Elephants and mahouts —early days in semiconductor physics

Research on band structures of diamond-type crystals and germanium-silicon alloys, undertaken in the 1950s to advance the development of transistors, led to a surprisingly different outcome.

Frank Herman

Since World War II, better understanding of the properties of solid and liquid matter has wrought a virtual revolution in microelectronics and materials science. Somewhat surprisingly, however, the rapid developments in the field have been relatively neglected by historians of science. To right this wrong, a group of historians of science have organized the International Project on the History of Solid-State Physics, and since 1980 they have been working in Paris, Munich, New York, Urbana (Illinois) and Birmingham (England), conducting interviews with scientists, gathering papers on the subject and preparing scholarly accounts for publication. Their work will culminate with a book on the history of solid-state physics to be published by Oxford University Press and a catalog of source materials by the American Institute of Physics.

As part of this ongoing project, Frank Herman, a research physicist at the IBM Research Laboratory at San Jose, California, was interviewed by Kris Szymborski, a member of the US group now working at the University of Illinois in Urbana. A pioneer in the application of modern computers to the study of the band structure of solids, Herman had predicted theoretically in the 1950s, while working at the RCA Laboratories in Princeton, New Jersey, that diamond and germanium are indirect gap materials. This led to a more complete understanding of the properties of semiconductors. Herman's work has been conducted entirely in industrial laboratories, and the story he tells illustrates how fuzzy the distinction is between pure and applied science.

The accompanying article is based on Szymborski's interview with Herman.

I didn't really decide to become a physicist until I was about 23 years old, working as a research engineer at the RCA Laboratories in Princeton, New Jersey. To understand my decision, you will need some background. Ever since I was a boy I was fascinated by the operation of machines. I liked to take them apart, and often I was even able to put them back together again. I used to make model airplanes and construct mechanical toys. Originally, I wanted to become a civil engineer and design complex mechanisms.

My interest in mathematics and science took a big leap forward when I was admitted to the Bronx High School of Science in New York City and fell under the spell of outstanding teachers who had been brought together in the late 1930s to form a specialized high school. The school remains one of the best of its kind in the world. (See PHYSICS TODAY, February 1983, page 53.) Not only were the teachers inspiring, but the students were exceptional, having been selected by stiff competitive examinations. Many of my classmates went on to become scientists and engineers. One of my high-school (and college) classmates was Harold Brown, who later became president of Caltech, then Secretary of Defense. Others who come to mind are Norman Austern, Joseph L. Birman, and Sidney A. Bludman, now professors of physics at, respectively, Pittsburgh University, City College of the City University of New York, and the University of Pennsylvania.

I was graduated from Bronx Science in January 1943—a member of its fourth graduating class. I continued my education at Columbia University, where I received a bachelor's degree in electrical engineering in the fall of 1945 just after the end of World War II. During the war, Columbia, like other universities, had an accelerated academic program, which enabled me to complete four years of college in less than 3 years. I then spent the next two years in the Navy, attending electronics schools and doing electronics at various places in communications security. By the time I concluded my Navy hitch in the fall of 1947, I knew quite a bit about the practical side of electronics and, in fact, was able to modify and repair some complex electronic equipment, including advanced radar and sonar systems. The experience complemented my earlier academic studies by placing me in contact with the practical aspects of electronics.

Once out of the Navy, I returned to Columbia to work toward a master's degree in electrical engineering, which I received in 1949. During that period I also taught electrical engineering at the Cooper Union for the Advancement of Science, a small, outstanding engineering school in New York City. One of my colleagues at Cooper Union was Jesse B. Sherman, who had once worked as an engineer at RCA. Sher-

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The author constructs a molecular model of crystals in this photograph taken in 1961 at the David Sarnoff Research Center of RCA.

man suggested that I might enjoy doing research for a large electronics company, and he urged me to apply for a position at the RCA Laboratories.

I was hired by RCA Laboratories in June 1949 as a research engineer. I spent my first six months in experimental research on gaseous electronics under Louis B. Malter, and my second in the new field of semiconductor electronics under Arnold R. Moore. By this time I was more interested in physics than in engineering, so I enrolled at Columbia in December 1949 as a candidate for a doctorate in physics. During my first year and a half at RCA, I completed the PhD requirements in a most unusual way. I did all the necessary laboratory work at Columbia on Saturdays, but I did not attend any classes during the week. I had made a special arrangement with my teachers: As long as I did all the homework satisfactorily and got high grades on my exams, my absence from class would be overlooked. I was grateful to Columbia for making this accomodation, which enabled me to work full time at RCA while continuing my studies.

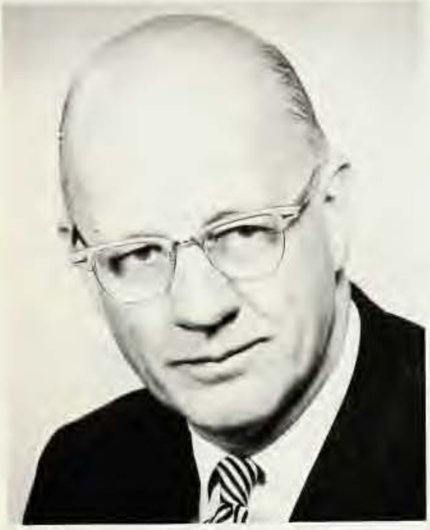
I should mention that I did not enjoy doing research on gaseous electronics because I was left to myself and did not feel I was part of a vital activity. On the other hand, once I began working on semiconductors with Moore, I became enthralled and decided to go into solid-state electronics or solid-state physics. By a happy chance, Moore and I carried out an experiment that applied television techniques (electron-beam scanning in a cathode-ray tube) to semiconductors. Because of the television aspect, this work was highly appreciated by RCA. We put a germanium point-contact rectifier inside a cathode-ray tube as a target, connected the rectifier to an external circuit, and scanned the electron beam across the germanium sample. When the electron beam came close to the point-contact, it generated electron–hole pairs, causing an increase in current through the external circuit. Moore and I received a patent for a device based on electron-bombardment-induced conductivity. This patent was to play an important role in my career.

Amid the RCA elite

After completing my six months with Moore, I joined the physical electronics group and reported to Dwight O. North, who was known affectionally as "Don," after his initials. North was a brilliant theoretician who had carried out pioneering studies in noise theory before and during World War II. I was delighted to join this group because it consisted of some of the most eminent RCA scientists, including Albert Rose, the inventor of the image orthicon and an authority on vision; Leon S. Nergaard, an expert on high-power tubes; and Henry T. Devore, an authority on photoconductivity.

Most of the scientists at RCA had made their reputations in vacuum science and technology, electronic circuitry and acoustics. They had invented and designed high-power tubes, television-camera tubes, and gas-discharge devices. They also had made detailed studies of thermionic emission, cathodoluminescence and photoconductivity. Research on these subjects was still in full swing in the early 1950s. More important to RCA at the time was a laboratory-wide crash program on color television. My immediate colleagues were among the world's leading authorities in their fields, but they lacked a strong background in the quantum theory of solids. In retrospect, I was lucky to have worked with Moore, who had gotten his degree at Cornell working on color centers and was thus one of the few physicists with a solid-state background at RCA.

The transistor had been invented a few years earlier (in 1947) at Bell Telephone Laboratories. Few inventions, as it turned out, would have a greater impact on the electronics industry. With it the entire industry moved rapidly to use semiconductor devices. Accordingly, many senior people needed to learn more about semiconductors; and to that end a number of us organized courses in quantum mechanics, semiconductor electronics and solid-state physics. The challenge was to teach scientists and engineers with strong backgrounds in classical physics



Frederick Seitz, whose *Modern Theory of Solids* became a guidebook to entering the uncharted frontiers of electrical conductivity.

and vacuum electronics the fundamentals of the quantum theory of solids. Because they were highly motivated, most of the senior people succeeded in making a graceful transition from their earlier fields of expertise into the world of semiconductors. From my own point of view, the courses proved useful: They greatly facilitated my getting into solid-state physics, and they made up for many of the courses that I had taken "*in absentia*" at Columbia.

After joining the physical electronics group, I began looking for a suitable research problem—one that would be relevant to RCA's interests, acceptable as a thesis problem at Columbia and at the same time intellectually challenging to me. Initially, I intended to do a theoretical study of the change that electron bombardment induces in the conductivity of germanium, extending the work I had done earlier with Moore. This topic was close enough to the experimental activities at my lab that it received RCA's blessing. By way of getting into research or electrical conductivity, I read Frederick Seitz's *Modern Theory of Solids* literally from cover to cover, as well as current literature on the electronic and optical properties of germanium. I became interested in band theory principally through the influence of three textbooks—Seitz's book, Leon Brillouin's *Wave Propagation in Periodic Structures* and Alan Herries Wilson's *Theory of Metals*. From my readings, it became clear to me that we knew little about the electronic structure of germanium and that many interpretations of the physical properties of germanium were based on highly simplified band-structure models of doubtful validity. As in football, when a hole is found in the line, the best move is to charge through.

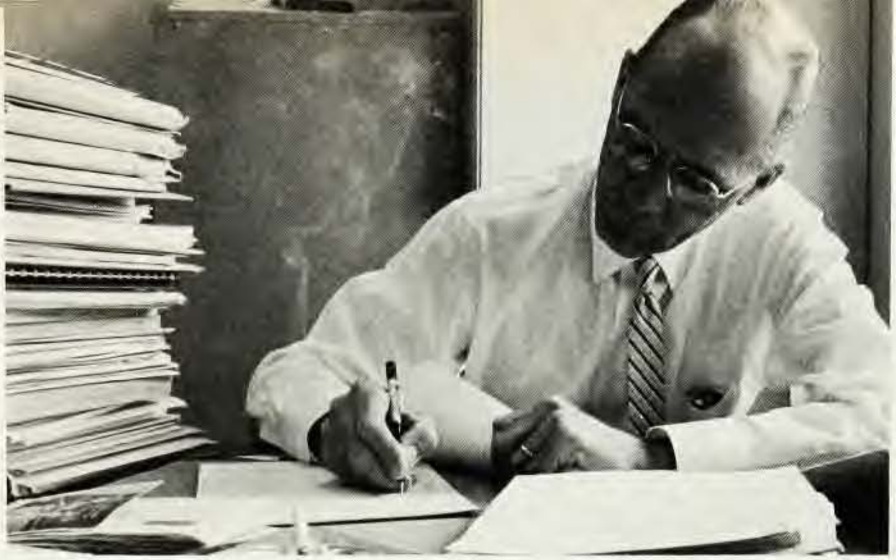
At the time there were few people at RCA or Columbia that I could talk to about germanium. My reading suggested that band structure played an important role in determining many of the electronic and optical properties of solids. It was evident that our understanding of the band structure of actual materials such as germanium—as opposed to idealized textbook models—was rudimentary. Details of band structure affect transport properties, such as magnetoresistance, and optical properties, such as the shape of the absorption curve just above threshold. Such phenomena were than beyond our ken. I couldn't get very excited about the phenomenological analysis of transport measurements, for example, but I became intrigued at the prospect of obtaining more definitive information about the band structure, and I hoped to contribute to a better understanding of germanium and related materials.

Calculating with a slide rule

In the final analysis, my choice of problem was highly subjective and was determined in part by the experimental situation at the time, by the research atmosphere in which I found myself, including the people and the facilities and, perhaps most important of all, by my own temperament. I wasn't out for a Nobel Prize. I simply wanted to do something theoretical that would lead to a substantial result—one that could be verified experimentally and move the field forward. In view of the nature of the problem, it was clear that I had to carry out what today would be called a large-scale calculation, armed only with a slide rule and desk calculator. I wanted to work on a problem that I could solve all by myself, in keeping with the spirit of a doctoral dissertation.

The first issue that had to be resolved was whether RCA would approve of this project, which was considered "blue-sky" for that time. Fortunately, RCA managers remembered my success in getting a patent on the electron-bombardment induced-conductivity device with Moore shortly after joining the company. They reasoned that I was a practical person who would generate other patentable ideas in the course of time, particularly if I became active in a new, rapidly developing subject. So RCA agreed to the project, and North, as my supervisor, accepted the responsibility of guiding my research, even though his own expertise was in noise theory and applied mathematics. As it turned out, Don was extremely supportive, providing mathematical guidance and considerable encouragement. He was also an excellent writer, and his insistence on high literary standards improved my ability to write scientific articles clearly. But I was certain that if it hadn't been for my patent on electron conductivity, it is not likely that RCA would have permitted me to work on such a far-out project as the band structures of diamond and germanium. In any event, I now had a research problem acceptable to RCA.

The next step was to find a thesis adviser at Columbia. The only member of the Columbia physics faculty who had any professional interest in solid-state physics was an elderly professor, Shirley L. Quimby, who taught an excellent course in analytical dynamics. He was interested in the elastic properties of copper, but he had no interest whatsoever in the quantum theory of solids. After extensive discussions with various physics professors, I persuaded Henry M. Foley to become my thesis adviser. He was an expert on hyperfine structure in atoms and molecules and had a strong quantum-mechanical background. He regarded my work as a challenge and as an opportunity to learn something himself about solid-state physics. Foley proved to be an excellent adviser. Actually, I had three advisers—one at Columbia, a second at RCA Laboratories, and a third at Bell Telephone Laboratories, whom I will come to later. I began working on the band-structure calcula-



Dwight North, whose theoretical studies of the behavior of solid-state materials at the RCA Physical Research Laboratory were often hailed as "brilliant."

tions in the fall of 1950. Because of its more complicated core structure, I decided to defer study of germanium until I had dealt with its simpler prototype—diamond.

In doing this work I had to chose a computational method. It so happened that in 1935 George E. Kimball, then a graduate student at MIT working under the direction of John C. Slater, carried out a pioneering calculation of the band structure of diamond using Slater's cellular method.[1] Even though this calculation was rather crude, leading to a forbidden band width of about 50 eV (the experimental value was about 5.4 eV), it provided the model that was used to represent germanium during the late 1940s and early 1950s. According to this model, elemental tetrahedrally coordinated materials such as diamond, silicon and germanium are direct-gap insulators or semiconductors whose valence and conduction band edges are located at the center of the reduced zone. During the mid 1930s William Shockley, then also a student at MIT working with Slater, calculated the band structure of sodium chloride and found the top of the p valence bands to be degenerate and to lie at the center of the reduced zone.[2] He also analyzed the complicated nature of the fluted dispersion curves in the neighborhood of the valence-band edge. In the light of Shockley's results for sodium chloride, it is surprising that nobody worried about the possible consequences of an analogous degenerate valence band edge in diamond-type crystals in the late 1940s and 1950s.

After I became interested in the band structure of diamond in the fall of 1950, I spoke to Kimball, who was then a professor of chemistry at Columbia, about his classic 1935 calculation. Kimball said that his calculation explained why diamond was an insulator, but was not to be trusted quantitatively because the calculation was at a primitive state. He also said that I could probably carry out a more accurate cellular calculation than he did 16 or so years earlier, but it would require a great deal of computational effort.

Ironically, the computational tools at my disposal at RCA in the early 1950s consisted of slide rules and desk calculators—essentially no better than those used by Kimball at MIT in 1935. In retrospect, it was lucky that I didn't use the cellular method, because it would have taken an enormous amount of computational effort to get accurate results, as I discovered about 25 years later while using large electronic computers. Incidentally, these later calculations, done with Jose R. Leite and Baird I. Bennett at IBM, confirmed the numerical accuracy of Kimball's results, but showed how inadequate his basis set and choice of boundary conditions really were.

Calculating by OPW method

So I proceeded to examine other computational methods, and I rejected one after another because they required excessive computational effort or were based on empirical parameters that could not be estimated reliably. Thus, first-principles "linear combination of atomic orbitals" or "augmented plane wave" calculations would have required evaluation of multicenter integrals or many integrations of the Schrödinger equation, while the semiempirical LCAO method would have been based on highly uncertain parameters. Finally, I came across Conyers Herring's beautiful 1940 paper on the "orthogonalized plane wave" method, and another paper written the same year by Herring and Hill on its application to metallic beryllium.[3]

With the OPW method, I could use existing self-consistent Hartree or Hartree–Fock atomic wave functions to represent the valence- and conduction-band orbitals in the ion core region, bypassing the need to integrate the Schrödinger equation from scratch. (This is not rigorously true, but represents an excellent first approximation.) It was necessary to calculate Fourier transforms of atomic orbitals, but this could be readily done with a desk calculator, as could the calculation of the OPW matrix elements. The major computational problem would be evaluating high-order secular equations.

It was difficult to estimate the highest order in advance, but I anticipated secular equations of order 150×150 or even 200×200. It all depended on how rapidly the characteristic solutions converged, particularly those representing pure p-type valence- and conduction-band states. For these, the orthogonalized plane waves reduced to ordinary plane waves, and in principle it might take thousands of plane waves to reach effective convergence. Although I never did achieve convergence of energy levels on an absolute scale, I did reach effective relative convergence, so that I could determine energy-level differences (interband separations) reasonably well. The key, of course, was to factor the high-order secular equations using group-theoretical methods. To learn how to do this, I taught myself group theory and, in the course of reading the literature on space groups, came across some interesting papers by Herring, one dealing in fact with the diamond space group.[4] As it turned out, the symmetrized OPW secular equations ranged in order from 4×4 to 16×16 or so. Although I still had a formidable computational job facing me, it was one that I felt confident I could handle, using primitive electromechanical computers such as those located at the IBM Thomas J. Watson Computing Laboratory at Columbia. It was quite some time before electronic computers made their appearance at the RCA Laboratories in Princeton.

After getting into the calculations, I met Conyers Herring for the first time, as I recall it, at a meeting of the

Conyers Herring of Bell Laboratories, who is characterized in this article by Herman as "a virtual encyclopedia of solid-state knowledge."

American Physical Society.[5] I told him that I was applying the OPW method to diamond and would be interested in his opinions and advice. Herring was somewhat surprised at first that I was attempting to treat a covalent crystal by a method he had developed for metals, but he encouraged me to try the OPW method on diamond in the expectation that the results would be interesting. He also agreed to serve as an informal thesis adviser. I now had a veritable troika of advisers, consisting of North, my RCA supervisor; Foley, my formal thesis adviser at Columbia; and Herring. I was delighted to have the opportunity to talk about the OPW method with its orginator and about group theory with an expert.

Herring was then at Bell Labs in Murray Hill, New Jersey, just about an hour's drive from Princeton, and he invited me to visit him. When I came to see him, which I did many times during my doctoral studies, I was always impressed by the number of people lined up in the hallway outside his office waiting to ask him questions on a wide variety of scientific subjects. Herring was a voluminous reader, having probably read every important paper in solid-state physics. He had a dispatch case containing hundreds if not thousands of 3 × 5 index cards, each containing the essential information culled from a particular paper. Herring was a virtual encyclopedia of solid-state knowledge. He would provide theoretical advice, constructive criticism, and useful references on an astonishingly wide range of topics.

North and Herring both had carved out creative and productive careers as theoreticians in industry. Their successes showed me clearly that one could aspire to be a theoretical solid-state physicist in industry equally well as at a university.

To return to the calculations, all went smoothly until I had to face up to the problem of factoring high-order secular equations. I knew how to do this by hand, but the task was time-consuming, and it was necessary to check and recheck the factoring to make such no errors were made. (Years later I devised ways for automating this process, using large electronic digital computers, but in 1951 I hadn't yet learned how to use "automatic" computers.) It then occurred to me that my mother could help me with some of this work. I had read about the Hartrees—how the younger Hartree (Douglas R.) had been aided by his father (William), who was a retired railroad engineer and enjoyed doing "sums" on a desk calculator. I showed my mother how to set up the OPW secular equations and how to factor them, and she agreed to do some of this in her spare time in order to save me time.

Mother, the committed computer

Now my mother was by no means a mathematician, but she had enormous patience and an aptitude for numbers, and she was only too happy to help me out. So she did the factoring by hand—the total effort may have taken her many hundreds of hours—and I was saved many weeks, if not months of detailed work. Many years later, when I repeated these calculations using electronic computers, I found that my mother had not made a single mistake. I wanted to acknowledge my mother's assistance in my doctoral dissertation, but she wouldn't let me. So I am happy to have the opportunity to do so now, however belatedly. In 1954, at Cambridge, I had a conversation with D. R. Hartree and I told him how my mother had helped me in the same way that his father had helped him.

Even though the factored secular equations were of much lower order than the original unfactored ones, it was still impractical to evaluate them on a desk calculator. So I obtained permission to spend a few months at the Watson Lab at Columbia in early 1952, writing programs and carrying out the numerical work on early electromechanical computers such as the IBM 602A. I greatly enjoyed learning to program this computer, which involved wiring program boards. How different from today! Finally I was able to obtain the electronic eigenvalues and eigenvectors for diamond, and from these emerged the band structure of diamond.

There were many side benefits to my sojourn at the Watson Lab. First and foremost, I met my future wife, Sondra, who was then a senior at Barnard College. Secondly, I met Leon Brillouin and Llewelyn H. Thomas, both of whom were on the staff at the Watson Lab and were also affiliated with Columbia. I had many interesting conservations with both of them. I vividly recall Brillouin discussing his development of the zone concept. In retrospect, he was delighted that a purely mathematical construct could play such an important role in determining the electronic structure of solids, with such dramatic physical and technological consequences, ranging all the way from the existence of holes, to roles played by electrons and holes as minority and majority carriers in semiconductors. Here were the practical effects in transistor action itself. Finally, the visit to the Watson Lab reinforced my interest in large-scale scientific computation, a consideration that greatly influenced my future career.

Incidentally, about the time I was completing my doctoral studies at Columbia, I was offered a position at the IBM Watson Lab by Polykarp Kusch, then a consultant to the director, Wallace J. Eckert. I sometimes wonder what turn my career would have taken if I had accepted this job offer and joined IBM in 1953 instead of 16 years later. As it was, I felt extremely loyal to RCA and turned down the offer. Some of my Columbia classmates—Robert Gunther-Mohr, Seymour H. Koenig, Sol Triebwasser, and Gardner L. Tucker—did join IBM at the time.

After completing my diamond calculations, I went on to study the band structure of germanium in the summer of 1952, assisted by a young graduate student at Princeton, Joseph Callaway, who has since made a name for himself in theoretical atomic and solid-state physics. My doctoral dissertation covered both the diamond and germanium band-structure calculations. My final oral examination took place in the fall of 1952. The doctoral committee included Brillouin, Foley, Kimball, Thomas, and, as chairman, Charles Townes. My research clearly indicated that the band structure of diamond was quite different from the one calculated by Kimball. While Kimball's pioneering studies suggested that diamond was a direct-gap insulator, with the lowest conduction-band level lying at the zone center, my calculations showed that the lowest conduction band level lay somewhere along the [100] axis in the reduced zone, away from the zone center. My calculations also indicated that the edge of the valence band in diamond was degenerate and occurred at the zone center; thus diamond has the same degeneracy found earlier for sodium chloride by Shockley.

The results for germanium were similar, except that the calculations were too crude for us to decide whether the lowest conduction-band states occurred along the [100] axes, as in diamond, or along the [111] axes, as was later shown to be the case in laboratory experiments.

Kimball questioned me closely about my calculations and in the end accepted the findings as quite reasonable. In doing so, he emphasized that, in contrast to mathematical theorems, numerical calculations inevitably were based on approximations and that my results were more reliable than his because I had used fewer and better approximations as well as more powerful numerical methods. To emphasize his open-mindedness, he predicted jokingly that others would publish still better calculations for diamond, which might or might not confirm my results. Actually, subsequent calculations as well as subsequent experimental studies all confirmed the essential correctness of my indirect-band-gap model for the diamond crystal, though some of the numerical details were certainly improved.

The first papers on diamond and germanium appeared in *Physical Review* in December 1952 and January 1953, respectively.[6,7] These calculations were important because they showed for the first time that diamond and germanium had multivalley conduction-band structures, and hence were indirect-band-gap materials. I received my PhD degree in physics from Columbia in January 1953, and my doctoral dissertation was published[8] in *Physical Review* in 1954.

In late 1952, I wrote a letter to Seitz, informing him of my results. Being closer to solid-state physics than Kimball, who was by then more concerned with chemistry, Seitz recognized the technical as well as the scientific implications of my band-structure results. I also discussed my results with Herring, who was very close to the semiconductor research at Bell Labs and also appreciated such implications as, for instance, the effect of complicated band structures on electron and hole scattering and hence on electron and hole mobilities.

After the diamond and germanium results were published, I received many invitations to lecture about the band structures. It was exciting to discuss the results with experimentalists and to understand how they were planning to investigate the detailed band structure of diamond-type crystals. Shockley was also interested in the results. He and Herring realized that a degenerate valence-band edge and a multivalley conduction-band structure could account for some of the puzzling effects observed in magnetoresistance and other types of measurements on germanium. Herring went on to do important theoretical work dealing with transport properties in multivalley semiconductors.

Arnold Moore, who became Herman's mentor in the new field of semiconductor electronics at the RCA David Sarnoff Laboratory in the early 1950s.

Breaking a company tradition

Shortly thereafter, I received an invitation to present the leading invited paper at the International Conference on the Physics of Semiconductors, to be held in Amsterdam in the summer of 1954. When I showed the invitation to my bosses at RCA, their initial response was that young scientists did not get sent to international conferences. Such honors were reserved for senior scientists and managers. It wasn't a question of money, for the conference had offered to pay all my expenses. It was a question of tradition. Eventually, however, the RCA management realized that my participation at Amsterdam would be a singular honor for RCA as well as for me, and they relented. I felt highly privileged to be an invited speaker, with an opportunity to meet such distinguished European scientists as Hendrik B. G. Casimir of the Netherlands, Georg Busch of Switzerland and Heinrich Welker of Germany. Thus, my wife and I took our first trip to Europe, and we visited many legendary figures and places, including the Hartrees in Cambridge.

The Amsterdam conference was the first at which the band structure of semiconductors figured significantly. Having relied on simplified models for so long, people were just beginning to realize that the band structure of actual materials could be quite complicated. They also began to realize that it was essential to understand such complicated features thoroughly if they ever hoped to interpret electronic and optical experiments properly. The highlight of the Amsterdam conference was the discussion of our improved understanding of the band structure of germanium, based primarily on experimental work by the Bell, Berkeley and MIT groups, along with my own theoretical work. (At the 16th International Conference on the Physics of Semiconductors, held at Montpellier, France, in September 1982, Pierre Aigrain also characterized the 1954 Amsterdam conference in precisely these terms in his plenary address.) Experimental and theoretical studies of semiconductor band structures represented one of the major scientific themes for the next decade and a half.

On a more personal level, the greatest benefit of attending the conference was meeting experimentalists and theoreticians interested in fundamental semiconductor research. At this and subsequent national and international conferences, I formed friendships that I have maintained to this day. Such friendships represent a vital communication network, providing advance information on research in progress and suggesting talented postdocs looking for jobs. I also remember with particular pleasure the after-dinner speeches by Casimir on broken English and Busch on the current state of semiconductor research. I was fascinated by Casimir's urbanity and by Busch's facility to switch from English to French to German to Swiss dialect virtually in mid-sentence. Up to that time I had stood in awe of great men of science. It was refreshing to meet so many of them face to face and discover their human side.

After Amsterdam, I continued to

study the band structure of semiconductors and learned quite a bit more about diamond-type crystals and alloys. Let me illustrate this period in my life with a few incidents. Two of my RCA colleagues, Everett R. Johnson and Schuyler M. Christian, showed me some optical absorption curves for a series of germanium–silicon alloys covering the entire composition range from pure germanium to pure silicon. The striking feature about the absorption threshold (band gap) vs. composition curve was a discontinuity at about 15 atomic percent silicon. The curve was roughly linear on both sides of this composition, but the slopes were significantly different above and below this point. Having found from my theoretical studies that different types of conduction-band minima responded differently to various types of perturbations (changes in crystal potential), I was able to interpret this discontinuity.[9]

The picture of the band structure of germanium–silicon alloys was subsequently confirmed by magnetoresistance measurements carried out by Maurice Glicksman, who had come to RCA after working in nuclear physics with Fermi at Chicago. More detailed studies of optical absorption were later carried out at RCA by Ruben Braunstein, Moore, and myself, and much additional work on the germanium–silicon alloy system was done by others still later at RCA and elsewhere. Initially, it was thought that these alloys would make better transistors than silicon or germanium, because suitably chosen alloys would combine the high-temperature performance of silicon with the higher mobility of germanium. It was eventually realized that the alloys were unsuitable for transistors because carrier mobilities were greatly reduced by disorder scattering, so research on such alloys was discontinued. Somewhat later, it occurred to RCA scientists, particularly Fred Rosi, that the alloys would make excellent thermoelectric elements for auxiliary power sources in spacecraft, and indeed this is one of their major applications today. This story clearly illustrates how fundamental research begun with one practical objective in mind ended up with an entirely different and wholly unanticipated practical outcome.

My explanation of the optical threshold of germanium–silicon alloys showed that such disordered semiconductors actually did have band structures that are well enough defined in the reduced zone to exhibit different conduction-band symmetry properties, as evidenced by their magnetoresistance. When I first discussed the band structure of such alloys in 1954, many people said that semiconductor alloys didn't have band structures because of disorder. Of course, the fluctuating potential associated with the disorder in the germanium–silicon alloy system is rather small. Moreover, the effective atomic potentials of silicon and germanium—or pseudopotentials, as they would be called today—are rather similar, so that substitutional replacement of one atom by the other introduces only a weak perturbation locally. This all seems rather obvious today, but it came as a big surprise to many people in 1954.

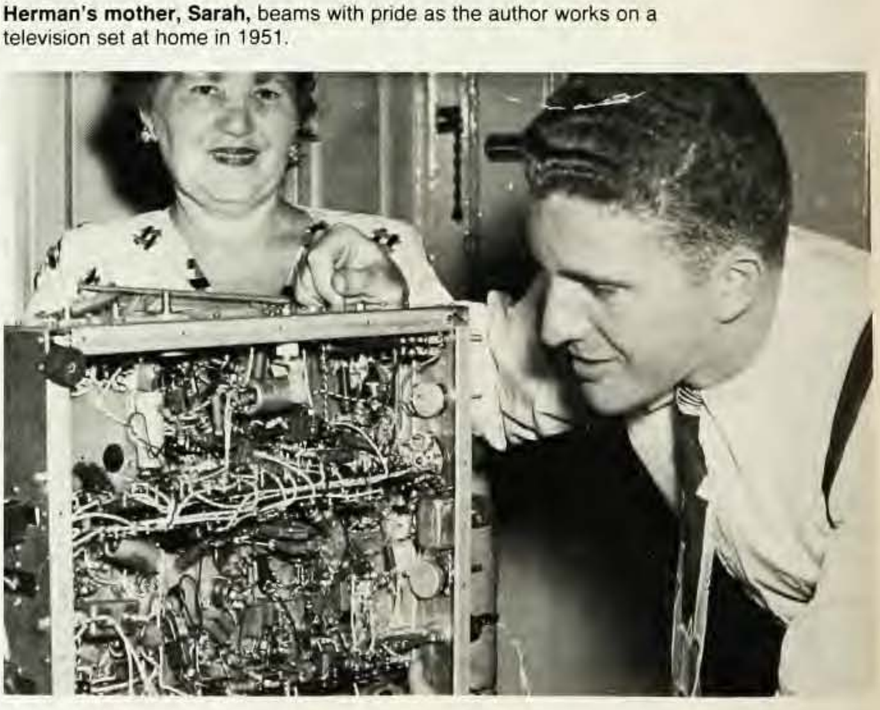
Pride in a theoretical idea

Another perturbation that I studied was associated with the hypothetical transformation of a group-IV elemental semiconductor such as Ge into other members of its isoelectronic series—in this case GaAs, ZnSe, and CuBr. Because the lattice constants of the members of this series are essentially the same, I assumed that the symmetric part of the potential in the unit cell (which contains two atoms) remains the same for all members, while the antisymmetric part becomes increasingly larger as one proceeds along the series. This work was important because it showed that the band structures of structurally and chemically related semiconductors could be related to one another in a very simple and physically compelling manner. I have always been very proud of this particular theoretical idea, which was published[10] in 1955. It implied that if you have paid your dues and done detailed calculations for a few representative crystals, you could draw many important conclusions for a wide class of materials without doing additional detailed calculations.

At that time I was in touch with several individuals interested in group theoretical aspects of the band structure of diamond-type crystals. In addition to Herring, they included Roger J. Elliott and Gene F. Dresselhaus, both then at Berkeley, and others from Japan, especially R. Sugita and E. Yamaka. Some of the formal work by scientists in Japan had been published only in Japanese, though translations into English were sent to certain scientists here. I also maintained close ties with theoreticians interested more explicitly in band-structure calculational methods and techniques, particularly Slater's group at MIT. The scientific community concerned with this specialty was rather small then and one could meet virtually everybody in the field by attending a few conferences in the US and Europe.

The subject was much more exciting then than it is now because there was still so much to learn. The growth of solid-state physics was phenomenal during the 1950s. Its expansion was a direct consequence of the interplay between theory and experiment and exemplified the richness and diversity of the subject. The burgeoning of solid-state physics was stimulated by industrial and governmental support, which, in turn, was influenced by the tangible transfer of basic research ideas into important commercial and military products. In contrast to high-energy physics, which requires big machines, solid-state physics could be done by small groups using modest experimental equipment and relatively small electronic computers. More interesting than its rapid growth in the 1950s and 1960s are the reasons for its reduced growth in subsequent decades.

Herman's mother, Sarah, beams with pride as the author works on a television set at home in 1951.



During the 1950s and into the 1960s there was a sharp dichotomy between those doing formal solid-state research and those doing computational work in the field. Many physicists were strongly prejudiced against numerical studies. Considerable prestige was attached to formal theory. Those doing calculations were not doing physics, but rather engineering. Such attitudes persisted for a long time. For example, at the 1969 Electronic Density of States Conference at Gaithersburg, Maryland, John Ziman, a distinguished British theorist, referred to electronic computers as elephants and to band theorists as their mahouts. In my rebuttal, I pointed out that our understanding of the band structure of solids did not progress very far while it was confined to textbooks and treated in terms of Kronig–Penney models and similar idealizations. But once the band theorists rolled up their sleeves and began doing realistic calculations on actual materials, and checking their results against experiment, real progress began to be made.

Many physicists have gotten over this prejudice by now, having seen how important the results of numerical studies can be and how ingenious the methods themselves can be. What is now happening is that the most sophisticated computational methods are being adapted to implement some of the most advanced formal theories. This may have been the case also in the 1950s, but computations hadn't yet achieved the speed and accuracy that are almost taken for granted today, so that the power of formal theory overshadowed the computational studies.

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