Constitution of Matter, for thanks to the Kinetic Theory of Gases our conceptions of the processes other than electrical which occur in gases are much more vivid and definite than they are for liquids or solids; in consequence of this the subject has advanced very rapidly and I think it may now fairly be claimed that our knowledge of and insight into the processes going on when electricity passes through a gas is greater than it is in either of solids or liquids. The possession of a charge by the ions increases so much the ease with which they can be traced and their properties studied that, as the reader will see, we know far more about the ion than we do the uncharged molecule.

13. Jed Z. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago: University of Chicago Press, 1985).

14. J. J. Thomson, *Notes on Recent Researches in Electricity and Magnetism* (Oxford: Oxford at the Clarendon Press, 1893), 189. Hereafter, *Recent Researches.*

15. Lord Kelvin, "Presidential Address," *Proceedings of the Royal Society,* 54 (1893): 376–394.

16. Ibid., 386.

17. Ibid., 389. The passage, ending the address, continues: "and if, as I believe is true, there is good reason for hoping to see this step made, we owe a debt of gratitude to the able and persevering workers of the last forty years who have given us the knowledge we have: and we may hope for more and more from some of themselves and from others encouraged by the fruitfulness of their labours to persevere in the work." For details on the problem of the interaction between electricity and matter and its impact on the history of physics at the end of the nineteenth century, see Buchwald, *From Maxwell to Microphysics.*

18. Ibid., 388. Kelvin goes on to list several of Crookes's findings: "the non-importance of the position of the positive electrode; the projection of the torrent *perpendicularly* from the surface of the negative electrode; its convergence to a focus and divergence thenceforward when the surface is slightly convex; the slight but perceptible repulsion between two parallel torrents due, according to Crookes, to negative electrifications of their constituent molecules; the change of direction of the molecular torrent by a neighboring magnet; the tremendous heating effect of the torrent from a concave electrode when glass, metal, or any ponderable substance is placed in the focus; the phosphorescence produced on a plate coated with sensitive paint by a molecular torrent skirting along it; the brilliant colours—turquoise-blue, emerald, orange, ruby-red—with which grey colourless objects and clear colourless crystals glow on their struck faces when lying separately or piled up in a heap in the course of a molecular torrent; 'electrical evaporation' of negatively electrified liquids and solids; the seemingly red hot glow, but with no heat conducted inwards from the surface, of cool solid silver kept negatively electrified in a vacuum of 1/1,000,000 of an atmosphere, and thereby caused to rapidly evaporate."

19. William Crookes, "On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules," *Philosophical Transactions of the Royal Society,* A 170 (1879): 87–134.

20. Eugen Goldstein, "On the Electric Discharge in Rarefied Gases," *Philosophical Magazine,* 10 (1880): part I, 173–190, and part II, 234–247. This is the English translation of a paper that had initially appeared in German. The discussion of Crookes is in part II.

21. "Versuche über die Glimmentladung," *Annalen der Physik und Chemie* 19 (1883): 782–816; translated as "Experiments on the cathode discharge," in Heinrich Hertz, *Miscellaneous Papers* (London: Macmillan and Company, 1896), 224–254.

22. Ibid., 253.

23. Arthur Schuster, "Experiments on the Discharge of Electricity through Gases. Sketch of a Theory," The Bakerian Lecture, *Proceedings of the Royal Society* 37 (1884): 317–339.

24. Arthur Schuster, "The Discharge of Electricity through Gases," *Proceedings of the Royal Society* 47 (1890): 527–559. Schuster's bounds for m/e were 10^{-6} and 10^{-3}, in electromagnetic units.

25. Heinrich Hertz, "Uber den Durchgang der Kathodenstrahlen durch dünne Metallschichten," *Annalen der Physik und Chemie* 45 (1892): 28–32.

26. *Recent Researches,* 126.

27. Phillip Lenard, "Uber Kathodenstrahlen in Gasen von atmosphärischen Druck un in áussersten Vacuum," *Annalen der Physik und Chemie* 51 (1894): 225–267; and "Uber die magnetische Ablenkung der Kathodenstrahlen," 52 (1894): 23–33.

28. *Recent Researches,* 136ff. Thomson does not cite Schuster's Bakerian Lecture here, though he does so elsewhere in the book.

29. J. J. Thomson, "On the Velocity of the Cathode-Rays," *Philosophical Magazine* 38 (1894): 358–365.

30. Ibid., 359. Specifically, Thomson argues that, if the view that cathode rays are aetherial waves "is admitted, it follows that the aether must have a structure either in time or space." See also Heilbron, *A History of the Problem of Atomic Structure,* p. 67.

31. Reminiscent of the preface of Maxwell's book, Thomson remarks in the Preface of *Recent Researches* (vi), "The physical method has all the advantages in vividness which arise from the use of concrete quantities instead of abstract symbols to represent the state of the electric field; it is more easily wielded, and is thus more suitable for obtaining rapidly the main features of any problem; when, however, the problem has to be worked out in all its details, the analytical method is necessary."

32. At the end of the first chapter, entitled "Electric Displacment and Faraday Tubes of Force," Thomson notes: "The theory of Faraday tubes which we have been considering is, as far as we have taken it, geometrical rather than dynamical; we have not attempted any theory of the constitution of these tubes, though the analogies which exist between their properties and those of tubes of vortex motion irresistibly suggest that we should look to a rotatory motion in the ether for their explanation." (Ibid., 52.)

33. Ibid., 2.

34. Ibid., 3.

35. See Buchwald, *From Maxwell to Microphysics,* for details on the problems faced at the time in trying to offer an account of electric conduction.

36. *Recent Researches,* 56.

37. Ibid., 128.

38. Ibid.

39. Ibid., 189.

40. Ibid. In the second clause Thomson is quoting his "On a Theory of the Electric Discharge in Gases," *Philosophical Magazine* 15 (1883): 427–434, where he outlined an approach to ionization based on his vortex atom.

41. p. 190. For more details on Thomson's treatment of electrical conduction in *Recent Researches,* see Buchwald *From Maxwell to Microphysics,* 49–53, and Falconer, "Corpuscles, Electrons and Cathode Rays," 255f.

42. Ibid., 181.

43. Ibid., 190.

44. J. J. Thomson, "On the Effect of Electrification and Chemical Action on a Steam-Jet, and of Water-Vapour on the Discharge of Electricity through Gases," *Philosophical Magazine* 36 (October 1893): 313–327.

45. J. J. Thomson, "The Relation between the Atom and the Charge of Electricity carried by it," *Philosophical Magazine* 40 (December,1895): 511–544.

46. Ibid., 512.

47. Wilhelm Röntgen, *Sitzungsberichte der Würzburger Physikalischen-Medicinischen Gesellschaft,* December 28, 1895. An English translation appeared immediately in *Nature* 53 (January 1896): 274.

48. J. J. Thomson and E. Rutherford, "On the Passage of Electricity through Gases exposed to Röntgen Rays," *Philosophical Magazine* 42 (November 1896): 392. Thomson had announced this effect of Röntgen rays in "On the discharge of electricity produced by the Röntgen rays," *Proceedings of the Royal Society* 59 (February 1896): 391.

49. J. G. Crowther, *The Cavendish Laboratory: 1874–1974* (New York: Science History Publications, 1974): 121. Two years of residence and a thesis on their research work gave these students a Cambridge M.A.; later this became a PhD.

50. J. J. Thomson and J. A. McClelland, "On the leakage of electricity through dielectrics traversed by Röntgen rays," *Proceedings of the Cambridge Philosophical Society* 9 (1896): 126; also J. J. Thomson, "The Röntgen Rays," *Nature,* 53 (February 27, 1896): 391–392.

51. The paper cited in note 48 was first presented at a meeting of the British Academy in November 1896. Rutherford published two further papers in *Philosophical Magazine* the next year: "On the Electrification of Gases exposed to Röntgen Rays, and the Absorption of Röntgen Radiation by Gases and Vapours," 43 (April 1897): 241–255; and "The Velocity and Rate of Recombination of the Ions of Gases exposed to Röntgen Radiation," 44 (November 1897): 422–440. Thomson appended a short note to the former of these, proposing that if x-rays are a form of electromagnetic radiation, they can be regarded as groups of "Faraday tubes travelling outwards through space;" these cause molecules to dissociate, and an ion then forms when precisely one tube becomes detached from its group and its ends become anchored to dissociated parts of a molecule.

52. Jean Perrin, "Nouvelles propriétés des rayons cathodiques," *Comptes Rendus* 121 (1895): 1130–1134.

53. "On the Cathode Rays," *Proceedings of the Cambridge Philosophical Society* 9 (February 1897): 243–244; and *Nature* 55 (March 11, 1897): 453. Townsend immediately followed Thomson's talk with a presentation on electricity in gases and the formation of clouds in charged gases, indicating that "the gases, given off when certain chemical actions are going on, have sometimes a very large electrostatic charge" (ibid., 244–258). I will return to this paper below.

54. *The Electrician* 39 (July 2, 1897): 299.

55. "Cathode Rays," cited in n. 1, 108.

56. Ibid., 109.

57. "On the Influence of Magnetism on the Nature of the Light emitted by a Substance," *Philosophical Magazine* 43 (March 1897): 226–239. Zeeman had announced the effect that bears his name in November 1896 in Holland; an account appeared in the February 11, 1897 issue of *Nature.* Zeeman followed his *Philosophical Magazine* paper with a second that added support to his *m/e* calculation, "Doublets and Triplets in the Spectrum produced by External Magnetic Forces," ibid., 44 (September 1897): 55–60 and 255–259, and a third that began to complicate matters, "Measurements concerning Radiation-Phenomena in the Magnetic Field," ibid., 45 (February 1898): 197–201.

58. "Dissociation of Atoms," cited in n. 10, 104. 1897 was the sixtieth year of Queen Victoria's reign—hence FitzGerald's reference to "this Jubilee year."

59. Thomson was the first to call attention to the electromagnetic inertia of a moving charge in his "On the Electric and Magnetic Effects produced by the Motion of Electrified Bodies," *Philosophical Magazine,* 11 (1881): 229–249—the paper that first brought him to prominence. FitzGerald had made important additions to this finding.

60. G. Johnstone Stoney, "On the Physical Units of Nature," *Philosophical Magazine* 11 (1881): 381–390; see specifically page 387. This paper had been presented to the British Association in 1874. The term "electron" does not occur in the paper, but Stoney had apparently begun using it, and others had picked it up from him.

61. Joseph Larmor, "A Dynamical Theory of the Electric and Luminiferous Medium," *Philosophical Transactions of the Royal Society,* A 185 (1894): 719–822. See Buchwald, *From Maxwell to Microphysics,* for details of this theory, which was based largely on emission and absorption spectra.

62. Thomson's assistant at the time was Ebenezer Everett (incorrectly spelled 'Everitt' at the end of the 1897 paper). Thomson was legendarily inept in the laboratory, and Everett apparently always endeavored to keep him away from the apparatus. The experiments were therefore most likely carried out by Everett.

63. "Cathode Rays," cited in n. 2, 302.

64. Ibid., 312.

65. Ibid., 296.

66. Thomson introduced just such corrections in *Conduction of Electricity Through Gases;* see, for example, the second edition (1906), chapter V, 118–121, as well as elsewhere in this chapter.

67. R. J. Strutt, "The Dispersion of the Cathode Rays by Magnetic Force," *Philosophical Magazine* 48 (November 1899): 478–480. One must wonder whether this paper would have been so readily accepted had it not been communicated to the journal by Lord Rayleigh.

68. Our current value for *m/e* for the electron is $0.56856314 \times 10^{-7}$ emu, corresponding to a mass of $9.1093897 \times 10^{-28}$ grams and a charge of $1.60217731 \times 10^{-19}$ coulombs

($4.80653193 \times 10^{-10}$ esu). See W. N. Cottingham, "The Isolated Electron," in *Electron: A Centenary Volume* (Cambridge: Cambridge University Press, 1997): 24–38, for a discussion of current methods of measurement.

69. Four decades later, Thomson said, "These experiments were of an exploratory nature; the apparatus was of a simple character and not designed to get the most accurate results. . . . These results were so surprising that it seemed more important to make a general survey of the subject than to endeavour to improve the determination of the exact value of the ratio of the mass of the particle to the mass of the hydrogen atom." J. J. Thomson, *Recollections and Reflections* (New York: Macmillan, 1937), 337f.

70. The fact that these values are well below Hertz's eleven earth-quadrants per second is further evidence that the electric fields in his attempted electrostatic-displacement experiments were lower than he thought.

71. An often remarked irony of this episode in the history of science is that Thomson's son George shared in the Nobel Prize given for establishing the wave-like character of electrons.

72. "Ergebniss einer Messung der Geschwindigkeit der Kathodenstrahlen," cited in n. 6. A year later Wiechert went a step further in determining *m/e,* using a pair of high frequency coils oscillating in phase to determine the velocity of the cathode rays from the timing required for the second coil to cancel the deflection of the first; see "Experimentelle Untersuchungen über die Geschwindigkeit und die magnetische Ablenkbarkeit der Kathodenstrahlen," *Annalen der Physik und Chemie* 69 (1899): 739–766.

73. "Die magnetische Ablenkbarkeit der Kathodenstrahlen und ihre Abhängigkeit vom Entladungspotential," cited in n. 7. Like Thomson, Kaufmann also determined that *m/e* does not vary with the gas in the tube or the material of the electrode. Unlike Thomson, he put a good deal of subsequent effort into determining more precise values for *m/e*. See Kaufmann and E. Aschkinass, "Uber die Deflexion der Kathodenstrahlen," *Annalen der Physik und Chemie* 62 (1897): 588–595; and Kaufmann, "Nachtrag zu der Abhandlung: 'Die magnetische Ablenkbarkeit der Kathodenstrahlen und ihre Abhängigkeit vom Entladungspotential'," *Annalen der Physik und Chemie* 62 (1897): 596–598; "Die magnetische Ablenkbarkeit electrostatisch beeinflusster Kathodenstrahlen," *Annalen der Physik und Chemie* 65 (1898): 431–439; and "Bemerkungen zu der Mittheilung von A. Schuster: 'Die magnetische Ablenkung der Kathodenstrahlen'," *Annalen der Physik und Chemie* 66 (1898): 649–651.

74. Phillip Lenard, "Uber die electrostatischen Eigenschaften der Kathodenstrahlen," *Annalen der Physik und Chemie* 64 (1898): 279–289.

75. Wilhelm Wien, "Untersuchungen über die elektrische Entladung in verdünnten Gasen," *Annalen der Physik und Chemie* 65 (1898): 440–452.

76. For a review of measurements of *m/e* in these early years, see J. J. Thomson and G. P. Thomson, *Conduction of Electricity Through Gases,* 3rd ed., vol. I (New York: Dover, 1969), chapter VI, 229–290. (Volume I of the original of this Dover republication appeared in 1928 and volume II in 1933.)

77. *Philosophical Magazine* 45 (February 1898): 172–183.

78. Cited in n. 3.

79. *Philosophical Magazine* 44 (November 1897): 422–440. This paper was submitted on July 19, one month before Thomson submitted his cathode ray paper.

80. C. T. R. Wilson, "Condensation of Water Vapour in the Presence of Dust-free Air and other Gases," *Philosophical Transactions of the Royal Society,* A 189 (1897): 265–307. For a detailed discussion of Wilson's development of the cloud chamber, both in the period of 1895–1900 and subsequently, see Peter Galison and Alexei Assmus, "Artificial Clouds, Real Particles," in *The Uses of Experiment: Studies in the Natural Sciences,* ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge University Press, 1989), 225–274. J. J. Thomson had published a key theoretical result underlying the use of droplet formation to detect ions two years before Wilson joined the Cavendish Laboratory in "On the Effect of Electrification and Chemical Action on a Steam-Jet, and of Water-Vapour on the Discharge of Electricity through Gases," *Philosophical Magazine* 36 (1893): 313–327.

81. John Townsend, "On Electricity in Gases and the formation of Clouds in Charged Gases," *Proceedings of the Cambridge Philosophical Society* 9 (February 1897): 244–258. Townsend obtained a value of 2.8×10^{-10} for the positively charged ion of oxygen and 3.1×10^{-10} for the negatively charged ion. As remarked in n. 53, Townsend's main point in this paper, presented some ten weeks before Thomson's first announcement of *m/e* for cathode rays, was that the gases released in electrolysis, contrary to what had been thought before, are electrified. Nothing in the paper indicates that Townsend was trying to measure a fundamental unit of charge; the paper does not compare the result he obtained with the charge per atom in electrolysis.

82. This correction is presented more clearly in Thomson's December 1899 paper, cited in n. 4, p. 562. For details of C. T. R. Wilson's wrestling with the electrification produced by cosmic rays, see Galison and Assmus, *The Uses of Experiment,* 254–257.

83. "On the Charge carried by the Ions produced by Röntgen Rays," cited in n. 3, 543.

84. R. A. Millikan's *The Electron* (Chicago: University of Chicago Press, 1924), 31, is the source for this claim. The text he refers to is O. E. Meyer's *Kinetische Theorie der Gase* (335, 1899).

85. "On the Charge carried by the Ions produced by Röntgen Rays," cited in n. 3, 544.

86. *Philosophical Magazine* 47 (March 1899): 253–268.

87. Cited in n. 4.

88. Rutherford's measurements on gases electrified by uranium radiation led him into the research on transmutation for which he won the Nobel Prize.

89. "On the Theory of Conduction of Electricity through Gases by Charged Ions," cited in n. 86, 254.

90. William Sutherland, "Cathode, Lenard, and Röntgen Rays," *Philosophical Magazine* 47 (March 1899): 268–284.

91. Ibid., 269. Sutherland goes on to acknowledge the experimental work of Thomson and Kaufmann: "Whatever proves to be the right theory of the nature of the cathode rays, the quantitative results which these experimenters [Thomson and Kaufmann] have obtained (as did also Lenard), in a region, where, amid a bewildering wealth of qualitative work, the quantitative appeared as if unattainable, must constitute a firm stretch of the roadway to the truth."

92. Ibid., 284.

93. "Note on Mr Sutherland's Paper on the Cathode Rays," *Philosophical Magazine* 47 (April 1899): 415–416.

94. Ibid., 416.

95. "On the Masses of Ions in Gases at Low Pressures," cited in n. 4, 548.

96. This was not the only complication in these measurements. The schematic shown in figure 1.4 is misleading in hiding from view the special efforts required in isolating and controlling the discharges sufficiently to allow meaningful measurements.

97. There appears to be a misprint in the table of results for the incandescent filament. The value of V in the last row should be 100×10^8, not 120×10^8. This is not the only misprint in this paper. A more egregious error occurs on line 4 of p. 563, where the exponent should be -10, not -8.

98. Ibid., 556.

99. Ibid., 557.

100. C. T. R. Wilson, "On the Condensation Nuclei produced in Gases by the Action of Röntgen Rays, Uranium Rays, Ultra-violet Light, and other Agents," *Philosophical Transactions of the Royal Society* A 192 (1899): 403–453.

101. Ernest Rutherford, "The Discharge of Electrification by Ultra-violet Light," *Proceedings of the Cambridge Philosophical Society* 9 (1898): 401–417.

102. John Townsend, "The Diffusion of Ions into Gases," *Philosophical Transactions of the Royal Society,* A 193 (1899): 129–158; and also "The Diffusion of Ions produced in Air by the Action of a Radio-active Substance, Ultra-violet Light and Point Discharges," ibid., 195 (1901): 259–278.

The first of these papers is remarkable in its own right, for like Thomson's seminal papers, it presents a difficult, theory-laden experiment and then combines the results of this experiment with other Cavendish results to draw several basic conclusions. The experiment, predicated on Maxwell's diffusion theory, determined values for the coefficient of diffusion for x-ray generated ions of different gases by having the ions pass down a long, narrow tube and measuring the rate at which they became neutralized by contact with the metal walls of the tube. From this value of the coefficient of diffusion, together with Rutherford's previously determined value of the velocity u of such ions under a potential gradient (see n. 79), Townsend inferred a magnitude for Ne, where N is the number of molecules per cubic centimeter under standard conditions. The uniformity of this magnitude for ions of different gases and its close correspondence to the value for NE from electrolysis (where E is the charge per hydrogen atom), then allowed Townsend to conclude, *independently of any specific value of e or N,* that the charge per ion, when generated by x-rays, is the same as the charge on the hydrogen atom in electrolysis. Adopting Thomson's 1898 value of 6.5×10^{-10} for *e,* Townsend goes on in the paper to infer, among other things, values of 20×10^{18} for N and 4.5×10^{-24} grams for the mass of the hydrogen molecule. The subsequent paper extends the results to ions produced in other ways.

103. "On the Masses of the Ions in Gases at Low Pressures," cited in n. 4, 563.

104. Ibid.

105. It goes without saying that conclusions reached by means of this rule, whether in this or Newton's original form, are not guaranteed to be true. Newton recognized this in his fourth, and last, rule of reasoning: "In experimental philosophy, propositions gathered from phenomena by induction should be considered either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions."

106. Ibid.

107. Ibid. 565.

108. The text of the paper gives as value of the mass "about 3×10^{-26} of a gramme." (ibid.). This is an obvious error insofar as the number is being obtained from an *e* of 6.5×10^{-10} esu (i.e. 2.17×10^{-20} emu) and an e/m of 7×10^{6}. Whether Thomson himself made the error in calculation is unclear. The value might well have been given as '$.3 \times 10^{-26}$' in the manuscript. The uncorrected typographical errors noted in n. 97 above are clear evidence that Thomson either did not proof read the galley proofs of this paper at all or did so extremely hurriedly.

109. Ibid., 566.

110. J. J. Thomson, "Indications relatives à la constitution de la matiere par les recherches récentes sur le passage de l'électricité à travers les gas," *Congres International de Physique* 3 (Gauthier-Villars, Paris): 138.

111. Paul Drude's initial theory of 1900 allowed both positively and negatively charged electrons ("Zur elektronen Theorie der Metalle," *Annalen der Physik und Chemie* 1 (1900): 566–613 and 3 (1900): 369–402. This further underscores the importance of Thomson's December 1899 paper, for it is where he first presents the evidence for the asymmetric role of the negatively charged corpuscle in electric conduction. For more details of early free-electron theories of electrical conduction in metals, see A. B. Pippard, "J. J. Thomson and the discovery of the electron," *Electron: A Centenary Volume,* ed. Michael Springford (Cambridge: Cambridge University Press, 1997), 1–23, especially 14–17. For a discussion of the Drude theory, its limitations, and the subsequent quantum free-electron model, see Brian K. Tanner, *Introduction to the Physics of Electrons in Solids* (Cambridge: Cambridge University Press, 1995).

112. J. J. Thomson, "On the Genesis of the Ions in the Discharge of Electricity through Gases," *Philosophical Magazine* 50 (September 1900): 278–283. Thomson opens the paper by reminding readers of his Grotthus chains in *Recent Researches* and then noting: "Since

that was written, many investigations have been made which have proved that where electrified particles move through a gas ions are produced under certain circumstances, at any rate if the particle is negatively electrified." (279)

113. Ibid., 282.

114. I have not assigned the same importance to this paper of 1900 as to the three seminal papers of 1897 to 1899 because, unlike them, it presents no experiments yielding new results, but only proposes an extension of the working hypothesis that culminates the 1899 paper.

115. The advances discussed here and below involved so many papers that I have generally chosen not to give citations. References can be found in the readily available Thomson and Thomson, *Conduction of Electricity.* Details on Thomson's work during the decade can be found in George Thomson's *J. J. Thomson: Discoverer of the Electron* (Garden City: Anchor, 1966).

116. For Millikan's version of all of this, see his book, cited in n. 84.

117. See Tanner, *Introduction to the Physics of the Electrons.*

118. *Electricity and Matter* (New Haven: Yale, 1904) and *The Copuscular Theory of Matter* (New York: Charles Scribner's Sons, 1907). The latter is especially concerned with the conduction of electricity in metals.

119. *The Corpuscular Theory of Matter,* 1f.

120. Our current value for e, $4.80653193 \times 10^{-10}$ esu, is 2.27 percent greater than the value Bohr used, versus the 0.64 percent difference between his and our current value, 1.7588×10^{7} emu, for e/m. The discrepancy in e, taken to the fifth power, amounts to 11.86 percent, which was partly, but not completely compensated by his using 6.5×10^{-27} for h, 1.91 percent below our currrent value.

121. *Philosophical Magazine* 11 (1906): 769–781. See also *The Corpuscular Theory of Matter,* chapter VII.

122. Charles G. Barkla, "Note on the Energy of Scattered X-radiation," *Philosophical Magazine* 21 (1911): 648–652. Ironically, this paper was in response to J. G. Crowther, a research student working under Thomson, who had argued that the number of scattering electrons in aluminum is greater, not less, than its atomic weight. The specific values Barkla used in 1911 were: $e/m = 1.73 \times 10^{7}$ (from Bucherer), $e = 4.65 \times 10^{-10}$ (from Rutherford and Geiger), and $n = 28 \times 10^{18}$ (from Rutherford); Thomson had used $e/m = 1.7 \times 10^{7}$ and $e = 3.6 \times 10^{-10}$ in 1906.

123. "On the Constitution of Atoms and Molecules," part II, *Philosophical Magazine* 26 (September 1913): 29.

124. Friederich Kurylo and Charles Susskind, *Ferdinand Braun: A Life of the Nobel Prizewinner and Inventor of the Cathode-Ray Oscilliscope* (Cambridge: MIT Press, 1981), 90f. Braun used crossed magnetic fields. The modern cathode ray tube uses crossed electric fields, and hence Thomson's experiment did contribute something of value to this technology.