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CLINTON JOSEPH DAVISSON

1881—1958

A Biographical Memoir by
MERVIN J. KELLY

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BY MERVIN J. KELLY

DAVISSON'S EARLY YEARS AND FAMILY HISTORY

CLINTON JOSEPH DAVISSON was born in Bloomington, Illinois, October 22, 1881. His father, Joseph Davisson, after service in the Union Army, settled there in 1865, where he lived the remainder of his life, dying at the age of sixty-five. He was a contract painter. He married Mary Calvert, who lived for more than ninety years. Davisson had one sister, Carrie (Mrs. L. B. Jackson), who lives in Bloomington.

After graduating from Bloomington High School in 1902, he won a scholarship at the University of Chicago and entered in the fall of 1902. Throughout his college and graduate school years, Davisson was almost entirely self-supporting. After only four quarters at the University of Chicago, his work was so brilliant that, upon Professor R. A. Millikan's recommendation, he went to Purdue University in the middle of the 1903–1904 school year to take the place of a member of the teaching staff in physics who had died.

He returned to Chicago in the fall of 1904. After a year's residence there, he went to Princeton University in the fall of 1905 with the title of Instructor in Physics. He continued at Princeton until 1911, when he received his Ph.D. with physics as his major and mathematics as his minor field. He had received the B.S. degree from the University of Chicago in 1908; he had returned to Chicago for the summer quarters of 1905, 1906, 1907, and 1908.

While Davisson had the title of Instructor at Princeton, he was assigned more to assisting the research of Professor Owen Richardson than to teaching. He did his thesis with Richardson. It was entitled "The Positive Thermions from Salts of Alkaline Earths."

While doing his graduate work at Princeton, Davisson met, wooed, and won for his wife and life companion Charlotte Sara Richardson, who was visiting her brother, Professor Richardson. Professor Richardson was then Head of the Physics Department at Princeton, and later became Sir Owen Richardson, Nobel Laureate in Physics. Oswald Veblen, the mathematician, had similarly met, wooed, and won for his wife Charlotte's sister Elizabeth, only a year or so previously. The Richardsons were born and educated in England.

Clinton and Charlotte were married on August 4, 1911. He was appointed Assistant Professor of Physics at Carnegie Institute of Technology in the summer of 1911, and the Davissons established their first home there in September, 1911. It was there that their first two children were born: Clinton Owen in 1912 and James Willans in 1914.

The Davissons purchased several acres overlooking the sea at Brooklin, Hancock County, Maine, in 1913. He built their delightful summer home there with his own hands in successive yearly stages. The family vacations were always spent there. His last summer in Brooklin was in 1957, only a few months before his death.

Davisson, affectionately known to his large circle of friends as "Davy," came to the Engineering Department of the Western Electric Company (later the Bell Telephone Laboratories) in New York City in May, 1917, on leave from Carnegie Tech to aid in research for the Army and Navy in the First World War. After the close of the war, being assured of freedom to do basic research, he stayed at the Laboratories, where he remained until the retirement age of sixty-five; he then accepted a research professorship in the Department of Physics at the University of Virginia. He made his decision to stay at Western Electric on the basis of opportunity for full time at basic research. At Carnegie Tech, his teaching duties had been so heavy that in his six years there he had been able to carry only one research to the point where, with his lifelong high standards, he would publish it.

Having decided to remain in this laboratory of industry, the Davissons purchased a comfortable house with about one acre of land in near-by Short Hills, New Jersey. Here Mrs. Davisson could give expression to her lifelong interest in flower gardening. She also always had a flower garden at their Maine summer home. Two more children were born: Elizabeth Mary in 1921 and Richard Joseph in 1922. James and Richard have followed their father in their professional interests. Both are pursuing careers as research physicists. Owen's interests are also close to those of his father, as his career is in engineering. Elizabeth had a serious illness when she was about twelve years old, and its effects continued for many years. This was a source of great sadness to Davisson. Fortunately, she had an almost complete recovery some years ago, and her continued living at home was a source of much happiness to him.

DAVISSON'S CAREER AT BELL TELEPHONE LABORATORIES

Beginning in 1912, the Western Electric Company, under the able leadership of Dr. H. D. Arnold, pioneered in the development of the thermionic high-vacuum tube for communications applications. Although such devices already had important application as voice-frequency amplifiers in long-distance circuits at the time of our entrance into the First World War, vacuum tubes were really not yet out of the laboratory, and the relatively few that were required for extending and maintaining long-distance telephone service were made in the laboratories of the Engineering Department. Research and development programs directed to military applications of these new devices brought about a large expansion in the work of the laboratories. Davisson was assigned to the development of tubes for military use.

Important applications resulted from this work, and thermionic high-vacuum tubes had to be produced in what was for that time astronomical quantities. The science, technology, and art essential to such quantity production did not exist and had to be created concurrently with a most rapid build-up in production. All of the tubes employed an oxide-coated cathode, which was later to become the universal standard the world over for low-power thermionic vacuum tubes.

Davisson early took a position of leadership in problems of fundamental physics relating to the emitter and high-vacuum techniques. War tempo forced the work to move so rapidly that much of it was empirical. Even in this atmosphere of empiricism, his work was unusually fundamental and analytical. Increasingly, all of his associates went to him to discuss fundamental problems that were in urgent need of an answer. He was always available and displayed a friendly interest; they rarely left him without benefit from the discussion. Frequently, he would continue his study of the problem and come later to give the benefit of his more mature consideration.

During this period of intensive work performed in an atmosphere of urgency, Davisson displayed the characteristics that were important in determining the pattern of his work through the years and the nature of his contributions to Bell Telephone Laboratories and to science. His inner driving force was always for complete and exact knowledge of the physical phenomena under study. Thoroughness was an outstanding characteristic. The rapid tempo of the work with the necessity of accepting partial answers and following one's nose in an empirical fashion were foreign to his way of doing things. As a war necessity, he yielded to it and performed like a good soldier. His interests were almost wholly scientific, but the needs of the situation forced upon him somewhat of an engineering role for which he had little appetite. As an adviser and consultant, he was unusually effective. In this he had few equals among scientists. His success here was due to the high level of his interest in solving problems, to his broad area of curiosity about physical phenomena, and to his warm, friendly, and unselfish interest in the scientific aspects of the work of his associates.

Industry's scientific and technologic support of the war effort led to a rapid expansion of industrial laboratories in the postwar period. The expansion of the laboratories at West Street during the war period was continued at a rapid rate throughout the following decade. The scientists who had come to the Laboratories during the war and the years immediately preceding it, with few exceptions, moved out of the laboratory and assumed places of management and leadership in the research and development sections. At that early period in the life of industrial laboratories, the major emphasis was on applied research and development; there was very little basic research.

Davisson was one of the few who did not gravitate to positions of management and leadership. He had no desire or taste for such assignments. His compelling interest in scientific research led Dr. Arnold to make a place in it for him, very rare in industrial laboratories of that time. A pattern of work of his own choosing gradually evolved, and he worked within it throughout his career. One or two young physicists and a few laboratory technicians made up the team that worked with him on his research problems. The young physicists and technicians did most of the work in his laboratory, although Davisson would frequently be found there making observations in association with his co-workers. He took a leading part in planning the experiments and in designing the apparatus. His thoroughness and absorbing interest in detail were especially rewarding in this area, for his experiments were always well conceived and their instrumentation was beautiful.

The maximum of reliability, long life (measured in years), and the highest electron-emitting efficiency from the cathode were early recognized as essential to the full utilization of the thermionic high-vacuum tube in telecommunications. For several years after the close of the First World War, Davisson's researches were directed toward a complete understanding of the emission phenomena of oxide-coated cathodes. This emitter is an unusually complex system. Chemical, metallurgical, and physical problems of great complexity are interleaved. Over the years, Bell Laboratories has made great progress in reliability, long life, and high electron-emitting efficiency of thermionic vacuum tubes for telecommunication uses. The benefits of this work to the telephone user have been large, and annual savings to the Bell System of many tens of millions of dollars have resulted. Davisson's researches during the five years following the close of the war and his continuing advice to others through a longer period were significant in the advances that the Laboratories has made.

As vacuum tubes with multigrid structures came into use and were placed in circuits of ever-increasing complexity, unwanted secondary electron emission from the grid structures became a major problem. The presence of this emission and its variation in amount from tube to tube brought about malfunctioning and unreliability. If it were to be controlled, its complete understanding was essential. A basic study of secondary emission was Davisson's next area of research. In these studies he came upon patterns of emission from the surface of single crystals of nickel that aroused his curiosity. His examination of these patterns led to his discovery of electron diffraction and the wave properties of electrons. In recognition of this masterful research with its important highly significant results, he was awarded the Nobel Prize in 1937.

After the discovery of electron diffraction, Dr. Lester H. Germer, who had worked with Davisson on the secondary-emission researches, took the problem of applying electron diffraction to the study of the structure of thin surface films. Under Davisson's inspiring but quiet leadership, Germer was the pioneer in utilizing electron diffraction in studies of surface structure and made a large contribution to the science and technology of this new and important analytical technique. After he had perfected an electron-diffraction spectrometer, he operated it for a number of years as an analytical aid to many of the research and development projects of the Laboratories. The interpretation of the patterns and the determination of the crystalline structure of surface films were complex problems. During the period that Germer was developing techniques and getting order into the analysis of the patterns, Davisson often joined him in puzzling out the crystal structure revealed in photographs of the diffraction patterns of many different kinds of surfaces.

As a logical consequence of Davisson's interest in electron diffraction, he next concerned himself with a variety of problems in electron optics. He was one of the first to develop analytical procedures in the design of structures for sharply focusing electron beams. For many years, beginning in the early 1930's, he gave much attention to the analytical side of electron optics and designed and constructed many structures for electron-focusing. Prior to his work, much of the vacuum-tube development in Bell Laboratories, as elsewhere where electron-focusing was required, was largely empirical. Unfortunately, he did not publish much of the fine work that he did, although he reported on portions of it to scientific and technical groups. However, the effects of his work and his ever-increasing knowledge of electron optics on the programs and men of Bell Laboratories concerned with electron dynamics were large. Dr. James B. Fisk, Dr. John R. Pierce, Dr. Leroy A. MacColl, Dr. Frank Gray and others of the Laboratories obtained guidance and inspiration from Davisson, the consultant and adviser.

Davisson's work in electron optics came at a fortunate time in relation to Bell Laboratories' studies of the transmission of television signals over coaxial conductor systems. Although it was possible to measure the amount and characteristics of the electrical distortion of signal currents, there were not available cathode-ray tubes precise enough in their design for evaluating the degradation in the picture's quality resulting from the passage of the signal through the coaxial system. He undertook the development of a cathode-ray tube for this test purpose, employing the principles of electron optics that he had worked out. In doing this, he made one of his few excursions into technology. There resulted from his work a cathode-ray tube of great precision. By virtue of the fundamental design of the beam and deflecting system, the tube provided an extremely small rectangular spot on the fluorescent screen that remained in sharp focus over the entire screen area and had a much improved response characteristic. He took unusual pride in this project and played a leading part in the design of every element of the complicated structure. The tube proved to be a useful tool in the evaluation of picture impairment resulting from different types of signal distortion.

Bell Laboratories steadily increased its participation in research and development activities for the military beginning in 1938. This effort expanded with terrific speed at the beginning of the Second World War and soon became its major activity, continuing until the close of hostilities. Davisson was most anxious to contribute in any way that he could to the Laboratories' military work. While continuing his researches, he turned his attention to the new and important multicavity magnetron that was receiving so much emphasis in Bell Laboratories. His background in electron optics made him invaluable as a consultant to Fisk, who led the magnetron work. As in the First World War, speed was again the driving force in the programs of the Laboratories, and substantially all of its research people turned to development. By keeping aloof from the rapidly moving development stream, Davisson was able to give unhurried consideration to many of the basic electron dynamics problems of the magnetron.

When Dr. John C. Slater joined Bell Laboratories in 1943 to participate with Fisk in the basic magnetron problems, Davisson then turned his attention to problems of crystal physics relevant to the Laboratories' development programs on quartz crystal plates as circuit elements. Bell Laboratories was the focal point of a large national effort for the development, design, and production of quartz crystal plates for a multitude of military electronic circuit applications. Dr. Warren P. Mason, Dr. Walter L. Bond, Dr. Gerald W. Willard, and Dr. Elizabeth Armstrong-Wood were the basic science team to whom Davisson gave invaluable consultation and inspiration. They worked on a multitude of problems that arose from the tremendous expansion of quartz plate application and production.

Davisson spent most of his time from 1943 until his retirement from Bell Laboratories in 1946 on a variety of crystal physics problems. He brought a fresh viewpoint to the crystal physics area. Through consultation, analyses, and experiments, he was of material assistance to the crystal physics group in the large contribution they made to the application of quartz plates to electronic systems for the military.

Davisson exerted a constructive influence on programs and men in many research and development areas of Bell Laboratories throughout the thirty years of his active service. His door was open to all, and through his constructive interest in the problems presented, he developed large and continuing consultant contacts. This was not an assigned task but rather one that was personal to him, and its amount and continuance through the years were expressions of a facet of his personality. His contribution to the adjustment of the young men to their change in environment from the university to industry as they came to Bell Laboratories was considerable. It became a habit of the research directors to place with him for a year or so junior scientists on their entrance to the Laboratories. Dr. Joseph A. Becker and Dr. William Shockley are typical of the men who were introduced into the Laboratories through a period of association with him.

Bell Laboratories always welcomes young scientists from the graduate schools of the universities for summer work. This gives them a view of the operation of an industrial laboratory, and is an aid to the Laboratories in the selection of young research men from the schools. Several of them were assigned to work with Davisson. Some have since had distinguished careers in science. Dr. Lee A. DuBridge, Dr. Merle Tuve, and Dr. Philip Morse are among the graduate students who worked with Davisson during their summer employment with Bell Laboratories.

It was fortunate for the Laboratories and for science that Davisson, who had come to stay for the duration of the First World War, elected to stay and work as a scientist in areas of physics important to its programs rather than return to university life. He established a pattern of fundamental research that has continued and enlarged in scope as Bell Laboratories evolved and reached maturity. Across the forefronts of physics, mathematics, and chemistry, which are basic to telecommunication technology, Bell Laboratories now has many scientists whose programs are directed, as were Davisson's, only at expanding fundamental knowledge, and who do not divert their energies even to the fundamental development phases of its technology. It is a tribute to Davisson's overpowering interest in science and to his steadfastness in the pursuit of knowledge through the scientific method of experiment and analysis, that during the pioneering and rapid expansion years of Bell Laboratories, when development demanded the attention of most of its scientists, he gave almost undivided attention to the scientific aspects of its work. Throughout his career, he remained a scientist and maintained a working knowledge at the forefront of a wide area of physics.

Throughout his thirty years at the Laboratories, Davisson's circle of friends among scientists steadily grew, not only within his own country but extending to Europe and the Orient. His capacity for friendships was large, and each scientist of the Laboratories in daily contact with him enjoyed a close friendship of exceptional warmth. The integrity and quality of his work are universally appreciated. He was held in high regard, not only for this, but also because of his fine personal qualities. He was shy and modest. Because of this, it required an association of some duration to know Davisson, the man. He had a keen sense of humor, which flashed upon you in most unexpected ways. Unusually slight of stature with a fragile physical frame, his weight never exceeded 105 pounds and for many years it hovered around 100. While his health was good, his store of energy was limited, and it was necessary for him to husband it carefully.

Davisson's modesty caused him to undervalue the importance and scope of his contributions. This characteristic, the low level of his energy, and the high standard he always set for his work, combined to limit the amount of his publication. His influence on science and technology generally was therefore not as great as it was within the

Laboratories, where his personal contact with individuals and their work was more effective than publication.

DAVISSON'S YEARS AT THE UNIVERSITY OF VIRGINIA AND CHARLOTTESVILLE

Upon Davisson's retirement from Bell Telephone Laboratories in 1946, he accepted a Visiting Professorship in Physics at the University of Virginia in Charlottesville. Professor Jesse Beams, Head of the Department of Physics, wished him to accept a professorship in residence, but the ever-shy and modest Davisson chose the more tenuous connection. The Davissons sold their lovely home in Short Hills, where they had spent some twenty-five happy years, and acquired a comfortable but smaller residence in Charlottesville. At the University, Davisson gave lectures in both the undergraduate and graduate schools, and directed doctoral thesis research work of graduate students.

Professor Beams, who greatly admired Davisson and, as he came to know him better through their close association, developed a deep feeling of friendship and respect for him, reports that Davisson entered most enthusiastically into the activities of the Department of Physics. Davisson was considered an excellent teacher, both by his colleagues in the Department and by the students.

"One might think," says Beams, "that because of his rather low speaking voice, the undergraduates would not like his classes, but his charm and thoroughness made his classes very popular, especially among the better students. He was of inestimable value here in constructively criticizing the research that was going on. He was always more than willing to help the graduate students and the staff with any physics problem they brought to him. Sometimes, when he did not immediately know the answer, he would come around in a few days with a complete answer all typed out in the greatest detail. He seemed to have the ability to concentrate deeply on any problem that interested him.

"Shortly after he came to Virginia, he became interested in using a magnetic suspension for the measurement of the gyromagnetic ratios in ferromagnetic material and worked out a most ingenious way of doing it. I recall that he calculated everything out in so much detail that the first time we put the apparatus together, it worked almost perfectly. My principal part of the experiment was to take the data while his was to analyze it, but every afternoon he turned up in the laboratory to help take the data. He always seemed very anxious to find out what the next data would be like. It was the same as some people who read serial stories and can't wait to find out what comes next."

After eight years as Visiting Professor at Virginia, Davisson again retired, and for the last time. He was now seventy-four and his physical strength, always low, was declining. But his mind was as keen as ever, and his scientific interest remained high as long as he lived.

Each summer the Davissons went to their beloved summer home in Brooklin, Maine, where the three Davissons—Davy, Lottie, and daughter Elizabeth—spent a happy three months. Davy always had some theoretical problem that challenged him. He would sit by the hour with a writing pad and pencil working through different lines of attack. Mrs. Kelly and I spent a day with them in the summer of 1957, only a few months before his death. He was indeed frail in body, but not in spirit. As of yore, he was attempting to solve a problem in physics that intrigued him.

He died peacefully while asleep at their home in Charlottesville on the night of February 1, 1958. Thus ended the life of a truly great physicist. He was the first scientist at Bell Telephone Laboratories who had remained, in every sense of the word, a truly basic scientist, active in research throughout a career there extending over twenty-eight years. He might well be called the father of basic research at Bell Telephone Laboratories.

SOME PERSONAL RECOLLECTIONS AND EVALUATIONS BY THE BIOGRAPHER

I came to the Western Electric Company's Engineering Department in January, 1918, only a few months after Davisson. We officed together during our first few years. A friendship developed that endured throughout the remainder of Davy's life. He was perhaps my closest friend throughout this period, and the association will always be one of my most prized memories. Our homes in Short Hills were separated by a little more than a mile, and there were family associations most intimate and highly valued by the Kellys.

Davy had an enduring interest in physics and research. In the early years of the Laboratories at West Street, pioneering years of an industrial laboratory, there was not an atmosphere conducive to detached basic research, but Davy, through his love of research and his devotion to physics, continued to work on the problems that intrigued him with only a small staff. His dedication was greatly appreciated by the administration. The successive Directors of Research —Arnold, Buckley, and then I—gave him complete freedom of choice in problems and actions concerning them. Only during the periods of the two world wars and once in the thirties, when his interest in electron optics led him to develop a precision cathode-ray tube for television viewing, did he enter the applied physics area.

Davisson rendered a valuable service by introducing to the industrial laboratory some of the nation's most promising young physicists. Becker, Shockley, DuBridge, Tuve, and Morse are typical of the young scientists assigned to work with him.

Davisson's thoroughness was a most outstanding characteristic. He planned every experiment in the greatest detail before undertaking it. The precision of this planning is almost unbelievable. He was never in a hurry to publish, and established his conclusions by check and rechecks in a most painstaking way before he would announce them. The description by Professor Beams of Davy's way of work at the University of Virginia admirably portrays his ways at Bell Telephone Laboratories.

Since he always had marginal physical stamina, Davy worked slowly. He described himself as lazy, which was not true. He worked at a slow pace but persistently. This pace was suited to his limited store of energy. He was frequently at home for short periods because of minor illnesses, but always worked there at theroetical aspects of his problems or at plans for the next experiment. When I visited him at his home at the time of an illness, invariably I would find him in dressing gown, writing pad on his knee and pencil in hand, smoking his pipe and puzzling over his problem.

He enjoyed his family and intimate friends. He did not have time to waste with those he found uncongenial, but the fortunate ones whom he did find congenial had great pleasure in his company.

Davisson had a good life and a highly productive one in science. His own work and his influence on and contribution to the work of others will long be living testimonials of his value to the world in which he lived.

A PERSPECTIVE OF DAVISSON'S SCIENTIFIC WORK

Contributed by Karl K. Darrow

The very first piece of work 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 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-focusing 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 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 *The 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. This work ended the contributions of his student years, and next we find him publishing as an independent investigator.

From Davisson's years at the Carnegie Institute of Technology (1912–17) there is a paper embodying an attempt to calculate the optical dispersion of molecular hydrogen and 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 May, 1917, in the midst of the First World War, Davisson came for what he thought would be a temporary job to the New York laboratories of the Western Electric Company, thereafter, from 1925, the 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 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 by 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

$$T^{1/2} \exp(-b/T)$$

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 the 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 expenses 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 $T^{1/2}$ 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 is 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 emission of electrons from 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 an American Physical Society 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 constant *b* obtained from the newer form of "Richardson's law," and 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 α 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 α. 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 α 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 α over a range of this angle which extends from -90° to $+90^\circ$; but the part of the curve which runs from $\alpha = -90^\circ$ to $\alpha = 0^\circ$ is the mirror image of the other part, and either by itself suffices. The curve cannot be plotted in the immediate vicinity of $\alpha = 0^\circ$, 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 $\alpha = 0^\circ$ and presumably pointing in exactly that direction, consisting therefore of electrons which had been turned clear around through 180°; 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 also 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, just as though the shell were not there at all; outside 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 $\alpha = 0°$, so long as the speed of the electrons did not exceed a certain ceiling value! And there was more: "the main features of the distribution-curves (scattering-patterns) 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 reasonably 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 would 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 (1927) 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 *The 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 recrystallization 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 *The 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 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 did also 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 return transatlantic voyage Davisson spent his time trying to understand Schrödinger'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 the sixth of 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 Erwin Schrödinger to the discovery of wave mechanics, but I mention it here because Schrödinger'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 Göttingen. 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 1 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 explanation. 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 *The 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 1 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 noncrystalline 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 in which Davisson exhibited much interest. He frequently puzzled out with Germer the crystalline structure and composition of surfaces under study.

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

The work which an electron must do in order to quit a metal has been mentioned; two ways were 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 nonzero amount, which will be denoted by X. One is tempted to identify X with ϕ and with kb/e; but this is an oversimplification. 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 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 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, 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, three notes should be mentioned—two abstracts of papers given before the American Physical Society in 1931 and 1934, and one letter to the Editor of *The Physical Review* bearing on what has been described, 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.

HONORS

Fellow of the American Physical Society
Member of the National Academy of Sciences (elected in 1929)
Member of the American Philosophical Society

PRIZES

Comstock Prize, National Academy of Sciences, 1928
Elliott Cresson Medal, Franklin Institute of Philadelphia, 1931
Hughes Medal, Royal Society (London), 1935
Nobel Prize in Physics, 1937
Alumni Medal of the University of Chicago, 1941

HONORARY DEGREES

D.Sc., Purdue University, 1937
D.Sc., Princeton University, 1938
Honorary Doctor, University of Lyon, France, 1939
D.Sc., Colby College, 1940

KEY TO ABBREVIATIONS

Bell Lab. Record = Bell Laboratories Record
Bell Sys. Tech. J. = Bell System Technical Journal
J. Appl. Phys. = Journal of Applied Physics
J. Franklin Inst. = Journal of the Franklin Institute
J. Opt. Soc. Am. Rev. Sci. Instruments = Journal of the Optical Society of America and Review of Scientific Instruments
Phil. Mag. = Philosophical Magazine and Journal of Science
Phys. Rev. = The Physical Review
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Sci. Monthly = Scientific Monthly

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