Electron diffraction: fifty years ago

A look back at the experiment that established the wave nature of the electron, at the events that led up to the discovery, and at the principal investigators, Clinton Davisson and Lester Germer.

Richard K. Gehrenbeck

An article that appeared in the December 1927 issue of *Physical Review*, "Diffraction of Electrons by a Crystal of Nickel," has been referred to in countless articles, monographs and textbooks as having established the wave nature of the electron—in principle, of all matter.[1] Now, fifty years later, it is fitting to look back at the events that led up to this historical discovery and at the discoverers, Clinton Davisson and Lester Germer. Figure 1 shows them in their lab in 1927, together with their assistant Chester Calbick.

A shy midwesterner

Clinton Joseph Davisson, the senior investigator, was born in Bloomington, Illinois, on 22 October 1881, the first of two children. His father, Joseph, who had settled in Bloomington after serving in the Civil War, was a contract painter and paperhanger by trade. His mother, Mary, occasionally taught in the Bloomington school system. Their home was, as Davisson's sister, Carrie, characterized it, "a happy congenial one—plenty of love but short on money."

Davisson, slight of frame and frail throughout his life, graduated from high school at age 20. For his proficiency in mathematics and physics he received a one-year scholarship to the University of Chicago; his six-year career there was interrupted several times for lack of funds. He acquired his love and respect for physics from Robert Millikan; Davisson was "delighted to find that physics was the concise, orderly science [he] had imagined it to be, and that a physicist [Millikan] could be so openly and earnestly concerned about such matters as colliding bodies."

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Before finishing his undergraduate degree at Chicago, he became a part-time instructor in physics at Princeton University, where he came under the influence of the British physicist Owen Richardson, who was directing electronic research there. Davisson's PhD thesis at Princeton, in 1911, extended Richardson's research on the positive ions emitted from salts of alkaline metals. Davisson later credited his own success to having caught "the physicist's point of view—his habit of mind—his way of looking at things" from such men as Millikan and Richardson.

After completing his degree, Davisson married Richardson's sister, Charlotte, who had come from England to visit her brother. After a honeymoon in Maine Davisson joined the Carnegie Institute of Technology in Pittsburgh as an instructor in physics. The 18-hour-per-week teaching load left little time for research, and in six years there he published only three short theoretical notes. One notable break during this period was the summer of 1913, when Davisson worked with J. J. Thomson at the Cavendish laboratory in England.

In 1917, after he was refused enlistment in the military service because of his frailty, Davisson obtained a leave of absence from Carnegie Tech to do war-related research at the Western Electric Company, the manufacturing arm of the American Telephone and Telegraph Company, in New York City. His work was to develop and test oxide-coated nickel filaments to serve as substitutes for the oxide-coated platinum filaments then in use. At the end of World War I he turned down an offered promotion at Carnegie Tech to accept a permanent position at Western Electric. It was at this time that he began the sequence of investigations that ultimately led to the discovery of electron diffraction; it was also at this time that he was joined by a young colleague, Lester Halbert Germer, just discharged from active service.

An adventurous New Yorker

Germer was born on 10 October 1896, the first of two children of Hermann Gustav and Marcia Halbert Germer, in Chicago, where Dr Germer was practicing medicine. In 1898 the family moved to Canastota in upper New York state, the childhood home of Mrs Germer. Germer's father became a prominent citizen in the little town on the Erie canal, serving as mayor, president of the board of education and elder in the Presbyterian church.

Germer attended school in Canastota and won a four-year scholarship to Cornell University, graduating from there in the spring of 1917, six weeks early because of the outbreak of the war. The local newspaper, after applauding 18-year-old Lester for working as a laborer for the local paving contractors during his summer vacation, proceeded to ridicule his lazier contemporaries for sitting "day after day in the lounging places of the village," saying there is "nothin' doin'" and that "a young feller has no chanst in this durn town." (Lester, must have taken a bit of ribbing from the "idle boys" after this appeared!) Germer's studies at Cornell were partly self-directed; in their junior year he and two classmates, finding themselves "unsatisfied with the course in electricity and magnetism given ... bought a more advanced text and met regularly in the vacant class room ... and really learned something."

Upon graduation from Cornell, Germer obtained a research position at Western Electric, which he held for about two months before volunteering for the Army (aviation section of the signal corps). He

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apparently made no contact with Davisson then. Lieutenant Germer, among those piloting the first group of airplanes on the Western Front, was officially credited with having brought down four German warplanes. Discharged on 5 February 1919, Germer was treated in New York City for severe headache, nervousness, restlessness and loss of sleep, conditions attributed to his military campaigns, but he refused to file for compensation because "others were worse off." After three weeks of rest, he was re-hired by Western Electric—and had as his first assignment the preparation of an annotated bibliography for a new project being directed by his new supervisor, Davisson.

That fall Germer married his Cornell sweetheart, Ruth Woodard of Glens Falls, New York.

Electron emission—in court

The assignment that engaged Davisson and Germer in their first joint effort reflects one of the chief interests of the parent company, AT&T, at this time: to conduct a fundamental investigation into the role of positive-ion bombardment in electron emission from oxide-coated cathodes. Although Germer later remembered this project as having been directly related to the famous Arnold–Langmuir patent suit, that occupied Western Electric (Harold Arnold) and General Electric (Irving Langmuir) from 1916 until it was finally settled[2] by the US Supreme Court (in favor of Western Electric) in 1931, a careful examination of the documents makes it clear that Davisson and Germer's project could have related to it only in a very indirect way. The patent case concerned improvements to the earliest deForest triode tubes with metallic (tungsten or tantalum) cathodes; it dealt with evidence obtained in the years 1913 to 1916, before Davisson and Germer appeared on the scene. Nevertheless, because AT&T was deeply concerned about the efficiency and effectiveness of its triode amplifiers—key components in its recently constructed transcontinental telephone lines—Arnold assigned Davisson and Germer the task of conducting tests on oxide-coated cathodes. They published their results in the *Physical Review* in 1920, concluding that positive-ion bombardment has a negligible effect on the electron emission from oxide-coated cathodes.[3]

With this problem settled, a related question came up: What is the nature of secondary electron emission from grids and plates subjected to electron bombardment? Davisson was assigned this new task and given an assistant, Charles

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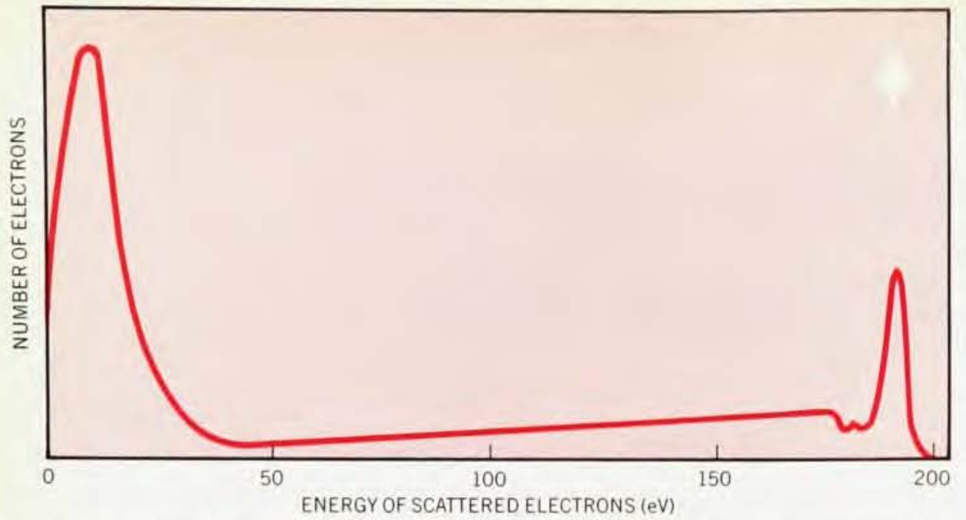
Davisson, Germer and Calbick in 1927, the year they demonstrated electron diffraction. In their New York City laboratory are Clinton Davisson, age 46; Lester Germer, age 31, and their assistant Chester Calbick, age 23. Germer, seated at the observer's desk, appears ready to read and record electron current from the galvanometer (seen beside his head); the banks of dry cells behind Davisson supplied the current for the experiments. Figure 1



Electron-scattering peak. The energy of the scattered electrons varies from almost zero to that of the incident beam (indicated by the arrow). This is a reconstruction of the type of observation that led Davisson and Charles Kunsman to conclude that some electrons were being scattered elastically. Davisson saw these as possible probes of the electronic structure of the atom, in analogy to Rutherford's use of alpha particles to explore the nucleus. Figure 2

H. Kunsman, a new PhD from the University of California. For this work they were able to convert the positive-ion apparatus to an electron-beam apparatus. Meanwhile Germer was shifted to a project on the measurement of the thermionic properties of tungsten, a topic he pursued for about four years, both under Davisson's direction and as part of a graduate program he undertook at nearby Columbia University part time.

A startlihg observation

Soon after Davisson and Kunsman began their secondary electron emission studies, they observed an unexpected phenomenon that was to have crucial importance for their future experimental program: A small percentage (about 1%) of the incident electron beam was being scattered back toward the electron gun with virtually no loss of energy—the electrons were being scattered elastically. Figure 2 reconstructs this phenomenon. Previous observers had noticed this effect for low-energy electrons (about 10 eV), but none had reported it for electrons of energies over 100 eV.

Although this discovery undoubtedly had no immediate impact on the stockholders of AT&T, it affected Davisson profoundly. To him these elastically scattered electrons appeared as ideal probes with which to examine the extranuclear structure of the atom. Ernest Rutherford announced his nuclear model of the atom in 1911, the year Davisson completed his PhD; Hans Geiger and Ernest Marsden completed their definitive experimental tests of Rutherford's theory and Niels Bohr announced his planetary model of the atom in 1913, when Davisson worked with Thomson at Cambridge. So it is not surprising that Davisson was enthusiastic about the prospect of using these electrons for basic research on the structure of the atom. In Davisson's own words,

"The mechanism of scattering, as we pictured it, was similar to that of alpha ray scattering. There was a certain probability that an incident electron would be caught in the field of the atom, turned through a large angle, and sent on its way without loss of energy. If this were the nature of electron scattering it would be possible, we thought, to deduce from a statistical study of the deflections some information in regard to the field of the deflecting atom . . . What we were attempting . . . were atomic explorations similar to those of Sir Ernest Rutherford . . . in which the probe should be an electron instead of an alpha particle."

In fact, Davisson was so enthusiastic about a full-scale assault on the atom that he was able to convince his superiors to let him and Kunsman devote a large fraction of their time to it, and to give them the necessary shop backup.

The basic piece of apparatus, built to order by a talented machinist and glassblower, Geroge Reitter, was a vacuum tube with an electron gun, a nickel target inclined at an angle of 45° to the incident electron beam and a Faraday-box collector, which could move through the entire 135° range of possible scattered electron paths; it is diagrammed in figure 3. The Faraday box was set at a voltage to accept electrons that were within 10% of the incident electron energy.

After two months of experimentation, Davisson and Kunsman submitted a two-column paper to *Science*, in which they sketched the main features of their scattering program, presented a typical curve of their data, proposed a shell model of the atom for interpreting these results, and offered a formula for the quantitative prediction of the implications of the model.[4] Unfortunately their attempts to link together their data, the model and the predictions were anything but definite—quite out of keeping with the Rutherford-Geiger-Marsden tradition.

Although Davisson (and Kunsman) must have been somewhat disappointed at the limited success of their initial venture, they pressed on with additional experiments. In the next two years they built several new tubes, tried five other metals (in addition to nickel) as targets, developed rather sophisticated experimental techniques at high vacuum ("the pressure became less than could be measured, i.e., less than 10^{-8} mm Hg,") and made valiant theoretical attempts to account for the observed scattering intensities. The results were uniformly unimpressive; several of the studies were not even published. In fact, the generally disheartened atmosphere that seems to have prevailed by the end of 1923 is indicated by the fact that Kunsman left the company and Davisson abandoned the scattering project.

A year later, however, Davisson was ready to have another try at electron scattering. Was this change of heart prompted by Davisson's strong attraction to the project? Was it his eagerness to obtain additional information about the extranuclear structure of the atom? In any case, in October 1924 Germer was put back on the scattering project in place of the departed Kunsman. Germer, who had already completed several thermionic-emission studies, was returning to Western Electric after a 15-month illness. Regarding his development as a physicist by this time, Germer later recollected:

"I learned relatively little at Columbia . . . but was nevertheless fortunate in working . . . with Dr C. J. Davisson. I learned a simply enormous amount from him. This included how to do experiments, how to think about them, how to write them up, how even to learn what other people had previously done in the field . . . I am quite certain that I do really owe to Dr Davisson much the best part of my education, and I am not really convinced that it is so inferior to that obtained in more conventional ways. It is certainly different."

A "lucky break" and a new model

So the scattering experiments were finally resumed. One can easily imagine, then, the feelings of disappointment and frustration that Davisson and Germer must have shared when, soon after the project had been restarted, they discovered a cracked trap and badly oxidized target on the afternoon of 5 February 1925, as the notebook entry in figure 4 shows. What it meant in simple terms was that the experiments with the specially polished nickel target, discontinued for almost a year, were to be delayed again. Apparently Germer's attempts to revitalize the tube after its long period of disuse by repumping and baking (outgassing) were to be for nought; an additional delay for repairs was necessary.

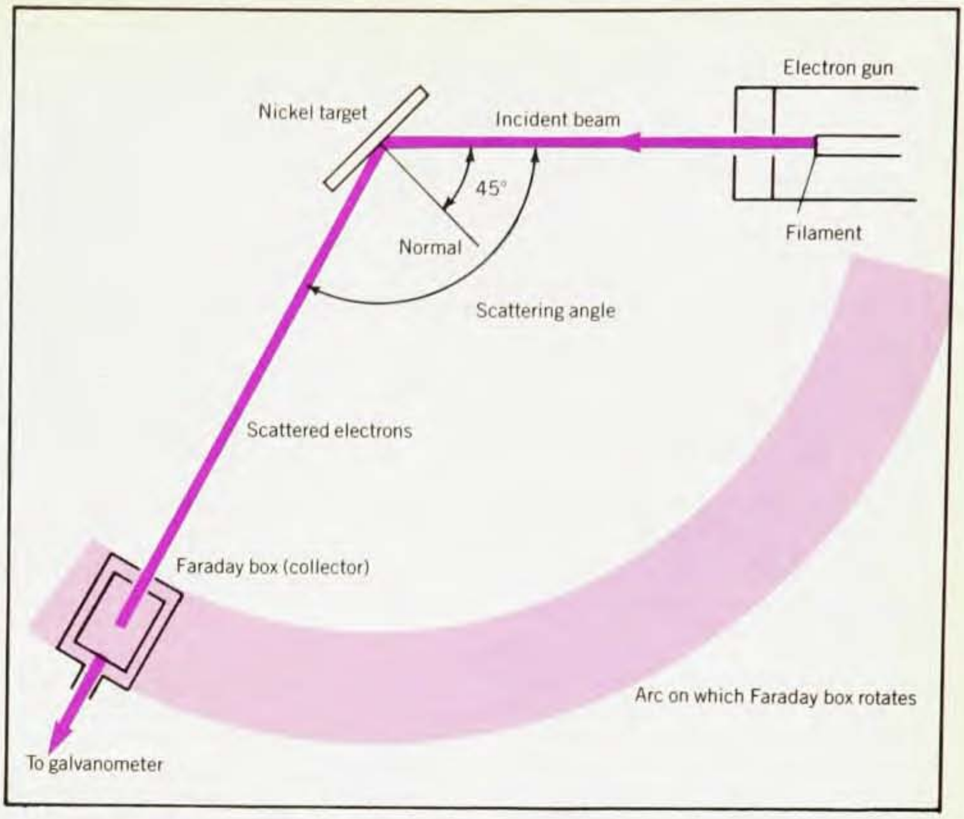
This was not the only time that a tube had broken during a scattering experiment, nor was it to be the last. Nor was the method of repair unique, for the method of reducing the oxide on the nickel target by prolonged heating in vacuum and hydrogen had been used once before (unsuccessfully; that time it had led to the formation of a "black precipitate" and "no apparent cleaning up of the nickel"). This particular break and the subsequent method of repair, however, had a crucial role to play in the later discovery of electron diffraction.

By 6 April 1925 the repairs had been completed and the tube put back into operation. During the following weeks, as the tube was run through the usual series of tests, results very similar to those obtained four years earlier were obtained. Then suddenly, in the middle of May, unprecedented results began to appear, as shown in figure 5. These so puzzled Davisson and Germer that they halted the experiments a few days later, cut open the tube, and examined the target (with the assistance of the microscopist F. F. Lucas) to see if they could detect the cause of the new observations.

What they found was this: The polycrystalline form of the nickel target had been changed by the extreme heating until it had formed about ten crystal facets in the area from which the incident electron beam was scattered. Davisson and Germer surmised that the new scattering pattern must have been caused by the new crystal arrangement of the target. In other words, they concluded that it was the arrangement of the atoms in the crystals, not the structure of the atoms, that was responsible for the new intensity pattern of the scattered electrons.

Thinking that the new scattering patterns were too complicated to yield any useful information about crystal structure, Davisson and Germer decided that a large single crystal oriented in a known direction would make a more suitable target than a collection of some ten small facets randomly arranged. Because neither Davisson nor Germer knew much about crystals, they, assisted by Richard Bozorth, spent several months examining the damaged target and various other nickel surfaces until they were thoroughly familiar with the x-ray diffraction patterns (note!) obtained from nickel crystals in various states of preparation and orientation.

By April 1926 they had obtained a suitable single crystal from the company's metallurgist, Howard Reeve, and cut, etched and mounted it in a new tube that allowed for an additional degree of freedom of measurement; the collector could now rotate in azimuth (the 360° angle circling the beam axis) as well as in colatitude. The design of the new tube reflected their expectation of finding certain "transparent directions" in the crystal along which the electrons would move with least resistance. They expected these special directions to coincide with the unoccupied lattice directions.



Scheme of the first scattering tube, which served as a prototype for the group's later models. Davisson and Germer later included mechanisms for rotating the target azimuthally 360° about the beam axis and for changing the angle of the incident beam with respect to the normal to the target. In their 1926–27 work the incident beam was perpendicular to the target face, and the scattering angle was called the "colatitude angle." Figure 3

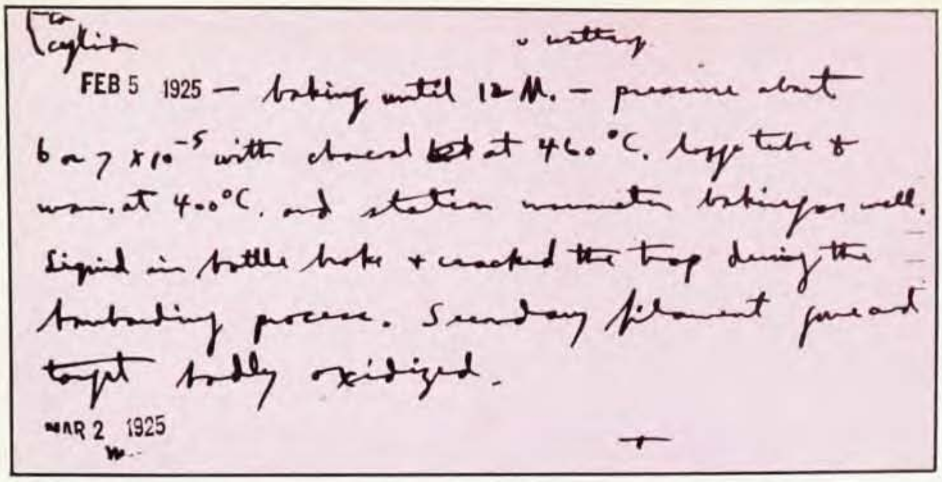
More than a "second honeymoon"

Having suffered disappointment with the results of the original scattering experiments performed with Kunsman, Davisson must have been doubly disheartened by the meager returns he and Germer obtained with the new tube. After an entire year spent in preparation, and with a new tube and a new theory in hand, they obtained experimental results that were even less interesting than those from the earliest experiments. The new colatitude curves showed essentially nothing, and even the new azimuth curves gave at best only a weak indication of the expected three-fold symmetry of the nickel crystal about the incident beam.

Davisson must have been quite pleased with the prospect of getting away for a few months during the summer of 1926, when he and his wife had planned a vacation trip to relax and visit relatives in England. Mrs Davisson recalled that this summer had been chosen for the trip because her sister, May, and brother-in-law, Oswald Veblen of Princeton University, were available to stay with the Davisson children at that time. As Davisson wrote to his wife, then at the Maine cottage making arrangements for the children: "It seems impossible that we will be in Oxford a month from today—doesn't it? We should have a lovely time—Lottie darling—It will be a second honeymoon—and should be sweeter even than the first." Something was to happen on this particular trip, however, to turn it into more than the "second honeymoon" Davisson envisioned.

Theoretical physics was undergoing fundamental changes at this time. In the early months of 1926 Erwin Schrödinger's remarkable series of papers on wave mechanics appeared, following Louis de Broglie's papers of 1923–24 and Albert Einstein's quantum-gas paper of 1925. These papers, along with the new matrix mechanics of Werner Heisenberg, Max Born and Pascual Jordan, were the subject of lively discussions at the Oxford meeting of the British Association for the Advancement of Science. Davisson, who generally kept abreast of recent developments in his field but appears to have been largely unaware of these recent developments in quantum mechanics, attended this meeting. Imagine his surprise, then, when he heard a lecture by Born in which his own and Kunsman's (platinum-target) curves of 1923 were cited as confirmatory evidence for de Broglie's electron waves![15]

After the meeting Davisson met with some of the participants, including Born and possibly P.M.S. Blackett, James



The notebook entry for 5 February 1925 records, in Germer's handwriting, the discovery of the broken tube that interrupted the scattering experiments once again. It was this break, however, which initiated a chain of events that eventually led to the preparation of a single crystal of nickel as the target, and to a shift of Davisson's interest from atomic structure to crystal structure. Reproduced by courtesy of Bell Laboratories. Figure 4

Franck and Douglas Hartree, and showed them some of the recent results that he and Germer had obtained with the single crystal. There was, according to Davisson, "much discussion of them." All this attention might seem strange in light of the relatively feeble peaks Davisson and Germer had obtained, but even these may have been exciting to physicists already convinced of the basic correctness of the new quantum theory. It may also reflect the fact that several European physicists, Walter Elsasser (Göttingen), E.G. Dymond (Cambridge, formerly Göttingen and Princeton), and Blackett, James Chadwick and Charles Ellis of Cambridge[6] had attempted similar experiments and abandoned them because of the difficulties of producing the required high vacuum and detecting the low-intensity electron beams. Apparently they were encouraged by these results, which appeared so unimpressive to Davisson. At any rate, Davisson spent "the whole of the westward transatlantic voyage ... trying to understand Schrödinger's papers, as he then had an inkling ... that the explanation might reside in them"—no doubt to the detriment of the "second honeymoon" in progress.

Back at Bell Labs (as the engineering arm of Western Electric has been called since 1925), Davisson and Germer examined several new curves that Germer had obtained during Davisson's absence. They found a discrepancy of several degrees between the observed electron intensity peaks and the angles they expected from the de Broglie–Schrödinger theory. To pursue this matter further they cut the tube open and carefully examined the target and its mounting. After finding that most of the discrepancy could be accounted for by an accidental displacement of the collector-box opening, they "laid out a program of thorough search" to pursue the quest of diffracted electron beams. In typical Davisson fashion, however, this quest was preceded by a period of careful preparation, including an important change in the experimental tube. As Davisson wrote to Richardson in November,

"I am still working at Schrödinger and others and believe that I am beginning to get some idea of what it is all about. In particular I think that I know the sort of experiment we should make with our scattering apparatus to test the theory."

Found—a "quantum bump"

It was three weeks before the "thorough search" was begun. The importance that Davisson (and Bell Labs) had come to attach to this project can be surmised from the addition to it of a new assistant, Chester Calbick, a recently graduated electrical engineer. After about a month of experimenting, during which time Calbick took charge of operating the experiment, they gave the newly prepared tube a thorough set of consistency tests. During one attempt by Germer to reactivate the tube in late November the tube broke, but with little damage. (Strangely, little damage can be considered "lucky" in this case, whereas it would have been "unlucky" in the case of the 1925 break!)

The first experiments with the new tube yielded no significant results; the colatitude and azimuth curves looked much as before, and the new experiments added by Davisson "to test the theory" were uninformative as well. These tests consisted of varying the accelerating voltage, and hence electron energy E, for fixed colatitude and azimuth settings, and were designed to see if any effect could be discerned for a changed electron wavelength λ, according to the de Broglie relationship, $\lambda = h/(2\,mE)^{1/2}$.

A concerted search for "quantum peaks" (voltage-dependent scattered electron beams) was launched by late December. These attempts revealed only "very feeble" peaks. The situation changed dramatically on 6 January 1927, however; the data for that day are accompanied by the remark, in Calbick's neat handwriting: "Attempt to show 'quantum bump' at an intermediate [colatitude] angle. Bump develops at 65 V, compared with calculated value for 'quantum bump' of V = 78 V." Then, stretched across the bottom of the page in Germer's unmistakable bold strokes, is the additional remark: "First Appearance of Electron Beam." A portion of the notebook page is reproduced in figure 6.

The data for this curve are extremely interesting. Noting from the figure that the readings were taken in one-volt intervals on either side of 79 volts, whereas the steps are 2, 5 and then 10 volts elsewhere, we see that a peak was expected at about 78 volts. But the experiment yielded a single large current at 65 volts. The experimenters took immediate notice of this spike, making a second run in one-volt steps around 65 volts, which on a graph shows a clear peak centered on 65 volts. It is easy to imagine the excitement that must have accompanied this sudden turn of events, moving Germer to sprawl his glad tidings across the bottom of the page!

With this single critical result in hand, the experimental situation changed suddenly. The next day, 7 January, they ran several additional voltage curves, one for each of four different colatitude positions. A voltage peak appeared at a colatitude angle of 45° that was even greater than that at 40°, where the collector had been set the previous day. On the eighth, a new colatitude curve was run at a voltage of 65 volts, and the first true and unmistakable colatitude peak was observed—this was what Davisson had been looking for since 1920! Skipping Sunday, they next ran an azimuth curve at 65 volts and a colatitude of 45°. This time the three-fold azimuthal symmetry was immediately apparent. Figure 7 shows these curves.

The experiments that were carried out during the next two months show that Davisson, Germer and Calbick, having finally found and positively identified one set of electron beams, could now find and identify others quickly. This block of experiments continued through 3 March, when Calbick left for a month on family business. Comparing this with earlier periods of Davisson's long contact with electron scattering, we see that not since the early days of the original Davisson–Kunsman experiments had there been such intense and concentrated effort in a single well defined direction. The presence of a clear, unambiguous goal certainly must have been a major factor in the two cases, an ingredient lacking at other times.

Another factor undoubtedly urging Davisson on to rapid (but careful) experimentation and possible early publication was his feeling that others might be pursuing similar investigations at that time. Recalling his conversations at Oxford and the comments that had been made about the interest of others in this matter, he sent off an article to Richardson in March with the accompanying note:

> "I hope you will be willing, if you think it at all desirable, to get in touch with the editor of *Nature* with the idea of securing early publication. We know of three other attempts that have been made to do this same job, and naturally we are somewhat fearful that someone may cut in ahead of us."

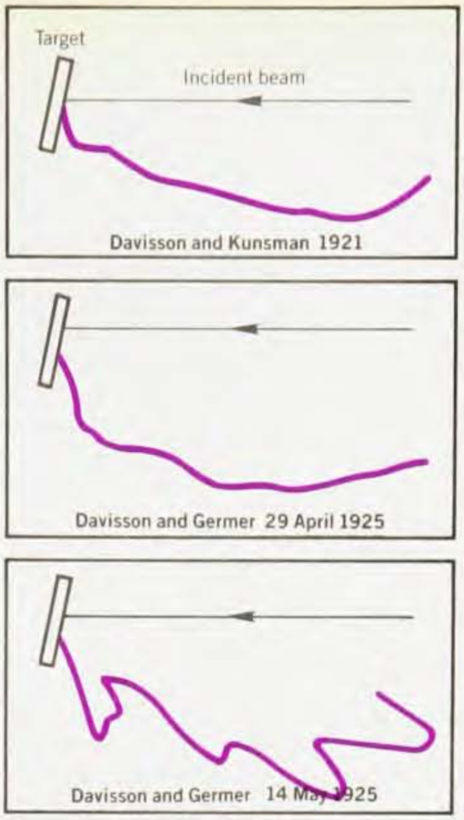
As it turned out these efforts had long been abandoned, but he had no way of knowing that. Nevertheless, another investigator, unknown to Davisson at that time, was indeed making progress at revealing the phenomena of electron diffraction with high-voltage electrons and thin metal foils. This was J.J.'s son, G.P. Thomson; his and Andrew Reid's first note was published in *Nature* just one month after Davisson and Germer's.[7]

A conservative note and a bold one

Davisson and Germer's *Nature* article was an extremely conservative expression of the new experimental evidence for electron diffraction.[8] Its title, "The Scattering of Electrons by a Single Crystal of Nickel," bears a closer connection to the early work of Davisson and Kunsman than it does to the new wave mechanics. Although the paper included a table linking the scattered electron peaks to the corresponding de Broglie wavelengths, it was not until the last two paragraphs that a tentative suggestion was made about the important implications of the work: The results were "highly suggestive . . . of the ideas underlying the theory of wave mechanics."

This cautious attitude may have been due to the problem that Davisson and Germer had in making the proper correlation between their data points and the theory; they found it necessary to hypothesize an *ad hoc* "contraction factor" of about 0.7 for the nickel-crystal spacing to get approximate correspondence between the de Broglie wavelengths and their data. Even at that, only eight of the thirteen beams described were clearly amenable to this analysis.

This cautious attitude appears to have been abandoned in a concurrent article by Davisson alone for an in-house publication, the *Bell Labs Record*.[9] The very title, "Are Electrons Waves?" suggests this difference. After reviewing the evidence that led Max von Laue to think of x rays as being wave-like, he cited his and Germer's recent work with electrons, urging a similar conclusion in this case. Although this article gave its readers no actual data on the experimental evidence for electron waves, it clearly indicates that



Before and after the accident of 5 February 1925. Although the first scattering curves after the repair of the broken tube (middle curve) resembled the 1921 results of Davisson and Kunsman (top curve), striking peaks soon made a sudden appearance (bottom). This development led Davisson and Germer to make a major change in their program. Figure 5

Davisson's thoughts (and certainly Germer's as well) on the subject were not nearly as reserved as the *Nature* article suggests.

One other public announcement of the recent discoveries was made at this time. In a paper presented at the Washington meeting of The American Physical Society on 22–23 April 1927 and abstracted in the *Physical Review* in June,[10] Davisson and Germer basically repeated what they had stated in their *Nature* article, and then added an intriguing final paragraph. Referring to the three anomalous beams that could not be fitted into the analysis in the *Nature* article, they suggested that these "offer strong evidence that there exists in this crystal a structure which has not been hitherto observed for nickel." This statement implies Davisson and Germer had already gone beyond the point of using the "known" structure of the nickel crystal to find out about the possibility of the wave properties of the electron; they were now using the "known" electron waves to learn new facts about the nickel crystal. Between March, when the *Nature* article was submitted, and April, when the *Phys. Rev.* abstract was prepared, results that had been embarrassing to the theory had become a potential new application of that very theory!

True to form, however, Davisson and Germer did not sit back and rest on a "job well done"; they recognized the considerable work necessary to resolve a number of questions still outstanding. Among these were:

- the problem of the "anomalous" beams mentioned above,
- the *ad hoc* "contraction factor" that they had found necessary to attribute to the nickel crystal and
- extension of their electron energies over a greater range, and sharpening and refining their diffraction peaks.

Instant acclaim

Toward this end they initiated an extensive experimental and theoretical attack that lasted from 6 April (when Calbick returned from his month's absence) until 4 August. At that time the tube was cut open for a final careful examination of the target and the other tube components. As it turned out, this intention was foiled when, in the process of being brought back to room temperature, the tube "blew up and [was] partially ruined . . . the leads being broken, filament also, and a large part of the nickel oxidized." A broken tube had served to initiate the decisive experiments on 5 February 1925, and a broken tube ended them on 4 August 1927, two and a half years later. The cover of this issue of PHYSICS TODAY shows the Davisson–Germer tube as it appears today.

The most interesting of this last group of experiments was a series designed to investigate "the anomalous peaks after bombardment," which appeared for a restricted period of time after the target had been heated by bombardment. The experiments showed that the nature—even the existence—of certain beams was not static but varied with temperature and time (and hence conditions of the target in terms of occluded gases). The notebook entries include a great variety of different terms, diagrams and calculations designed to try to make sense out of these data. Davisson and Germer found a "gas crystal" model, in which "gas atoms fit into the crystal," to be the most effective.

The task of welding data and interpretation into a comprehensive report for publication was begun in mid June, well before the experiments were completed. It appears that Davisson was responsible for most, of not all, of the writing; in a letter to his family at the summer cottage he wrote:

> "I'm busy these days writing up our experiment—It's an awful job for me. I didn't get much done yesterday as Prof. Epstein from Pasadena turned up and had to be entertained and shown things—and today I'm too sleepy [after having spent last evening at the theater with Karl Darrow]. However, I must keep at it."

More than three weeks later (23 July) he

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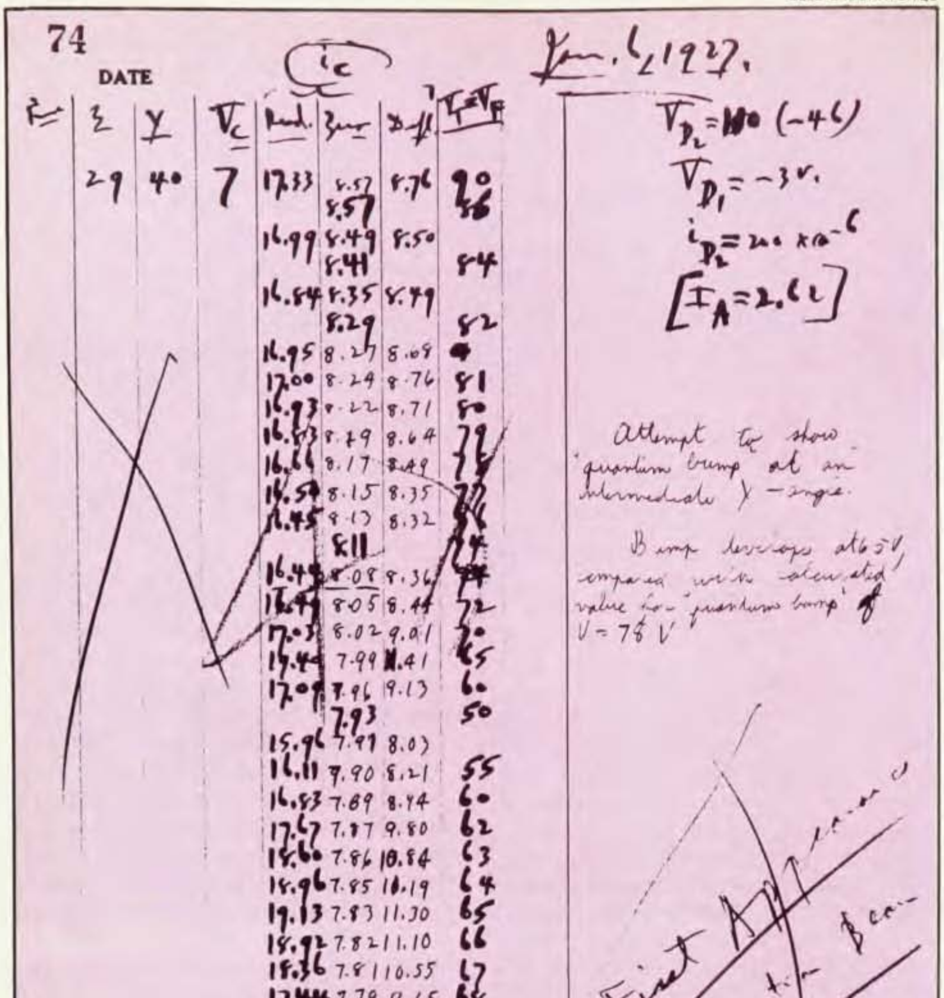


The sixth of January 1927 might well be regarded as the birthday of electron waves, for it was the day that data directly supporting the de Broglie hypothesis of electron waves were first observed. Note the peak deflection at 65 volts, and the detailed study of the region directly below. Calbick's handwriting is neat and cautious; Germer's is bold and expansive. Davisson made no entries in any of the research notebooks kept in the Bell Labs files. Figure 6

was at last able to exclaim,

"I finished the first draft of our paper this morning. It is going to take a lot of going over and revising ... I will leave [the drawings] to Lester—and also the thing is full of blanks in which he will have to stick in the right numbers."

A week later he made his final changes before departing for Maine. Germer, too, needed a break, and after finishing his tasks he left on 14 August for a canoe trip with several friends. The final copy was sent to the *Physical Review* in August and the article appeared in December.

The paper itself was a detailed, comprehensive report on experiments performed, conclusions reached and questions left unanswered. One of the significant features of the paper was its thoughtful examination of the possible ways of interpreting the systematic differences between observed and calculated electron wavelengths (either the suggested "contraction factor" or an "index of refraction" proposed by Carl Eckart, A.L. Patterson, and Fritz Zwicky in independent responses to the *Nature* article).[11] Summarizing the evidence, the paper concluded that of the 30 beams that had been observed, 29 were adequately accounted for by attributing wave properties to free electrons. It acknowledged, however, that the wave assumption implied the existence of eight additional beams, which had not been observed.

The discrepancies between theory and experiment, apparently fairly minor, that Davisson and Germer recorded, evidently did not reduce their fundamental belief that free electrons behave like waves. The physics community appears to have concurred, for I have not found a single voice raised in opposition. This may well have been due as much to the success of the earlier theory of wave mechanics and the acceptance of a wave–particle duality for light as to the force of the evidence inherent in the paper itself.

This may be illustrated by some remarks made by prominent physicists prior to the publication of the *Phys. Rev.* article. In the reports and discussions of the fifth Solvay Conference held in Brussels in October 1927, Niels Bohr, de Broglie, Born, Heisenberg, Langmuir and Schrödinger all hailed the experiments of Davisson and Germer (as described in the *Nature* article) as being, in the words of de Broglie, "very important results which [appear] to confirm the general provisions and even the formulas of wave mechanics."[12] Bohr, speaking before the International Congress of Physics assembled in Como, Italy, on 16 September 1927, drew upon these experiments in establishing his views on complementarity:

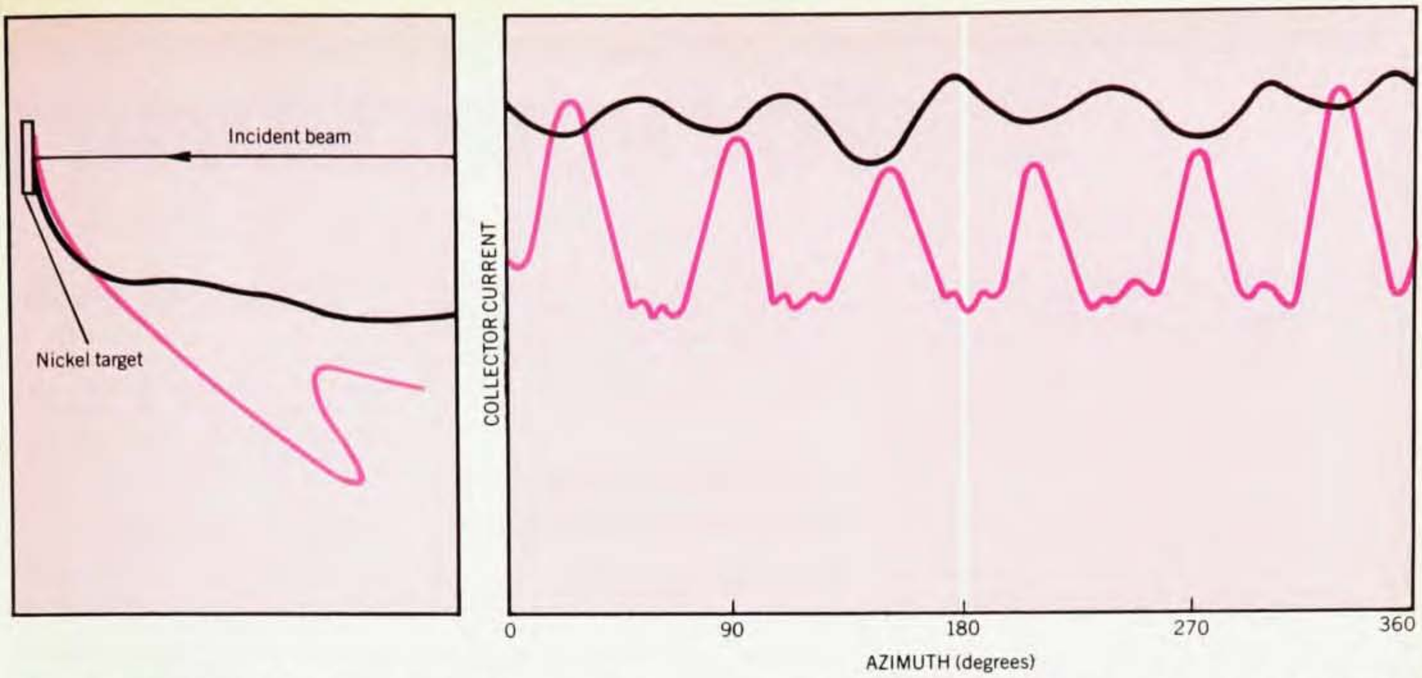
"... the discovery of the selective reflection of electrons from metal crystals ... requires the use of the wave theory superposition principle ... Just as in the case of light ... we are not dealing with contradictory but with *complementary* pictures of phenomena."[13]

Planck, addressing the Franklin Institute on 18 May 1927, even *before* he had heard of the Davisson–Germer results, stated about the electron: "[Its] motion [in the atom] resembles ... the vibrations of a standing wave ... [Thanks] to the ideas introduced into science by L. de Broglie and E. Schrödinger, these principles have already established a solid foundation."[14] Yet in the same address Planck stated that he was *still* (in 1927, four years *after* the decisive Compton experiments) reluctant to accept the corpuscular implications for electromagnetic radiation inherent in his own quantum hypothesis! It appears that physicists were willing to accept the experimental evidence for electron waves almost before those experiments were performed!

The world that is physics

Davisson and Germer succeeded where others had failed. In fact, the others mentioned above (Elsasser, Dymond, Blackett, Chadwick and Ellis), who had the *idea* of electron diffraction considerably *ahead* of Davisson and Germer, were *not able* to produce the desired experimental evidence for it. G.P. Thomson, who did find that evidence by a very different method, testified to the magnitude of the technical achievement as follows:

"[Davisson and Germer's work] was indeed a triumph of experimental skill. The relatively slow electrons [they] used are most difficult to handle. If the results are to be of any value the vacuum has to be quite outstandingly good. Even now [1961] ... it would be a very difficult experiment. In those days it was a veritable triumph. It is a tribute to Davisson's experimental skill that only two or three other workers have used slow



New colatitude and azimuth curves. The black lines show the appearance of the colatitude (left) and azimuth (right) distributions of the scattered electrons when Davisson took the curves to England in 1926. The colored curves are from data taken after 6 January 1927, when the first "quantum bump" was observed. The azimuth curves also confirm the threefold symmetry of the nickel crystal. Figure 7

electrons successfully for this purpose."[15]

Davisson and Thomson shared in the Nobel Prize for physics in 1937 for their accomplishments. Germer and Reid, as junior partners to Davisson and Thomson, did not share in the prize. Reid was tragically killed in a motorcycle accident shortly after his and Thomson's definitive papers appeared in 1928.

Davisson and Germer actively pursued the topic of electron diffraction for about three years after 1927, publishing, together and separately, about twenty more papers on the subject; reference 16 gives three of the most important. By the early 1930's, both Davisson and Germer had turned to new fields: Davisson to electron optics (including early television); Germer to high-energy electron diffraction and later still to electrical contacts. Davisson retired from Bell Labs in 1946 and spent the remaining twelve years of his life in Charlottesville, Virginia, summering as usual in Maine. Germer regained his interest in low-energy electron diffraction in 1959–60, at which time he and several co-workers at Bell Labs perfected a technique, eventually referred to as the "post-acceleration" technique,[17] which had been devised in 1934[18] and then abandoned, by Wilhelm Ehrenberg. With this work Germer was able to follow up with great success the study of surfaces, to which he had been attracted in his original work with Davisson; the field of low-energy electron diffraction (LEED) is now widespread and very active. Germer retired from Bell Labs in 1961 and remained active in this "new" field and in his favorite recreation, mountain climbing, until his death in 1971.

In trying to answer the question of "Why Davisson and Germer, and not someone else?" one's thoughts leap to such things as the "luck" of the broken tube in 1925 and the trip to England in 1926. Davisson and Germer themselves freely admitted the key importance of these events. But to dwell on them exclusively would be a mistake. Neither of these events would even have been remembered had they not been followed by thorough, careful and creative experiment and reflection. Perhaps of equal importance is the habit of attention to technical detail established by Davisson in his student days and extended in the long series of Davisson–Kunsman and earlier Davisson–Germer experiments. Another important factor is the time for pure research provided by Western Electric–Bell Labs, and the technical support in areas such as high vacua and electrical detection techniques available at that industrial laboratory.

All in all, this case history on the discovery of electron diffraction appears to illustrate the complex nature of the world that is physics, the difficulty of singling out any one factor as being responsible for a great discovery, and the importance of establishing and nurturing the ties that bind together the generations of physicists, as well as the physicists of each generation.

References

References to correspondence, personal remarks and other archival material are documented in the author's PhD dissertation, "C.J. Davisson, L. H. Germer, and the Discovery of Electron Diffraction," University of Minnesota, 1973, available from Xerox University Microfilms, 3000 North Zeeb Road, Ann Arbor, Michigan 48106, Order No. 74–10 505.

1. C. J. Davisson, L. H. Germer, Phys. Rev. **30,** 705 (1927).
2. US Reports **283,** 665 (1931).
3. C. J. Davisson, L. H. Germer, Phys. Rev. **15,** 330 (1920).
4. C. J. Davisson, C. H. Kunsman, Science **54,** 523 (1921).
5. M. Born, Nature **119,** 354 (1927).
6. W. Elsasser, Naturwissenschaften **13,** 711 (1925); E. G. Dymond, Nature **118,** 336 (1926).
7. G. P. Thomson, A. Reid, Nature **119,** 890 (1927).
8. C. J. Davisson, L. H. Germer, Nature **119,** 558 (1927).
9. C. J. Davisson, Bell Lab. Record **4,** 257 (1927).
10. C. J. Davisson, L. H. Germer, Phys. Rev. **29,** 908 (1927).
11. C. Eckart, Proc. Nat. Acad. Sci. **13,** 460 (1927); A. L. Patterson, Nature **120,** 46 (1927); F. Zwicky, Proc. Nat. Acad. Sci. **13,** 518 (1927).
12. L'Institut International de Physique Solvay, *Electrons et Protons: Rapports et Discussions du Cinquieme Conseil de Physique*, Gauthier-Villars, Paris (1928), pages 92, 127, 165, 173, 274, 288.
13. N. Bohr, *Atomic Theory and the Description of Nature*, Macmillan, New York (1934), page 56; italics supplied.
14. M. Planck, J. Franklin Inst. **204,** 13 (1927).
15. G. P. Thomson, *The Inspiration of Science*, Oxford U.P., London (1961); reprinted by Doubleday, Garden City, New York (1968), page 163.
16. C. J. Davisson, L. H. Germer, Proc. Nat. Acad. Sci. **14,** 317 (1928); Proc. Nat. Acad. Sci. **14,** 619 (1928); Phys. Rev. **33,** 760 (1929).
17. A. U. MacRae, Science **139,** 379 (1963).
18. W. Ehrenberg, Philosoph. Mag. **18,** 878 (1934). □