1 Planck and de Broglie in the Thomson Family

Jaume Navarro

Introduction

The 1927 Solvay Conference is usually regarded as one of the central moments in the history of the development and acceptance of the new quantum physics. In that meeting, the latest developments in both wave and matrix mechanics were publicly discussed by the main characters of this drama. In the years after the Great War, German and Austrian scientists had been banned from international meetings, a fact that eventually helped Bohr's institute, in neutral Copenhagen, to become the focal point of international discussions on the new physics. The fifth Solvay conference was the first to waive the boycott that had been in place in the two previous councils (the ones of 1921 and 1924), and thus it became a unique opportunity to gather all the major actors of quantum physics under the same roof.[1]

Only a month before the Solvay meeting in Brussels, a huge international event gathered hundreds of physicists in Como, Italy, in a celebration of Alessandro Volta and Italian science. There, Niels Bohr was invited to open a discussion on quantum physics by giving an overview of the latest developments in the new science. Werner Heisenberg, Wolfgang Pauli, Enrico Fermi and Max Born were among the participants in the discussion. But others were missing: Albert Einstein and Erwin Schrödinger were absent, probably for political reasons. Louis de Broglie, whose contribution had been so crucial in the last three years, was also absent in Como.

At one point of his speech in Como, Bohr signalled that "recent experience, above all the discovery of the selective reflection of electrons from metal crystals, requires the use of the wave theoretical superposition principle in accordance with the original ideas of L. de Broglie",[2] better known as the principle of wave-particle duality. Bohr was referring here to the experiments reported by American physicists Clinton J. Davisson and Lester H. Germer. Oblivious to the intricacies of the new quantum physics, Davisson and Germer were working on the scattering of electrons on metallic surfaces, as part of their research in the industrial Bell Laboratories. The anomalous results they had been obtaining since 1923 only made sense after a young student of Max Born in Göttingen, Walter M. Elsasser, suggested in 1925 that they could be interpreted in terms of electron diffraction. In early 1927, Davisson and Germer reported final evidence of the diffraction of electrons in their scattering by metal plates, thus proving the wave behaviour of the

[1] See Pierre Marage and Gregoire Wallenbron, eds., *The Solvay Conferences and the Birth of Modern Physics* (Basel; Boston, Mass : Birkhäuser Verlag, 1999).

[2] Niels Bohr, "The quantum postulate and the recent development of atomic theory", in Atti del Congresso Internationale dei Fisici, vol. 2, (Bologna : N. Zanichelli, 1928), 568.

electrons.

When, a month later, Louis de Broglie presented his paper at the Solvay Conference, he made a serious effort to update on the experimental results that might support his theory of wave-particle duality. Together with the experiments of Davisson and Germer, to which he devoted thorough attention, he also mentioned the latest observations of George Paget Thomson and his student, Alexander Reid, only recently published as a preliminary note in *Nature*. In that note, Thomson and Reid published a photograph obtained after "a beam of homogeneous cathode rays is sent through a thin celluloid film", in which "the central spot formed by the undeflected rays is surrounded by rings, recalling in appearance the haloes formed by mist round the sun".[3] In de Broglie's words, "these observations are very interesting and they confirm, although only roughly, the new conceptions".[4]

Ten years later, in 1937, Davisson and Thomson shared the Nobel Prize for their experimental confirmation of the undulatory nature of the electrons and, therefore, the principle of duality wave-particle put forward by de Broglie. Their experimental paths were largely distinct, and this helped to have two independent confirmations of the radical principle of wave-particle duality. Historical analysis on the work of Davisson and Germer was done by historian of science Arturo Russo more than 25 years ago.[5] On the contrary, the work of G. P. Thomson has never received sufficient attention by historians of quantum physics. This neglect can be partly due to the fact that British physicists played but a minor role in the theoretical developments of quantum physics and, therefore, are of little significance in a whiggish history of science. However, the current project in the analysis of the early developments of quantum physics opens the door to the study of what one might be tempted to call 'the losers', i.e., those who didn't fully accept the radical changes of the new physics.

In this paper I want to discuss the intellectual setting in which G.P. Thomson developed his early career, a career that was boosted by the experiments of 1927. As I shall argue, the influence of his father, Sir Joseph John Thomson, proved to be a crucial factor in the way G.P. received quantum physics. As most Victorian scientists, J.J. was not prepared to accept the quantum of action as a metaphysical principle: his world was a world of ether and, therefore, essentially continuous. Any discreteness in physical theories was only phenomenological. This worldview was preserved within the Thomson family and this explains G.P's difficulties in understanding the relevance of the new physics. Far from becoming an *experimentum crucis*, electron diffraction was a proof, to J.J.'s eyes, of the correctness of his worldview of ether. Only partly did G.P. manage to cut the umbilical cord that had kept him tied to his father's metaphysics in the beginning of his career.

[3] George P. Thomson and Andrew Reid, "Diffraction of Cathode Rays by a Thin Film", *Nature* 119 (1927), 890.

[4] Louis de Broglie, "La Nouvelle Dynamique des Quanta", in *Electrons et photons. Rapports et discussions du cinquieme conseil de physique tenu a Bruxelles du 24 au 29 Octobre 1927 sous les auspices de l'Institut International de Physique Solvay* (Paris, 1928), 130.

[5] See Arturo Russo "Fundamental research at Bell Laboratories: The discovery of electron diffraction" *Historical Studies in the Physical Sciences* 12 (1981), 117–160.

J.J. and the quantum of light (c. 1925)

J.J. Thomson's world was a world of ether. Ever since his work for the 1881 Adams Prize, Thomson had attempted to understand matter as an epiphenomenon of the ether. First, it was the vortex-ring theory, which had it origins in Helmholtz and Kelvin;[6] and then, around 1891, he shifted to the notion of Faraday tubes of force, a theoretical tool he kept, in different formats, all his life.[7] Faraday tubes were bundles of lines of force in the ether, acting as the channels of energy between charged bodies. In 1891 he defined them in the following way:

> "...the electric field is full of tubes of electrostatic induction, that these are all of the same strength, and that this strength is such that when a tube falls on a conductor it corresponds to a negative charge on the conductor equal in amount to the charge which in electrolysis we find associated with an atom of a univalent element. These tubes must either form closed circuits, or they must end on atoms, any unclosed tube being a tube connecting two atoms".[8]

Always eager to promote mental images in the development of physical theories, Thomson rejected the interpretation of Maxwell's theories only in terms of dimensional equations.[9] Eventually, however, the tubes of force ceased to be only a mental image and became a physical reality. In 1925, he explained: "I suppose that these lines are not merely geometrical figments, but that they, or rather the groups of them forming tubes of force (...), are physical realities, and that the energy in the electric field is bound up with these tubes".[10]

Since the tubes of force were real physical entities, and not merely ideal devices, this meant that there should be an actual physical limit to their divisibility.[11] This idea opened the door to a quantification of energy and charge within the framework of a continuous ether. Continuity and discreteness were, in this way, aspects of nature which were not mutually exclusive.

The tubes of force were instrumental in the experiments that led J.J. to the discovery of the electron; and he retrieved them in the early 1900s to give a particular account that would explain the structure of light. Ever since the discovery of X-rays, the old problem of explaining light in terms of either waves or particles gained particular momentum. J.J. was no alien to this problem. As an expert on the interaction between electricity and matter in discharge tubes, Thomson became increasingly aware of the discrete behaviour

[6]See Helge Kragh, "The Vortex Atom: A Victorian Theory of Everything" *Centaurus* 44 (2002), 32–126; and Jaume Navarro, "J.J. Thomson on the nature of matter: corpuscles and the continuum" *Centaurus* 47 (2005), 259–282.

[7]For the reasons why he shifted from vortex rings to Faraday tubes, see Isobel Falconer, "Corpuscles, Electrons and Cathode Rays: J.J. Thomson and the 'Discovery of the Electron'" *British Journal for the History of Science* 20 (1987), 241–276.

[8]Joseph J. Thomson, "On the Illustration of the Properties of the Electric Field by Means of Tubes of Electrostatic Induction" *Philosophical Magazine* 31 (1891), 149–171, 150.

[9]See David Topper, "'To reason by means of images': J.J. Thomson and the mechanical picture of Nature" *Annals of Science* 37 (1980), 31–57.

[10]Joseph J. Thomson, *The Structure of Light* (Cambridge: Cambridge University Press, 1925), 20.

[11]Joseph J. Thomson, *Notes on recent researches in electricity and magnetism: intended as a sequel to Professor Clerk-Maxwell's Treatise on electricity and magnetism* (Oxford: The Clarendon press, 1893), 3.

of both matter and light. Being one of the first researchers in Britain to use X-rays in his laboratory work (an innovation that was, by the way, instrumental to his discovery of the corpuscle-electron), he soon engaged in efforts to explain the apparently dual nature of Röntgen rays and, by extension, of any light wave. The properties of the photoeffect confirmed Thomson in his intuition that there was a certain discreteness in the wave front of electromagnetic waves.

His idea that the tubes of force, the carriers of electromagnetic energy, were both continuous and discrete led him to suggest a theory of light in which the wave front was discontinuous:

> "This view of light as due to the tremors in tightly stretched Faraday tubes raises a question which I have not seen noticed. The Faraday tubes stretching through the ether cannot be regarded as entirely filling it. They are rather to be looked upon as discrete threads embedded in a continuous ether, giving to the latter a fibrous structure; but if this is the case, then on the view we have taken of a wave of light the wave itself must have a structure, and the front of the wave, instead of being, as it were, uniformly illuminated, will be represented by a series of bright specks on a dark ground, the bright specks corresponding to the places where the Faraday tubes cut the wave front".[12]

This theory was, however, never developed beyond the realm of ideas and suggestions. There was never a complete mathematical development that would validate the theory or, otherwise, point at its limitations.

It was not until 1909 that Thomson publicly gave his opinion on what he called the "light-quantum hypothesis" of Planck. It was the beginning of his long controversy with the increasingly popular quantum theory, a controversy in which "Thomson's basic position was that energy itself has no coherence, or inherent structure, but rather that the carriers of the energy—Faraday tubes, electrons, etc.—are the permanent, indivisible entities".[13] This was, however, already an advanced mentality compared to most British physicists in the first decade of the 20th century. J.J. was, at least, ready to accept a certain discontinuity in the electromagnetic waves and the ether, as his early theory on the structure of light shows.

J.J. could not accept Planck's theory basically for two reasons. First, because the way he read Planck (closer to Einstein's interpretation of the quantum) involved a quantification of energy itself. Thomson was ready to accept a quantification of the carriers of energy (as he had accepted a quantification of the carriers of charge), but nothing beyond this point. And second, because Planck's theory, while explaining the discrete phenomena in radiation was not able to explain the wave characteristics of light such as diffraction. Not that his models could, but preserving the ether was a way to keep the door open to both discrete and undulatory characteristics.

The following quotation helps us to illustrate the grounds of his opposition to Planck's hypothesis while, at the same time, accepting the possibility of a certain quantification:

> "Again, if all the atoms were made of vortices of the same 'strength', we should find that certain mechanical quantities would all be integral multiples

[12] Joseph J. Thomson, *Electricity and Matter* (London: Archibald Constable & Co, 1906), 62–63.

[13] Russell McCormmach, "J.J. Thomson and the Structure of Light" *British Journal for the History of Science* 3 (1967), 362–387, 375.

> of a definite unit, i.e. these dynamical quantities, though not matter, would resemble matter in having an atomic constitution, being built of separate indivisible units. The quantity known as 'circulation' is proportional to its moment of momentum, and we see that in a theory of this kind the moment of momentum of particles describing circular orbits would always be an integral multiple of a definite unit. We see from this example that when we have a structure as fine as that associated with atoms, we may find dynamical quantities such as moment of momentum, or it may be kinetic energy, assuming the atomic quality and increasing or decreasing discontinuously by finite jumps. In one form of a theory which has rendered great service to physical science—I mean Planck's theory of the 'quantum'—the changes from radiant to kinetic energy are supposed to occur not continuously, but by definite steps, as would inevitably be the case if the energy were atomic in structure. I have introduced this illustration from the vortex atom theory of matter, for the purpose of showing that when we have a structure as fine as that of atoms we may, without any alteration in the laws of dynamics, get discontinuities in various dynamical quantities, which will give them the atomic quality. In some cases it may be that the most important effect of the fineness of the atomic quality in some dynamical quantity such as the kinetic energy. If then we postulate the existence of this propriety for the energy, it may serve as the equivalent of a detailed consideration of this structure itself. Thus, for many purposes (...) Planck's quantum theory serves as the equivalent of a knowledge of the structure of the atom".[14]

In 1910, J.J. introduced a new modification in his theory of light which is relevant for the purposes of this paper. He suggested that every single electron was the origin of only one Faraday tube of force and, therefore, each of the electrons exercised its influence only in one direction.[15] This enabled him to visualize better the concentration of energy in apparently corpuscular form: the impulse produced by a rapid displacement of an electron would be represented as a kink in the Faraday tube, a kink that would not spread but only travel in the direction of the tube.[16] In this way he had no need to postulate a quantum of light, since the apparent quantification was only a consequence of the way energy spread within a physical tube of force.

With these elements in mind, and with a growing interest to disprove Planck's notion that energy was essentially discrete, J.J. kept presenting different modifications to his theory of light. The one that interests me for the argument of this paper is a mechanism he presented in 1924 to account for the apparent discrete behaviour of light. Based, yet again, on the Faraday tubes of force, J.J. suggested that,

> "on this view the mutual potential energy of an electron E and a positive charge P is located in the tube of force stretching between E and P. If the electron falls from E to E' this potential energy is diminished by the energy

[14] Joseph J. Thomson, *The Atomic Theory. The Romanes Lecture* (Oxford: The University Press, 1914), 26–27.

[15] Joseph J. Thomson, "On a Theory of the Structure of the Electric Field and its Application to Röntgen Radiation and to Light" *Philosophical Magazine* 20 (1910), 301–313.

[16] See Bruce R. Wheaton, *The Tiger and the Shark: Empirical roots of wave-particle dualism* (Cambridge: Cambridge University Press, 1983), 140–142 for an analysis of this theory.

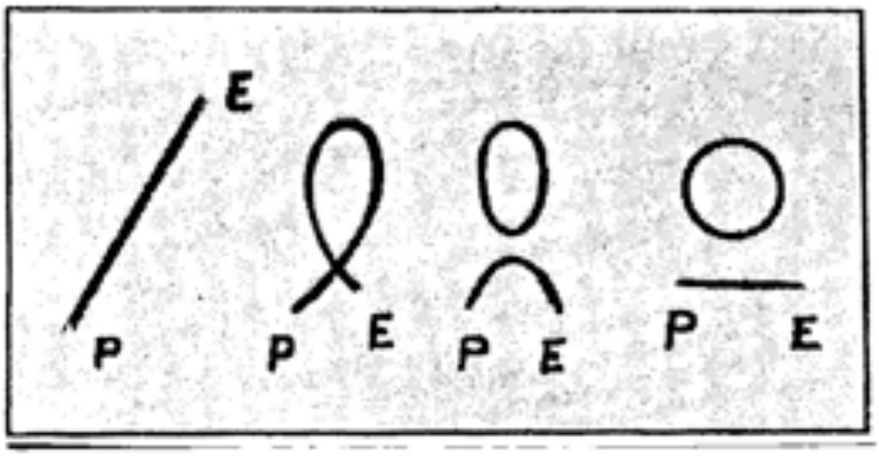

Figure 1.1: Bending of the Faraday tube

> in the portion EE' of this tube of force; for the energy in this portion to get free and travel out as light, the piece EE' of the tube must get into a state where it can travel freely with the velocity of light and not be associated with a charge of electricity whether positive or negative".[17]

As illustrated in Fig. **??**, when an electron jumped from a certain energy state to a lower one, the Faraday tube would bend. If the bent was big enough as to create a closed loop of tube of force, this would disassemble from the main body of the tube, giving rise to a "closed ring, which rapidly becomes circular and travels with the velocity of light. (...) The energy of this ring, (...) remains constant as long as the ring is unbroken".[18] That would be, in the terminology of quantum physics, the quantum of light. Analogously, the reverse process would explain the absorption of light and the jumping of the electron to a level of higher energy.

> "Thus we see that the death of a ring means either the birth of a high-speed electron or the emission of a unit of characteristic radiation. (...) The rings are the centres in which the energy from light to matter involves the destruction of these rings; thus the amount of energy transferred from a beam of monochromatic light or homogenous Röntgen radiation must be an integral multiple of a unit".[19]

This unit was, of course, Planck's constant.

A last aspect to point at is the way J.J. was trying not only to account for discrete phenomena but also for the continuous aspects of light, especially diffraction. The process of creation and emission of a ring of Faraday tube is such that before and after the emission of the ring the ether around the vibrating electron is set in motion. The ring itself, when liberated, is also vibrating. This gives us the picture of a ring which "will be

[17] Joseph J. Thomson, "A suggestion as to the Structure of Light" *Philosophical Magazine* 48 (1924), 737–746.
[18] *Ibid.*, 738.
[19] *Ibid.*, 739.

the centre of a system of electrical waves of the normal type, and predominant among these are those which have the same periodicity as the vibrations of the ring".[20] If the ring is assimilated to a unity of light, we find that this is accompanied by an extended wave. Thus, Thomson gets a picture in which discrete entities go hand in hand with undulatory characteristics, by which he aimed at an understanding of the dual nature of light and radiation.

The occurrence of diffraction in the passage of light through a slit would be explained in the following terms:

> "If the waves surrounding the ring fall on a narrow slit in a metal plate parallel to the plane of the ring, the electric and magnetic forces in the parts of the wave in the slit are much greater than they were before the wave reached the slit. The directions of these forces change as well as their intensities, so that the Poynting vector, i.e. the direction of the flow of energy, will change in direction from place to place in the neighbourhood of the slit. Thus the flow of energy gets diverted when the wave passes through the slit; it is no longer always in one direction, but spreads out fanwise after leaving the slit".[21]

In a famous statement in 1925, J.J. referred to the tension between discrete and undulatory conceptions of light as the battle between a tiger and a shark: "the position is thus that all optical effects point to the undulatory theory, all the electrical ones to something like the corpuscular theory; the contest is something like one between a tiger and a shark, each is supreme in its own element but helpless in that of the other".[22] His mental model was a step towards solving this entanglement; and it was a model that predisposed him favourably towards de Broglie's ideas. Furthermore, his theory of light was, in J.J.'s mind, more powerful than the, by then, "universally accepted" law of Planck. The latter was giving a good account of discrete phenomena in light, but "it is quite foreign to the undulatory theory which postulates a continuous and not an atomic distribution of energy".[23] Thus, J.J. always considered Planck's law as an incomplete theory that was solving the corpuscular aspects of light without explaining its undualtory properties.

G.P. and de Broglie's Principle

George Paget Thomson belongs to that special brand of British physicists whose entire life evolves around the University of Cambridge. Born in that university town in 1892, G.P. was the first and only son of J.J. who subtly led him into a career in physics. He was prepared by private coaching even before his enrolment in the university and, as a result, he was able to sit for both the Mathematical Tripos and the Natural Sciences Tripos in the three customary years that people took for only one degree. This gave him a special training in which both theoretical and experimental aspects of physics

[20] *Ibid.*, 740.
[21] *Ibid.*, 741.
[22] J. J. Thomson, op. cit. (10), 15.
[23] *Ibid.*

were present. Nevertheless, the particular training G.P. received in pre-World War I Cambridge was totally oblivious to the new developments in quantum theory.

In Cambridge, quantum physics and relativity were not formally taught until after the Great War, and even then only in the form of 'special courses': Charles Galton Darwin gave a course on spectra and quantum physics, and Arthur Eddington a course on relativity. Darwin was, together with Ralph Fowler, one of the first to introduce the new quantum physics in Cambridge.[24] A good life-long friend of G.P., Darwin became very critical of "the deficiencies of the syllabus [in Cambridge] which was disconnected from the subjects then coming into importance".[25] After graduating in Cambridge, Darwin moved to Manchester, where he met Niels Bohr in the crucial years of the development of his atomic model. This was his first real contact with the new physics and, after the war, when he returned to Cambridge as fellow of Christ's College, he was ready to embrace and work on quantum physics. Fowler's engagement with the new science was more independent than Darwin's. It was during the war, after being wounded in Gallipoli, that Fowler could study quantum physics from German scientific journals. Both Darwin and Fowler were Cambridge contemporaries and good friends of G.P., and he relied on them to get introduced into the new quantum physics in the late 1920s. During his formative years, quantum principles were rarely mentioned at home or in the university, and when they were, it was with high doses of contempt.

A faithful and devoted son of his father, G.P. relied on the advice of J.J. who became his mentor and supervisor in his first research work at the Cavendish. This is the reason why G.P. started his career as a researcher in the Cavendish laboratory on a project to study the nature and behaviour of positive rays. This project would eventually lead J.J.'s other assistant, F.W. Aston, to the manufacturing of the mass spectrometer and the discovery of isotopes. For J.J., however, as much as for G.P., this project had a different interest: first, the study of positive electricity emulating J.J.'s early work on cathode rays, and later, in the 1920s, as an instrument for chemical analysis. And the latter was the project that G.P. took with him to Aberdeen when he was appointed Professor of Natural Philosophy, in 1923.

While he was working with valves, sealing glass tubes, and pursuing the fine tuning of the vacuum pump in Aberdeen, G.P. was not oblivious to the theoretical developments of physics. His very good friends from the days of Cambridge—Darwin, Fowler and Bragg—would keep him up-to-date on their respective researches. It thus comes as no surprise that G.P. was well aware of de Broglie's principle, recently translated into English with the backing of Fowler.[26] In that paper, de Broglie was presenting the results of his recent PhD dissertation, from which he was "inclined to admit that any moving body may be accompanied by a wave and that it is impossible to disjoin motion of body and propagation of wave".[27] This is what soon came to be understood as the

[24]See the *Cambridge University Reporter*. In 1919 Darwin offered a course on 'Quantum Theory and Origin of Spectra'. This course changed to 'Recent Developments on Spectrum Theory' the following year, and a joint course on isotopes with Aston in 1921. In 1922 Fowler gave his first special course on 'The Theory of Quanta'.

[25]George P. Thomson, *J.J. Thomson and the Cavendish Laboratory in his Day* (London and Edinburgh: Thomas Nelson and Sons, 1964), 70.

[26]Louis de Broglie, "A tentative theory of Light quanta" *Philosophical Magazine* 47 (1924), 446–458. This paper was communicated by Ralph Fowler.

[27]*Ibid.*, 450.

principle of duality wave-particle, and which Schrödinger eventually turned into a full formulation of a wave quantum mechanics.[28]

De Broglie's English paper was entitled "A Tentative Theory of Light Quanta", a title which had very strong resonances in the Thomson family. As seen above, the nature of light and the other radiations had been a topic of heated debates for the past twenty years; a debate in which J.J. had been one of the main actors. This paper by de Broglie was an attempt to design a new theory of light, as much as J.J.'s 1924 paper was. Both were published in the same year and G.P. tried to unite them in a paper in the Philosophical Magazine. In retrospect, G.P. would regret publishing this paper, calling it "an example of a thoroughly bad theoretical paper",[29] even though it was proof, in his reconstructions of history, that he had paid attention to de Broglie's theory as soon as it was published in the British milieu: "I think in retrospect I was in advance of my time, I think I paid more attention to de Broglie than probably anybody else in this country on the whole. Some people thought it was just nonsense".[30]

The point to stress here is that G.P. knew of de Broglie's theory as a theory of light and electronic orbits, not as a theory of electron diffraction.[31] As we shall see, the idea of electron diffraction as an experimental application of de Broglie's theory came to him only some time in the summer of 1926, not in 1924. The title of his 1925 paper is "A Physical Interpretation of Bohr's Stationary States", and in it he tries to dismiss de Broglie's radical hypothesis as unnecessary. If the trajectories of electrons were understood in terms of waves as much as of particles, only those orbits in which the path is a multiple of the wavelength can be stable orbits around the nucleus, a suggestion that was totally in tune with Bohr's quantification. G.P.'s suggestion was that these stationary states could be equally achieved following his father's 1924 atomic model explained above. If proton and electron were united by a Faraday tube of force, "it will thus be able to transmit waves, and the condition that will be taken as determining the possible states is that the vibrations in this tube shall be in tune with the period of the orbit".[32] In this manner, G.P. Thomson was doing away with the main characteristic of de Broglie's hypothesis—the fact that electrons were actually waves—by ascribing the wave motion to the tube of force *outside* the electron.

[28]For this process, see Varadaraja V. Raman and Paul Forman, "Why was it Schrödinger who developed de Broglie's ideas?" *Historical Studies in the Physical Sciences* 1 (1969), 291–314.

[29]George P. Thomson, "Early Work in Electron Diffraction" *American Journal of Physics* 29 (1961), 821–825, 821.

[30]Oral interview with George P. Thomson, *Archive for the History of Quantum Physics*, Tape T2, side 2, 8.

[31]In his reconstruction of the events, G.P. presented a different version of the facts. G.P. Thomson, op. cit., (29), 821: "At that time we were all thinking of the possible ways of reconciling the apparently irreconcilable. One of these ways was supposing light to be perhaps particles after all, but particles which somehow masqueraded as waves; but no one could give any clear idea as to why this was done. The first suggestion I ever heard which did not stress most of all the behaviour of the radiation came from the younger Bragg, Sir Lawrence Bragg, who once said to me that he thought the electron was not so simple as it looked, but never followed up this idea. However, it made a considerable impression on me, and it pre-disposed me to appreciate de Broglie's first paper in the Philosophical Magazine of 1924".

[32]George P. Thomson, "A physical interpretation of Bohr's stationary states" *Philosophical Magazine* 1 (1925), 163–164, 163.

G.P.'s experiments on electron diffraction

"By 1926 I was feeling depressed by having failed to produce anything of real note. In fact, positive rays, as distinct from the study of isotopes, were nearly worked out, at least for the time".[33] Looking at his laboratory notebooks, however, no hint of G.P.'s disappointment is evident: in the first half of the year he keeps accumulating data and changing the experimental conditions in his work on positive rays. The last entry before the summer break is from June 23rd, in which he is testing the scattering of positive rays in Argon; the next entry, on August 23rd, clearly signals a shift of research project: "Alteration to apparatus. A slip of gold leaf mounted on brass carrier + partly covering aperture in camera".[34] His quest for electron diffraction had started.

The different autobiographical notes by G.P. on the events leading up to his measurement of electron diffraction are a bit hazy. They all coincide, however, as does all other evidence, in assigning a central role to the month of August 1926, both in Oxford and in Cambridge. From the 4th to the 11th the British Association for the Advancement of Science held its annual meeting in Oxford; and it became the forum in which many British and American physicists learnt about the latest developments in wave quantum mechanics. During the spring that year Erwin Schrödinger, based on de Broglie's ideas, had reinterpreted wave mechanics from a quantum perspective. Max Born, present at the meeting, explained these developments to the participants, and the topic became one of the highlights in the informal discussions in the meeting.[35]

Straight after the Oxford meeting G.P. stopped over in Cambridge, where he could continue discussions on electron diffraction. Actually, in the Cavendish he must have met with Charles D. Ellis, who had, as early as 1924, unsuccessfully tried to convince Rutherford to allow him to look for electron diffraction in the Cavendish.[36] The case is that be it in conversations in Oxford or in Cambridge, G.P. saw—or was led to understand—that his experimental device in Aberdeen was all that was needed to try electron diffraction through solids and that he was in the best of conditions to give it a try. And that's what he did, first with his research student Andrew Reid, and then, after the unfortunate death of the latter in a motorcycle accident, on his own. The first tentative results were published in a note in *Nature* in June 1927,[37] and this was followed by a full account of his work in several articles later that year and the following one.[38]

[33] George P. Thomson Archives, Trinity College, Cambridge, A6, 7.

[34] *Ibid.*, C24, 13.

[35] Born's paper had a strong impact on many of the present, but especially on the American physicist working at the Bell laboratories, Clinton J. Davisson, when he heard that the anomalous results he had been obtaining in experiments on electron dispersion with his colleague Lester H. Germer might be signs of electron diffraction. That branch of the story, which was studied in detail by historian of science Arturo Russo, ends with the confirmation of electron diffraction in the Bell laboratories and the sharing of the Nobel Prize with G.P. Thomson for their experimental proof of de Broglie's principle. Born also mentioned the experiments of the young German physicist, Walter M. Elsasser, who had unsuccessfully tried to detect diffraction patterns in the passage of an electron beam through a metallic film. See Arturo Russo, op. cit. (5).

[36] *Ibid.*, 141.

[37] George P. Thomson and Andrew Reid, "Diffraction of Cathode Rays by a Thins Film" *Nature* 119 (1927), 890.

[38] George P. Thomson, "The Diffraction of Cathode Rays by Thin Films of Platinum" Nature 120 (1927), 802; "Experiments on the Diffraction of Cathode Rays" *Proceedings of the Royal Society* 117 (1928), 600–609; "Experiments on the Diffraction of Cathode Rays. II" *Proceedings of the Royal Society* 119

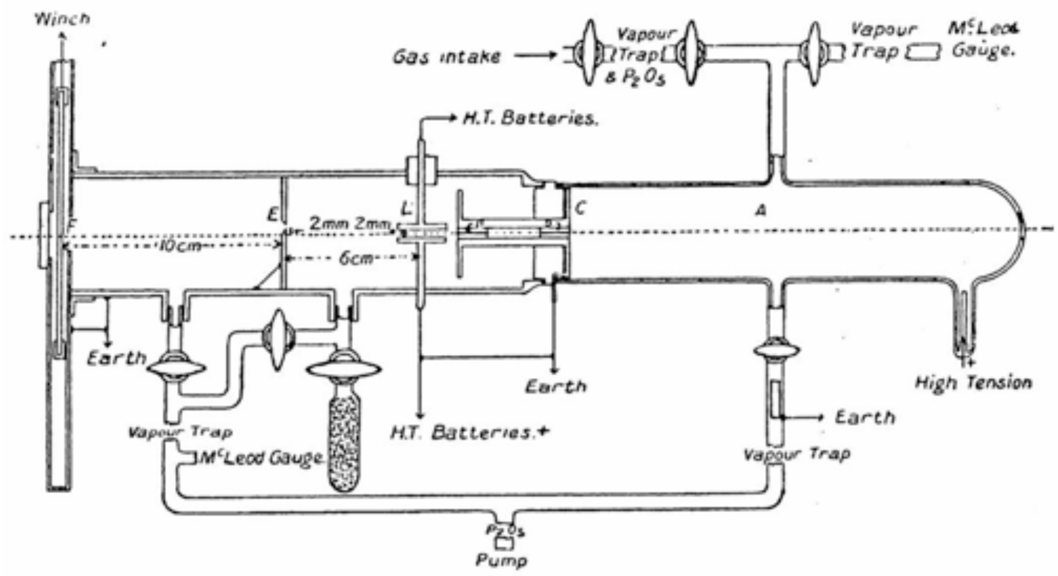

Figure 1.2: Experimental Arrangement

A quick comparison between the experimental arrangement he had so far used for his experiments on positive rays (Fig. ??) with the one he used in his work of 1926–1927 clearly shows that few changes were needed for the new measurements. His original display provided positive rays using a cathode rays tube; and now, the same tube could be the source of a beam of electrons. The "apparatus for studying the scattering of positive rays (...) could be used for this experiment with little more change than reversing the current in the gaseous discharge which formed the rays".[39] The rest of the arrangement only varied in the fact that instead of scattering the positive rays in a gas, he would attempt their diffractive dispersion through a thin metallic plate. The latter was, in a way, the only real experimental change, one in which he depended on the good skills of his assistant C.G. Frazer, who succeeded in obtaining the extremely thin metallic films that were needed.

The aim of this paper is not to give a detailed account of G.P.'s work in the period 1926–1928. But one element needs to be highlighted: the close connection between his experiments and the long tradition in research on X-ray diffraction, to which G.P. was certainly no stranger. After the discovery of X-ray diffraction by Planck's protégé Max von Laue in Munich in 1912, G.P.'s life-long friend Lawrence Bragg had modified his father's research project on X-rays and understood that X-ray diffraction could be used as a tool to determine the crystalline structure of metals. This other father-son story culminated in the shared Nobel Prize that both Braggs received in 1915 and, most importantly, consolidated the emergence of the new science of X-ray crystallography in Britain. G.P. certainly followed closely these developments due to his friendship with

(1928), 651–663; "Experiments on the Diffraction of Cathode Rays. III" *Proceedings of the Royal Society* 125 (1929), 352–370.

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the young Bragg with whom he spent summer holidays in G.P.'s boat, the Fortuna.[40] His other life-long friend, C. G. Darwin, was responsible for the formulation of the most successful theory of X-ray diffraction between 1913 and 1922.[41]

The parallelism between G.P.'s experiments and X-ray diffraction was almost complete, since the order of energy of the waves de Broglie was talking about was the same as that of hard X-rays. The only real difference between X-rays and the waves of cathode rays was that the latter could be deflected with electric and magnetic fields due to their electric charge, a difference that proved essential in order to make sure that the diffracted patterns were not due to secondary X-rays but to the cathode rays themselves.[42] Again, this was a feature that the experimental arrangement for G.P.'s project on positive rays already included: like the experiment that had led to the hypothesis of the corpuscle in 1897, the Thomsons' study on positive rays involved their deflection by electric and magnetic fields in the glass tube.

The pictures G.P. obtained were powerful enough to convince his audience (Fig. **??**). The circular halos were widely recognised as the Hull-Debye-Scherrer patterns of diffraction, already known for X-ray diffraction. Therefore, if those pictures were really obtained from dispersed cathode rays, there was no other way out but to accept that the electrons behaved like waves: "The detailed agreement shown in these experiments with the de Broglie theory must, I think, be regarded as strong evidence in its favour".[43]

If the period between the summer of 1926 and the spring of 1928 required only a few changes in the experimental culture of G.P. Thomson, it did however involve a radical change in his conceptual framework. Distancing himself from his classical tradition, he was suddenly coming to terms with the fact that, as he said in his November 1927 paper, his experiments involved "accepting the view that ordinary Newtonian mechanics (including the relativity modifications) are only a first approximation to the truth, bearing the same relation to the complete theory that geometrical optics does to the wave theory".[44] This statement strongly suggests a connection with Niels Bohr's correspondence principle, formulated in 1923, by which it is assumed that classical physics is the limit of quantum physics for large quantum numbers. If that is so, that would mean a first abandonment of the classical mechanics he had thus far been immersed in, and one

[40]See Graeme K. Hunter, *Light is a Messenger: The life and science of William Lawrence Bragg* (Oxford: Oxford University Press, 2004), 70 and 104.

[41]Darwin came back to Cambridge after the war and was made a fellow of Christ's College while G.P. was a fellow in Corpus Christi. On Darwin, see George P. Thomson, "Charles Galton Darwin" *Biographical Memoirs of Fellows of the Royal Society* 9 (1963), 69–85.

[42]The following anecdote helps to illustrate the importance of electromagnetic deflection. Probably around the beginning of March 1928, he also had the opportunity to discuss his experimental results with Schrödinger himself as the latter recalled in 1945: "After mentioning briefly the new theoretical ideas that came up in 1925/26, I wish to tell of my meeting you in Cambridge in 1927/28 (I think it was in 1928) and of the great impression the marvellous first interference photographs made on me, which you kindly brought to Mr Birthwistle's house, where I was confined with a cold. I remember particularly a fit of scepticism on my side ("And how do you know it is not the interference pattern of some secondary X-rays?") which you immediately met by a magnificent plate, showing the whole pattern turned aside by a magnetic field." Schrodinger to G.P. Thomson, 5th February 1945, George P. Thomson Archives, Trinity College, Cambridge, J105, 4. The exact date can be traced by the minutes of the Kapitza Club, which says that Schrödinger gave a paper to the Club on March 10th, 1928. See *Churchill Archives*, CKFT, 7/1.

[43]George P. Thomson, op. cit. (38), I, 608.

[44]*Ibid.*, 608–609.

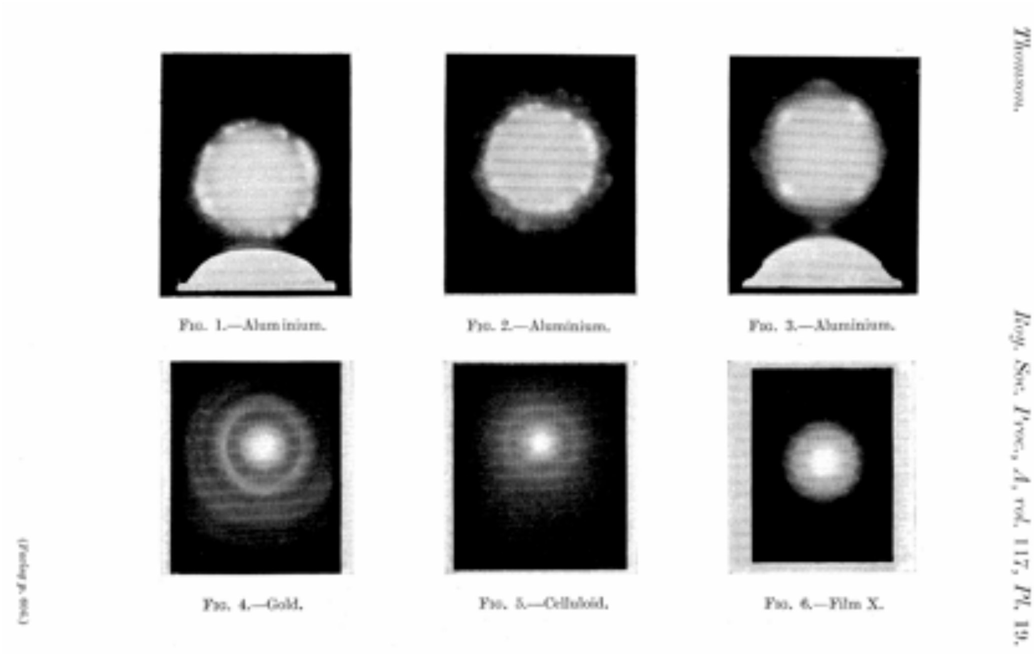

Figure 1.3: Pictures from G.P.'s experiments on electron diffraction.

might want to consider when and how G.P. got in touch with the latest developments going on in Copenhagen.

Besides the impetus that the BAAS Oxford meeting of 1926 meant for many British physicists, G.P. benefited, once again, from his close friendship with C. G. Darwin who, since 1924 the Tait Professor of Natural Philosophy in Edinburgh, spent two months in Copenhagen in the spring of 1927, where he learned about the latest developments in quantum physics and complementarity from Bohr and Heisenberg themselves. On his way back, Darwin spent some time in Aberdeen, in G.P.'s home. This way, G.P. learned all about it from Darwin's explanations: "we had long talks about all this, and really began to get an idea about it".[45] The timing was just right. As G.P. was seeing with his own eyes the diffraction patterns of cathode rays, he understood their importance in the context of the latest theoretical developments of quantum mechanics from possibly the British physicist best suited for understanding them at the moment. In his biographical memoir on Darwin, G.P. said that "I am inclined to think that his most useful work was as an interpreter of the new quantum theory to experimental physicists. (...) I should like to record my great debt to him for the many ideas in physics he helped me to understand".[46]

The pictures convinced G.P. of the validity of de Broglie's principle. But contrary to what had happened in 1925 when he first learnt about the new theory, G.P. was no longer interpreting it in terms of his father's metaphysical framework. In the last section we will explore the change of mindset that can be perceived in the early explanations about electron diffraction that G.P. gave, and the uses he made of it. However, let's

[45]Oral interview with G.P. Thomson, *Archive for the History of Quantum Physics*, Tape T2, side 2, 15.
[46]George P. Thomson, op. cit. (41), 81.

pause for a moment before that and look at the reaction of his father, J.J., in the face of the unavoidable experimental evidence.

J.J.'s reaction

The father saw, in the experiments of his son, the final proof of his life-long metaphysical project and a clear sign of the invalidity of quantum physics as an ultimate explanation. His world had always been, and still was, a world of ether, in which discrete entities, including the electrons, were but epiphenomena in the ether. Now, in 1928, J.J. Thomson felt his metaphysical idea had proved true and that electron diffraction was a sign that discrete models of matter were only rough approximations to reality. In his mind, the "very interesting theory of wave dynamics put forward by L. de Broglie", and experimentally proved by his son, was not in contradiction with classical mechanics. In the first of a series of papers he would publish in Philosophical Magazine, J.J. tried to show that "the waves are also a consequence of classical dynamics if that be combined with the view that an electric charge is not to be regarded as a point without structure, but as an assemblage of lines of force starting from the charge and stretching out into space".[47]

Thomson had never accepted the idea put forward by Larmor and Lorentz at the turn of the century of an electron being a point charge of electricity in the ether. Now, the detection of a train of waves associated with the movement of electrons was proof that he had been right: Maxwell's equations had not predicted such a wave for a point electron, and *therefore* such a view of the electron had to be wrong. On the other hand, de Broglie's wave could be obtained on purely classical grounds if he assumed the electron to be a two-part system: a "nucleus which (...) is a charge e of negative electricity concentrated in a small sphere",[48] and a sphere surrounding it "made up of parts which can be set in motion by electric forces (...) consist[ing] either of a distribution of discrete lines of force, or of a number of positively- and negatively-electrified particles distributed through the sphere of the electron".[49] With this *ad hoc* structure J.J. deduced the relationship between the speed of an electron and the wavelength of its sphere to be the same as that expected by de Broglie and measured by G.P.

In a conference given in Girton College, Cambridge, in March 1928 entitled *Beyond the Electron*, J.J. argued that talking about a structure for the electron was not ludicrous. Thirty years earlier, when he first suggested that corpuscles would be constituents of all atoms, thus initiating the exploration of the structure of the atom, he had been accused of being an alchemist. The developments of the physics of the electron had dismissed that accusation. Now he felt justified to talk about the structure of the electron in the light of the latest developments by his son. "Is not going beyond the electron really going too far, ought one not draw the line somewhere?", he would ask rhetorically. To

[47] Joseph J. Thomson, "Waves associated with Moving Electrons" *Philosophical Magazine* 5 (1928), 191–198, 191.

[48] Joseph J. Thomson, "Electronic Waves and the Electron" Philosophical Magazine 6 (1928), 1254–1281, 1259.

[49] *Ibid.*, 1254. J.J.'s model for the electron sphere would soon be expressed in terms only of what he came to call "granules", particles "having the same mass μ, moving with the velocity of light c, and possessing the same energy μc^2". See Joseph J. Thomson, "Atoms and Electrons" *Manchester Memoirs* 75 (1930–31), 77–93, 86.

which he would reply that "It is the charm of Physics that there are no hard and fast boundaries, that each discovery is not a terminus but an avenue leading to country as yet unexplored, and that however long the science may exist there will still be an abundance of unsolved problems and no danger of unemployment for physicists".[50]

The diffraction experiments showed that "we have energy located at the electron itself, but moving along with it and guiding it, we have also a system of waves".[51] Following the similarities with his structure of light of 1924, he supposed that the electron "had a dual structure, one part of this structure, that where the energy is located, being built up with a number of lines of electric force, while the other part is a train of waves in resonance with the electron and which determine the path along which it travels".[52] For him, the association of a wave with an electron was not a new phenomenon. It had already happened when, in the late 18th century, the corpuscles of light that Newton had postulated needed to be complemented by wave explanations. It was not so strange to see that the new corpuscles, the electrons, had to undergo a similar process. Furthermore, discussions on the nature of light in the previous two decades had paved the way for the acceptance of the duality of the electron.

In the world of J.J, electron diffraction brought with it the possibility of challenging, rather than accepting, the new quantum physics. A continuous metaphysics in which all phenomena and entities could be seen as structures of the ether was, in his view, still possible. Furthermore, J.J. felt that at last electron diffraction provided the final argument to defend the old worldview, something that the developments of the previous two decades had, only apparently, jeopardised. Electron diffraction was proof of the complexity of the electron and, therefore, of the validity of classical mechanics. Quantification of magnitudes such as momentum or energy "is the result and expression of the structure of the electron; only such motions are possible, or at any rate stable, as are in resonance with the vibrations of the underworld of the electron".[53]

At the root of his models there was a metaphysical problem as much as an epistemological one. As already stated, J.J.'s metaphysics involved a continuum in terms of which all discrete phenomena could, and should, be explained. Parallel to that was an epistemological problem: for Thomson, de Broglie's and Schrödinger's theories, as much as Planck's, were valid only from a mathematical point of view. Their results were valid, but they did not entail real, true physics. And that was the strength J.J. saw his theory had over de Broglie's: "The coincidences are remarkable because two theories could hardly be more different in their points of view. M. de Broglie's theory is purely analytical in form; the one I have brought before you (...) is essentially physical".[54] It comes to no surprise that, true to the spirit in which he was educated in the old Mathematical Tripos, physical meant mechanical.

In an ironical remark on the situation of physics in previous years he would state in 1930 that "when the waves are taken into account, the classical theory of dynamics gives the requisite distribution of orbit [of the electrons] in the atom, and as far as these go the properties of the atom are not more inconsistent with classical dynamics than are the properties of organ pipes and violin strings, in which, as in the case of the electron,

[50]J.J. Thomson, *Beyond the Electron* (Cambridge: Cambridge University Press, 1928), 9.
[51]*Ibid.*, 22.
[52]*Ibid.*, 23.
[53]*Ibid.*, 31.
[54]*Ibid.*, 34.

waves have to be accommodated within a certain distance. It is too much to expect even from classical dynamics that it should give the right result when supplied with the wrong material".[55] Obviously, the fact that the proof had come in the family was only an added reason to rejoice.[56] In the decade between these events and his death in 1940, J.J. did not change his mind. The last paper he ever published, sent in October 1938 at age 81, still reclaimed his son's experiments as proof of the validity of the old classical mechanics.

G.P.'s evolution

G.P. presented his first preliminary results in a short note in Nature dated May 1927 and in a presentation at the Kapitza Club, in Cambridge, on the 2nd of August.[57] In November he was ready to publish a long and detailed paper in the *Proceedings of the Royal Society* preceded by another short note in *Nature*.[58] Although these papers are basically a cold description of the experimental methods and results, some distance from his father's metaphysics is already apparent. As noted above, G.P. realised that his experiments meant a proof of de Broglie's principle and, *therefore*, undermined the validity of classical mechanics. In his words, his experiments involved "accepting the view that ordinary Newtonian mechanics are only a first approximation to the truth (...). However difficult it may seem to accept such a sweeping generalisation, it seems impossible to explain the results obtained except by the assumption of some kind of diffraction".[59]

For the first time in his career we can see a strong contrast between his and J.J.'s position. Both father and son accepted the law of de Broglie, but in different terms. The father wanted to obtain the same relationship between speed, mass and wavelength of the electron by creating an *ad hoc* mechanical model; the son saw the incompatibility of both approaches and opted for a correspondence between the old and the new, between Newton and de Broglie, in terms advocated by Bohr and the school of Copenhagen. G.P. was cutting the umbilical cord that had kept him tied to his father and to the old worldview for far too long.

But this change was no easy business. Two basic questions were at stake: the relationship between the particle and the wave associated with it, and the nature of the medium in which these waves propagate. Before turning to his answer to these questions, we should reflect on G.P.'s attitude towards experimental and theoretical science. A quotation from his Friday speech at the Royal Institution of 1929 describes his approach to theoretical speculation in this period of his life. After explaining with full detail the experiments on electron diffraction he would venture into trying to answer the "great difficulties of interpretation. What are these waves? Are they another name for the electron itself? (...) Some of these questions I should like very briefly to discuss, but we now leave the sure foothold of experiment for the dangerous but fascinating paths

[55] Joseph J. Thomson, *Tendencies of recent investigations in the field of Physics*, (London: British Broadcasting Corporation, 1930), 26–27.

[56] Oral interview with G.P. Thomson, *Archive for the History of Quantum Physics*, Tape T2, side 2, 9: "Well, I think he was very pleased [with my developments], largely because it was in the family".

[57] Thomson and Reid, op. cit. (37), and *Churchill Archives*, CKFT 7/1.

[58] George P. Thomson, op. cit. (38).

[59] George P. Thomson, op. cit. (38), I, 608–609.

traced by the mathematicians among the quicksands of metaphysics".[60] The contrast between the security of experimental data and the uncertainty of theorising is, in this quotation, very strong, and shows how G.P. was sticking to what he considered to be the facts, and distrusting unnecessary speculation.

This speech is also the first time in which he publicly and explicitly distances himself from his father's ideas. Contrary to J.J.'s explanation of the electron waves in terms of a modification of the ether, G.P. dismisses the need of an ether and takes, for the first time, a pragmatic and positivistic stand. "Personally—he says—I see no necessity for there to be any vibration of a material or quasi-material object. (...) The easiest way of looking at the whole thing seems to be to regard the waves as an expression of the laws of motion".[61] And to give authority to his point of view, he finished his speculations by quoting Newton's famous 'hypothesis non fingo'.

The best and most exhaustive document we have to understand G.P.'s views at the time of his experiments is a series of lectures he gave at the University of Cornell the last term of 1929, and immediately published in the form of a book, *The Wave Mechanics of Free Electrons*. Here we find a thorough explanation of the reasons why he wanted to avoid the question of the ether. The wave-lengths of electron waves and X-rays are in the same range, but they clearly behave differently, for the first can be deflected, and the second can't. If that is the case, one might need to assume two different media to account for the different behaviour of the two waves, "but it is not a very attractive idea to have two ethers filling the space, especially as the waves of protons—if they exist—would demand yet a third. Space is becoming overcrowded".[62] G.P.'s decision was to apply Ockham's razor, to do away with the ether and stick to the information given by the wave formulation, and "perhaps simple physicists may be content as long as the waves do their job guiding the electron, and it is possible that, after all, the question will ultimately be seen to be meaningless".[63] G.P. seems to be here in close agreement with the spirit of the Copenhagen Interpretation, mixing epistemology and metaphysics, and reducing what there is to what can be described.

One of G.P.'s most surprising speculations in these years was his adherence to the possibility, put forward by Bohr, that energy conservation might have to be abandoned in order to explain beta radioactive decay.[64] Closely following calculations made by his friend Darwin on his way back from Copenhagen, G.P. suggested a mechanism to account for the dispersion of energy. Essentially, G.P. was suggesting that the actual beta emission did conserve energy, only that the huge acceleration suffered by the electron in its ejection from the nucleus involved the creation of an energetic wave, like "the sound produced by the firing of an atomic gun whose bullet is the electron".[65] Such a wave could be supposed to "possess energy when highly concentrated which it loses on spreading out,"[66] giving rise to an indeterminacy in the energy of the electron. The

[60] George P. Thomson, "The Waves of an Electron" *Nature* 122 (1928), 279–282, 281.
[61] *Ibid.*, 282.
[62] George P. Thomson, *The Wave Mechanics of Free Electrons* (New York & London, 1930), 11.
[63] *Ibid.*, 12.
[64] For a thorough analysis of the problems with beta decay and the conservation of energy, see Carsten Jensen, *Controversy and Consensus: nuclear beta decay, 1911–1934* (Basel: Birkhäuser, 2000).
[65] George P. Thomson, "On the Waves associated with β-Rays, and the Relation between Free Electrons and their Waves" *Philosophical Magazine* 7 (1929), 405–417, 410.
[66] *Ibid.*, 415.

actual mechanism G.P. was thinking of was based on basic mathematical properties of waves: the Fourier transformation of the initial pulse would give all possible monochromatic wavelengths and, therefore, all possible energies. The emitted electron would choose only one of these monochromatic waves, thus explaining the indeterminacy in their energy.[67] G.P. did not follow this idea any further since he was step-by-step coming to understand that the new physics was totally alien to the old notion of explanation by way of mechanical models.

As for the relationship between the wave and the particle, the question arises as to which is the *real* thing. Here his position is less clear, but there doesn't seem to be a total identification of both. The wave *guides* the electron but is not totally identifiable with the electron, since what one really observes is the particle, not the wave: "Whenever an electron produces any detectable effect it does so as a particle, and it seems easiest to suppose that even when it is not producing an effect the particle is somewhere round".[68] An example he would often use in his popular lectures is that of the gossamer spider:

> "When at rest this spider is a minute insect. When it wants to move it sends out streamers into the air, and floats away owing to the action of the air on these filaments which stretch out a foot or more all round it. Just so the electron, when it is part of an atom its waves are limited to that atom, or even to a part of it. They are curled round on themselves, as it were. Suppose, now, an electron escapes from the hot filament of a wireless valve and gets free. Its waves will spread far out into the space round it. I regard it as still a particle at the centre of its wave system. The analogy can be pressed further. If the wind sweeps the spider past an obstacle the filaments will catch. The pull on filaments will move the spider, and he will feel that there is something in the way, even though his body does not actually hit it. In the same way the waves are a means by which the motion of the electron is affected by things which the main body of the electron never comes very near".[69]

The Aberdeen experience came to an end in 1930, when he was offered the chair at Imperial College, London, after his close friend W.H. Bragg had declined the offer. In his new appointment, G.P. made use of his experimental skills to study the minutiae of electron diffraction and some possible applications, soon to move, following the steps

[67] See George P. Thomson, "The Disintegration of Radium E from the Point of View of Wave Mechanics" Nature 121 (1928), 615–616: "[The apparent non conservation of energy] is to be expected on the new wave mechanics, if the ejection of a β-particle is produced by anything like a sudden explosion. In such a case one would expect that the wave-group which accompanies, and on some views actually constitutes, the electron, would be of the nature of a single pulse, that is, the damping factor of the amplitude would be of the order of the wave-length. Such a wave-group, being very far from monochromatic, would spread rapidly lengthwise owing to the large dispersion of the phase waves, and so the distance within which the electron may occur becomes large, implying a marked 'straggling' in velocity. Similarly, if the waves pass through a magnetic field, which is for them a refracting medium, the group will split into monochromatic waves going in different directions, just as white light is split up by a prism. Thus an observer who forms the magnetic spectrum of the β-rays will find electrons in places corresponding to paths of various curvatures, that is, he will find a spectrum continuous over a wide range".

[68] George P. Thomson, op. cit. (62), 10.

[69] George P. Thomson, "New Discoveries about Electrons" *The Listener* 1 (1929), 219–220, 220.

of Fermi, to the more fashionable and very promising field of slow neutrons. Electron diffraction, for which he would become universally known and receive the Nobel Prize in 1937, soon became a closed chapter of his scientific life.

Conclusion

A textbook history of the early developments of quantum physics will, at most, only contain a footnote mentioning G.P.'s experiments on electron diffraction. And certainly no reference will be made to the antagonism and speculations of J.J. Thomson. However, a good history of quantum physics should analyse the attitudes, ideas and reactions of both the 'winners' and the 'losers', to avoid being whiggish. J.J. Thomson can be considered to be one of such losers; but in the 1920s he was still a public icon of British science. True, he didn't play a major role at the forefront of science, but he was a very influential figure among second line scientists and the general public. The study of his reaction against quantum physics is certainly necessary if we want to understand the public perception of quantum physics in the 1920s.

At a less social level, one can also use this case study as a way to analyse the role that experimental physics played in the configuration and acceptance of quantum physics. Electron diffraction was proof that electrons behaved like waves, and it triggered in G.P. a certain conviction that wave mechanics, with all the epistemological implications, was worthwhile embracing. But, as we have seen, the experiments were not necessarily an *experimentum crucis* for wave mechanics, certainly not for J.J. and his generation, who were, in Kuhnian terms, excessively immersed in the paradigm of ether physics.[70] This case study reveals the complexities in interpreting experimental results. The same experiments triggered different, almost opposite, responses in the father and in the son.

[70] The antagonism to quantum mechanics was not exclusive to Cambridge. In Oxford, for instance, the head of the Clarendon Laboratory stubbornly rejected quantum physics. See Benoit Lelong, "Translating Ion Physics from Cambridge to Oxford: John Townsend and the Electrical Laboratory, 1900–24", in Physics in Oxford 1839–1939. Laboratories, Learning and College Life, ed. Robert Fox and Graeme Gooday (Oxford: Oxford University Press, 2005), 209–232, 229: "The break of international physics became more marked after the war. Townsend first ignored and then rejected the emerging quantum theories".