The Scientific Work of C. J. Davisson

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THE very first piece of work which is published by a physicist who is destined to be great is not often outstanding; but sometimes it has curious affinities, accidental rather than causal, with aspects of the work that was to come thereafter. In the first paper published by C. J. Davisson, we find him working with electrons, concentrating them into a beam by the agency of a magnetic field, directing them against a metal target, and looking to see whether rays proceed from the target. True, the electrons came from a radioactive substance, and therefore were much faster than those of his later experiments. True also, he did not actually focus the electron-beam. True also, the rays for which he was looking were X-rays, and in these he took no further interest. Yet in nearly all of his subsequent researches he was to use some of the principles of electron-focussing or electron-microscopy; in many, he was to look for things that were emitted by the target on which his electrons fell. This maiden paper was presented before the American Physical Society at its meeting in Washington in April 1909; the printed version may be found in the Physical Review, page 469 of volume 28 of the year 1909. It was signed from Princeton University, whither Davisson had gone as a graduate student.

Another characteristic of Davisson's work in his later years was his frequent study and use of thermionics. Already in 1911 we find him working in this field—but it was thermionics with a difference. The word "thermionics" now signifies, nearly always, the emission of electrons from hot metals; but at first it included also the emission of positive ions from hot metals and hot salts. Though neither useless nor uninteresting, the emission of positive ions is now rated far below the effect to which we now confine the name of thermionics: emission of electrons from hot metals is one of the fundamental phenomena of Nature, and its uses are illimitable. It may be plausibly conjectured that in 1911 the difference in the importance of the two phenomena—emission of positive ions and emission of electrons—was far less evident than it is now. Davisson, working under the British physicist O. W. Richardson who was then professor at Princeton, established that the positive ions emitted from heated salts of the alkali metals are once-ionized atoms of these metals—that is to say, atoms lacking a single electron. He also showed that if gas is present in the tube, it may enhance the number of the ions but does not change their character. This work was presented before the April meeting of the American Physical Society in 1911. Abstracts of the papers which he there gave orally may be found in *Physical Review*, but the publications in full appeared (in 1912) in *Philosophical Magazine*. Davisson's choice of a British journal was advised by his transplanted teacher, but it must be realized that in 1912 the *Physical Review* had by no means ascended to the rank that it holds today. With this work came to their end the contributions of his student years, and next we find him publishing as an independent investigator.

From Davisson's years (1912–17) at the Carnegie Institute of Technology there is a paper embodying an attempt to calculate the optical dispersion of molecular hydrogen and of helium from Bohr's earliest atom-model. It shows him possessed of no mean mathematical technique, but is based—as the date by itself would make evident—on too primitive a form of quantum-theory.

In June 1917, in the midst of World War I, Davisson came for what he thought would be a temporary job at the institution then known as the Research Laboratories of the American Telephone and Telegraph Company and the Western Electric Company, thereafter—from 1925—as Bell Telephone Laboratories. Not for a year and a half was he able to devote himself to work untrammeled by the exigencies of war. So far as publication is concerned, his second period began in 1920, when he presented two papers before the American Physical Society: one at the New York meeting in February, one at the Washington meeting in April. In the former of these his name is linked with that of L. H. Germer, a name associated with his in the great discovery of electron waves; in the latter it is linked with that of the late H. A. Pidgeon.

These two papers are represented only by brief abstracts; and this is the more regrettable, as they form the only contributions published under Davisson's name to the dawning science of the oxide-coated cathode. In the former, he established that the remarkably high electron-emission of oxide-coated metals—as contrasted with bare metals—is *not* due, as had been elsewhere suggested, to the impacts of positive ions from the gas of the tube against the coatings: it is true thermionic emission. In the latter, he studied the rise and eventual fall of the thermionic emission as more and more oxide is laid down upon the metal surface, and concluded that the emission occurs when a definite number of oxide molecules is assembled into a patch of definite size on the surface: the number of patches of just the right size first rises, then declines as the deposition continues. According to colleagues of his, these two papers fall short by far of indicating the extent of his contributions to this field; and one of them has said that Davisson was excessively scrupulous about putting his work into print, being unwilling to publish his observations until he felt sure that he understood all that was taking place. It is in an article by another—the late H. D. Arnold, first to hold the post of Director of Research in Bell Telephone Laboratories and its antecedent organization—that we find a description of Davisson's "power-emission chart," now standard in the art. In Arnold's words: "Dr. Davisson has devised a form of coordinate-paper in which the coordinates are power supplied to the filament (abscissae) and thermionic emission (ordinates). The coordinate lines are so disposed and numbered that if the emission from a filament satisfies Richardson's relation, and the thermal radiation satisfies the Stefan-Boltzmann relation, then points on the chart coordinating power and emission for such a filament will fall on a straight line."

In a paper presented at a meeting toward the end of 1920 (it was a joint paper of himself and J. R. Weeks) Davisson gives the theory of the emission of light from metals, deduces a deviation from Lambert's law and verifies this by experiment. A connection between this and the study of thermionics may be inferred from the words which I quoted earlier from Arnold's description of Davisson's power-emission chart. This work was published in full, some three years later, in the *Journal of the Optical Society of America*.

We turn now to Davisson's investigations of thermionic emission from metals.

Those whose memories go back far enough will recall that two laws have been proposed for the dependence of thermionic emission on temperature. Both were propounded by O. W. Richardson, and each, somewhat confusingly, has at times been called "Richardson's law." The earlier prescribed that the thermionic current i should vary as $T^{\frac{1}{2}}\exp(-b/T)$, T standing for the absolute temperature; the later prescribes that i should vary as $T^2\exp(-b/T)$. The former is derived from the assumption that the velocities and energies of the electrons inside the metal are distributed according to the classical Maxwell-Boltzmann law. The latter follows from the assumption that these velocities and energies are distributed according to the quantum-theory or Fermi-Dirac law: it was, however, derived from thermodynamic arguments some thirteen years before the Fermi-Dirac theory was developed, and the experiments about to be related were performed during this thirteen-year period.

In the interpretation of either law, b is correlated with the work of egress which an electron must do (at the expense of its kinetic energy) in order to go from the inside to the outside of the metal. I will leave to a later page the phrasing of this correlation, and say for the moment that b multiplied by Boltzmann's constant k represents what used to be called and is still sometimes called the "thermionic work-function" of the metal. If a given set of data is fitted first by the $T^{\frac{1}{2}}$ law and then by the T^2 law, different values of b and therefore different values of the thermionic work-function are obtained. Which is right?

This question can be answered if the thermionic work-function can be measured with adequate accuracy by some other method. Such a method exists: it is called the "calorimetric" method. Suppose an incandescent wire surrounded by a cylindrical electrode. If the latter is negative with respect to the former, the emitted electrons will return to the wire, and there will be no net thermal effect due to the emission. If, however, the cylinder is positive with respect to the wire, the electrons will be drawn to it, and the wire will fall in temperature: this is the "cooling-effect" due to the emission. The resistance of the wire will decrease, and if the current into the wire is held constant, the voltage between its terminals will be lessened.

The experiment may sound easy, and so it might be if all of the current flowed within the wire from end to end; but the bleeding of electrons through the entire surface makes the current vary from point to point along the wire, and complicates the test enormously. Others elsewhere had tackled this difficult problem of experimentation; but Davisson and Germer found a better way to handle it, and their results for tungsten were presented at a meeting at the end of 1921 and published fully the following year. From their data they calculated the thermionic work-function of the metal, which when thus determined we may denote by $e\phi$. It agreed with the value of kb obtained from the newer form of "Richardson's law," disagreed with the other. Thus Davisson was in the position of having confirmed the Fermi-Dirac distribution-law before it had been stated!

It remains to be said that, years later, Davisson and Germer repeated this experiment upon an oxide-coated platinum wire. Here they came upon a complication from which clean metal surfaces are fortunately exempt. The character of the oxide-coated wire changed with the temperature; and, since the measurement of the "constant" b requires a variation of the temperature, its value did not provide a reliable measure of the work at any single temperature, whereas the "calorimetric" measurement did.

Now at last we are ready to attend to the early stages of the studies which were destined to lead to the discovery of electron-waves. These were studies of what I shall call the "polycrystalline scattering patterns" of metals: the name is descriptive rather than short. A beam of electrons is projected against a metal target which is in the condition, normal for a metal, of being a complex of tiny crystals oriented in all directions. Some of these electrons swing around and come back out of the metal with undiminished energy: these are the electrons that are "elastically scattered," (Davisson records that elastic scattering had previously been observed only with electrons having initially an energy of 12 electron-volts or less). A collector is posted at a place where it collects such electrons as are scattered in a direction making some chosen angle θ with the direction exactly opposite to that of the original or "primary" beam. There may be inelastically-scattered or secondary electrons which travel toward the collector: its potential is so adjusted as to prevent the access of these.

The collector is moved from place to place so as to occupy successively positions corresponding to many values of the angle θ. It is always in the same plane passing through the primary beam, and so the curve of number-of-scattered-electrons (per unit solid angle) plotted against θ is a cross-section of a three-dimensional scattering pattern; but, for obvious reasons of symmetry, the three-dimensional pattern is just the two-dimensional pattern rotated around the axis which is provided by the primary beam. This two-dimensional pattern is what I have called the polycrystalline scattering-pattern. It is a curve plotted, in polar coordinates or in Cartesian, against θ over a range of this angle which extends from $-90°$ to $+90°$; but the part of the curve which runs from $\theta = -90°$ to $\theta = 0°$ is the mirror-image of the other part, and either by itself suffices. The curve cannot be plotted in the immediate vicinity of $\theta = 0°$, because the source of the electrons gets in the way.

The first published report of such an experiment is to be found, under the names of Davisson and C. H. Kunsman, in *Science* of November 1921; in that same November Davisson presented the work before the American Physical Society. The metal was nickel, and the pattern had two most remarkable features. These were sharp and prominent peaks; one inferred from the trend of the curve in the neighborhood of $\theta = 0°$ and presumably pointing in exactly that direction, consisting therefore of electrons which had been turned clear around through 180 degrees; the other pointing in a direction which depended on the speed of the electrons, and for 200-volt electrons was at 70°.

Any physicist who hears of experiments on scattering is likely to think of the scattering-experiments performed by Rutherford now more than forty years ago, which established the nuclear atom-model. These were measurements of the scattering-pattern of alpha-particles, and this does not look in the least like the curve observed by Davisson and Kunsman: it shows no peaks at all. Alpha-particles, however, are seven thousand times as massive as electrons: they are deflected in the nuclear fields, and so great is the momentum of an alpha-particle that it does not suffer any perceptible deflection unless and until it gets so close to a nucleus that there are no electrons at all between the nucleus and itself. But with so light a particle as an electron, and especially with an electron moving as slowly as Davisson's, the deflection commences when the flying electron is still in the outer regions of the atom which it is penetrating. The deflection of the individual electron and the scattering-pattern of the totality of the atoms are, therefore, conditioned not only by the nuclear field but by the fields of all the electrons surrounding the nucleus. How shall one calculate the effect of all these?

This is a very considerable mathematical problem, and Davisson simplified it to the utmost by converting the atomic electrons into spherical shells of continuous negative charge centered at the nucleus. The simplest conceivable case—not to be identified with that of nickel—is that of a nucleus surrounded by a single spherical shell having a total negative charge equal in magnitude to the positive charge of the nucleus itself. Within the shell the field is the pure nuclear field, the same as though the shell were not there at all; outside of the shell there is no field at all. This is what Davisson called a "limited field." Calculation showed that the scattering-pattern of such a system would have a peak in the direction $\theta = 0°$, so long as the speed of the electrons did not exceed a certain ceiling-value! And there was more: "the main features of the scattering-patterns (Davisson said "distribution-curves") for nickel, including the lateral maximum of variable position, are to be expected if the nickel atom has its electrons arranged in two shells."

Nickel in fact is too complicated an atom to be represented, even in the most daring allowable approximation, as a nucleus surrounded by a single shell; two shells indeed seem insufficient, but the fact that a two-shell theory leads in the right direction is a significant one. Magnesium might reasonbly be approximated by a single-shell model; Davisson experimented on this metal, and published (in 1923) scattering-patterns which lent themselves well to his interpretation. He measured scattering-patterns of platinum also, and these as to be expected are much more wrinkled with peaks and valleys; the task of making calculations for the platinum atom with its 78 electrons was too great.

Nickel continued to be Davisson's favorite metal, and four years later (1925) his study of its polycrystalline scattering-pattern was still in progress. In April of that year occurred an accident, of which I quote his own description from *Physical Review* of December 1927. "During the course of his work 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 in-rushing air. The oxide was eventually reduced and a layer of the target removed by vaporization, but only after prolonged heating at various high temperatures in hydrogen and in vacuum. When the experiments were continued it was found that the distribution-in-angle of the scattered electrons had been completely changed. . . . This marked alteration in the scattering-pattern was traced to a re-crystallization of the target that occurred during the prolonged heating. Before the accident and in previous experiments we had been bombarding many small crystals, but in the tests subsequent to the accident we were bombarding only a few large ones. The actual number was of the order of ten."

I do not know whether Davisson ever cried out *O felix culpa!* in the language of the liturgy; but well he might have. The exploding liquid-air bottle blew open the gate to the discovery of electron-waves. Fatal consequences were not wanting: the accident killed the flourishing study of polycrystalline scattering-patterns, and countless interesting curves for many metals are still awaiting their discoverers. This may illustrate a difference between the industrial and the academic career. Had Davisson been a professor with a horde of graduate students besieging him for thesis subjects, the files of *Physical Review* might exhibit dozens of papers on the scattering-patterns of as many different metals, obtained by the students while the master was forging ahead in new fields.

Now that we are on the verge of the achievement which invested Davisson with universal fame and its correlate the Nobel Prize, I can tell its history in words which I wrote down while at my request he related the story. This happened on the twenty-fifth of January, 1937: I have the sheet of paper which he signed after reading it over, as also did our colleague L. A. MacColl who was present to hear the tale. This is authentic history such as all too often we lack for other discoveries of comparable moment. Listen now to Davisson himself relating, even though in the third person, the story of the achievement.

"The attention of C. J. Davisson was drawn to W. Elsasser's note of 1925, which he did not think much of because he did not believe that Elsasser's theory of his (Davisson's) prior results was valid. This note had no influence on the course of the experiments. What really started the discovery was the well-known accident with the polycrystalline mass, which suggested that single crystals would exhibit interesting effects. When the decision was made to experiment with the single crystal, it was anticipated that 'transparent directions' of the lattice would be discovered. In 1926 Davisson had the good fortune to visit England and attend the meeting of the British Association for the Advancement of Science at Oxford. He took with him some curves relating to the single crystal, and they were surprisingly feeble (surprising how rarely beams had been detected!). He showed them to Born, to Hartree and probably to Blackett; Born called in another Continental physicist (possibly Franck) to view them, and there was much discussion of them. On the whole of the westward transatlantic voyage Davisson spent his time trying to understand Schroedinger's papers, as he then had an inkling (probably derived from the Oxford discussions) that the explanation might reside in them. In the autumn of 1926, Davisson calculated where some of the beams ought to be, looked for them and did not find them. He then laid out a program of thorough search, and on 6 January 1927 got strong beams due to the line-gratings of the surface atoms, as he showed by calculation in the same month."

Now I will supplement this succinct history by explanations. The first name to be mentioned in the explanations must be one which does not appear in the quotation: that of Louis de Broglie.

Louis de Broglie of Paris had suggested that electrons of definite momentum—let me denote it by p—are associated with waves of wavelength λ equal to h/p, h standing for Planck's constant. This suggestion he made in an attempt to interpret the atom-model of Bohr, a topic which is irrelevant to this article. Irrelevant also is the fact that Louis de Broglie's suggestion led Schroedinger to the discovery of "wave-mechanics," but I mention it here because Schroedinger's name appears in the quotation. Highly relevant is the inference that the "de Broglie waves," as they soon came to be called, might be diffracted by the lattices of crystals, and that the electrons of an electron-beam directed against a crystal might follow the waves into characteristic diffraction-beams such as X-rays exhibit.

This inference was drawn by a young German physicist Walther Elsasser by name, then a student at Goettingen. It was one of the great ideas of modern physics; and, in recording that its expression in Elsasser's letter was not what guided Davisson to its verification, I have no wish to weaken or decry the credit that justly belongs to Elsasser for having been the first to conceive it. Dr. Elsasser has authorized me to publish that he submitted his idea to Einstein, and that Einstein said "Young man, you are sitting on a gold-mine." The letter which I have mentioned appeared in 1925 in the German periodical *Die Naturwissenschaften*. As evidence for his idea Elsasser there adduced the polycrystalline scattering-patterns, in particular those for platinum, that had been published by Davisson and Kunsman. But Davisson as we have seen did not accept this explanation of the patterns; and never since, so far as Elsasser or I are aware, has anyone derived or even tried to derive the polycrystalline scattering-patterns from the wave-theory of electrons. This must be listed as a forgotten, I hope only a temporarily forgotten, problem of theoretical physics.

Essential to the application of Elsasser's idea is the fact that the wavelengths of the waves associated with electrons of convenient speeds are of the right order of magnitude to experience observable diffraction from a crystal lattice. It is easy to remember that 150-volt electrons have a wavelength of one Angstrom unit, while the spacings between atoms in a solid are of the order of several Angstroms. This fact of course did not escape Elsasser, and it figures in his letter.

From the quotation it is clear that the earliest patterns obtained from the complex of large crystals were obscure, and the definitive proof of Elsasser's theory was obtained only when Davisson instituted his "program of thorough search" and simultaneously in England G. P. Thomson instituted his own. Two other items in the quotation require to be explained. The hypothesis of "transparent directions" I will consider to be explained by its name. Were it correct, the directions of the beams would be independent of the speed of the electrons; since they are not, the hypothesis falls. The reference to the "line-gratings of the surface atoms" induces me to proceed at once to one of the principal contrasts between diffraction of electrons and diffraction of X-rays.

An optical grating is a sequence of parallel equidistant grooves or rulings on a surface of metal or glass. The atoms on a crystalline surface are arranged in parallel equidistant lines, and one might expect X-rays or electrons to be diffracted from them as visible light is diffracted from an optical grating. This expectation is frustrated in the case of X-rays, because their power of penetration is so great that a single layer of atoms, be it the surface-layer or any other, diffracts but an inappreciable part of the incident X-ray beam; only the cumulative effect of many layers is detectable. Electrons as slow as those that Davisson used are not nearly so penetrating. With these indeed it is possible, as he was the first to show, to get diffraction-beams produced by the surface-layer only. Such beams, however, are detectable only when the incident (or the emerging) beam of electrons almost grazes the surface; and nearly always, when a beam is observed, it is due to the cumulative effect of many atom-layers as is the rule with X-rays. But the cumulative effect requires more specific conditions than does diffraction by the surface-layer: if the incident beam falls at a given angle upon the surface, the momentum of the electrons and the wavelength of their waves must be adjusted until it is just right, and, reversely, if the momentum of the electrons has a given value the angle of incidence must be adjusted until it is just right. This also Davisson verified.

As soon as Davisson made known his demonstration of electron-waves, he was bombarded by entreaties for speeches on his work and for descriptions to be published in periodicals less advanced and specialized than *Physical Review*. To a number of these he yielded, and I recommend especially the talk which in the autumn of 1929 he gave before the Michelson Meeting of the Optical Society of America; one finds it in print in volume

18 of the Journal of that Society. It is written with such clarity, grace and humor as to make one regret that Davisson was not oftener tempted to employ his talents for the benefit not of laymen precisely, but of scientists who were laymen in respect to the field of his researches. I quote the first two sentences: "When I discovered on looking over the announcement of this meeting that Arthur Compton is to speak on 'X-rays as a Branch of Optics' I realized that I had not made the most of my opportunities. I should have made a similar appeal to the attention of the Society by choosing as my subject 'Electrons as a Branch of Optics.' "

Though in this period his duties as expositor took a good deal of his time, Davisson found opportunity to prosecute his work and to begin on certain applications. One obvious development may be dismissed rather curtly, as being less important than it might reasonably seem. One might have expected Davisson to strive to verify de Broglie's law $\lambda = h/p$ to five or six significant figures. This would have been difficult if not impossible, since the diffraction-beams of electrons are much less sharp than those of X-rays; this is a consequence of the fact that the diffraction is performed by only a few layers of atoms, the primary beam being absorbed before it can penetrate deeply into the crystal structure. But even if it had been easy the enterprise would probably have been considered futile, for de Broglie's law quickly achieved the status of being regarded as self-evidently true. Such a belief is sometimes dangerous, but in this case it is almost certainly sound: the law is involved in the theories of so many phenomena, that, if it were in error by only a small fraction of a per cent, the discrepancy would have been noted by now in more ways than one. Davisson established the law within one per cent, and there are few who would not regard this as amply satisfactory.

The greatest of the uses of electron-diffraction lies in the study of the arrangement of atoms in crystals and in non-crystalline bodies. Here it supplements the similar use of X-ray diffraction, for it serves where X-ray diffraction does not, and *vice versa*. Once more I quote from a lecture of Davisson's: "Electrons are no more suitable for examining sheets of metal by transmission than metal sheets are suitable for replacing glass in windows. To be suitable for examination by electrons by transmission, a specimen must be no more than a few hundred angstroms in thickness. It must be just the sort of specimen which cannot be examined by X-rays. Massive specimens can be examined by electrons by reflection. The beam is directed onto the surface at near-grazing incidence, and the half-pattern which is produced reveals the crystalline state of a surface-layer of excessive thinness. . . . Invisible films of material, different chemically from the bulk of the specimen, are frequently discovered by this method." Many experiments of this type were done at Bell Telephone Laboratories by L. H. Germer; they do not fall within the scope of this article, but we may be sure that Davisson was interested in them.

Davisson also studied the refraction of electrons at the surface of nickel, and this is work which in my opinion has never received the attention that it merits. Let us consider its importance.

I have already spoken of the work which an electron must do in order to quit a metal, and have mentioned two ways employed by Davisson (and by others) to ascertain its value—the measurement of the constant b which figures in Richardson's equation, and the measurement of the quantity ϕ by the calorimetric method. It is customary to ascribe this work of egress to the presence of a "surface potential-barrier," usually imagined as an infinitely sudden potential-drop occurring at the surface of the metal: the potential immediately outside the metal is supposed to be less than the potential immediately inside by a non-zero amount, which I will denote by X. One is tempted to identify X with ϕ and with kb/e; but this is an over-simplification. By the classical theory there is a difference which is small but not quite negligible. By the new theory there is a difference which is neither small nor by any means negligible. By the new theory, in fact, X is greater than ϕ by an amount which is equal to the so-called "Fermi energy"—the kinetic energy of the electrons which, if the metal were at the absolute zero of temperature, would be the fastest-moving electrons in the metal. Now, this last amount is of the order of half-a-dozen electron-volts for the metals of major interest in thermionic experiments, and so also is the value of ϕ. Thus, if there were a method for determining the height of the surface potential-barrier, this would be expected to yield a value of the order of six volts if the old theory were right and a value of the order of twelve volts if the new theory were correct.

Well, there *is* such a method, and it consists precisely in observing and measuring the refraction of the electron-waves as they pass through the surface of the metal. This refraction has a deceptive effect; it alters the orientations of the diffraction-beams as though the crystal were contracted in the direction normal to its surface. Once this is comprehended, the refractive index may be calculated from the observations, and from the refractive index the value of X the surface potential drop. This was done by Davisson and Germer for nickel, and published in the *Proceedings of the National Academy of Sciences* for 1928. The value which they found for the surface potential-drop was 18 volts—three times as great as the value prescribed by the old theory, half again as great as the value afforded by the new. Thus the experiments speak for the new theory over the old, yet not with unambiguous support of the new. This has been described to me, by a distinguished physicist, as one of the situations in which the concept of a single sharp potential-drop becomes most palpably inadequate. Work of this kind continued to be done, especially in Germany, until the later thirties, and then regrettably flickered out.

In 1937 the Nobel prize was conferred on Davisson, and he had the opportunity of enjoying the ceremonies and festivities which are lavished upon those who go to Stockholm and receive it. He shared the prize with G. P. Thomson, who must not be entirely neglected even in an article dedicated explicitly to Davisson. There was little in common between their techniques, for Thomson consistently used much faster electrons which transpierced very thin polycrystalline films of metal and produced glorious diffraction-rings. He too founded a school of crystal analysts.

Finally I mention three notes—two abstracts of papers given before the American Physical Society in 1931 and 1934, and one Letter to the Editor of Physical Review—bearing on what has been described to me, by an expert in the field, as the first publication of the principle of the "electrostatic lens" useful in electron-microscopy. These are joint papers of Davisson and C. J. Calbick. They report, in very condensed form, the outcome of an analysis which showed that a slit in a metal cylinder treats electrons as a cylindrical lens treats light, and a circular hole in a metal plate treats electrons as a spherical lens treats light: in both cases the field-strengths on the two sides of the metal surface (cylinder or plate) must be different. Experiments were performed to test the theory, and succeeded; and in the latest of the notes we read that Calbick and Davisson used a two-lens system to form a magnified image of a ribbon-filament upon a fluorescent screen. Calbick recalls that the magnification was of the order of twentyfold.

During the time of his researches on electron-waves, Davisson's office was on the seventh floor of the West Street building, on the north side about seventy-five paces back from the west facade: his laboratories were at times beside it, at times across the corridor. This illustrates a disadvantage of our modern architecture. If Davisson had done his work in a mediaeval cathedral, we could mount a plaque upon a wall which had overlooked his apparatus, and plaque and wall would stand for centuries. But the inner walls of Davisson's rooms are all gone, and the outer wall consisted entirely of windows; and nothing remains the same except the north light steaming through the windows, which we may take as a symbol of the light which Davisson cast upon the transactions between electrons and crystals.