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Chemistry and History

J. J. Thomson and The Electron: 1897–1899 An Introduction

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Three Seminal Papers of J. J. Thomson

This being the 100th anniversary of J. J. Thomson's discovery of the electron, the October 1897 paper in which he presented his case that cathode rays are streams of subatomic "corpuscles" is attracting a great deal of attention. Viewed from 100 years later, this paper stands out as the starting point for the research into the structure of the atom that has dominated 20th-century science. Viewed in its original historical context, however, this paper was but one of a group by Thomson and his Cavendish Laboratory research students and is matched in importance by his two ensuing papers: "On the Charge of Electricity carried by the Ions produced by Röntgen Rays" published in December 1898 and "On the Masses of the Ions in Gases at Low Pressures" published in December 1899. All three of these seminal papers, which appeared in the prestigious *Philosophical Magazine*, are included here, along with the published text of Thomson's talk

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of April 30, 1897, in which he first put the subatomic proposal forward and George Fitzgerald's commentary on this talk.

Thomson's October 1897 paper is extraordinary, a model of clarity as well as a watershed in the history of science. For twenty years I have been using it to initiate my course in the philosophy of science. Student interest in it is easy to arouse, even without my feeble witticisms about the knowledge of cathode ray tubes that they must have gained from the number of hours they have spent staring at them. Students also have little trouble following the paper. In contrast to most papers of the time about electrical phenomena, it is not laden with talk of æthereal processes; the only terminological adjustment students need to make is from Thomson's "corpuscle" to our "electron". The most important virtue of Thomson's paper from my point of view, however, is that I can use it to show students what is involved in *doing* science. I know of no paper that better displays the problem of marshaling evidence in the early stages of theory construction.

The key experiments in this paper proceed 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. The logic underlying these experiments is complex, involving a number of principles from previous science and many simplifying assumptions, some of them not so transparently justified. The experiments themselves, besides requiring special precautions to execute, included an anomaly that Thomson had to work his way around. Thomson's data were less than perfect with more than a factor of four variation in the m/e values he obtained. Students love to critique the experiments, and they often react with amusement to the conjectures on atomic structure at the end of the paper. Yet, they know perfectly well that a huge amount of 20th century science starts with this paper, and a compelling case can be made that it fully deserves the acclaim it has long received.

As good as this paper is for showing students what is involved in doing science, it is still better when read together with Thomson's papers of December 1898 and December 1899. Reading his 1897 paper by itself, or even together with his April 1897 talk, gives a misleading impression of what he was up to at the time. Both the paper and the talk 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

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æthereal 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 [1].

A second aim stated in Thomson's 1897 paper was to answer the question, "What are these particles?" The increasing importance of this question to Thomson as he wrote the paper becomes clearer from reading the text of his April talk. Partly in response to Fitzgerald's commentary, the paper advances considerably more evidence than the talk that the particles are subatomic. Nevertheless, in contrast to the rapid acceptance of the particle view of cathode rays, the subatomic claim, while drawing a great deal of attention, did not begin to be accepted until after his December 1899 paper. A good question for students is, What exactly did the October 1897 paper show about the particles forming cathode rays, and what remained to be shown in order 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 standard picture of Thomson's 1897 achievement is that he discovered the electron and then went off on the garden path about 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.

This is not an accurate picture of what Thomson accomplished. His central concern at the time was with "the connexion between ordinary matter and the electrical charges on the atom [2]." 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 is the same as the charge per hydrogen atom in electrolysis. His December 1899 paper reported that the m/e of both the electrical

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discharge in the photoelectric effect and the electrical discharge from incandescent filaments is the same as the *m*/*e* of cathode rays he obtained in 1897, and the *e* in the photoelectric effect is the same as the charge per ion in gases ionized by x-rays that he obtained in 1898. At the end of the 1899 paper Thomson put forward a new "working hypothesis" for electrical phenomena in gases in which his 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. The 1897 paper deserves its fame, for it truly is a watershed in the history of science. Nevertheless, the line of research that led Thomson into his investigations of cathode rays culminates with the 1899 paper. The three papers together, prefaced by the April 1897 talk and Fitzgerald's response to it, enable students to see the full magnitude of the problem that Thomson faced in his pursuit of ways of penetrating experimentally into the microstructure of electricity.

Some Historical Background

Thomson's talk of April 30, opens with a review of the history of cathode ray research, so I need not go into great detail here [3]. The phenomena of rays emanating from a cathode in an evacuated tube was first announced by Julius Plücker in 1858 [4]. The case for their consisting of charged particles was made by William Crookes in 1879 [5], though mean-free-path considerations raised questions about their being charged molecules. The case against their consisting of charged particles was made by Heinrich Hertz in 1883 [6]. 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 (Figure 1). In a third set of experiments he was unable to deflect cathode rays electrostatically, from which he concluded that the only way they 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 [7]." Nevertheless, Hertz's findings did not stop Arthur Schuster from

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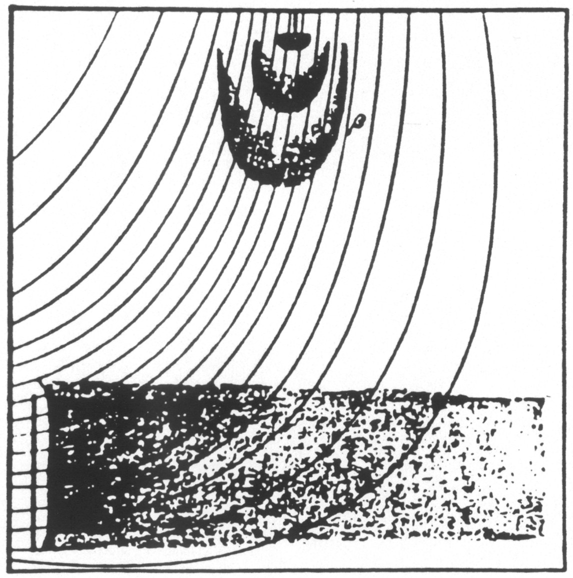

FIGURE 1: HERTZ'S FIGURE SHOWING LINES OF CURRENT DEPARTING FROM THE CATHODE RAYS IN HIS 1883 PAPER.

continuing with the particle hypothesis. In 1884 he formulated an algebraic relationship between the *m*/*e* and the velocity of the particles implied by their curved trajectory in a magnetic field [8], and in 1890 he estimated upper and lower bounds for their *m*/*e* [9].

Two results on cathode rays from the early 1890s were important. First, in 1894 Hertz's protégé Phillipp Lenard reported experiments in which the cathode rays appeared to penetrate right through thin aluminum foils into the open air [10]. The question as to whether the rays in the air were truly cathode rays was held open in England by calling them "Lenard rays," allowing for the possibility that they are generated when cathode rays strike thin metal foils. Second, in 1895 Jean Perrin reported an experiment in which, contrary to Hertz, the negative electric charge does accompany the cathode rays [11]. A refined version of Perrin's experiment is the centerpiece of Thomson's talk of April 30, 1897.

J. J., as he was generally called, had succeeded Lord Rayleigh as the third Cavendish Professor and head of the Cavendish Laboratory in 1884, at the tender age of 28. After training, first in engineering and then in physics and mathematics at Owens College in

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Manchester where Schuster was one of his teachers, he matriculated at Cambridge, graduating in 1880. Although he was not a student of Maxwell's (who died at age 48 in 1879), Thomson's research between 1880 and 1897 was very much in the tradition of Maxwell's work in electricity and magnetism. The title page of Thomson's 1893 book, *Notes on Recent Researches in Electricity and Magnetism*, includes as subtitle, "Intended as a Sequel to Professor Clerk-Maxwell's Treatise on Electricity and Magnetism." In surveying the progress made in the field in the 20 years after Maxwell's *Treatise*, Thomson's book was no less committed than Maxwell's to combining abstract mathematical theory and experiment with concrete models of the mechanisms and processes underlying electric and magnetic phenomena [12]. The model dominating Thomson's book is not the æther, but the Faraday tube [13]—"tubes of electric force, or rather of electrostatic induction, ...stretching from positive to negative electricity [14]." 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 [15]."

Whether modeled in terms of the æther or Faraday tubes, charge at the time was not thought of as a property of matter akin to mass, and the problem of the relation between electricity and ordinary matter had become a focus of research [16]. In extending Clausius's kinetic theory of gases into statistical mechanics, Maxwell had established the molecular composition of matter, and such molecular properties as their (approximate) sizes and mean free paths had been inferred from macroscopic phenomena. Electrically, gases are peculiar: though normally non-conducting, they become conductors under certain distinctive, often dramatic, circumstances. This made electrical phenomena in gases a natural place to turn to in an effort to gain insights into the interaction of electricity and atoms or molecules. Chapter II of Thomson's 1893 book, following the introductory chapter on Faraday tubes, presents 154 pages covering recent research on the "passage of electricity through gases," including investigations of his own, which he had begun in 1890. The chapter reviews a wide array of experimental results, many pointing to seemingly anomalous nonsymmetries between positive and negative electricity. Even though Thomson ends the chapter with a sketch of a theory, the main impression readers today will draw from it is that no one

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at the time was in a position to assemble the complicated, often conflicting experimental results into a body of coherent evidence.

Thomson's chapter on the passage of electricity through gases includes several sections on electrical discharges in rarified gases and hence on cathode rays. In the year after the book was published, Thomson made a stab at determining the velocity of cathode rays in an effort to show that they are not electromagnetic waves [17]. That cathode rays themselves were not his principal concern at the time, however, is clear from a long theoretical paper he published in December 1895 entitled "The Relation Between the Atom and the Charge of Electricity Carried by It." The following remark motivates the conjectures put forward in this paper:

> 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 [18].

The rest of this paper, mostly devoted to trying gyroscope-like analogies and their directional asymmetries to model the different preferences of atoms for positive and negative electricity, was made obsolete by Thomson's research over the next four years.

December 1895 was more notable for the publication of Wilhelm Röntgen's paper announcing the discovery of x-rays [19]. 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 [20]." 1895 was also the year in which Cambridge University first began admitting graduates of other universities as "research students [21]." Ernest Rutherford from New Zealand and John Townsend and J. A. McClelland from Ireland became research students at Cavendish at the end of

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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 Rutherford worked closely 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 [22].

J. J. Thomson on Cathode Rays—1897

The first public indication that Thomson was doing experiments on cathode rays was a February 8 talk he gave to the Cambridge Philosophical Society, reported a month later in *Nature* [23]. 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 [24]."

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."

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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 electrostatic 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 $I = \rho H$, where ρ is the radius of curvature of the rays deflected by a magnetic field of strength *H*. Of course, 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." Thomson 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 [25].

As the title, "Dissociation of Atoms," suggests, Fitzgerald's comments focus 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 underscores 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 [26].

Fitzgerald's stance is 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 notes how little was known "about the inner nature of conduction and the transference of electricity from one atom of matter to another"—makes other interpretations not hard to find. The alternative line of interpretation Fitzgerald develops is that cathode rays consist of æthereal "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 [27]."

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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 [28]. In the early 1890s Joseph Larmor of Cambridge had adopted the term at Fitzgerald's instigation for the unit "twists" of æther comprising the atom in his theory of atomic structure [29]. (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 æthereal 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. For the results reported 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 [30]. Because the rays liberated gas from the walls of the tube, the rays had to be run in the tube and the tube then be re-evacuated several times in order to eliminate sufficiently the nullifying effects of ions in the residual gas.

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Thomson submitted his paper on August 7, 1897, three months after his first going public with the subatomic hypothesis. The paper has three principal parts. After posing the particle versus æther-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 negatively electrified body moving along the path of these rays, I can see no escape from the conclusion that they are charges of negative electricity carried by particles of matter. 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.

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.

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 his paper of 1894 and on effects observed with different cathode materials.

Only the experiments for m/e in the third part of the paper require much comment here. Figure 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

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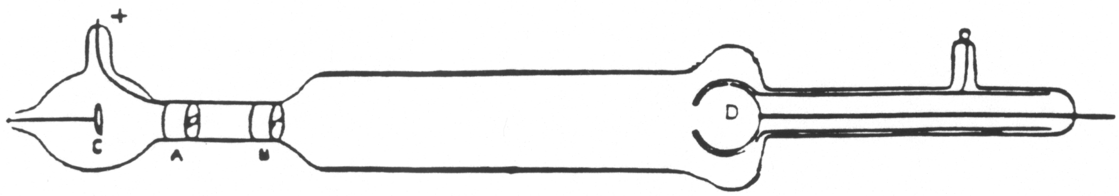

FIGURE 2. A SCHEMATIC OF ONE OF THE THREE KINDS OF TUBES THOMSON USED IN HIS FIRST APPROACH TO MEASURING *m*/*e* OF CATHODE RAYS (BASED ON THE DESCRIPTION IN HIS TEXT).

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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 (p 4), 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 ϕ 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 involved only macrophysical measurements.

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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 they were less firmly entrenched in 1897 than they are now. The derivations also presuppose a variety of further assumptions. (A good exercise for students is to list all the nonmathematical assumptions required as premises in order to derive the expressions for *m*/*e*.) Some of these assumptions serve only to simplify the mathematics. For example, in deriving the angular displacement of the beam in a magnetic field, 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. For example, he assumes 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.

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 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

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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 it 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 the 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 he was still an undergraduate at Trinity College, Cambridge, and it was announced in a paper in the November 1899 issue of *Philosophical Magazine* [31]. No one at Cavendish appears to have repeated the cathode ray *m*/*e* measurements when this discovery was made.)

Of course, 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 (p 14) range from a low of 0.31×10^{-7} to a high of 1.0×10^{-7} and, from the second method (p 17) they range from a low of 1.1×10^{-7} to a high of 1.5×10^{-7} [32]. Looking at his *m*/*e* numbers by themselves, therefore, one can

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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 what one might call 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 really 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} *esu—presumably subatomic corpuscles of a single, universal type* [33]. He set the question of 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. 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?; (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?

Two 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 wavelike. 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 s^{-1}—that is from roughly 7 to 43% of the speed of light [34]. 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 wavelike character is via the tacit premise that anything consisting of particles cannot have a wavelike character. But this premise is 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 wavelike character of electrons was established three decades later [35].

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Second, it must be pointed out that Thomson was not the only one measuring *m*/*e* for cathode rays at the time. Both Emil Wiechert [36] and Walter Kaufmann [37] 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 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. The question thus arises as to whether Thomson has received more credit than he is due for being *the* discoverer of the electron. I prefer to leave this question to others, instead calling attention to three respects in which Thomson's work was distinctive. 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. Third, he alone immediately proposed that the particles forming cathode rays 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 [38]. In 1886 Eugen 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 [39]. By contrast, while others were pursuing refined measures of *m*/*e* for cathode and related rays, Thomson, though noting their results [40], 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 [41]. In it Thomson derives theoretical expressions for the magnetic force

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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 wavelength. The second paper, "On the Charge of Electricity carried by the Ions produced by Röntgen Rays," is the one that appeared in December, included here. 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 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, titled "The Velocity and Rate of Recombination of the Ions of Gases exposed to Röntgen Radiation [42]." 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 s^{-1} per V cm^{-1} potential gradient (i.e., 480 cm s^{-1} 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 [43]. Presumably, the ions act as nuclei around which droplets of water form.

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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 [44]. 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 was 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. (Students who have read only Thomson's paper have no hope of identifying all the assumptions lying behind this experiment.) 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 on p 8 of the paper) has a distinctly "Rube Goldberg" character. The ionized gas is contained in the vessel A, which is covered by a grounded aluminum plate and 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

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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 (pp 1–3) presents the basic ideas underlying the experiment. The second (pp 3–7) describes precautions taken to assure that the level of radiation and the amount of expansion were appropriate. The third (pp 7–11) 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 (pp 11–16) 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 (pp 16–18) presents the results for e obtained from several trials for air and for hydrogen. The last part (pp 18–19) 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 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 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?

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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 [45]. (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 by moisture coating the walls of the vessel. Neither of these corrections seem to have been introduced solely to make the data appear better behaved.

The values Thomson reported for *e* in air (p 16) have a range about their mean of roughly ±16%. His corrected mean value for air is 6.5×10^{-10} electrostatic units, around 35% above our 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 (p 17) 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.” 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 prefers, 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

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suggest. For example, a prominent 1899 textbook in kinetic theory gave 60×10^{18} as the value [46]. 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."

Just as with his *m/e* experiments, the aim 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 is struggling to find experiments involving macrophysical measurements that will yield some reasonably dependable conclusions about microphysical processes. In this early stage of research, working hypotheses are having to stand in for established theory in the logical design of experiments. The results of his *e* experiment could, in principle, have provided good reasons for abandoning the wishful thought that nature is simple in the way the working hypothesis italicized above says it is. They did not. Instead, in spite of their roughness and uncertainty, his results showed this 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 [47], and "On the Masses of Ions in Gases at Low Pressures" in December. 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 photo-electric discharge produced by ultraviolet light [48], 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

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> velocities of the ions produced in different ways differ from each other, yet they all show this peculiarity.

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. Nevertheless, this case allows him 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, titled "Cathode, Lenard, and Röntgen Rays [49]." 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 [50]." The principles Sutherland has in mind are those of æther theory and Larmor's æthereal 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 [51]." 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 [52], Thomson points to questions about whether an impacting quasi-mass is sufficient to produce x-rays and to questions about how æthereal 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 follows:

> 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

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> the view that cathode rays can be produced without the interposition of matter at all by splitting up neutrons into electrons [53].

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 19th century from this journal. The paper consists of five parts. The first (pp 1–2) 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." The second part (pp 2–8) 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 (pp 8–11) 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 (pp 11–17) 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 (pp 17–21) 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 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. 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 (Figure 3). 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

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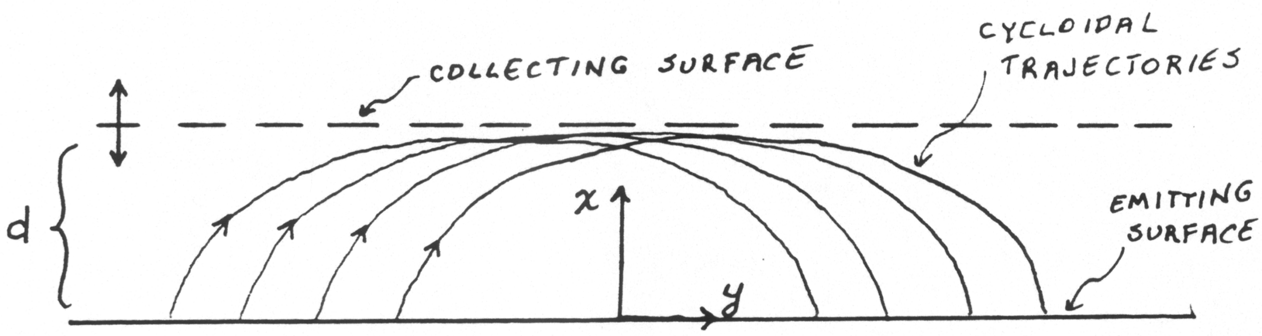

FIGURE 3: THE CYCLOIDAL PATH OF THE PHOTOELECTRIC DISCHARGE UNDER THE ACTION OF AN ELECTRIC FORCE PARALLEL TO THE *X*-AXIS AND A MAGNETIC FORCE PARALLEL TO THE *Z*-AXIS. FOR AN APPROPRIATE COMBINATION OF ELECTRIC AND MAGNETIC FORCE AND THE DISTANCE *d*, THE PARTICLES WILL CEASE REACHING THE COLLECTING PLATE.

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:

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

According to this theory, there should be a sharp cut-off 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.

In the case of the photoelectric discharge, the paper gives the results of seven trials of the experiment with different distances d (p 8). 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 crossed-field method. The same is true of the five e/m values obtained in the case of the incandescent filament discharge (p 10). Again inverted for

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ease of comparison, they all lie between 1.04×10^{-7} and 1.36×10^{-7}, except for one at 0.88×10^{-7} [54]. 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."

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."

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 will move away from the emitting surface [55]. 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* [56]. 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* (p 16) 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. Experiments on the diffusion of ions that were being carried out at Cavendish by Townsend had in the meantime provided clearer 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 [57]. 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."

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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.

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 April 30, 1897 talk before the Royal Institution.

We best pause briefly 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

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precisely, *same distinctive order of magnitude for the value of a characteristic property of two things, two things of a single kind* [58]. 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.” 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 mass equal to or greater than that of the hydrogen atom, and carriers of negative charge with mass 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

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> 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.

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.” I cannot see how to summarize the three pages that follow without diminishing them. They should be read on their own. Nevertheless, three points regarding them should be noted. First, even though the 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 in the atom. Thomson leaves the question of the number of corpuscles per atom open for subsequent investigation. Second, even though he extends his working hypothesis beyond gases to the electrolysis of liquids and ionic bonding, he does not here extend it to conduction in metals. This problem involved special phenomena, like the Hall effect, that the electron by itself did not shed much immediate light on [59].

Third, one should note the absence of the æther—more precisely, the æther continuum—in the working hypothesis elaborated in these three pages. The negatively charged electron, not some state or process in the æther, 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 vast array of electrical phenomena could be developed without having to address the question of the ultimate constitution of electricity at all. The æther had ceased having a role to play in ongoing research in the areas Thomson was concerned with. It had become an unnecessary relic. Earlier I remarked that his December 1899 paper would

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have been a fitting final word of the 19th century for *Philosophical Magazine*. The experiments reported in the three papers included here are very much a product of 19th 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 19th century. Nineteenth century science had reached a position that allowed Thomson to penetrate experimentally into the microphysics of electrical phenomena. At the same time, however, the December 1899 paper takes a large step into the 20th century. The research it culminates had undercut one of the chief elements of 19th century science, the æther [60].

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% 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 (see Table 1 from Thomson 1906) [61]. The value *m*/*e* was obtained with increasing precision over the next decades as can be seen from the corresponding table in the third edition of Thomson's book, twenty-two years later (Table 2).

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

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TABLE 1. Measured values of *e*/*m* from the 1906 edition of Thomson's *Conduction of Electricity Through Gases*.

Source of Ions	Observer	Date	Method of Determination	Value of *e*/*m*	$v \times 10^{-4}$
Cathode rays	J. J. Thomson	1897	Magnetic and electrostatic deflection	7.7×10^4	2.2–3.6
Cathode rays	J. J. Thomson	1897	Magnetic deflection and heat effect	1.17×10^7	2.4–3.2
Cathode rays	Kaufmann	1897–8	Magnetic deflection and potential difference	1.86×10^7	
Cathode rays	Simon	1899	Magnetic deflection and potential difference	1.865×10^7	
Cathode rays	Wiechert	1899	Magnetic deflection and velocity of ions	1.01×10^7–1.55×10^7	
Cathode rays	Seitz	1901	Magnetic and electrostatic deflection	6.45×10^6	7.03
Cathode rays	Seitz	1902	Magnetic and electrostatic deflection, heating effect and potential difference	1.87×10^7	5.7–7.5
Cathode rays	Starke	1903	Magnetic and electrostatic deflection	1.84×10^7	3.8–12
Cathode rays	Reiger	1905	Magnetic deflection and potential difference	1.32×10^7	
Cathode rays	Becker	1905	Magnetic and electrostatic deflection	1.8×10^7	10
Lenard rays	Lenard	1898	Magnetic and electrostatic deflection	6.39×10^6	
Lenard rays	Lenard	1898	Magnetic deflection and retardation in electric field	6.8×10^6	3.4–10
Ultra-violet light	J. J. Thomson	1899	Retardation of discharge by magnetic field	7.6×10^6	
Ultra-violet light	Lenard	1900	Magnetic deflection and potential difference	1.15×10^7	
Ultra-violet light	Reiger	1905	Magnetic deflection and potential difference	9.6×10^6–1.2×10^7	
Incandescent metals	J. J. Thomson	1899	Retardation of discharge by magnetic field	8.7×10^6	
Incandescent oxides	Owen	1904	Retardation of discharge by magnetic field	5.6×10^6	

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TABLE 1. Measured values of *e*/*m* from the 1906 edition of Thomson's *Conduction of Electricity Through Gases* (continued).

Source of Ions	Observer	Date	Method of Determination	Value of *e*/*m*	$v \times 10^{-4}$
Incandescent oxides	Wehnelt	1904	Magnetic deflection and potential difference	1.4×10^7	
Radium	Becquerel	1900	Magnetic and electrostatic deflection	10^7 approximately	2×10^{10}
Radium	Kaufmann	1901–2	Magnetic and electrostatic deflection	1.77×10^7 for small velocities	
Polonium (slow rays)	Ewers	1906	Magnetic and electrostatic deflection	1.7×10^7	

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}.

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 [62]."

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TABLE 2. Measured values of *e*/*m* from the 1928 edition of Thomson's *Conduction of Electricity Through Gases* (jointly with G. P. Thomson).

Source of Ions	Observer	Date	Method of Determination	Value of *e*/*m*	$v \times 10^{-9}$
Cathode rays	J. J. Thomson	1897	Magnetic and electrostatic deflection	7.7×10^6	2.2–3.6
Cathode rays	J. J. Thomson	1897	Magnetic deflection and heat effect	1.17×10^7	2.4–3.2
Cathode rays	Kaufmann	1897–8	Magnetic deflection and potential difference	1.86×10^7	
Cathode rays	Simon	1899	Magnetic deflection and potential difference	1.865×10^7	
Cathode rays	Wiechert	1899	Magnetic deflection and velocity of ions	1.01×10^7–1.55×10^7	
Cathode rays	Seitz	1901	Magnetic and electrostatic deflection	6.45×10^6	7.03
Cathode rays	Seitz	1902	Magnetic and electrostatic deflection, heating effect and potential difference	1.87×10^7	5.7–7.5
Cathode rays	Becker	1905	Magnetic deflection and retardation in electric field	1.8×10^7	10
Lenard rays	Lenard	1898	Magnetic and electrostatic deflection	6.39×10^6	
Ultra-violet light	J. J. Thomson	1899	Retardation of discharge by magnetic field	7.6×10^6	
Ultra-violet light	Lenard	1900	Magnetic deflection and potential difference	1.15×10^7	
Incandescent metals	J. J. Thomson	1899	Retardation of discharge by magnetic field	8.7×10^6	
Radium	Becquerel	1900	Magnetic and electrostatic deflection	10^7 approximately	2×10^{10}
Radium	Kaufmann	1901–2	Magnetic and electrostatic deflection	1.77×10^7	
X-rays	Bestelmeyer	1907	Magnetic and electrostatic deflection (crossed fields)	1.72×10^7	6×10^9 9.7×10^9
Incandescent oxide	Bestelmeyer	1911	Magnetic deflection and potential difference	1.767×10^7	1.7×10^9
Incandescent oxide	Classen	1907	Magnetic deflection and potential difference	1.775×10^7	2×10^9 4×10^9

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TABLE 2. Measured values of *e*/*m* from the 1928 edition of Thomson's *Conduction of Electricity Through Gases* (jointly with G. P. Thomson) (continued).

Source of Ions	Observer	Date	Method of Determination	Value of *e*/*m*	*v*
Ultra-violet light	Alberti	1912	Magnetic deflection and potential difference	1.756×10^7 1.766×10^7	7×10^9 8.5×10^9
Radium	Bucherer	1909	Magnetic and electrostatic deflection (crossed fields)	1.763×10^7	1.1×10^{10} 2×10^{10}
Radium	Wolz	1909	Magnetic and electrostatic deflection (crossed fields)	[I]1.7706×10^7	1.2×10^{10} 2.1×10^{10}
Radium	Neumann	1914	Magnetic and electrostatic deflection (crossed fields)	1.765×10^7	1.2×10^{10} 2.4×10^{10}
----	Fortrat	1912	Zeeman effect	1.7636×10^7	----
----	Paschen	1916	Bohr's theory	1.7649×10^7	----

The values of *e/m* in the lower half of the table are corrected to zero velocity.
[I] Corrected by Neumann

Thomson published the first edition of *Conduction of Electricity Through Gases* in 1903. With the exception of a section on radioactivity, this book amounts to a rewrite of the long chapter on the subject in *Notes on 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, needless to say, they added immensely to it, expanding the work to two volumes and 1100 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.

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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 the War, 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 in order to obtain additional experimental basis for elaborating his "plum-pudding" model of the atom. Much of his effort in the first decade of the 20th 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 [63]. 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 at the beginning of this introduction: an explanation of the connection between ordinary matter and the electrical charges on the atom should go a long way towards 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 papers included here, 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:

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> 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 co-ordinate 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 [64].

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 remained for still another of his research students, though by the time Bohr developed his model of the atom in 1913 he had left Cavendish for Manchester where Rutherford offered an atmosphere more agreeable to the approach Bohr was trying to take.

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:

$$\frac{2\pi^2 me^4}{h^3} = 3.1 \times 10^{15}$$

Bohr used 4.7×10^{-10} for e and 1.77×10^7 for e/m in this calculation, obtaining a value within 6% of the observed value.

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 [65]. 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

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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 [66]." Bohr cites Barkla on this in 1913 [67].

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1. Isobel Falconer has made a strong 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 cathode 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*, **1987**, *20*, 241. I have been helped by this paper in several places, as well as by John Heilbron's doctoral dissertation, Heilbron, J. "A History of the Problem of Atomic Structure from the Discovery of the Electron to the Beginning of Quantum Mechanics" Ph.D. Dissertation, University of California, Berkeley, 1964. I should also mention Anderson, D. L. *The Discovery of the Electron*; Van Nostrand: Princeton, 1964, which first drew my attention to this episode in the history of science and is unfortunately now out of print; and Weinberg, S. *The Discovery of Subatomic Particles*; Freeman: New York, 1983, which I often assign to my students. The main difference between all of these and the present introduction is that they treat Thomson's work primarily from the perspective of the problem of atomic structure, not from that of the problem of the conduction of electricity.

2. Thomson, J. J. "The Relation between the Atom and the Charge of Electricity Carried by It" *Philosophical Magazine*, **1895**, *40*, 512.

3. More detailed histories of cathode ray research can be found in Anderson, D. L. *The Discovery of the Electron*; Van Nostrand: Princeton, 1964; Chapter 2 and in Whittaker, E. *A History of the Theories of Æther and Electricity*; Humanities Press: New York, 1973; Vol. 1, Chapter XII.

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5. Crookes, W. "On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules" *Philosophical Transactions of the Royal Society*, *A*, **1879**, 87.

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7. Hertz, H. *Annalen der Physik und Chemie*, **1883**, *19*, 782; translated as "Experiments on the Cathode Discharge" In *Miscellaneous Papers*; Macmillan: London , 1896; p 253.

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9. "The Discharge of Electricity through Gases" *Proceedings of the Royal Society*, **1890**, *47*, 527.

10. Lenard, P. "Uber Kathodenstrahlen in Gasen von atmosphärischen Druck un in áussersten Vacuum" *Annalen der Physik und Chemie*, **1894**, *51*, 225; (b) "Uber die Magnetische Ablenkung der Kathodenstrahlen" **1894**, *52* 23.

11. Perin, J. "Nouvelles Propriétés des Rayons Cathodiques" *Comptes Rendus* **1895**, *121*, 1130.

12. Reminiscent of the Preface of Maxwell's book, Thomson remarks in his preface, "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." (Thomson, J. J. *Notes on Recent Researches in Electricity and Magnetism*; Clarendon Press: Oxford, 1893; p vi.)

13. At the end of the first chapter, entitled "Electric Displacement 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." (Thomson, J. J. *Notes on Recent Researches in Electricity and Magnetism*; Clarendon Press: Oxford, 1893; p 52.)

14. Thomson, J. J. *Notes on Recent Researches in Electricity and Magnetism*; Clarendon Press: Oxford, 1893; p 2.

15. Thomson, J. J. *Notes on Recent Researches in Electricity and Magnetism*; Clarendon Press: Oxford, 1893; p 3.

16. A detailed account of the problem of the interaction between electricity and matter and its impact on the history of physics at the end of the 19th Century can be found in Buchwald, J. Z. *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*; University of Chicago Press: Chicago, IL, 1985.

17. Thomson, J. J. "On the Velocity of the Cathode-Rays" *Philosophical Magazine*, **1894**, *38*, 358.

18. Thomson, J. J. "The Relation between the Atom and the Charge of Electricity Carried by It," *Philosophical Magazine*, **1895**, *40*, 511.

19. Röntgen, W. *Sitzungsberichte der Würzburger Physikalischen-Medicinischen Gesellschaft*, **December 28, 1895**. An English translation, "On a New Kind of Rays" appeared immediately in *Nature*, **1896**, *53*, 274.

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20. Thomson, J. J.; Rutherford, E. "On the Passage of Electricity through Gases exposed to Röntgen Rays" *Philosophical Magazine*, **1896**, *42*, 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*, **1896**, *59*, 274.

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

22. The paper cited in 20 was presented at a meeting of the British Academy before its publication in November 1896. Thomson and McClelland had earlier presented related results in "On the leakage of electricity through dialectrics traversed by Röntgen rays" *Proceedings of the Cambridge Philosophical Society* **1896**, *9*, 126. 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" **1897**, *43*, 241; and "The Velocity and Rate of Recombination of the Ions of Gases exposed to Röntgen Radiation" **1897**, *44*, 422. 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 traveling 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.

23. Thomson, J. J. (a) "On the Cathode Rays" *Proceedings of the Cambridge Philosophical Society*, **1897**, *9*, 243; (b) Thomson, J. J. *Nature*, **55** (*No. 1428, 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 [23a]." We will return to this paper below.

24. Editorial, *The Electrician*, **July 2, 1897**, *39*, 299.

25. Zeeman, P. "On the Influence of Magnetism on the Nature of the Light emitted by a Substance," *Philosophical Magazine*, **1897**, *43*, 226. 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" *Philosophical Magazine* **1897**, *44*, 55, and a third that began to complicate matters, "Measurements concerning Radiation-Phenomena in the Magnetic Field" *Philosophical Magazine* **1898**, *45*, 197.

26. 1897 was the 60th year of Queen Victoria's reign.

27. 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* **1881**, *11*, 229. This is the paper that first brought him to prominence. Fitzgerald had made important additions to this finding.

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28. Stoney, G. J. "On the Physical Units of Nature" *Philosophical Magazine* **1881**, *11*, 381; see specifically p 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.

29. Larmor, J. "A Dynamical Theory of the Electric and Luminiferous Medium" *Philosophical Transactions of the Royal Society*, A, **1894**, 185, 719. See Buchwald, J. Z. *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*; University of Chicago Press: Chicago, IL, 1985 for details of this theory, which was based largely on emission and absorption spectra.

30. Thomson's assistant at the time was Mr. 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 actually carried out by Everett.

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

32. Our current value for m/e for the electron is $0.56856314 \times 10^{-7}$ esu.

33. 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." Thomson, J. J. *Recollections and Reflections*; Macmillan: New York, 1937; p 337f.

34. 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.

35. 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 wavelike character of electrons.

36. Wiechert, E. "Ergebniss einer Messung der Geschwindigkeit der Kathodenstrahlen" *Schriften der physikalischökonomisch Gesellschaft zu Königsberg* **1897**, *38*, 3. Wiechert identified the particle with the immaterial electron. A year later he 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.

37. Kaufmann, W. "Die magnetische Ablenkbarkeit der Kathodenstrahlen und ihre Abhängigkeit vom Entladungspotential" *Annalen der Physik und Chemie*, **1897**, *61*, 544. The results led Kaufmann to conclude that "the hypothesis which assumes the cathode rays to be charged particles shot from the cathode is insufficient." Like Thomson, Kaufmann also determined that m/e does not vary with

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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*.

38. Lenard, P. “Uber die electrostatischen Eigenschaften der Kathodenstrahlen” *Annalen der Physik und Chemie* **1898**, *64*, 279.

39. Wein, W. “Untersuchungen über die elektrische Entladung in verdünnten Gasen” *Annalen der Physik und Chemie* **1898**, *65*, 440.

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

41. Thomson, J. J. *Philosophical Magazine* **1898**, *45*, 172.

42. Rutherford, E. *Philosophical Magazine* **1897**, *44*, 422. This paper was submitted on July 19, one month before Thomson submitted his cathode ray paper.

43. Wilson, C. T. R. “Condensation of Water Vapour in the Presence of Dust-free Air and other Gases” *Philosophical Transactions of the Royal Society*, *A*, **1897**, *189*, 265.

44. Townsend, J. “On Electricity in Gases and the Formation of Clouds in Charged Gases” *Proceedings of the Cambridge Philosophical Society* **1897**, *9*, 244. 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 note 23, Townsend’s main point in this paper, presented some 10 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 even compare the result he obtained with the charge per atom in electrolysis.

45. This correction is presented more clearly in Thomson’s December 1899 paper, p 562.

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

47. Thomson, J. J. *Philosophical Magazine* **1899**, *47*, 253.

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

49. Sutherland, W. *Philosophical Magazine* **1899**, *47*, 268.

50. Sutherland, W. *Philosophical Magazine* **1899**, *47*; p 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

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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."

51. Sutherland, W. *Philosophical Magazine*, **1899** *47*; p 284.

52. Thomson, J. J. "Note on Mr. Sutherland's Paper on the Cathode Rays" *Philosophical Magazine* **1899**, *47*, 415.

53. Thomson, J. J. *Philosophical Magazine* **1899**, *47*; p 416.

54. 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 page 17 where the exponent should of course be –10, not –8.

55. Wilson, C. T. R. "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*, **1899**, *192*, 403.

56. "The Discharge of Electrification by Ultra-violet Light," *Proceedings of the Cambridge Philosophical Society* **1898**, *9*, 401.

57. Rutherford, E. "The Diffusion of Ions into Gases" *Philosophical Transactions of the Royal Society*, *A*, **1899**, *193*, 129; (b) Rutherford, E. "The Diffusion of Ions Produced in Air by the Action of a Radioactive Substance, Ultra-violet Light and Point Discharges" **1901**, *195*, 259.

58. 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."

59. A few months later, at an international conference in Paris, Thomson did propose an account of electrical conduction in metals, centered on the electron, which has much in common with our current picture (Thomson, J. J. "Indications relatives à la constitution de la matiere" *Congres International de Physique*; Gauthier–Villars: Paris; Vol. 3, p. 138). By the time he delivered the lectures at the Royal Institution that became *The Corpuscular Theory of Matter*, however, he had backed off this view. For more details, see Pippard, A. B. "J. J. Thomson and the discovery of the electron" in *Electron: A Centenary Volume*; Springford, M., Ed. Cambridge University Press: Cambridge, 1997.

60. Thomson added to his new working hypothesis in a short paper, "The Genesis of the Ions in the Discharge of Electricity through Gases" a few months later (Thomson, J. J. *Philosophical Magazine* **1900**, *50*, 278). There he introduced the idea of corpuscles liberating further corpuscles

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in a cascading fashion at high voltages, producing among other things the separate regions of glow and the striations observed in cathode ray tubes.

61. 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, J. J.; Thomson, G. P. *Conduction of Electricity Through Gases*; 3rd ed., Dover: New York, 1969. Details on Thomson's work during the decade can be found in Thomson, G. *J. J. Thomson: Discoverer of the Electron*; Anchor: Garden City, 1966.

62. For Millikan's version of all of this, see his book. (Millikan, R. A. *The Electron*; University of Chicago Press: Chicago, IL, 1924.) The relevant papers appeared in Volume 2, Number 3 of *The Chemical Educator.*

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

64. Thomson, J. J. *The Corpuscular Theory of Matter*; Charles Scribner's Sons: New York, 1907; p 1f.

65. Thomson, J. J. *Philosophical Magazine* **1906**, *11*, 769. See also *The Corpuscular Theory of Matter*; Charles Scribner's Sons: New York, 1907; Chapter VII.

66. Barkla, C. G. "Note on the Energy of Scattered X-radiation" *Philosophical Magazine* **1911**, *21*, 648. Ironically, this paper was in response to J. G. Crowther, a research student working under Thomson, who had just 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.

67. Bohr, N. "On the Constitution of Atoms and Molecules, Part II" *Philosophical Magazine* **1913**, *26*; p 29.

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2. Thomson, J. J. "Cathode Rays," *The Electrician*, May 12, **1897**, *39*, 104.

3. Thomson, J. J. "Cathode Rays," *Philosophical Magazine* **1897**, *44*, 293.

4. Thomson, J. J. "On the Charge of Electricity carried by the Ions produced by Röntgen Rays," *Philosophical Magazine* **1898**, *46*, 528.

5. Thomson, J. J. "On the Masses of the Ions in Gases at Low Pressures," *Philosophical Magazine* **1899**, *48*, 547.