Physics Today

Fifty years of matter waves

Heinrich A. Medicus

Citation: Physics Today **27**(2), 38 (1974); doi: 10.1063/1.3128444
View online: http://dx.doi.org/10.1063/1.3128444
View Table of Contents: http://scitation.aip.org/content/aip/magazine/physicstoday/27/2?ver=pdfcov
Published by the AIP Publishing

Fifty years of matter waves

Louis de Broglie's conception of the wave-particle duality in 1923 opened up new experimental possibilities, initiating the era of modern quantum mechanics.

Heinrich A. Medicus

Fifty years have passed since Louis de Broglie created the theory of matter waves and published his first papers on the subject—inaugurating the era of modern quantum mechanics. De Broglie's undertaking was a very bold one. Unlike Planck's work in the older quantum theory, which had its origin in the measurements of black-body radiation, and unlike the photon hypothesis of Einstein, where the early experiments on the photoelectric effect offered some corroboration, de Broglie's theory lacked the support of any direct experimental evidence. Had it not been for the intervention of such established figures as Langevin and Einstein, who recognized the importance of what he had accomplished, de Broglie's work probably would have had little immediate effect.

Although the history of matter waves has been told before—in *The Conceptual Development of Quantum Mechanics* by Max Jammer,[1] in *Geschichte der Quantentheorie* by Friedrich Hund[2] and in *Geschichte der Wellenmechanik* by Johannes Gerber,[3] for example—there is certainly room for another account. Here I will place the emphasis not so much on the wave-mechanical aspects, as on the concept of the matter waves, attempting to trace the immediate influence of de Broglie's ideas on his contemporaries—notably on Einstein, Schrödinger, Elsasser, Davisson and Thomson—at the same time correcting certain inaccuracies and providing new material.

The origin of de Broglie's concept

When did de Broglie arrive at the revolutionary idea of attributing wave properties to particles? It is not correct to trace the concept to his thesis of 1924. The history of the events leading up to his thesis, as evidenced by published documents, follows.

Heinrich A. Medicus is a professor in the department of physics at the Rensselaer Polytechnic Institute.

Early in 1922 de Broglie wrote an article[4] about black-body radiation in which he derived Wien's radiation law by means of thermodynamics, kinetic gas theory and quantum theory, without using electromagnetic theory. Here he says, "The hypothesis of quanta of light is adopted." (This article was written one year before the explanation of the Compton effect, which settled definitively the existence of light quanta.) He treats the photons as particles, or "atoms of light" with mass $h\nu/c^2$ and momentum $h\nu/c$. On assuming a mixture of photon gas "molecules," each consisting of 1, 2, 3, . . . atoms of light, he obtains Planck's law. In a sense, this paper was a precursor of the Bose statistics. Yet another article,[5] written in the same year, deals with the energy fluctuations in the blackbody radiation.

In a 1963 interview in Paris with Thomas S. Kuhn, Andre George and Théo Kahan,[6] de Broglie indicated that this paper on blackbody radiation was the point of departure for his later work: "I began to have the idea—it was not yet born. I probably would not have dared to tell about—but I began to have it in my mind." This interest in the properties of quanta motivated his search for a theory that would unify the wave and particle aspects. In addition, the occurrence of integral quantum numbers in descriptions of the movement of electrons in an atom reminded him of wave theory, where integers appear in interference phenomena and at many other places.

Suddenly, in the summer of 1923, the idea occurred to de Broglie to generalize this wave-particle duality to include material corpuscles as well. (He was thinking of electrons in particular.) This idea, to be more fully developed the following year in his thesis, initially came to light in 1923—in three short articles published in the 10 and 24 September and 8 October issues of the *Comptes Rendus*,[7] and in a very short note submitted to *Nature*[8] on 12 September, in which he sketches some of the main points and refers for details to the first two *Comptes Rendus* articles. These preliminary notes, which contained the essentials of his new theory on a total of seven or eight pages, were written with the thought of developing these ideas more completely in a thesis.[9] In October of the same year he also submitted a paper to the *Philosophical Magazine,*[10] which, however, did not appear until 1924.

Thus, de Broglie's discovery actually took place in 1923. The year of his thesis,[11] 1924, has gained common acceptance, particularly outside France, as the date of the discovery. But de Broglie himself, in the volume[12] published on the occasion of his 60th birthday in 1952, said that 1923 would be a more accurate date. The thesis itself was published as an article of over 100 pages in the *Annales de Physique* of 1925.

It should be noted in passing that, contrary to popular understanding, de Broglie was not a young, unknown student when he wrote the thesis for which he won the Nobel Prize in 1929. Because he had spent six years in the military, stationed at the military radiotelegraphic station on the Eiffel tower, he was 32 years old when he received his doctorate and had already published about two dozen scientific papers on electron, atomic and x-ray physics. Upon discharge from the military, de Broglie resumed his theoretical studies, at the same time working in the private laboratory of his considerably older brother, the Duc Maurice de Broglie, a highly respected x-ray physicist, with whom he had many conversations about wave-particle duality in x rays.

De Broglie's theory in brief

It will be helpful to briefly sketch the thought behind de Broglie's discovery. Basically, his idea was an extension, to include all particles, of Einstein's 1905 theory about the wave-particle duality of photons: Corpuscles

ARCHIVES DE L'ACADEMIE DES SCIENCES DE PARIS

are accompanied by waves. Whereas the equation $E = h\nu$ applied for photons, and $E = mc^2$ for material particles, de Broglie assigns a frequency ν_0 as the frequency of an internal vibration as measured in the reference system fixed to the particle of rest mass m_0. However, if a stationary observer sees the particle passing with a certain velocity, he will see the frequency of the internal vibration decrease to $\nu_1 = \nu_0\,(1 - \beta^2)^{1/2} = (m_0c^2/h)\,(1 - \beta^2)^{1/2}$, because moving clocks go slowly. On the other hand, the energy of the moving particle is $m_0c^2\,(1 - \beta^2)^{-1/2}$, which corresponds, according to the quantum relation, to a frequency $\nu = (m_0c^2/h)\,(1 - \beta^2)^{-1/2}$. The frequencies ν_1 and ν are different. De Broglie overcomes this apparent difficulty by stating that the periodic phenomenon that is inherent in the moving particle and that for an observer at rest has the frequency ν_1, appears to this observer to be constantly in phase with a wave having the frequency ν and propagating with a velocity $V = c/\beta$ in the same direction as the particle. This velocity V is thus the phase velocity of the wave.

Therefore, de Broglie speaks of phase waves and concordance, or harmony, of phases. The particle itself, and hence also the energy, moves with the group velocity v. These considerations are of relativistic nature because they are based on the difference between the relativistic transformations for waves and for a moving clock, which represents the particle. De Broglie also shows that, in the case where the propagation of the wave can be described in the approximation of geometrical optics, one has to identify Fermat's principle with the principle of least action of Maupertuis, and this leads again to the same phase waves. He also expresses his belief that the new dynamics of mass points exhibits a relationship to the classical one (including relativity) similar to the relationship existing between wave optics and geometrical optics.

NIELS BOHR LIBRARY

Clinton Davisson and Lester Germer in 1927. The accidental breakage of a tube similar to the one Davisson is holding led to experimental confirmation of de Broglie's theory.

As early as the *Comptes Rendus* article of 10 September, de Broglie applied his electron-wave hypothesis to electron orbits in an atom, requiring that the wave be in phase with itself, that the circumference be an integral multiple of the wavelength. In the summary concluding his thesis he comments, "We believe that this is the first physically plausible explanation for the Bohr–Sommerfeld stability rules."

It is noteworthy that the famous formula $\lambda = h/mv$ is found in this explicit form only once in de Broglie's thesis, namely in the chapter on statistical mechanics, where he calculates the momentum of molecules in an enclosure forming standing waves. For de Broglie, it is not the wavelength of the particle that is in the foreground, but its frequency. There is no essential difference between photons and particles. In order to avoid difficulties in his theory, de Broglie at first assumes that light quanta, or atoms of light, "have an extremely small mass (not infinitely small in the mathematical sense) .... It seems that m_0 [the rest mass of the quanta] should be at most of the order of 10^{-50} gr."

Thus, the titles of all but one of his early papers dealing with matter waves make no explicit mention of the concept of matter waves. The first two *Comptes Rendus* articles have the headings "Ondes et quanta," "Quanta de lumière, diffraction et interférences"; the thesis and the article in *Annales de Physique* are entitled "Recherches sur la théorie des quanta"; the note in *Nature* is called "Waves and quanta," and the article in the *Philosophical Magazine*, "A tentative theory of light quanta." Only the title of the third *Comptes Rendus* paper, "Les quanta, la théorie kinetique des gaz et le principe de Fermat," promises perhaps more than a discussion of light. In a recent article,[13] de Broglie has provided an illuminating summary of his early ideas and his current interpretation of matter waves.

Experimental proof

How did de Broglie feel about experimental proof of his theory? In the *Comptes Rendus* note of 24 September 1923 he writes, "A beam of electrons passing through a very small opening could present diffraction phenomena. This is perhaps the direction in which one may search for an experimental confirmation of our ideas." The letter to *Nature*, written at the same time, likewise implies this possibility. However, the article in the *Philosophical Magazine* of 1924 contains nothing of that sort. Even the thesis, although much longer and more detailed, is silent about potential experiments. De Broglie said in an interview with Fritz Kubli[14] in November 1968 that his brother Maurice had suggested that the thesis should also include an experimental part. Louis declined, saying that he was not an experimentalist. Maurice de Broglie's attitude appears to indicate that he considered an experimental proof of the existence of matter waves within the realm of possibility.

Answering a questionnaire[15] prior to the interview of 1963, de Broglie wrote that during the doctoral examination in November 1924 at the Sorbonne Jean Perrin, the chairman of the examination committee, asked him how one could experimentally observe these matter waves. By that time de Broglie had a much more practical idea than in 1923 and suggested trying diffraction experiments on crystals with electrons. On 17 September 1973 he wrote to me: "At the time I wrote my thesis, I was working on x rays in the laboratory of my brother Maurice, and I am certain to have suggested to Mr Dauvillier [who later became a well known astrophysicist] that he undertake experiments to obtain diffraction or interference phenomena with electrons. But Dauvillier, held back by other work, did not follow my advice." In a short article, "Hommage à Louis de Broglie," also in the 60th-anniversary volume, Dauvillier writes, "The first experiments undertaken for the verification of these properties of the electron were negative. The cathode rays [electron beams] used in this experiment were too soft, and the mica crystal picked up parasitic charges in the high [*trop bon*] vacuum." Dauvillier does not give the name of the experimenter. However, in a letter to me, dated 26 September 1973, Dauvillier writes, "Indeed, it was myself who undertook the first [unsuccessful] experiments to verify the wave character of the electron." Thus, 50 years later, it appears that Dauvillier did not devote much effort to this investigation. He had little faith in the reality of these waves. In a letter of 16 November 1973 Dauvillier assessed the situation of that time by pointing out that "it is indicative that neither M[aurice] de Broglie, nor P. Langevin, nor J. Perrin saw to it that [such experiments] were carried out in their laboratories! Nobody believed in it . . ."

Dauvillier certainly must have been aware that a positive result might have earned him the Nobel Prize. In his 17 September letter, de Broglie said that he did not know of any unsuccessful attempt in this domain before the experiments of Clinton Davisson and Lester Germer in America.

De Broglie's examination committee, composed of Perrin, Paul Langevin as thesis advisor, Elie J. Cartan and Charles Mauguin, was impressed by the candidate's work. First-hand knowledge of the opinion of one of its members is available. In 1952, Mauguin, who was a crystallographer, recalled de Broglie's examination: "Today I have difficulty understanding my state of mind [in 1924] when I accepted the explanation of the facts [the Bohr-Sommerfeld quantization rules] without believing in the physical reality of the entities that provided this explanation."[16] Today, interestingly enough, we have no such inhibitions about accepting the quark model, for example, even though no one has ever found a quark! Of the other members of the committee, Langevin was sufficiently enthusiastic to send a copy of the manuscript (before the exam) to his old friend Einstein.

Sir George Thomson, who, independently of Davisson's and Germer's work, verified the wave-particle duality of electrons. Thomson and Davisson shared the Nobel Prize in 1937.

Einstein's intercession

De Broglie's theoretical discovery did not become widely known, nor did it win immediate credence, for several reasons. Although the *Comptes Rendus* were widely circulated in Europe, they were not intensively read, and thus de Broglie's articles had little impact. Varadarja Vendakata Raman and Paul Forman[17] have pointed out that de Broglie's somewhat controversial reputation among atomic physicists in those days also played a role. He had been involved in a number of disputes with the schools of Niels Bohr and of Arnold Sommerfeld concerning the interpretation of the correspondence principle, the role of the quantum numbers, the number of energy levels, the priority in the discovery of element 72, hafnium, and the application of the quantum conditions. Even his thesis might not have made much of an impression on the community of physicists, had it not been for Langevin, who saw to it that Einstein learned about it.

De Broglie's ideas found a sympathetic reception with Einstein, as he himself had gone through an extended struggle to convince his colleagues of the wave-particle duality of photons. Einstein had a liking for symmetry arguments in physics, and de Broglie's theory established such symmetry between photons and material particles. (I refer here to Martin J. Klein's extensive article on Einstein and the wave-particle duality.)[18] Einstein enthusiastically gave Langevin his judgment, writing that de Broglie had "lifted a corner of the great veil,"[19] and he in turn alerted others to the importance of this far-reaching work.

De Broglie's thesis reached Einstein just as he was working on the theory of the monatomic gas, applying the new Bose statistics. He immediately incorporated some of de Broglie's ideas. Thus in December 1924—the month after de Broglie's doctoral exam—Einstein completed his paper,[20] which was published in the *Proceedings of the Prussian Academy* in February 1925. Calculating the fluctuations of an ideal gas, he arrives at a fluctuation law that is completely analogous to the Planck radiation law. There are two terms: The first one describes the situation of completely independent molecules; the second one corresponds to interference fluctuations in the case of radiation. "One can interpret it in a corresponding way also in a gas, by attributing to the gas in some appropriate way a radiation and by calculating the interference fluctuations." Einstein goes on to show how "in a very noteworthy paper" de Broglie attributes a wave field to a system of material particles. The corresponding reference is to de Broglie's thesis, not to his *Comptes Rendus* articles or the article in the *Annales de Physique*, which had not yet appeared. The footnote provides additional emphasis by mentioning that this thesis also contains "a very remarkable geometrical interpretation of the Bohr-Sommerfeld quantization rule."

Somewhat later in this article Einstein writes, "It seems that each motion [of a particle] is associated with an undulatory field ... This field—whose physical nature presently lies still in the dark—must in principle be observable ..." He then mentions the diffraction of molecules as they pass through small openings, but he concludes that at thermal velocities the holes, in general, would have to be smaller than the molecules, rendering this type of diffraction experiment impractical.

The effect on Schrödinger

The remarks in Einstein's important paper, in which he presented the new Bose-Einstein statistics, influenced deeply at least two other physicists: Erwin Schrödinger in Zurich and Walter Elsasser in Göttingen. Like Einstein, Schrödinger had been prodded by Langevin to read de Broglie's thesis. In a footnote in his paper about the relationship between wave mechanics and matrix mechanics[21] he acknowledges that his theory was inspired by de Broglie's thesis and "by short but truly visionary [*unendlich weitblickende*] remarks of A. Einstein in the *Proceedings of the Prussian Academy*." Schrödinger expresses similar feelings in a letter[22] to Ein-

The de Broglie family

The French noble family de Broglie is of Piedmontese origin. In 1740 the King of France made a member of the family a duke (*duc*), a hereditary title belonging only to the head of the family. Louis XV sent the son of the first duke to the aid of the Austrians during the Seven Years War. In reward for his success in battle, the emperor named this son a prince (*Reichsfürst*) of the Holy Roman Empire of the German Nation. According to German custom the princely title (*Prinz*) is carried by all members of the family. In 1960, with the death of his older brother, the duke Maurice de Broglie, Louis de Broglie became duke. Thus, Louis de Broglie is concurrently a French duke and a German prince. The family pronounces its name "broïe," rhyming approximately with "Troy."

stein, dated 23 April 1926. He states that "certainly the whole [wave mechanics] would not have been created and perhaps never (I mean, by myself) if your second paper on the gas degeneracy had not pushed my nose onto the importance of de Broglie's ideas." And in an article, "On Einstein's Gas Theory," submitted in December 1925 to the *Physikalische Zeitschrift,*[23] Schrödinger quotes de Broglie's paper in the *Annales de Physique* and also the identical thesis, which in itself would make it probable that Schrödinger had read the thesis before the article appeared in the journal. Langevin, Einstein and Schrödinger, as well as de Broglie—who habitually referred to his thesis, and not to the identical journal article—are therefore all responsible for the custom of referring to the thesis, rather than to the article in the *Annales de Physique.* Probably only a few physicists today are aware that the famous thesis is also printed in an easily accessible journal.

Schrödinger's article on the gas theory, written shortly before his well known series of papers on wave mechanics, shows clearly how he felt about de Broglie's ideas. In the introduction he writes, "This means nothing else but to take seriously the de Broglie–Einstein undulation theory of moving corpuscles." This work elaborates on Einstein's and de Broglie's ideas and, incidentally, postulates that a molecule in an enclosure of volume V cannot be at rest because the wavelength associated with that state would have to be infinite, whereas the lowest state must be one with a wavelength of order $V^{1/3}$. This is the first place where the zero-point energy is discussed in quantum mechanics.

Thus, wave mechanics had its first serious application in statistical mechanics, before it made its grand entry into atomic physics. To some degree this development parallels that of the old quantum theory, which had its beginnings in the blackbody radiation or the photon gas. Only much later, after it was also used in solid-state physics, did it influence atomic physics.

In his first wave-mechanics paper[24] Schrödinger makes it clear that he "owes the inspiration for these considerations primarily to the ingenious [*geistvoll*] thesis of Mr Louis de Broglie." But Peter Debye must also get some credit for Schrödinger's start in wave mechanics. The physics colloquium in Zurich, which was held jointly by the university and the Swiss Federal Institute of Technology, where Debye was teaching, was usually run like a journal club, the local people reporting on papers from other places. Debye asked Schrödinger to report on de Broglie's work. That Schrödinger himself gave the colloquium talk, and not as usual one of the assistants, can be attributed to the fact that Schrödinger was not only well versed in statistical mechanics, but a few years earlier had also worked on the hydrogen atom.[25] There he had obtained results that bore a remote resemblance to de Broglie's ideas about this atom. "The preparation of that [colloquium] really got him started," said Debye in an interview in 1964.[26]

Elsasser's contribution

Einstein's article furthermore induced Walter Elsasser, a 21-year-old student studying physics with James Franck in Göttingen, to go to the university library to borrow de Broglie's thesis because he wanted to learn more about the background of Bose statistics.[27] Elsasser and others in Göttingen were familiar with Davisson's and Charles Kunsman's[28] work at the laboratory later to become the Bell Telephone Laboratories. These two researchers had studied the scattering of slow electrons from metal surfaces. Observing maxima and minima as a function of scattering angle, they discovered a relation between the angle of the maximum and the energy of the electrons. Davisson and Kunsman interpreted this to mean that slow electrons, when deflected from different shells of the atoms, would be deflected in different ways. The electrons could therefore serve to probe the interior of atoms. In addition, Elsasser was also well aware of Carl Ramsauer's[29] puzzling, and at that time quite novel, observations of the transparency of atoms of noble gases to electrons of certain low energies. On reading de Broglie's thesis, it occurred to Elsasser that the two experiments might be an indication of the wave nature of the electrons. In other words, they were diffraction phenomena.

After some weeks of work and discussions with Franck he submitted a short article to *Naturwissenschaften*[30] in which he pointed out that Ramsauer's and also Davisson's and Kunsman's strange results could be explained by taking the idea of matter waves seriously. The wavelength was just of the right magnitude to produce such effects. He compared the Ramsauer effect with the effect of a colloidal solution on the transmission of light waves, where one can observe selective transmission for certain wavelengths. In the case of Davisson's and Kunsman's reflection experiments, Elsasser calculated the approximate angle of reflection by assuming the depth of penetration of the electrons to be very small, so that a two-dimensional grating could be assumed, with a grating constant equal to that of a platinum lattice. Since a polycrystal was used in the experiment, the discrepancy of about a factor of two between experiment and de Broglie's theory would not be crucial. Elsasser concluded the article by saying it would be necessary "to wait for further experiments . . . in preparation here [in Göttingen]." He finally gave Franck special thanks for various hints.

Elsasser described the following incident to me. Some time after his note had appeared in *Naturwissenschaften,* James Franck told Elsasser that it had not gained immediate acceptance. The editor of the journal, Arnold Berliner, sent it for review to Peter Pringsheim, a well-known physicist and, moreover, a personal friend of Franck. Pringsheim felt sufficiently in doubt that he had the manuscript forwarded to Einstein, who according to Elsasser's report, wrote about as follows: "I did not take my theoretical exercises quite *that* literally when I made the calculations about the Bose gas theory; but I felt that Elsasser's paper must by all means be published." Here Einstein seems to back off slightly from what he had written in his own paper, where he had discussed in detail experimental aspects of matter waves. But at a later time, on occasion of a visit from Elsasser, Einstein said to him, "Young man, you are sitting on a gold mine!"[31]

Elsasser's *Naturwissenschaften* article reveals clearly Einstein's enormous influence on other physicists in those days. Elsasser gives him nearly as much credit for the matter waves as he gives de Broglie. The introductory paragraph reads, "Some time ago Einstein arrived at a very strange and remarkable result via statistics. For he makes the hypothesis probable that one has to attribute to every translation of a corpuscle a wave field that determines the kinematics of a particle. The hypothesis of such waves, which even before Einstein was put forward by de Broglie, is supported by Einstein's theory to such a degree that it seems appropriate to look for experimental tests." On 27 October 1973 de Broglie wrote me that he certainly must have known the work of Ramsauer and Elsasser in 1924–25, but that he did not consider it to be clear-cut confirmation [*confirmations bien nettes*] of his ideas.

Elsasser tried to set up the experiment he had promised in his note. He also asked Franck if he could provide the help of a more experienced person. Franck, however, declined this request, saying that although the experiment was very worthwhile, he could not scatter the efforts of his institute into too many directions. After a few months Elsasser gave up on the experiment and switched to theoretical physics, working from then on under Born on a different topic.[32]

Born's conflicting account

Max Born has provided, at different times and places, a somewhat conflicting version of the history of Elsasser's paper.[33] In an interview with Friedrich Hund, one of Born's former students, and the physics historian Thomas S. Kuhn, in 1962,[34] Born explained that Einstein had written him a short letter, now lost, urging him to read de Broglie's thesis. Born then wrote to de Broglie and received a copy with his dedication. At about that time, Born received a letter from Davisson with graphs of his electron-scattering results. He discussed these with his experimental colleague Franck, and the two concluded that these might be interference patterns for matter waves. After having made a rough order-of-magnitude calculation Born then suggested to Elsasser, who up to that time was an experimental physics student of Franck, that he work out this concept. A letter[35] exists, from Born to Einstein, dated only three days before Elsasser sent his article to the editor of *Naturwissenschaften*. The tenor of this letter seems to be somewhat more in line with Elsasser's account. Born says that, during a visit, Ehrenfest elucidated Einstein's work on gas degeneracy and Bose statistics and that Born subsequently read the work of de Broglie. There is no mention in this letter of Einstein's having urged him to do so. Born expresses the belief that the wave theory of matter will come to be of very great importance. He then discusses Elsasser's work, remarking that the "considerations of our Mr Elsasser are not yet in proper order ... but I think that the essential point of his remark, particularly that about the reflection of electrons, can be salvaged." This letter does not give the impression that Elsasser was under Born's direct supervision, nor that Born was responsible even for suggesting the idea to him. Rather, it reflects Born's somewhat indirect knowledge gained from discussions with Franck. This seems particularly evident, considering that Elsasser's manuscript was practically finished and that he was to send it away only three days later. In a commentary following his letter to Einstein, Born gives essentially the same account as in the interview. Commentary and interview, however, both date from the 1960's.

Disregarding all the accounts that Born and Elsasser gave 30 or so years after the fact, and taking into account only Elsasser's paper and Born's letter to Einstein, the following points are noteworthy: Elsasser does not mention Born in his paper. The acknowledgment to Franck is a very standard one; there are no thanks for suggesting the topic of the investigation. Elsasser's address is the II. Physikalische Institut, which was Franck's laboratory, not the Institut für Theoretische Physik. There is no reference to a private communication from Davisson to Born. Further experiments, presumably by the author, are promised. Finally, Born's letter to Einstein does not give the impression that Elsasser's paper is almost ready to be published.

It would be highly illuminating to find this letter from Davisson to Born, or at least to know its date. Born certainly was interested in the work of the Americans, as his student Friedrich Hund had been working for his PhD thesis on the scattering of slow electrons by atoms. It is well known that the experiments of Davisson and his new coworker, Germer, took on a new aspect "as a result of an accident which occurred in this laboratory in April 1925 ... A liquid-air bottle exploded at a time when the target was at a high temperature; the experimental tube was broken, and the target heavily oxidized by the inrushing air. The oxide was eventually reduced and a layer of the target removed ... after prolonged heating at various high temperatures. When the experiments were continued it was found that the distribution-in-angle of the scattered electrons had been completely changed."[36] This change was traced to a recrystallization of the target into a few relatively large single crystals during the heating process. The two physicists' first tentative interpretation was that the electrons were channeled between the lattice planes, but the observations did not agree with this assumption because the angles at which the maxima were observed changed with the velocity of the electrons.

It may have been at this time that Davisson wrote to Born. However, in Davisson's account recorded in early 1937 by Karl K. Darrow,[37] there is no mention of such a letter, but he does report that during a scientific meeting

NIELS BOHR LIBRARY

At the 1927 Solvay Conference in Brussels several physicists who played roles of varying importance in the discovery of matter waves were present. They are, from left: back row, Ehrenfest and Schrodinger (3rd and 6th); center row: Debye, L. de Broglie and Born (1st, 7th and 8th); and front row Planck, Einstein, and Langevin (2nd, 5th and 6th).

at Oxford in August 1926, he showed some curves to Born and other physicists, among them possibly also Franck. These discussions finally brought Davisson onto the right track. At this time, Schrödinger's work had just appeared, and the idea of matter waves no longer seemed so farfetched.

Elsasser's paper, which was published in the 14 August 1925 issue of *Naturwissenschaften*, and which would therefore have reached the laboratory where Davisson worked sometime in September or October, had no effect on Davisson. His preliminary note of only two pages in *Nature*, April 1927,[38] points out that "these results are highly suggestive, of course, of the ideas underlying the theory of wave mechanics." He makes no mention of Elsasser, who had not sent him a reprint of his article. However, in the extensive *Physical Review* article published in December of the same year, Davisson comments, "That evidence for the wave nature of particle mechanics would be found in the reaction between a beam of electrons and a single crystal was predicted by Elsasser two years ago ... Elsasser believed, in fact, that evidence of this sort was already at hand in curves, published [in 1923] from these laboratories ... We should like to agree with Elsasser in his interpretation of these curves, but are unable to do so ... The maxima in the scattering curves ..., we believe, are unrelated to crystal structure." According to the account Davisson gave to Darrow, he did not think much of Elsasser's theory, and it had no influence on the course of his experiments. What was crucial was the accident with the polycrystalline mass. It is interesting to note that this event preceded Elsasser's paper by a few months.

Thomson's diffraction experiments

The electron-diffraction experiments for which George P. Thomson won the Nobel Prize in 1937 were also inspired by de Broglie's work. Thomson, however, did not have to depend on the intercession of a scientific missionary. He has told his part in the story of matter waves in papers published in 1961 and 1968.[39] Thomson, while at the University of Aberdeen, read de Broglie's article of 1924, "A tentative theory of light quanta," in the *Philosophical Magazine*, and by 1925 he had already used these ideas in a short theoretical paper[40] on the hydrogen atom. Like Davisson, he had attended the meeting at Oxford in 1926, where there was some talk about de Broglie's theory, and he began to think about diffraction effects. At this time, Thomson was not aware of Elsasser's paper. Shortly thereafter, on a visit to Cambridge, he saw data of scattering experiments on helium gas, which could be interpreted as diffraction effects. (It later turned out that the experiments were faulty.) In any case, this gave him an added incentive to attempt electron diffraction on solids. He could quickly adapt equipment that had been used before for the scattering of positive ions.

ARCHIVES DE L'ACADEMIE DES SCIENCES DE PARIS

Louis (center) and Maurice de Broglie (on his right) at the latter's home around 1925. Alexandre Dauvillier, who worked in Maurice's x-ray laboratory, sits to the left of Louis. Also present are Jean Thibaud (far left) and Jean Jacques Trillat, prominent French physicists.

In contrast to Davisson and Germer, Thomson decided to do his experiments in transmission. This permitted him to use electrons of considerably higher energies, between 3.9 and 16.5 keV, which are easier to handle. The first films investigated were celluloid, approximately 3×10^{-6} cm thick, and very soon he and Alexander F. Reid observed diffraction rings. The first note by the two appeared in *Nature*[41] in June 1927, a few months after Davisson's and Germer's letter to the editor. Later, they investigated gold, aluminum and platinum, and they published a longer paper in the *Proceedings of the Royal Society*[42] in February 1928, a few months after Davisson's and Germer's *Physical Review* article. As is well known, Davisson and Thomson shared the 1937 Nobel Prize in physics.

The faulty electron-scattering experiments on helium gas mentioned above, which prompted Thomson to undertake his own investigations on solids, had been performed by the British physicist E. G. Dymond while on a fellowship at Princeton University. On 18 June 1926 Dymond sent a letter to *Nature*.[43] This letter conveys the impression that Elsasser's paper had a distinct influence on him, at least on the interpretation of his findings; in addition to Einstein's note, Ramsauer's and Davisson's and Kunsman's experiments, as well as de Broglie's theory, are quoted, thus reviewing the essence of the young German's work. Dymond goes on to conclude that "the occurrence of these maxima [at certain scattering angles] is strongly suggestive of an interference pattern, as suggested by Elsasser ..." Schrödinger's first paper on wave mechanics, which may have reached Princeton at just about that time, is not mentioned. However, in the more extensive article that Dymond submitted in November 1926 to the *Physical Review*,[44] he does quote Schrödinger's paper. Although Dymond's results were quoted several times during the Solvay conference in 1927,[45] they were nevertheless erroneous and he later had to retract them. Dymond's attitude toward Elsasser's work during the same period was clearly very different from that of Davisson, who did not consider Elsasser's ideas at all relevant to his own work. It is also interesting to note that Dymond was not alone in his failure to detect matter waves experimentally. Of the three earliest experimental attempts—by Dauvillier, Elsasser and Dymond—none succeeded.

Later milestones

Later milestones in the diffraction of matter waves followed. Immanuel Estermann and Otto Stern successfully investigated the diffraction of atoms and molecules in 1929.[46] Directing H_2 and He beams on cleaved surfaces of LiF crystals, they found quantitative agreement with de Broglie's formula. These experiments had been preceded by those of F. Knauer and Stern,[47] which had turned out only marginally conclusive, and by Stern[48] alone.

In 1936, four years after the discovery of the neutron, Walter Elsasser, who had emigrated to France, suggested[49] that neutrons could be diffracted from crystals, and he estimated the expected intensity of the Bragg-diffracted neutron beam. He also referred to experiments being conducted at the laboratory of Frédéric Joliot by Hans von Halban and Peter Preiswerk. Scattering neutrons from a Rn–Be source, at room temperature and at 90 K, on iron, these two physicists observed a maximum and a minimum as a function of angle. Their paper in the *Comptes Rendus*[50] of 6 July 1936 had the title "Experimental proof of neutron diffraction." About a month later Dana P. Mitchell and Philip N. Powers[51] at Columbia University also looked for Bragg diffraction and produced slightly more convincing results. In the following year, Gian Carlo Wick[52] worked out a somewhat more complete theory of neutron diffraction by crystals.

Good neutron diffraction patterns could be obtained only with monochromatic neutron beams. Requiring substantially greater beam intensities, they first became available only with the advent of reactors. Soon after World War II, the first experiments in this field became known. At the Chicago meeting of the American Physical Society in June 1946, papers[53] were presented by Herbert L. Anderson, Enrico Fermi and Leona Marshall; by Walter H. Zinn; and by William J. Sturm and Soloman H. Turkel, all from the Argonne Laboratory; and by Lyle B. Borst, A. J. Ulrich, Charles L. Osborne and B. Hasbrouck from the Clinton Laboratory in Oak Ridge. This latter group, as well as Zinn,[54] constructed the first neutron-crystal spectrometers, setting the stage for the new and fertile field of neutron diffraction. But these early experiments were soon to be eclipsed somewhat by the beautiful work of Ernest O. Wollan and Clifford G. Shull.[55]

It is noteworthy that in 1952—only a couple of years after the invention of holography by Dennis Gabor—the first experiments on microholography with electrons were carried out in England by M. E. Haine and T. Mulvey.[56] The idea was to make a hologram with electron waves and to reconstruct the wave front with visible light, thus obtaining a magnified image. Because lasers had not yet been invented, this method yielded only moderate success at that time.

Finally, in 1953 Ladislaus Marton and his coworkers at the National Bureau of Standards conducted experiments with an electron interferometer. This instrument consisted of three parallel thin crystal layers by which the traversing electron waves were split and again merged. The researchers succeeded in obtaining a great number of interference fringes, corresponding to path differences up to 276 Å, equal to 5800 electron wavelengths. This investigation set a lower limit for the length of an electron wave packet.

Thus, in the span of 30 years the speculative ideas of Louis de Broglie served not only as the starting point of quantum mechanical theory, but they also opened new experimental possibilities.

* * *

My sincere thanks are due to Louis de Broglie, who most graciously and with great care responded to several questions, to Walter M. Elsasser, who in conversations and letters recalled events of the past, and to Alexandre Dauvillier for his clarification of the first attempt to detect these strange waves. I am also most grateful for suggestions and criticisms from my colleagues Herta R. Leng, Robert Resnick and Paul F. Yergin. Finally, I wish to thank the library of the American Philosophical Society for making available to me unpublished material.

References

1. M. Jammer, *The Conceptual Development of Quantum Mechanics*, McGraw Hill, New York (1966).
2. F. Hund, *Geschichte der Quantentheorie*, Bibliographisches Institut, Mannheim (1967).
3. J. Gerber, Arch. Hist. Exact Sciences **5**, 349 (1969).
4. L. de Broglie, Journal de Physique, Series VI, **3**, 422 (1922).
5. L. de Broglie, Comptes Rendus **175**, 811 (1922).
6. Archive for the History of Quantum Physics.
7. L. de Broglie, Comptes Rendus **177**, 507, 548, 630 (1923).
8. L. de Broglie, Nature **112**, 540 (1923).
9. L. de Broglie, letter of 17 September 1973 to the author.
10. L. de Broglie, Phil. Mag. **47**, 446 (1924).
11. L. de Broglie, *Thèse de doctorat*, Masson, Paris (1924); reprinted 1963; also Annales de Physique **3**, 22 (1925).
12. *Louis de Broglie—Physicien et Penseur* (A. George, ed.), Albin Michel, Paris (1953).
13. L. de Broglie, Comptes Rendus B **277**, 71 (1973).
14. R. Kubli, Arch. Hist. Exact Sciences **7**, 26 (1970).
15. Reference 6.
16. Reference 12.
17. V. V. Raman, P. Forman, in *Historical Studies in the Physical Sciences* **1** (R. McCormmach, ed.), Univ. of Philadelphia Press (1969), page 291.
18. M. J. Klein, "Einstein and the Wave–Particle Duality," in *The Natural Philosopher* (D. E. Gershenson and D. A. Greenberg, eds.) Xerox College Publishing (1964); reprinted in The Bobbs-Merrill Reprint Series in the History of Science.
19. L. de Broglie, *New Perspectives in Physics*, Oliver and Boyd, Edinburgh (1962), page 139; trans. from *Nouvelles perspectives en microphysique*, Paris (1955).
20. A. Einstein, Sitzungsber. Preuss. Akad. Wiss., Mathem.-Naturwiss. Kl. **23**, 3 (1925).
21. E. Schrödinger, Annalen der Physik **79**, 734 (1926).
22. E. Schrödinger, in *Briefe zur Wellenmechanik* (K. Przimbram, ed.), Springer, Vienna (1963), page 24.
23. E. Schrödinger, Physikal. Zeitschr. **27**, 95 (1926).
24. E. Schrödinger, Annalen der Physik **79**, 372 (1926).
25. E. Schrödinger, Zeits. Physik **12**, 13 (1922).
26. P. J. W. Debye, D. R. Corson, E. E. Salpeter, S. H. Bauer, Science **145**, 554 (1964).
27. W. Elsasser, letter of 21 August 1973 to the author.
28. C. Davisson, C. H. Kunsman, Science **54**, 522 (1921); Phys. Rev. **22**, 242 (1923).
29. C. W. Ramsauer, Annalen der Physik **72**, 345 (1923).
30. W. Elsasser, Naturwiss. **13**, 711 (1925).
31. Reference 27.
32. Reference 27.
33. M. Born in *Albert Einstein—Philosopher and Scientist* (P. A. Schilpp, ed.) 1, Harper, New York (1959), page 161; *Nobel Lectures in Physics: 1942–1970*, Elsevier, Amsterdam (1964), page 256; reference 12, page 165; reference 6; reference 35.
34. Reference 11.
35. *Albert Einstein–Max Born Briefwechsel: 1916–1955*, Nymphenburger, Munich (1969), page 119; *The Born–Einstein Letters*, Walker, New York (1971), page 83.
36. C. Davisson, L. H. Germer, Phys. Rev. **30**, 705 (1927).
37. K. K. Darrow, Bell System Technical Journal **30**, 786 (1951).
38. C. Davisson, L. H. Germer, Nature **119**, 558 (1927).
39. G. P. Thomson, Am. J. Phys. **29**, 821 (1961); Contemp. Phys. **9**, 1 (1968).
40. G. P. Thomson, Phil. Mag. **50**, 163 (1925).
41. G. P. Thomson, A. Reid, Nature **119**, 890 (1927).
42. G. P. Thomson, A. Reid, Proc. Roy. Soc. (London) A, **117**, 601 (1928).
43. E. G. Dymond, Nature **118**, 336 (1926).
44. E. G. Dymond, Phys. Rev. **29**, 433 (1927).
45. *Electrons et Photons, rapports et discussions du cinquième conseil de physique*, Gauthier-Villars, Paris (1928).
46. I. Estermann, O. Stern, Zeits. Physik **61**, 95 (1930).
47. F. Knauer, O. Stern, Zeits. Physik **53**, 779 (1929).
48. O. Stern, Naturwiss. **17**, 391 (1929).
49. W. Elsasser, Comptes Rendus **202**, 1029 (1936).
50. H. von Halban, P. Preiswerk, Comptes Rendus **203**, 73 (1936).
51. D. P. Mitchell, P. N. Powers, Phys. Rev. **50**, 486 (1936).
52. G. C. Wick, Physikal. Zeits. **38**, 403, 689 (1937).
53. See Phys. Rev. **70**, 99 (1946).
54. W. H. Zinn, Phys. Rev. **71**, 752 (1947).
55. E. O. Wollan, C. G. Shull, Phys. Rev. **73**, 830 (1948).
56. M. E. Haine, T. Mulvey, J. Opt. Soc. Am. **42**, 763 (1952).
57. L. Marton, J. A. Simpson, J. A. Suddeth, Phys. Rev. **90**, 490 (1953); Rev. Sc. Instr. **25**, 1099 (1954); L. Marton, Science **118**, 470 (1953). □