RECOLLECTIONS FROM THE EARLY YEARS OF SOLID-STATE PHYSICS

Bardeen's devotion to solid-state physics began in the mid-1930s, when he was a graduate student and the field was young. By 1951 he had helped to discover the transistor and had set the stage for his later studies of superconductivity.

Conyers Herring

I first met John Bardeen in the fall of 1934, when I arrived at Princeton as a second-year graduate student in astronomy. John was then a graduate student in mathematics, but considerably more experienced than I. He had taken a bachelor's and a master's degree in electrical engineering several years earlier at the University of Wisconsin and had worked for about three years at Gulf Research and Development Laboratories as a geophysicist, concentrating particularly on electromagnetic means of prospecting. He had become interested in physics during his years at Wisconsin, where he had heard lectures by John H. Van Vleck, Peter J. W. Debye and Paul A. M. Dirac, and he had finally decided that he wanted to do work more fundamentally mathematical than geophysics.[1]

Princeton: Physics of a metal surface

Like over half of the graduate students, John and I were housed in the Graduate College, and we took our meals there. For dinner in the evening we marched in wearing black academic gowns (see figure 1) and began the meal with an invocation in Latin by the Master in Residence. John and I usually found ourselves among the physicists, along with a few physical chemists and others. The group usually included Frederick Seitz, who had received his PhD the year before. Earlier, he and his professor, Eugene Wigner, had published their historic work[2] on the electronic structure of sodium, which showed how the new quantum mechanics could be applied to the calculation of electronic properties of simple metals in a quantitatively significant way. But solid-state theory didn't spark much dinner discussion, and as neither John nor I was particularly lively at initiating conversations, our acquaintance didn't develop very fast at mealtime.

Fortunately we had other contacts. Especially interesting was a series of informal meetings Edward Condon, then an associate professor, had initiated for the discussion of currently interesting topics in physics. In the academic year 1934–35, to the best of my memory, the attendees usually consisted of Condon himself, Seitz, Bardeen, me and John Blewett, who, though primarily an experimentalist, had a great talent for and interest in theoretical subjects. Typically a session would be divided between a little beer drinking at the Nassau Inn, a physics presentation by one of us in Condon's office and some discussion. When Bardeen's turn came, he told us about his thesis on the sodium surface.

John had undertaken to extend the quantum mechanical methods Wigner and Seitz had just introduced in such a way as to make possible a first-principles calculation, at zero temperature, of the electronic work function of a sodium metal surface. Though his work was rooted in that of his mentor, Wigner, his approach, as Wigner himself later described it, was very independent and self-directed. John made no attempt to glamorize what he had done, and my reaction at the time was one of distress at seeing so obviously intelligent a mind bogged down in such a messy calculation. Only years later, when I had occasion to study his work carefully, did I realize the depth of his insights and his courage in facing the messy details.

The calculation had two parts. The first step, which involved only the properties of the deep interior of the metal, was to calculate the change in the ground-state energy of the entire system if one electron were removed from the metal block and deposited not in the vacuum

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Procter Hall, where members of the Princeton Graduate College dined in their black academic gowns. This scene, photographed circa 1946, is typical of the meals Bardeen and Herring shared as graduate students in the mid-1930s. **Figure 1**

outside but simply in some isolated place having an electrostatic potential equal, say, to the mean electrostatic potential of the interior cells of the metal. The second step was to calculate the difference between the actual potential in the vacuum at infinity and this mean interior potential. This difference depends upon the way charge is distributed at the surface.

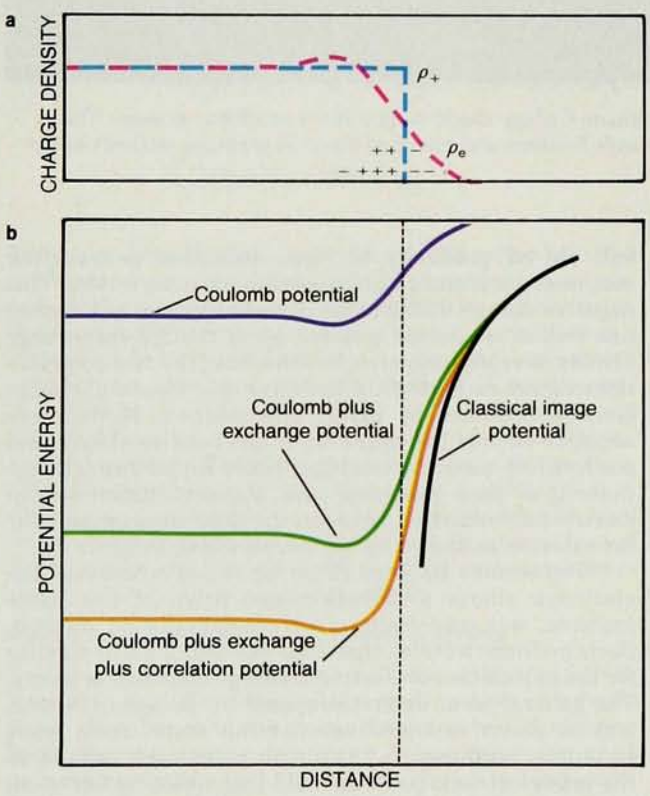
I shall not describe the solution of the first part of the problem, the bulk part, which was easily obtained from the Wigner–Seitz model of an infinite perfect crystal.[3] The second part of the problem, the surface part,[4] was less straightforward to deal with because it required treating the electrons of the metal in the highly inhomogeneous region at the surface where the metal adjoins the vacuum. Bardeen recognized that the essential physics of this inhomogeneous region is already manifested in a simplified metal in which the positive charge, rather than being localized in nuclei, is uniformly distributed over the metal and has a sharp plane boundary. One of the things Bardeen undertook in his thesis was to calculate from first principles what the charge distribution would be for this model (nowadays called "jellium"). Earlier attempts in the literature at such a calculation had given discordant and even unreasonable results.

Figure 2 shows what this type of calculation involves. Figure 2a shows the two contributions to the density of electric charge as a function of the distance normal to the surface. The positive background charge, represented by the blue dashed line, is constant within the boundary of the metal surface and abruptly drops to zero beyond. If one adds enough electrons to produce a neutral metal, the density of electronic charge in the deep interior will balance that of the positive charge. But at the surface the electron density, unlike the positive background charge, will die off gradually because, according to quantum mechanics, it is made up of smoothly decaying waves. This negative charge distribution is shown by the red dashed line (which is plotted positive, even though the charge density is really negative, to show how the two contributions cancel each other in the interior). The total charge density, indicated by pluses and minuses, is therefore negative outside the boundary of the positive charge and positive just inside it, forming a dipole with a strength per unit area that measures the classical difference in electrostatic potential between the deep interior and the far exterior, as shown by the purple curve in figure 2b.

The scheme Bardeen chose for the calculation of the electronic charge distribution, and hence of the dipole moment, was essentially a self-consistent field method. Such methods were at that time becoming quite popular for the calculation of electronic charge densities in atoms. The general idea, as first proposed by Douglas Hartree, was to derive occupied one-electron states from wave equations containing a "Coulomb potential," defined as the interelectronic potential field that would be felt by an electron positionally uncorrelated with the others. Then in 1930 John Slater and, independently, Vladimir Fock showed that addition of an "exchange potential"—the lowering of the electron–electron interaction energy resulting from the effect of the Pauli principle, which requires that parallel-spin electrons keep away from each other—could make a self-consistent field correspond to a minimum-energy wavefunction of the form of a determinant of one-electron wavefunctions.

Bardeen realized, however, that if one wanted to find a determinantal wavefunction that would yield an approximately correct electron density distribution rather than a minimum energy, then each one-electron wavefunction should obey a wave equation including, in addition to the Coulomb and exchange potentials, an additional "correlation potential" due to the additional mutual avoidance of electrons caused by their electrostatic repulsions. The various contributions to the effective potential for an electron are shown figure 2b. As mentioned above, the Coulomb potential (the purple curve) represents $-e$ times the classical potential of the mean charge distribution. The green curve shows how the attraction of the electron to the metal increases if the exchange potential is added, and the orange curve shows how inclusion of the correlation potential further increases the attraction. As Bardeen stressed, the latter potentials converge well outside the metal to the image potential $-e^2/4x$ that would be felt by a classical charge $-e$ a distance x outside the surface of a perfect conductor.

Because in the 1930s all numerical calculations had to be made by hand with mechanical calculators, John had to make a number of rather crude approximations to shorten the tedious task of iterating his wavefunctions to self-consistency. But his qualitative conclusions remain valid to this day, especially his conclusion that at typical metallic densities the exchange and correlation potentials, which prior to his time had not been included in theoretical calculations on surfaces, significantly reduce the electron spillover into the vacuum and so decrease the surface dipole moment.[5] And it is especially striking that his calculational approach, though it uses energy-dependent potentials and many approximations, is strikingly similar in its philosophy to the modern density-functional technique introduced by Pierre Hohenberg and Walter Kohn[6] in 1965, in that it sought a determinantal wavefunction that would reproduce the exact density.



Key concepts for calculating charge distribution near the surface–vacuum interface (black dashed line) for a simplified metal in which the positive charge is uniformly distributed. **a:** Contributions to the overall charge density ρ as functions of distance normal to the surface. The contribution from the continuous positive background charge is shown in blue; the negative of the contribution from the electrons, in red. **b:** Contributions to the effective potential energy felt by an average electron. **Figure 2**

Harvard

Let us return to Bardeen's personal history. In the mid-1930s, Harvard University set up a group called the Society of Fellows, intended to foster the ideal of a community of scholars devoted to the advancement of learning and enjoying interdisciplinary communication. The society had senior fellows drawn from the Harvard faculty and junior fellows who were scholars or scientists in their mid-20s. Bardeen was awarded a junior fellowship in 1935, so he went off to Harvard then, while I stayed two years more at Princeton. (Figure 3 shows a portrait of John taken at about this time.) However, when in 1937 I had the good fortune to get a National Research Council fellowship to work at MIT with Slater, I reestablished contact with Bardeen in Cambridge.

I had been very much impressed with some improvements that Bardeen had made in the Wigner–Seitz method of calculating electronic band structures of metals, improvements that he was even then applying to the calculation of the energies of metallic lithium and sodium[7] for comparison with Percy Williams Bridgman's experiments at high pressures. So I went up to Harvard from time to time to visit him and get educated about this and other aspects of the calculation of band structures and energies of metals. As the year went on, I got directly involved in band-structure work myself, through my association at MIT with Albert Gordon Hill, who the previous year had started a Wigner–Seitz-type calculation for beryllium with Seitz (by then at Rochester). I discovered early on that although Bardeen was, as I have already intimated, a very quiet person who never talked expansively about his work, he was really quite disposed to be helpful if one were patient in asking the right questions and thinking carefully about what he said.

In most of our contacts outside physics, for instance, at dinners at the Harvard Faculty Club or with an eating group to which Hill and I belonged at MIT, John displayed this same reticence underlain by amity. However, he could occasionally be jolly. And sometimes he could be quite angry, if he felt someone had overstepped tolerable behavior.

Bardeen in his mid-20s. In 1935, around the time this photo was taken, he joined Harvard University's Society of Fellows as a junior fellow. (Courtesy of Mrs. John Bardeen.) **Figure 3**

During his stay at Harvard John did other important work in solid-state theory, notably a first-principles calculation of the matrix elements for the scattering of conduction electrons in metals by phonons.[8] To calculate the matrix elements he utilized the self-consistent distortion of the valence electron distribution (and therefore of the potential due to it) during the vibrational motion. This calculation provided the definitive correction of the inadequacies of two previously used, rival theories: the "rigid ion" and "deformable potential" models. John was also interested in nuclear theory, both because of his intrinsically broad tastes and because of his association with Wigner. From Harvard he published a pair of papers[9] on the average density of nuclear energy levels at high excitation, a quantity important for the theory of slow-neutron capture.

Minnesota, the war and Bell Labs

In the middle of 1938 John finished his stay at Harvard, got married and became an assistant professor of physics at the University of Minnesota, where he remained until the beginning of World War II. His passion for thoroughness was typified by a paper[10] that showed how the use of the image force for an electron outside a metal surface could be justified in quantum mechanics. This placed his earlier thesis work on a firmer foundation. His continuing interest in fields outside solid-state physics resulted in a couple of papers on the theory of isotope separation. But perhaps his most noteworthy interest in this period, though it resulted only in the publication of a brief abstract, was superconductivity. Stimulated by Fritz and Heinz London's phenomenological theory[11] and by David Shoenberg's book,[12] he conceived the idea that superconductivity could result from a sufficiently strong electron–phonon interaction that would temporarily cause the electrons to suffer Bragg reflections from the phonon waves; near some of these reflections, electrons near the Fermi energy could have very small positive or negative effective masses and give large contributions to the diamagnetism. As history was to show, the difficulties of developing this idea into a convincing theory of superconductivity proved considerable. But the germ of an idea was there, and John never ceased to be fascinated by it.

World War II changed everything. Even well before the bombing of Pearl Harbor many scientists were being summoned to war work. While a few projects involved solid-state physics, most did not. Bardeen's experience with the classical problems of geophysics made it natural for him to be invited by one of his associates in that field to head a group at the Naval Ordnance Laboratory in Washington, DC, concerned with magnetic mines and torpedoes and countermeasures against them. I got involved elsewhere, in antisubmarine warfare. He and I saw each other no more than once or twice during the entire war. Though most scientists wanted to keep up with their fields, the pressures and often long hours of war work left little time or energy for reading or creating basic physics. As with most of us, John's list of published papers contains nothing between mid-1941 and 1946. However, as figure 4 shows, he did not neglect his growing family.

Because of his interest in nuclear physics and isotope separation, John was a likely candidate for recruitment to the Manhattan Project. However, perhaps out of loyalty to his earlier associates, he elected to stay at the Naval Ordnance Laboratory. At the end of the war recruitment of scientists into peacetime jobs resumed, and in John's case Bell Telephone Laboratories made an early bid to compete with Minnesota. Mervin Kelly, who had become director of research at Bell Labs in 1936 and subsequently became executive vice president and then president, had realized early on that solid-state electronic devices were likely to be of great importance in the future. Even before the war he had started to assemble a group of especially talented solid-state physicists. The most dynamic of these was William Shockley, who had come to Bell Labs in 1936 after taking his PhD with Slater at MIT. The war took Shockley away into operational research, but after the war Kelly's program got rolling again, and Shockley was placed as cohead of a new department dedicated to research in solid-state physics. Because Shockley and Bardeen had known each other when they were both in Cambridge during the mid-1930s, Shockley was well aware of John's exceptional talents and pushed to recruit him. Further support was given by James Fisk, also at Bell Labs and later to become its president; he had been a junior fellow at Harvard at the same time as Bardeen. When John came to Bell Labs for a job interview, he told his hosts that he was trying to make up his mind whether to focus his subsequent career primarily on solid-state physics or on nuclear physics. His final decision was for solid state and Bell, and in the middle of 1945 he and his wife, Jane, moved to Summit, New Jersey, with their three young children.

Naturally, things were in quite a state of flux at that time, and apparently office space was short. So John was temporarily given a desk in a room occupied by two solid-state experimentalists, Walter Brattain and Gerald Pearson. This arrangement turned out to be very felicitous: Not only did John make history in scientific collaborations with each of these men; it also turned out that all three shared an interest in golf, a fitting sport for suburban New Jersey and one that gave John great pleasure, though he could swear as fiercely as anyone when a shot went wrong.

Brattain used to tell an illuminating anecdote about a 1945 encounter with Walker Bleakney of the Princeton faculty, an old friend of Brattain's from student days at Whitman and Minnesota. Bleakney, though an experimentalist, had known Bardeen fairly well when John was a graduate student. When Brattain mentioned that Bardeen had just been hired and was going to become one of his coworkers, Bleakney offered his advice in words something like the following: "You'll find that Bardeen doesn't very often open his mouth to say anything. But when he does, YOU LISTEN!" The joy that Brattain exuded in his frequent retellings of this story typified the depth of his admiration and friendship for Bardeen.

Physics at Bell Labs: The transistor

Bardeen worked on a variety of topics over the next few years. However, his attention was increasingly drawn to electrical conduction in semiconductors and especially to the attempt to understand the experiments that Brattain and others were undertaking to see if they could make an amplifier using Shockley's idea of what we would today call a field-effect transistor.[13] Bardeen spent much time in the laboratory with his experimental colleagues. (See figure 5.)

It would be quite a long story to relate how the initial experiments, which failed, led to new theoretical concepts, which were tested with new experiments, which in turn led to new hypotheses about how a semiconductor amplifier might be made and to still further experiments, and ultimately, with a bit of serendipity, to the discovery of the point-contact transistor. I hope I can convey a feeling for what it was like being involved in this research by simply recalling the main theoretical concepts and experimental tools that were used and mentioning only a couple of the key experiments.

Figure 6 shows some of the important concepts, all of which play a role in our present understanding of semiconductors but which in the years 1945–47 came only successively to be recognized as important. The diagrams on the left show, for an n-type semiconductor, departures of charge density from the normal neutral bulk state. Mobile holes or electrons are indicated by simple blue or red dots, respectively, and similar immobile, or "trapped," charges by dots in circles. The diagrams on the right show the variation with depth of the edges of the conduction and valence bands, the band curvature being due to space-charge fields.

The 1945 suggestion by Shockley was that if a metal electrode were placed near a flat semiconductor surface and biased positive relative to the semiconductor (as shown in figure 6a), an excess of electrons would be drawn into a space-charge layer near the semiconductor surface. Therefore if the semiconductor is not enormously thicker

Reading to his son Bill during the war years. Bill, who would later become an elementary-particle theorist, had memorized every specification of the train engines described in the book pictured (his favorite) and would not allow his father to skip a single word. (Courtesy of Mrs. John Bardeen.) **Figure 4**

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Walter Brattain using his microscope was a typical sight for Bardeen around 1947. **Figure 5**

than the space-charge layer, its conductance parallel to its surface should be appreciably increased. Why such an increase was not observed was explained by Bardeen with the hypothesis that surface states (see figure 6b), whose possible existence had been known for some years, were immobile and present in such large numbers per unit energy near the Fermi energy that they could almost completely screen the interior of the semiconductor from the field of an external electrode.

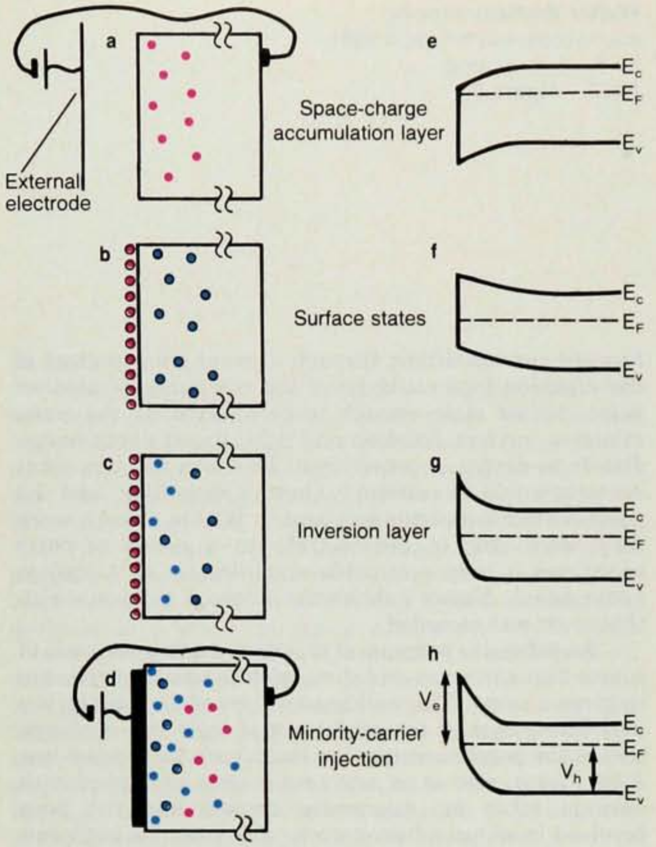
Such surface states can produce band bending even in the absence of an external field. Bardeen soon realized that this band bending could sometimes become great enough to produce an inversion layer (see figure 6c), that is, a region of the semiconductor very close to the surface with a high density of mobile charge carriers opposite in sign to those predominant in the deep bulk. For the n-type case shown in figure 6, this would be a region with hole conduction. But when an experiment intended to repel holes from an inversion layer, and thereby to decrease the conductivity of the near-surface layers, turned out instead to *increase* the conductivity seen by a neighboring probe electrode, Bardeen and Brattain were forced to conclude that a new phenomenon, hole injection, was occurring. In other words, a positively biased metal electrode in contact with the surface of an n-type semiconductor causes a current to flow into the latter that is primarily carried by minority carriers, in this case holes, moving into the semiconductor, rather than by electrons moving out. (See figure 6d.) This will occur if, as in the band-bending diagram in figure 6h, the barrier V_e for electron motion is greater than the barrier V_h for hole motion.

Once they realized that the holes injected by a forward current driven through a metal point contact of the injecting type could lower the resistance of another point contact close enough to be affected by the same minority carriers, Bardeen and Brattain set about immediately to design an experiment in which the two point contacts would be extremely close to each other, and the point-contact transistor was born.[14] Within about a week they were able to demonstrate to a group of their executives a very noticeable amplification of a spoken audio signal. Figure 7 shows the notebook page on which this event was recorded.

An extensive program of practical development was of course begun at once, and obviously the new device had to be given a name. The well-known story of the naming is a nice illustration of the confluence of logic and euphony. One of the people consulted in the search for a name was John Pierce, who as an engineer mainly concerned with vacuum tubes for microwave devices had not been involved in semiconductor work. But from his engineering viewpoint he knew that what Bardeen and Brattain had invented was a three-terminal device describable in the linear approximation by certain matrix coefficients relating input and output. As the device was normally used, its most important characteristic was the alteration of collector voltage by an alteration in the emitter current, in other words, a transresistance coefficient (in contrast to the situation for a vacuum tube, where transconductance is all important). Calling to mind words already in common use, like "resistor," "thermistor" and "varistor," Pierce tentatively mouthed words in response to Brattain's question about a name. Finally he said, thoughtfully, "Transconductance . . . transresistance . . . transistor." At once Brattain said, "Pierce, that is it!"

Return to superconductivity

Following the breakthrough discovery of minority-carrier injection, which soon led to the investigation of pn junction transistors, semiconductor physics at Bell Labs received greatly increased attention, and Bardeen continued to be strongly involved. However, he did not give up his interest in other solid-state topics. In particular, he had not forgotten his prewar ideas on the possible role of electron–phonon interaction in producing superconductivity. The discovery of the isotope effect[15]—the dependence of transition temperature on isotopic mass—was communicated to John by Bernard Serin in a telephone call, probably early in 1950. The discovery was tremendously exciting, because it seemed to confirm Bardeen's feeling that lattice vibrations were involved. He worked intensely to show how the electron–phonon interaction could lead to an effective electron–electron attraction and cause the ground state of a metal to be one with an occupation for one-electron states rather different from the usual simple filled Fermi sphere. This version of the theory, which John published in 1950, turned out (as Robert Schrieffer and David Pines explain in their articles on pages 46 and 64, respectively) to be incomplete, in that it failed to



Central ideas in semiconductor surface physics, arranged in the order in which their possible roles came to be appreciated through research at Bell Telephone Laboratories between 1945 and 1947. **a–d:** Spatial distribution of the change in electric charge density in or on an n-type semiconductor relative to that of a uniform neutral state. Mobile electrons are indicated by red dots and mobile holes by blue dots, while charges trapped on surfaces or at centers in the bulk are indicated by dots circled in black. In **a** and **d** metal electrodes, whose charges are not shown, are connected through a battery to a contact at the rear of the semiconductor. **e–h:** Energies of the conduction and valence band edges versus depth; the Fermi level is indicated by a dashed line. The barriers for electron and hole motion are V_e and V_h, respectively. Infinitesimal bias is assumed in **g**. **Figure 6**

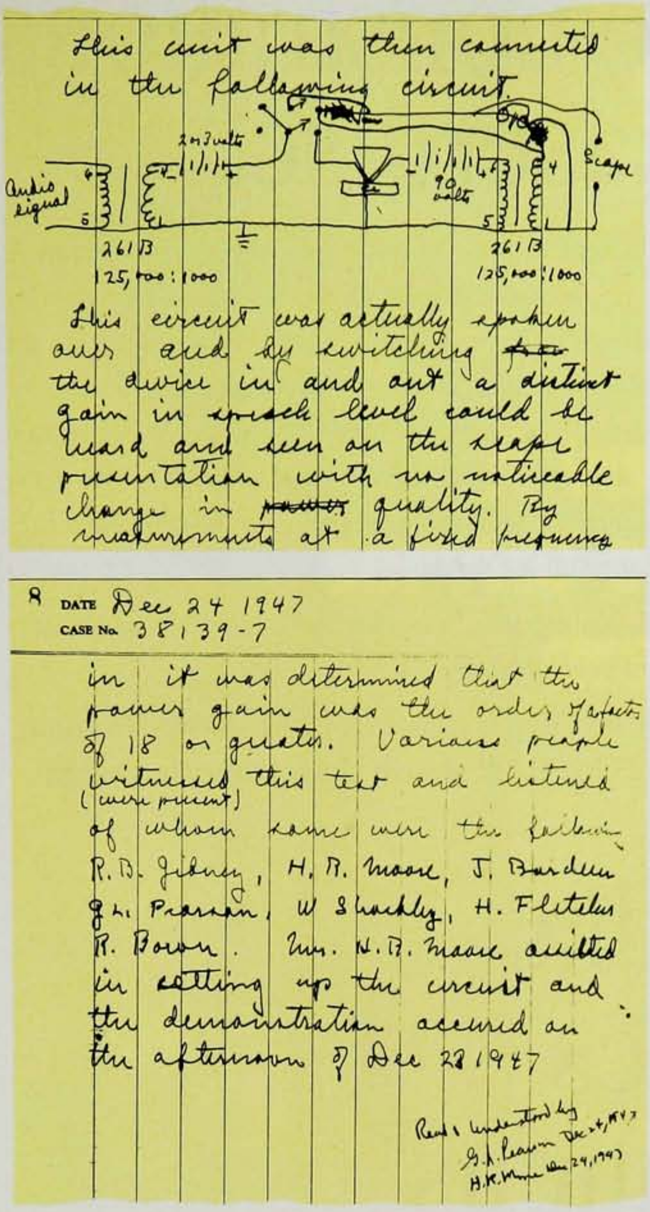
demonstrate logically how the phonon-induced interaction would produce perfect diamagnetism or persistent currents. However, though the currents weren't yet persistent, John's mind certainly was, and he became quite wrapped up in the effort to understand superconductivity.

At just this time, an interesting coincidence occurred. Herbert Fröhlich, at Liverpool University, whose work on electron–phonon interaction in insulators was already attracting attention, independently conceived the idea that in metals this interaction might cause an effective coupling of the electrons to each other. He tried to construct from this idea a theory of superconductivity, even before becoming aware of the isotope effect that was observed experimentally. In the summer of 1950, Fröhlich came to the United States and arranged to visit Bardeen at Bell Labs. Bardeen met Fröhlich at the railroad station and drove him to the laboratory, talking as they went about his own current work on superconductivity. After registering Fröhlich with the receptionist at Bell Labs, John personally escorted his visitor to his own office to continue the discussions. When they reached Bardeen's office, there was a pile of mail on the table, including a large sealed envelope from *Physical Review*. While Fröhlich watched, John opened the package, which turned out to contain the very paper on superconductivity that Fröhlich had recently submitted to *Physical Review*. John had been chosen as a referee. John remarked to me later that he was extremely glad for this event, which showed Fröhlich that the ideas John had just been telling him about were indeed John's own and not derived from an earlier encounter with Fröhlich's paper. It turned out that Bardeen had submitted his first paper on the subject, a letter to the editor of *Physical Review*, just a few days after Fröhlich submitted his paper; Bardeen submitted his full-length paper a couple of months later. There was quite a rivalry between the two, and it was very clear that John passionately aspired to lead the effort to decipher the mystery of superconductivity. As Schrieffer and Pines show in their articles, it was this determination that caused John to keep struggling with inadequate theories for years[16] until he finally to reached his goal.

Departure for Illinois

Bardeen left Bell Labs for the University of Illinois in 1951. Several factors brought about his decision. Though the research environment at Bell had unsurpassed quality and diversity over many areas of solid-state physics and materials science, universities could offer superior opportunities in some areas, for example, working with students. Bardeen also felt that strong concentration on superconductivity would be more appropriate in an academic environment.

But there was an additional motivation that was felt strongly by Bardeen and by several others in Shockley's group. (I myself was in a different organizational unit.) Shockley had been a bit chagrined that the discovery of the point-contact transistor had come suddenly in the course of a series of experiments in which he had not been a direct participant since his early field-effect suggestion. At any rate, he soon saw the immense potential of the new discovery and its extension to junction transistors. To lead the new developments he labored with a tremendous intensity that seemed at times to require his personal involvement in the details of everybody's work and his control over the flow of information. Notwithstanding his recognized ingenuity and depth of insight, this was not the way to manage a group of creative scientists. Especially exasperated were those people in his group who, like Bardeen, were strong-willed and self-directed, people whose success depended on working in their own ways. Similar conflicts even beset some workers in areas unrelated to transistor physics. Though anxious to keep Bardeen and encourage his superconductivity work, higher management didn't really help much, being inclined to give Shockley free rein. Some people, like Charles Kittel, John Richardson and Bardeen, left for careers elsewhere.


This circuit was then connected in the following circuit.

audio signal

2 1/3 volts

90 volts

scope

261 B 125,000 : 1000

261 B 125,000 : 1000

This circuit was actually spoken over and by switching the device in and out a distinct gain in speech level could be heard and seen on the scope presentation with no noticeable change in quality. By measurements at a fixed frequency

8 DATE Dec 24 1947
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in it was determined that the power gain was the order of a factor of 18 or greater. Various people witnessed this test and listened (were present) of whom some were the following R. B. Gibney, H. R. Moore, J. Bardeen, G. L. Pearson, W. Shockley, H. Fletcher, R. Bown. Mr. H. R. Moore assisted in setting up the circuit and the demonstration occurred on the afternoon of Dec 23 1947

Read & understood by G. L. Pearson Dec 24, 1947
H. R. Moore Dec 24, 1947

Pages from laboratory notebook of Brattain dated 24 December 1947 describe the demonstration of voice amplification by a point-contact transistor. The witnesses who signed the page were Gerald L. Pearson and H. R. Moore. (Courtesy of Walter Brown, AT&T Bell Laboratories.) **Figure 7**

Others, like Brattain and Philip Anderson, stuck it out and got relief when Shockley himself left for California in 1954 to start his own business.

I would like to mention, in closing, one very personal recollection from 1955. In the summer of that year John had occasion to pay a brief visit to Bell Labs. My wife, Louise, and I had invited him to come at the close of the day to our house to have dinner with our family. Now it happened that that day was our daughter's seventh birthday, and our original plans for a children's birthday party had to be canceled because she had come down with mumps; although mostly recovered, she might still have been contagious. So she was feeling rather unhappy. After checking with John that he had already had mumps, we asked him if he would mind being presented as the featured guest invited to celebrate our daughter's birthday. He was very willing, and we had a successful birthday party with a guest from out of town as the special attraction. It was typical of John that he enjoyed his role.

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I am greatly indebted to many colleagues who have reinforced and augmented my own remembrances and helped to improve my understanding. Special thanks are due to the other participants in the Bardeen Memorial Symposium, and also to Philip W. Anderson, Jane Bardeen, Walker Bleakney, Harvey Brooks, Albert Gordon Hill, Lillian Hoddeson, Charles Kittel, Walter Kohn, John Pierce, John M. Richardson, Viraht Sahni, Frederick Seitz and Charles H. Townes.

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