The Septuagenarian Electron

Concepts of the electron have evolved gradually from work on cathode rays, determinations of mass and charge, and predictions and tests of its wavelike properties.

by Sir George Thomson

THE YEAR 1897 HAS the best claim to be the birth year of the electron; for in it the main ideas, which for the next 30 years at least made up this concept, were first clearly stated and supported by strong evidence—though a little short of proof.

I have only space to pick out certain points in this long and intricate history; so I shall choose those that seem best to show how the many able searchers for truth in this matter have been helped or hindered by experimental and psychological causes.

The electron is a concept that has developed, and changed greatly in the process. It originated as the name of a unit of charge. Johnstone Stoney,[1] an Irish physicist who favored the idea that Michael Faraday's laws of electrolysis imply a natural unit of charge, introduced the word in 1891 as follows: "In electrolysis a definite quantity of electricity, the same in all cases, passes for each chemical bond which is ruptured . . . a charge of this amount is associated in the chemical atom with each bond . . . . These charges, which it will be convenient to call 'electrons' cannot be removed from the atom but they become disguised when atoms chemically unite." Stoney was to be proved partly wrong; the name he gave is now applied to a detachable part of the atom and implies a group of properties of which charge is only one.

In the latter part of the 19th century James Clerk Maxwell, following Faraday, had stressed the importance of the medium much above that of the electrical charges it separates. Thus, "According to this theory all charge is the residual effect of the polarization of the dielectric," and again, "It is extremely improbable, however, that when we come to understand the true nature of electrolysis we shall retain in any form the theory of electrical charges."

Faraday himself had been by no means convinced of what we now consider the obvious explanation of his laws. However, by about 1895 the evidence for a natural unit charge, which had been stressed by Hermann Helmholtz in the 1880's was very strong, but there was little evidence for its material character. It might have turned out to be just a unit governing the transfer of electricity, more like a quantum of radiation than the electron we know. The decisive evidence that changed Stoney's concept to one that lasted for the next 30 years came from the study of cathode rays.

The nature of cathode rays

Cathode rays had long been a problem. Were they charged particles or some phenomenon in the ether, perhaps the often surmised longitudinal waves? On 1 Jan. 1897 the relevant evidence was as follows:

They were deflected by a magnetic field as would be a wire carrying negative electricity from the cathode (known since their discovery).

They could go through thin solid films, for example gold foil, without

The son of Nobelist, J. J., Sir George won his own Nobel Prize in 1937 (shared with C. J. Davisson) for work at the University of Aberdeen; he demonstrated that electrons behave as waves. In 1930 he became professor at Imperial College, remaining there until 1952, when he became master of Corpus Christi College, Cambridge.

making holes in them. This fact, discovered by Heinrich Hertz[2] in 1892, was much used by Philipp Lenard, who studied them in the atmosphere and in a vessel filled with gas. Lenard proved that they were absorbed by matter and that equal weights per square centimeter of many widely different substances absorbed them nearly equally.

Lenard also had proved that the absorption coefficient of cathode rays depended on the voltage of the discharge that made them and that, if this was kept constant, the magnetic deflection was independent of the gas through which the rays were passed. He had not tried varying the gas in the discharge.

Jean Perrin had proved that when the rays were caught in a hollow container facing the cathode, they gave it a negative charge; this replaced erroneous results of William Crookes and Hertz.

Finally, according to Hertz's[3] paper of 1883 the rays were not deflected by an electric field.

This last was a serious hurdle for the supporters of charged particles, as was the observation that the rays could penetrate thin solids without leaving holes (more so than the modern physicist can easily realize).

In favor of the charged-particle explanation were the magnetic deflection and Perrin's experiment, then more convincing than it might be now with our wide experience of secondary radiations.

On 7 Jan. 1897 E. Wiechert[4] read a paper to the Königsberg Society for Science and Economics that deserves more credit than it usually receives. He believed in particles and tried to determine their m/e by combining mv/e found by magnetic deflection with a direct measure of v, using a Hertzian oscillator of known frequency to operate a shutter by its magnetic effect. This last was due to T. des Coudres.

Unfortunately in Wiechert's first paper the oscillation was not rapid enough actually to measure v but only gave a lower limit, with some indication that the true value was about twice as large, giving m/e about a two-thousandth part of the corresponding ratio M/e for the hydrogen ion in electrolysis.

A great merit of the paper was that it spoke boldly of the cathode rays as "electric atoms," and it stressed their universality, though not considering them as composing chemical atoms. The weakness of this early experiment is that it forms no real argument against those who believed in the ether-wave theory, for which one would expect an even larger velocity than Wiechert's limit, perhaps even the speed of light. In a later paper Wiechert was able to show that the speed varied with magnetic deflection about as it should for particles, but this was after Thomson's work. Kaufmann[5] in a paper sent to the *Annalen der Physik und Chemie* in May 1897 studied the magnetic deflections of rays formed in discharges of different potentials. He believed in waves, but calculated what m/e would have to be if the rays were particles, assuming that these rays acquired the full energy of the discharge potential—an assumption open to criticism but in fact nearly true under his conditions. He found m/e about $(^1/_{1000})$ M/e but rejected this as much too small and with it the idea of particles, because he supposed they should vary with the gas, the cathode material or both.

Cathode rays charged particles

On the last day of April, J. J. Thomson gave a lecture at Faraday's old home, the Royal Institution. He put forward evidence which had made him conclude that the cathode rays are particles 1000 times lighter than a hydrogen atom and a universal constituent of matter. It is interesting that among his experiments were some closely resembling those of Kaufmann with which they agree, but from which Thomson deduced precisely the opposite conclusions to Kaufmann's!

In this lecture, which was printed in extenso in the *Electrician*[6] of 21 May (and which contains some work that had appeared in *Nature*,[7] 15 March) J. J. makes an unanswerable case for the particle theory—unanswerable at least at the time.

He improves Perrin's experiment by showing that the rays when deflected carry the charge with them; therefore deflection is not a secondary phenomenon. He improves Lenard's experiment by showing that the deflection of the rays for equal potential is the same for any gas in the discharge; he measures m/e by measuring the magnetic deflection and also the heat given to a thermocouple per unit of charge carried by the rays, in other words mv/e and $(^1/_2)$ mv^2/e giving m/e of the order $(^1/_{1000})$ M/e. Most important of all he explains the negative experiment of Hertz on electric deflection. He was guided here by an old experiment of Eugen Goldstein, who had shown that if a discharge tube is provided with two cathodes, the rays from each are deflected when the other is connected to it. Thomson thought this occurred because the electric field from the second cathode deflected the rays. He improved on the experiment by putting two parallel plates in the dark space of the discharge from a single cathode and earthing each in turn. The rays passing between the plates were deflected.

This experiment was shown at the Royal Institution and is the basis of the e/m experiment illustrated in all the textbooks, itself the origin of the cathode-ray oscilloscope. Because this is the vital point in the matter, it is worth going back to Hertz's experiment of 1883 to see what went wrong there. In these experiments Hertz first made his discharge in a flat box formed by square glass plates held a few centimeters apart by a brass frame. The purpose of this shape was to explore the distribution of current in the flat discharge by measuring the magnetic field close to the glass plates. The visible cathode rays did not show any connection with the lines of current flow thus found. He then worked with a more conventional cylindrical tube, its cathode near one end being surrounded by an anode of wire gauze through which the cathode rays escaped. These he caught in a vessel further down the cylinder connected to an electrometer, but he could detect no charge.

Fairly convinced by now that the cathode rays were merely secondary (he compared them to the visible light of the discharge), he tried the crucial experiment of electric deflection, applying an electric field to the ray by connecting the poles of a battery to two metal plates between which the rays passed and found no deflection of

J. J. THOMSON at the Cavendish Laboratory.

the phosphorescence on the end of the tube. At first the plates were outside the tube, but guessing correctly that the field might cause electrification of the glass, which would cancel its effect inside, Hertz put the plates themselves inside. Again no effect. It is strange that he did not realize the possibility of ions from his discharge polarizing his electrodes. Thomson did so, helped I suspect by his study in the previous year of the conductivity caused in air by rays. He was working on the space-charge effects, as we now call them, of the ions.

A universal constituent

Though these experiments of Thomson proved that cathode rays were electrified particles, the further step of claiming them as a universal constituent of matter depended on other evidence. In doing so J. J. was influenced by the dependence of their absorption on density alone, which contrasted so strongly with the dependence on chemistry of the absorption of light, visible and ultraviolet. Even x rays, whose absorption is atomic, do not show the simple proportionality to density that Lenard found. In his definitive paper, sent to the *Philosophical Magazine*[8] (published October), J. J. not only claimed that the particles of cathode rays are a universal constituent of matter, in number roughly proportional to the density, so that collisions with them account for the absorption of cathode rays, but he indicated a way in which they might be arranged. He used an experiment of the American physicist, Alfred Mayer, in which floating magnets arrange themselves in concentric rings on the surface of water under the influence of a powerful magnet held vertically above them.

The average charge on the ions produced by x rays was, as is well known, measured by Thomson in 1898, using C. T. R. Wilson's discovery of ions as centers of condensation. The result was equal to that on a monovalent ion in solution within the very wide limits to which this last was known.

A more accurate proof of this equality, though without determining either quantity, was made by John Townsend, who compared the motions of these ions moving under electric fields and diffusing through a gas under their own partial pressure.

There was, however, no proof that the charge on a cathode ray was equal to this "electron." J. J. then showed that two other processes produced particles with the same m/e as cathode rays, namely emission from a hot wire and the photoelectric effect.

This paper appeared in 1899 and contained also a measurement of "e" for the photoelectric ions by a slight modification of the method used for x-ray ions, getting the same result. In this way the connection between Stoney's definition of his term and the new concept was established. But in this paper, as in the Royal Institution lecture, J. J. used the word "corpuscle" for these elementary particles and continued to use it for some years. The word "electron" was still not in common use. Hendrik Lorentz, for example, spoke of "ions." No doubt electron is much the neater and more distinctive name, but its use for "corpuscle," even without the complication introduced by discovery of positrons, leaves one with no single word for Stoney's unit charge as a purely electrical quantity.

J. J. stated that the charge on the corpuscle was that on a monovalent ion, and that ionization involves breaking an atom by detaching a corpuscle.

Pieter Zeeman's discovery of the effect that carries his name and its explanation in terms of Lorentz's theory, was submitted to the Amsterdam Academy in October 1896. It appeared in Dutch in May of the next year but had already been published in English in the *Phil. Mag.*[9] of March, dated January with a postscript dated February. There is a curious error in this paper, the sign of the charge of the moving particle being given as positive.

Thomson referred to this paper in his Royal Institution lecture and especially the agreement in the value found for m/e, namely 10^{-7}.

Though Thomson's ideas were not accepted immediately, even after the Dover meeting of the British Association in 1899, the evidence after the 1899 paper was adequate except on one important point. All the measurements of the supposed unit charge from Faraday's law were statistical; they gave average values for e. This was not so for m/e; even Thomson's measurement using electrical deflection gave some resolution, as did Kaufmann's and though poor at first the measurements rapidly improved. Most people assumed that the charge was also a true constant of nature, not just a mean. But it needed proof. Conceivably m/e could be more fundamental than either m or e. Thus in a gas such as argon containing isotopes, the mean value of $(^1/_2)\ mv^2$ is the same for every atom (over a period of time), but that of m can vary. As you all know, this matter was brilliantly cleared up by Robert A. Millikan in 1909 and with him this chapter ends.

First-rate minds blinded

In looking back at it, one is impressed by the extent to which a theory long held can blind even first-rate minds to new ideas and by how easy it is to explain almost anything in terms of a favorite theory. Also, see how important technique can be. A slight reduction in the very small amount of gas present in a discharge changes the whole appearance of the problem. How cautious one should be in accepting the experiments even of able workers at their face value!

The electron thus established served as the basis of atomic physics for nearly 30 years with comparatively little modification. In the form of beta rays, electrons were used by Kaufmann, Bucherer and others to examine experimentally the variation of mass with velocity and to establish the truth of the formula given for this by the theory of relativity. Less successfully a number of physicists, including Thomson, used it to form theories of metallic conduction, but ran up against the difficulty that the necessary number of free electrons, regarded as Maxwellian particles, subject to equipartition of energy, would increase the specific heats of a metal much beyond the observed values.

Niels Bohr was the first to emancipate the electron from full obedience to the laws of Maxwellian electrodynamics by assuming that electrons could move forever round a nucleus without radiating energy, provided that their angular momentum was an integral multiple of $h/2\pi$. This made possible theories of atomic structure capable of explaining many of the facts of spectroscopy, but otherwise left the idea of a small spherical particle unchanged. In the middle twenties it was becoming clear that in spite of its many successes there was something wrong with the Bohr-Sommerfeld theory. I remember vividly hearing Arthur H. Compton give a paper to the Cavendish Laboratory Colloquium on the "ring electron," showing how it could deal with some of these difficulties; however, this idea never fully caught on and was superseded in 1926 by George Uhlenbeck and Samuel Goudsmit's idea of the spinning electron. Even this was not too satisfactory. The point electron failed because it had not enough degrees of freedom to account for the variety of spectral lines—why, for example, the sodium lines are double. The spinning electron had too many.

Waves or particles?

But of course the real difficulty of physics in the 1920's was the radiation problem, waves or particles? Solutions came nearly simultaneously from Werner Heisenberg and Louis de Broglie. The former did not immediately affect one's ideas on the nature of electrons, but de Broglie stated from the first that an electron as well as a photon has waves associated with it. In his first paper in English[10] he used the idea of the electronic wavelength to explain Bohr's condition for the permitted orbits. I had been partly prepared for this by a remark Lawrence Bragg once made to me that he thought the electron was not as simple as people then supposed. Anyhow I wrote a brief attempt to amplify de Broglie's ideas in the *Philosophical Magazine*[11] of July 1925.

Meanwhile Clinton Davisson and C. H. Kunsman had been working on production of secondary electrons from metals by the impact of electrons of energy around 200 volts. The first paper appeared in 1921. This was by no means a virgin field, and Davisson's research is an outstanding example of what can sometimes be achieved by greatly superior technique in a field that might have been supposed unpromising.

Davisson's earlier papers used polycrystalline targets and showed "peaks," that is, directions of preferred scattering that varied with the speed of the primary electron. The theoretical physicist Walter Elsasser in a letter sent to *Naturwissenshaften*[12] in July 1925 suggested that the effects were analogous to diffraction by an optical grating and that in de Broglie's theory deviations from normal mechanics were to be expected in "an accessible range of velocities."

Davisson read Elsasser's paper, but states that he was not influenced by it as he did not think Elsasser's theory valid. He was inclined at first to explain these experiments by shells of electrons in the atoms and later by "transparent directions." However, by a fortunate accident (April 1925) a nickel target was subjected to a prolonged heat treatment that converted it into a few large crystals. The scattering pattern was now completely different. Davisson decided to work in future with a single crystal.

In 1926 he attended the British Association meeting at Oxford where he showed some curves relating to single crystals to Max Born, to D. R. Hartree and perhaps to P. M. Blackett and James Franck. On his return he spent his time, as he says, "trying to understand Schrödinger's papers, as I had then an inkling (probably derived from the Oxford discussions) that the explanation might reside in them." On return he instituted a search for the beams and after some failures found on 6 Jan. 1927 strong beams due to the line gratings of the surface atoms. I was at the same meeting, where I heard de Broglie's theory discussed informally. I do not remember Elsasser's name being mentioned, but some of those who were discussing the theory may well have read his work. I think I did not meet Davisson and certainly did not discuss his experiments with him. The experiments of Davisson and Lester Germer, first published in a note to *Nature*[13] in April 1927, and later more fully in the *Physical Review*[14] of December of that year, completely established that electrons are diffracted by the surface atoms of a crystal of nickel cut with a (111) face in the surface, as would be waves of $\gamma = h/mv$—the de Broglie formula; the atoms below the surface modify the effects in a way that can be explained as due to the electrons acquiring energy on entering the metal. The electrons Davisson used had energies in the region 50–400 volts.

Meanwhile it had occurred to me that one ought to be able to get an electron analog of Thomas Young's optical "eriometer," in which grains or

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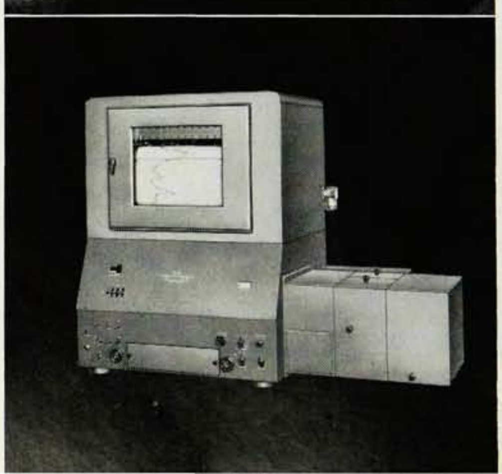

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fibers oriented at random on a plate of glass give halos whose diameters depend on the size of the fibers. Celluloid was known to be composed of long molecules and could easily be made in very thin films. As it happened a pupil of mine at Aberdeen, A. Reid (killed the next year in a motor accident), had an apparatus that could readily be used for this experiment, and I asked him to try it.

He sent cathode rays of about 30 kV through a thin film of celluloid onto a photographic plate, which, when developed, showed fuzzy rings.

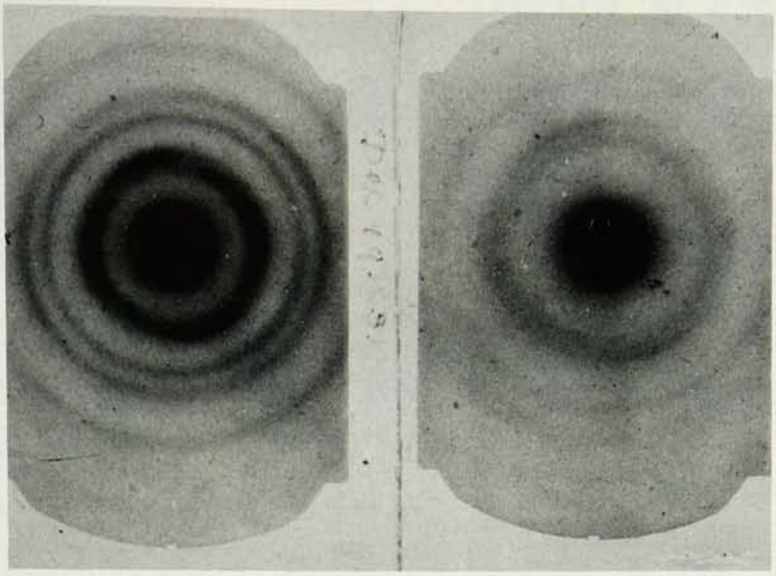
As it happened we had had trouble with some optical illusions on photographic plates, and I was chary of trusting these fuzzy rings though they were about the right size assuming $\lambda = h/mv$ and a reasonable guess for the effective thickness of the celluloid molecules. This delayed us a bit, but eventually there was no doubt and we published a note in *Nature*,[15] June 1927.

However, it was essential to get patterns from some substance whose crystal pattern was well established. Thanks to the dexterous manipulation of C. G. Fraser, I was able to use films of gold, aluminum and platinum, which gave patterns on the photographic plate that reproduced in all respects those to be expected on the de Broglie theory.

A note was published in *Nature*[16] in December 1927, and the paper appeared in *Proc. Roy. Soc.*[17] in February 1928. Because of the higher energy of the electrons there was no measurable correction for the inner potential of the metal.

In reply to a criticism I was able to show by the use of a magnetic deflection that the patterns were actually caused by the photographic action of electrons not by some kind of secondary x rays formed in the target. In a later paper I showed that the relative intensity of the rings formed in thin gold by Bragg reflection from the different crystal planes agreed with the predictions of a wave-mechanical theory due to Nevil Mott.

In comparing these experiments I must stress the far greater difficulty of those of Davisson and Germer, which are among the greatest experimental triumphs of physics, even now hard to repeat, while mine, once we learned how to make thin films, were among the easiest. This is mainly due to the difference in energy of the rays used. In addition, reflection experiments, though not very difficult with fast electrons, are naturally more complicated than transmission ones. I set out to test a theory; Davisson to explain an experiment.



EARLY ELECTRON DIFFRACTION PATTERN that author produced by passing cathode rays through an etched gold film.

The original simple form of de Broglie's theory is the wave-mechanical analog of the point electron with three degrees of freedom only. P.A.M. Dirac in 1928 put forward a more complicated theory that removes this objection and at the same time satisfies the requirements of relativity. It also led him to predict existence of electronic states of negative energy, an infinite number of which are supposed occupied. A vacancy in this sea of states then behaves like an electron of normal mass but positive charge. With the discovery of the positron by Carl Anderson in 1932 and the creation of pairs by the materialization of gamma rays, discovered by Seth Neddermeyer and Anderson the next year, this stage in the history of the electron came to an end.

No doubt there is much more to come. The relation of mu mesons to electrons is still a mystery 20 years after the discovery of muon decay. Perhaps electrons will lose their unique position and become merely senior partners in a firm of leptons; but 70 years is a good life.

References

1. J. Stoney, Trans. Roy. Dub. Soc. **4,** 583 (1891).
2. H. Hertz, Ann. Phys. Chem. **45,** 28 (1892).
3. H. Hertz, Ann. Phys. Chem. **19,** 809 (1883).
4. E. Wiechert, Proc. Phys. & Econ. Soc. Konigsberg **35,** 1 (1897).
5. W. Kaufmann, Ann. Phys. Chem. **61,** 544 (1897).
6. J. J. Thomson, Electrician **38,** 838 (1897).
7. J. J. Thomson, Nature **55,** 453 (1896–97).
8. J. J. Thomson, Phil. Mag. **44,** 293 (1897).
9. P. Zeeman, Phil. Mag. **43,** 226 (1897).
10. L. de Broglie, Phil. Mag. **47,** 446 (1924).
11. J. J. Thomson, Phil Mag. **50,** 163 (1925).
12. W. Elsasser, Naturwiss. **13,** 711 (1925).
13. C. J. Davisson, L. Germer, Nature **119,** 558 (1927).
14. C. J. Davisson, L. Germer, Phys. Rev. **30,** 707 (1927).
15. G. P. Thomson, A. Reid, Nature **119,** 890 (1927).
16. G. P. Thomson, A. Reid, Nature **120,** 802 (1927).
17. G. P. Thomson, A. Reid, Proc. Roy. Soc. **A117,** 600 (1928).
18. A. Reid, Proc. Roy. Soc. **A119,** 665 (1928). □