JOHN BARDEEN

BARDEEN FAMILY ARCHIVES

23 MAY 1908 · 30 JANUARY 1991

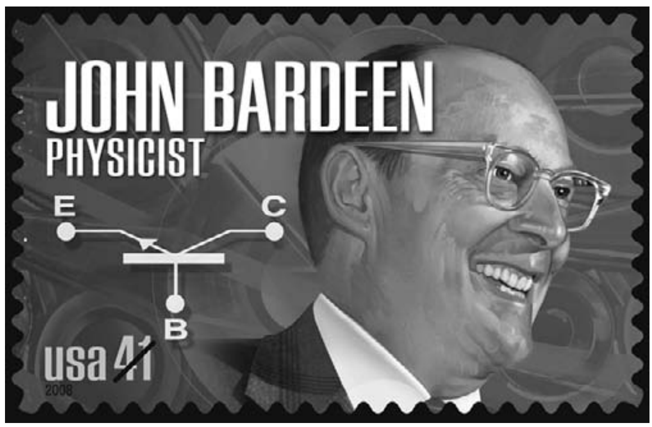
JOHN BARDEEN, whose hundredth birthday we celebrated on 23 May 2008, was arguably the most influential scientist/inventor of the latter part of the twentieth century. Through his scientific discoveries, his instinct for invention, and his impact on his colleagues, he made possible the electronics revolution and the information explosion that have changed dramatically our everyday lives. In common with our founder, Benjamin Franklin, he combined his passion for science with "the pursuit of useful knowledge."

Bardeen was an authentic American genius. Scientific genius is no easier to pinpoint than artistic genius. It derives from a combination of factors, including—but not limited to—intuition, imagination, far-reaching vision, and exceptional native gifts that blossom into significant technical skills, and the willingness and ability to challenge conventional wisdom. What is perhaps even more important, scientific genius depends on an instinct for invention, an ability to focus on the problem at hand, to juggle multiple approaches, and a fierce determination to pursue that problem to a successful conclusion.

The inventor of the transistor and leader of the team that developed the microscopic theory of superconductivity, John Bardeen possessed all of these qualities. But what made him different from so many fellow scientific geniuses in physics of the twentieth century—Einstein, Bohr, Dirac, Feynman, Landau, Pauli, and Oppenheimer? The answer lies not only in his two Nobel Prizes for Physics (in 1956 and 1972), but also his remarkable modesty, his deep interest in the application of science, and his genuine ability to collaborate easily with experimentalist and theorist alike. He was, moreover, a devoted and loving husband and father, whose marriage to Jane Maxwell in 1938 began a singularly happy union, and who imparted to his children his passion for science. His two sons—Jim and Bill—are distinguished theoretical physicists who work on cosmology and general relativity and high energy physics, respectively, while his daughter, Betsy, married an exceptionally able experimental low temperature physicist, Tom Greytak, and pursued an active career as a technology analyst and adviser before her untimely death from cancer in 2000.

With Walter Brattain, Bardeen invented the transistor at Bell Laboratories in 1947. Ten years later, with his young University of Illinois colleagues, Leon Cooper and Bob Schrieffer, he developed the microscopic theory of superconductivity. Their work not only solved the most challenging outstanding problem in fundamental physics, but changed the basic paradigm in condensed matter, nuclear, astro-, and particle physics, and opened the way for many practical applications of superconducting devices. Moreover, during a thirty-year period as the key scientific adviser to Xerox (and its predecessor the Haloid company)

Bardeen was a major player in the development of xerography, while his first UI electrical engineering graduate student, Nick Holonyak, went on to develop the light-emitting diode. It was therefore entirely fitting that the U.S. Postal Service marked his centenary by issuing the stamp in his honor reproduced here.



In the year following John Bardeen's death on 30 January 1991, *Physics Today* put out a special issue in his honor, and that April 1992 issue remains an invaluable source of first-hand tributes to John by friends and colleagues who knew him well (Conyers Herring, Nick Holonyak, Gloria Lubkin, George Pake, Bob Schrieffer, and the author); I draw heavily on it in writing this memoir. As we emphasized in our contributions, John Bardeen was a uniquely gifted scientist who combined superb physical intuition with analytic abilities of the highest order and a remarkable instinct for invention. He was an equally extraordinary human being: quiet, genuinely modest, a man of enormous integrity, with a wonderful sweetness of spirit. It is rare to find all these qualities coming together in a single individual. So it is natural to ask what there was in his youth that made this possible, and to examine how these qualities manifested themselves throughout his life.

John was the second of the five children of Charles and Althea Bardeen. His father, Charles Russell Bardeen, was the first graduate of the Johns Hopkins Medical School and founding dean of the medical school at the University of Wisconsin. Before marrying Charles Bardeen, his mother, Althea Harmer, had studied oriental art at the Pratt Institute and had been one of the first members of the Laboratory School of the University of Chicago, which had been created by John Dewey. She remained there for five years before resigning to practice interior design in Chicago. His mother died when John, their second-eldest child, was twelve years old, and his father soon remarried in order to have help in raising the five children.

In common with so many great scientists, John was a child prodigy who displayed an early talent for mathematics and learning. At the age of nine he was skipped from the third grade of elementary school directly to the seventh grade. The following year, aged ten, John won a citywide algebra contest. Because Madison's Uni High did not have adequate laboratory facilities, John switched to the public high school after his junior year. There he spent two years, graduating at the age of fifteen in the same class as his brother Bill, who was two years older.

John's personality, unlike those of so many prodigies, does not seem to have been shaped by the experience of being really different from his contemporaries. A number of factors may have been responsible: growing up in a university community, being a member of a large family, his closeness to his older brother, and, perhaps most important, his mother's guidance based on her experience as an educator in an experimental school for exceptional children. His brother Bill, like John, was deeply interested in sports, but, unlike John, he was garrulous and outgoing, with a bit of a wild streak and a marvelous sense of humor. Inspired by Laurel and Hardy movies, Bill and John tried all manner of gymnastic tricks. One day when John was eight or nine, a neighbor came over to ask John's mother if she was aware that John was hanging by his heels from a third-floor window sill.

His parents encouraged John to keep his school and after-school lives quite separate. He would thus come home from high school to join in the activities of the elementary school children his own age. His involvement in sports, for which he had a real aptitude, played a significant role in maintaining his social balance. John's lifelong passion for golf, perhaps inherited from his father, began when he was quite young. He was a good enough swimmer to make the varsity teams in both swimming and water polo at the University of Wisconsin for three years, and these accomplishments suggest that the keen sense of competitiveness that characterizes most successful scientists was present in John at an early age.

Though outwardly quiet and reserved, John demonstrated early on a genuine talent for making friends with contemporaries whose interests and outlooks were quite different from his own, and he maintained those friendships throughout his life. During his college years, John joined a fraternity a few doors away from his family home and happily participated in the fraternity life of the Roaring Twenties. He paid his fees and other college expenses from his winnings at poker, his youth and innocent appearance masking a keen sense for the cards. He may have inherited this from his grandfather, C. W. Bardeen, who went off to the Civil War at age fourteen to be a musician in the First Massachusetts Infantry, and who told stories of his own skill at poker during the Civil War in his *Little Fifer's War Diary*.

At Wisconsin, despite his obvious talent for mathematics and physics (including considerable exposure to the latter through courses from John Van Vleck and summer-institute lectures by Peter Debye and Paul Dirac), John's interest in "practical knowledge" led him to major in electrical engineering and later to focus on problems in geophysics, fields for which he also demonstrated very considerable aptitude. He was in no great rush to finish his university work; he found a summer job in Chicago at Western Electric so interesting that he remained there for a year before returning to Madison, where he received his bachelor's degree in 1928.

John stayed on in Madison for graduate work in electrical engineering. After working on applied problems in geophysics and antennas, and receiving his master's degree in 1930, he followed his favorite electrical engineering/geophysics professor, Leo Peters, to Gulf Research Laboratories in Pittsburgh, where he soon invented a quite novel electromagnetic method for oil prospecting. When his approach was finally made public in the early 1980s, more than fifty years after its invention, John told me with pride that the method had been sufficiently novel that Gulf decided not to make it public through submission of a patent application lest rival companies obtain too much useful information from it.

During those early years of the Depression, John shared a quite grand apartment in Pittsburgh with two friends who worked with him at Gulf Laboratories. At Gulf, John's world was that of the oil-exploration engineer, not the cloistered academic. Fred Seitz has told of stopping in Pittsburgh, during an auto trip with John from Princeton to Madison in the mid-1930s, and watching a completely different Bardeen persona emerge as John was reunited with his oil-prospecting friends.

After three years at Gulf, John decided he wanted to study mathematics and theoretical physics. Walter Osterhoudt, his close friend at Wisconsin and then Gulf, has described the moment of decision: "John wheeled his chair around . . . and said, 'I've sat here in this G.-D. room, looked at that blackboard, and out this window watching those lazy bastards working on that dummy rig for three years. I'm going back to school and get my Ph.D.' " So John left his highly paid position ($6,000 a year in 1933 was equivalent to a major university professorship) to begin graduate work.

He chose Princeton University, under the mistaken impression that he could obtain his Ph.D. working under Albert Einstein. However, on arriving in Princeton as a graduate student in mathematics, he learned that Einstein, who had left Germany for Princeton in 1933, had joined a quite different institution, the Institute for Advanced Study, where he no longer supervised graduate students. At Princeton, John lived at the Graduate College, where Fred Seitz was among his fellow students. Fred not only was the first person to show John around the mathematics and physics departments, but also introduced him to Eugene Wigner, who became his supervisor for a project that became his thesis: the behavior of solid-state surfaces.

Although trained as a chemical engineer, Wigner had become deeply interested in fundamental physics and especially the application of group theoretical methods in mathematics and physics. That he was carrying out research on solids in Princeton in the mid-thirties was something of an accident. An experiment on nuclear masses and energy levels that later turned out to be wrong had convinced him that the behavior of nuclei was too weird for him to comprehend; he decided he had better work on something else. So Wigner turned his attention to metals, and in so doing made a number of fundamental contributions to their understanding, beginning with his seminal paper with Seitz on the electronic structure of sodium. He trained a remarkable group of students, who included, in addition to Bardeen and Seitz, such seminal figures as Conyers Herring, Gregory Wannier, and Roman Smoluchowski.

Conyers Herring joined Bardeen as a Wigner grad student in 1934, and has written that while he often had dinner at Proctor Hall with John,

> neither [of us] was particularly lively at initiating conversations, so 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; that academic year the attendees usually consisted of Condon, Seitz, Bardeen, [Herring], 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.

One of these insights was his approach to the behavior of the electrons of the metal in the highly inhomogeneous region at the surface where the metal adjoins the vacuum. Bardeen developed a simple toy model (which Herring later named "jellium") to treat this, in which the positive charge was not localized in nuclei but uniformly distributed over the metal with a sharp plane boundary. The scheme Bardeen chose for the calculation of the electronic charge distribution was essentially a self-consistent field method in which he included both the exchange effects produced by the Pauli principle, and the correlation effects brought about by the Coulomb repulsion between the electrons. As Herring notes, "Bardeen's 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 in 1965, in that it sought a determinantal wave-function that would reproduce the exact density."

Bardeen left Princeton in 1935 before finishing his thesis (it was submitted the following year) to become one of the early members of Harvard's Society of Fellows, and stayed at Harvard for three years. There he continued his work on first-principles calculations of simple metals, making improvements on the Wigner-Seitz method of calculating electronic band structures, in order to compare theoretical calculation of cohesive energy and other thermodynamic properties with the experiments being carried out there by Bridgman on the behavior of lithium and sodium at high pressures.

Significantly, he also began to study the influence of electron interactions in more detail and carried out two seminal calculations involving these during his stay at Harvard. The first dealt with their influence on electron-phonon interactions in sodium; his mean field approach (which was a precursor to the random phase approximation introduced by David Bohm and the author in 1950) enabled him to calculate their influence on the matrix element for electron-phonon coupling and the combined effects of electron-phonon and electron-electron interactions on the phonon dispersion relation. As Herring writes, "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.

Bardeen's second calculation of the role played by electron interaction was to explore its influence on single-particle energies and the specific heat. In work that was published only as an abstract for an American Physical Society meeting, he found that because of the long range of the Coulomb interaction between the electrons, when one attempts to include its influence on single-particle energies at the lowest order perturbative level—the exchange or Hartree-Fock level—one obtains a logarithmically divergent result; this in turn leads to a specific heat that depends logarithmically on temperature, which is not observed. Bardeen had uncovered a paradox, not resolved for another fifteen years, that while it is absolutely essential to include exchange and, indeed, correlation terms in any calculation of the cohesive energy of simple metals, a straightforward attempt to do this for single-particle energies leads to difficulties and contradictions with experiment.

In mid-1938 Bardeen married Jane Maxwell, a science teacher whom he had met three years earlier in Pittsburgh, and the young couple went to Minneapolis, where John had accepted a position as an assistant professor of physics at the University of Minnesota. There he continued his work on a first-principles approach to the theory of metals, writing a paper that showed how the use of the image force for an electron outside a metal surface could be justified in quantum mechanics, and so placing his earlier thesis work on a firmer foundation. He also continued to publish in other fields of physics; at Harvard he had published a pair of papers on the average density of nuclear energy levels at high excitation, a quantity important for the theory of slow-neutron capture, and at Minnesota he published two papers on the theory of isotope separation.

It was while Bardeen was at Minnesota that Jane gave birth in 1939 to their first child, Jim, and it was there that Bardeen began to work seriously on superconductivity, the ability of some metals to flow without resistance at very low temperatures. Developing a microscopic theory of superconductivity was arguably at that time (and for nearly the next two decades) the outstanding problem in macroscopic quantum physics. Since its discovery in 1911 by Kamerlingh-Onnes, finding an explanation for superconductivity had attracted the attention of essentially every leading theoretical physicist, beginning with Einstein and Bohr, but very little progress had been made apart from the phenomenological theory developed by Fritz and Heinz London and a few others. In an abstract of a talk he gave at the spring (Washington) meeting of the American Physical Society in May 1941, Bardeen summarized what might be described as his first "wrong" explanation for superconductivity—suggesting that superconductivity might originate in a small periodic distortion of the lattice that produced a fine-grained zone structure in momentum space such that the energy gain from the resulting discontinuities would outweigh the cost of producing the distortion. He argued that one could achieve perfect diamagnetism (the ability of a superconductor to screen out external magnetic fields) in this way with only a fraction of the electrons near their ground state being involved and used his earlier calculations of the resistivity of simple metals to estimate the strength of the electron-lattice required to bring this about. He concluded that a high density of valence electrons and a strong electron-phonon interaction were favorable to superconductivity, a remarkably prescient argument of which it could be said that he was right for the wrong reasons.

World War II led most university scientists into war work, and Bardeen was no exception. In March 1941, he accepted an invitation from one of his former geophysics colleagues to come to Washington to head a group at the Naval Ordnance Laboratory that was concerned with magnetic mines and torpedoes and countermeasures against them. Later, although his knowledge of nuclear physics and isotope separation made him a natural candidate for the Manhattan Project, where his mentor Eugene Wigner and colleague Fred Seitz were playing active roles, he declined to join the project, when invited to do so, in favor of staying at the Naval Ordnance Laboratory throughout the war to continue his work on mines and torpedoes. It was during their stay in Washington that Jane gave birth to their two younger children, Bill, born in 1941, and Betsy, born in 1944.

At war's end, recruitment of scientists into peacetime jobs began. In John's case Bell Telephone Laboratories made an early bid to compete with Minnesota. Mervin Kelly, its president, had, in the summer of 1945, as the war was drawing to a close, formulated plans for an interdisciplinary solid state department that would apply the understanding of solids at the atomic level made possible by quantum theory to develop new materials for components in the telephone system. He named William Shockley and Stanley O. Morgan as co-heads of a new division that consisted of chemists, physicists, and engineers familiar with the problems of telephone communications, and sought to recruit into it some leading theorists to join Shockley, who had been at Bell since 1936. Through his Harvard days, Bardeen was well known to both Shockley, who had received his Ph.D. with John Slater at MIT in 1936, and Jim Fisk, who had been, like John, a member of the Society of Fellows; John was therefore in all likelihood their number one candidate for one of these newly created positions.

When Bardeen 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. In the fall of 1945 he, his wife, Jane, and their three young children, Jim, Bill, and Betsy, moved to Summit, New Jersey, where they remained for the next six years.

At Bell, space was in short supply, so Bardeen shared an office with Walter Brattain and Gerald Pearson and began a collaboration with them on semiconductors. Here let me quote from Bardeen's own description (in his NAS memoir of Walter Brattain) of those first years at Bell:

> A long-term goal was to make an amplifying device with a semiconductor to replace the vacuum tube. The result was the transistor, essentially an electrical valve with three electrodes such that a voltage applied to one can be used to control the current flowing between the other two. There had been considerable development of germanium and silicon as diodes for use as detectors for radar during the war. Being elements, they were easier to purify and their properties easier to understand than those of compound semiconductors. We decided to concentrate our efforts on these materials, then available in the form of reasonably pure polycrystalline ingots. Shockley had suggested what is now known as a thin-film field-effect transistor, but initial attempts to make such a device had failed.
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> None of us had worked on semiconductors during the war, so we were eager to learn about the developments that had taken place. With new materials to study and new concepts to help understanding, it was a very exciting time to be involved in semiconductor research. We followed the Bell Labs tradition of forming study groups to learn about what had been accomplished.
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> Those involved with the research were good friends socially as well as scientific collaborators. The Brattains were active members of a duplicate bridge club. Walter and I were partners in bridge games arranged at Bell Labs. We were also enthusiastic golfers and enjoyed many a match together. When we lived in different places we would try to get in a round of golf when we got together at scientific conferences in this country and abroad.

In an interview given in later years, Brattain recalled, "I cannot overemphasize the rapport of this group. We would meet together to discuss important steps almost on the moment of an afternoon. We would discuss things freely, one person's remarks suggesting an idea to another. We went to the heart of many things during the existence of this group and always when we got to the place where something had to be done, experimental or theoretical, there was never any question as to who was the appropriate man in the group to do it."

Bardeen writes,

> The close collaboration between experimentalists and theorists extended through all stages of the research, from the conception of the experiment to the analysis of the results. Most papers were authored jointly by an experimentalist and a theorist. Brattain concentrated on surface and interface phenomena, while Pearson concentrated on current flow in the bulk of a semiconductor. . . .
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> Current in a semiconductor can be carried in two different ways, by conduction electrons, extra electrons that do not fit into the valence bonds, and by holes, places where electrons are missing from the valence bonds. The first is called n-type, from the negative charge of

> an electron, and the second, p-type. Silicon and germanium are ambipolar; they can be either n-type or p-type, depending on the nature of the impurities present. By thermal excitation or by light (photoconductivity), electrons can be excited from the valence bonds, giving equal numbers of conduction electrons and holes to add to the conductivity. What we discovered in the course of research is that the conductivity can likewise be enhanced by current flow from an appropriate contact, the principle of the bipolar transistor.
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> Both the field-effect and bipolar principles are used in present-day transistors and integrated circuits. The experiments that led to the discovery of the bipolar principle and to the invention of the point-contact transistor were done in December 1947. The point-contact transistor consists of two metal (cat's whisker) contacts on the upper surface of a small block of germanium that had a large-area, low-resistance contact on the base. Each point contact by itself forms a rectifying contact relative to the base. One, the emitter, is biased in the direction of easy flow; the other, the collector, is biased to a higher voltage in the reverse direction. A signal applied between emitter and base appears in amplified form between collector and base.

A key role in the research leading to the transistor was the role played by surface states. Building on his knowledge of their behavior in metals, acquired in his thesis work, Bardeen realized that in a semiconductor these could be immobile and could act not only to screen the interior of a semiconductor from the field of an external diode, but to produce an inversion layer, a region very close to the surface with a high density of mobile charge carriers opposite in sign to those predominant in its interior.

As Herring writes,

> 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. . . . 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.
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> Within about a week [of this initial discovery], they were able to

> demonstrate to a group of their executives a very noticeable amplification of a spoken audio signal. 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!"

For the next two years, as the reader will discover from Bardeen's own account given below, although Bardeen continued work on semiconductors and semiconducting devices, he found it increasingly difficult to do so. Thus when he learned in a phone call from Bernard Serin in early 1950 of the discovery of the isotope effect (the dependence of the superconducting transition temperature on the inverse square root of the isotopic mass) on the superconducting transition temperature of lead, he essentially dropped everything else to return to his work of a decade earlier on a connection between lattice vibrations and superconductivity.

The experiments of Serin at Rutgers and similar experiments in the group of Emanuel Maxwell at the Naval Research Laboratory confirmed that quantized lattice vibrations, phonons, must be involved in bringing about superconductivity, but how? Bardeen, and independently (and in advance of the Serin-Maxwell result) the British theorist Herbert Frohlich, decided that the key physical effect must be a phonon-induced change in the self-energy of a fraction of the electrons near their ground state configuration, and both published their ideas in the early part of 1950. However, neither was able to demonstrate how this could lead to the formation of a coherent state of matter that could flow without resistance and screen out external magnetic fields ("perfect" diamagnetism), the key physical idea introduced by Fritz London to explain, at a phenomenological level, superconductivity. So this approach turned out to be Bardeen's second "wrong" effort to develop a microscopic theory of this remarkable quantum phenomenon.

It was during this period, in the spring of 1950, that I first met Bardeen, who had come to Princeton once a week to teach a seminar on the physics of semiconductors. Bardeen was already a legend among those of us who were Princeton graduate students—as an exceptionally gifted young theorist who had been Wigner's best student and gone on to invent the transistor. Bardeen's lectures on semiconductors were not memorable, but were typical of his lecturing style—clear, informative, low key, softly spoken, with little in the way of emphasis. (The notes for this seminar formed the basis of EE-PHYS 435, Bardeen's famous electrical engineering course at Illinois, described in his *Physics Today* article by Nick Holonyak.)

A highlight of those weekly visits was his discussions about superconductivity with my office-mate and thesis advisor, David Bohm, and the excitement he conveyed that a solution might be on the way. During these conversations, we also discussed John's early work on electron interactions in metals. John, always open to new ideas, encouraged us to pursue our approach to screening and plasma oscillation in metals, which subsequently turned out to resolve the difficulties he had found in extending the free electron model to take Coulomb interactions into account.

Although it became increasingly clear to him that a change in the electron self-energy would not in itself lead to superconductivity, Bardeen continued at Bell Labs to work on ways the coupling of electrons to lattice vibrations could play a significant role. He was, however, working in a highly unsatisfactory atmosphere (he was still a member of Shockley's group, and Shockley gave him no latitude to work with experimentalists) and began to explore opportunities elsewhere. When he told his old friend Fred Seitz, who had moved in 1949 to lead a group in solid-state physics at the University of Illinois in Urbana-Champaign, that he was ready to leave Bell Laboratories, Seitz quickly put together an offer for Bardeen to join him there, with a joint appointment in the Department of Electrical Engineering and the Department of Physics, which Bardeen accepted.

Bardeen's reasons for leaving Bell Laboratories were spelled out in detail in a memo written to its president, Mervin Kelly, on 24 May 1951. Because of its historical importance and its relevance to understanding John Bardeen, his perspective on the invention of the transistor, and his working conditions at Bell following its discovery, I reproduce that memo here in its entirety.

> The following is an account of the circumstances which led to my decision to leave the Laboratory. I had hoped to have a discussion with you before the final decision was made, but I first wanted to explore the decision at a lower level. After talking with Bown, Fisk, Morgan

and Shockley, and before I had a chance to talk with you, the time arrived when I had to decide if I were to start at Illinois next Fall.

The position at Illinois is a very attractive one. I am to be in both the Physics and Electrical Engineering Departments. Teaching obligations are a minimum; I will probably teach only one course and that will be of my own choosing. The present plan is to give a course on the electrical properties of solids. My own research, at least for the next year or so, will most likely be on superconductivity. Activities in Electrical Engineering will be confined mainly to teaching. The financial prospects appear to be as good as those at the Laboratories.

Nevertheless, I would not leave if I were not dissatisfied with conditions here. In fact I would not have received the offer if I had not let it be known that I was considering leaving the Laboratories. Fisk knew several months ago that I was thinking seriously about academic work and of another offer that I had at that time.

My difficulties stem from the invention of the transistor. Before that there was an excellent research atmosphere here. My own work, by choice, was in the field of semiconductors. None of us had worked in the field during the war. Shockley was instrumental in getting the work on semiconductors started, and was quite interested in it, but his main interest, in his own research, was in other fields.

After the invention, Shockley at first refused to allow anyone else in the group (1170) to work on the problem (that is, aside from Brattain, Gibney, and myself) and then did so only as he thought of problems of his own that he wanted investigated experimentally. In most cases these were problems in which he had already done some theoretical work or in which he wished to do some theoretical work himself in the future. In short, he used the group largely to exploit his own ideas. Since my own work is largely on the theoretical side, I could not contribute to the experimental program unless I wanted to work in direct competition with my supervisor, an intolerable situation.

This was a deliberate policy. Shockley himself was aware of the situation and indicated to me on numerous occasions that that was the way he wanted it to be. He suggested that I work with Morton's group, or with Ohl, but these solutions naturally did not appeal to me. This policy meant that I could not play an active role in the semiconductor research program, and could work directly only on the few problems (such as with Briggs on infrared absorption) in which Shockley was not interested himself, or at times on those on which Brattain was working.

For this reason, I seriously considered leaving the Laboratories about two years ago under much less favorable circumstances. At that time I discussed with Bown the difficulties outlined in the preceding paragraphs. Bown's reaction was that Shockley was in a highly emotional state (he was working 70 or more hours a week at the time) and that the difficulties would be resolved in time. In this and in later conversations he made it clear that it was the desire of the administration to give Shockley a free hand.

Instead of getting better, conditions if anything got worse. About a year ago I decided to give up work on semiconductors and work on superconductivity. This decision, discussed at the time with Fisk, was made before I learned about the isotope effect (which indicated the direction the theory of the effect should proceed) but I did not actually start work on superconductivity until afterwards. The theory on which I have been working for the past year is based on the isotope effect and is an outgrowth of the theory on which I worked before the war.

I have felt somewhat isolated working on superconductivity here as there are very few people in the Laboratories who are interested in the problem. Bown and Morgan have proposed getting some experimental work started, and this is no doubt desirable. However I feel that I can work on superconductivity more effectively in a university. The problem is one of more scientific than practical importance and there is great interest in it in academic circles.

Furthermore, in a university I will be able to work on semiconductors under more favorable circumstances than I can here. Before making the decision to leave, I again explored the possibility of working on the semiconductor program with Shockley. His attitude has not changed. He felt that he could supply all the ideas required, that he would want the people in his group to work on his ideas, and that I would not be happy in this situation. It has been suggested at various times that an independent semiconductor group be set up under my direction, but I did not feel that this was a satisfactory solution from my point of view. In the discussion with Shockley he indicated that he was unwilling to give up any significant part of the work.

To summarize, the invention of the transistor has led to the semiconductor program being organized and directed in such a way that I could not take an effective part in it. I could work on superconductivity, but I feel that I could do this better in a university where it is of primary rather than secondary interest.

I also feel that university work, in which one can set one's own pace, becomes relatively more desirable as one gets older. Therefore I have decided to leave, even though moving at this stage of my life is a difficult and costly business, for my family as well as myself. The move will also mean that I will have to start building a pension fund over again.

There are of course many regrets on leaving and breaking many pleasant associations. The work I have done while here has enhanced my reputation so that I have been able to get an excellent position outside the Laboratories.

I would be glad to discuss the matter further with you if you so desire.

Bardeen moved with his family to Urbana in the fall of 1951, with the understanding that he would be able to establish a laboratory devoted to semiconductor research in the EE department and pursue his theoretical research on superconductivity in the physics department. Illinois provided a most happy change of scene and a superb research environment for Bardeen, one in which he was now free to decide how he wished to allocate his research time between semiconductors and superconductivity, and had university, and soon government, support to work on both, with a grant from the Army Research Office to work on superconductivity.

Nowhere were Bardeen's approach to science, his mathematical skills, insight, imagination, and perseverance more evident than in his work at Illinois on superconductivity. As his first postdoctoral research associate, I had an opportunity to observe, from a desk in the corner of his office, how John pursued a multi-pronged attack on the problem during the period 1952–55.

Ere describing this, I remind the reader that the search for a microscopic theory of superconductivity was but one of John's activities during this period. He established a laboratory and a major semiconductor research group in the electrical engineering department, where in 1952 Nick Holonyak became his first EE Ph.D. student. In 1953, he directed the first international summer school in semiconductor electronics, which ran for two weeks in Urbana during an appalling heat wave. During this period he also became increasingly active as the major scientific and technological consultant to the Haloid Corporation, soon to become the Xerox Corporation, and took on his first Ph.D. advisee in physics, Bob Schrieffer.

In tackling superconductivity in Urbana, Bardeen first of all immersed himself in the experimental literature and devoted himself to developing a phenomenological description of the essential emerging experimental facts. In his masterly review article written in 1955 and published the following year in the *Handbuch der Physik*, he showed that an energy gap in the electronic excitation spectrum could explain many of these. Second, he tried to develop new methods to examine the behavior of a few electrons excited above the ground state, exploring matrix methods that might enable him to see how their mutual interaction might give rise to an energy gap in the excitation spectrum.

I was directly involved with two other approaches. Bardeen believed that among the new theoretical techniques that might be required would be treatments of a coupled electron-phonon system that went beyond the perturbative weak coupling approach he and Frohlich had adopted. So when I arrived in Urbana in the summer of 1952, he encouraged me to work on a strong coupling problem, the behavior of polarons, electrons in polar crystals that are coupled strongly to optical phonons. John started me off by giving me some papers by Frohlich and Solomon Pekar to read. Soon thereafter, in the course of a corridor conversation with Tsung-Dao Lee (who had a summer appointment in Urbana stemming from funds for basic research that the department head, Wheeler Loomis, had set aside from UI's operation of a governmentally sponsored classified project), we realized that the field theoretic intermediate-coupling approach T. D. had been applying in meson-related problems could be applied to polarons and soon wrote up our results, which provided a valuable step beyond a perturbation-theoretic treatment. Together with Francis Low we then showed how our intermediate-coupling solution could be obtained with an especially simple ground state wave function in which successive phonons coupled to an electron are emitted in the same quantum state.

At about this time, Frohlich found, using second-order perturbation theory, that the phonon-induced interaction between a pair of electrons was attractive for low excitation energies. This result was quite interesting, but was not on a firm footing, because Frohlich had ignored the very much stronger Coulomb repulsion between electrons. As the great Soviet theoretical physicist Lev Landau was fond of saying, "You cannot ignore Coulomb's law." So Bardeen encouraged me to continue work on a calculation I had already begun, developing a more complete theory of the interacting electron-phonon system by extending the collective description developed in my Ph.D. thesis at Princeton with David Bohm to include the influence of electron-electron interactions on electron-phonon interactions.

I made some progress on this, but found I was stuck, in that I could not arrive at a self-consistent account of the system behavior; then one morning, when I was describing my lack of progress to John, he suggested that I consider adding a term describing the phonon field to the Bohm-Pines collective coordinates that described the long range part of the electron interaction. This appeared to be a promising approach, and we started working together on the problem. By early 1954 we arrived at a consistent description, within the random phase approximation, of the combined consequences of electron-phonon and electron-electron interactions on a system of coupled electrons and ions. The resulting phonon dispersion relation turned out to be just what Bardeen had calculated seventeen years earlier in his seminal 1937 paper on lattice vibrations in simple metals, while the effective electron-electron interaction turned out to have properties similar to those Frohlich had found, but now with full account having been taken of all the interactions in play.

We found that for pairs of electrons whose energies differed from each other by less than a typical phonon energy, despite the repulsion coming from the now present and properly screened Coulomb interaction, the attractive interaction brought about by phonon exchange would win out, so that the net effective electron-electron interaction would be attractive; for larger energy differences, the repulsive screened Coulomb interaction would win out and the net interaction would be repulsive. Since a full account of the Coulomb interaction had now been taken into account, we argued in our paper published in 1955 that a microscopic theory of superconductivity could be developed based on this interaction.

This turned out to be the case, but much more work needed to be done.

In remarkable accord with Bardeen's intuition, each of his four thrusts (matrices involving a small number of electrons, the Bardeen-Pines effective electron interaction, the intermediate coupling solution of the polaron problem, and his phenomenological theory) turned out to play an important role in the development during the next two years of a successful microscopic theory of superconductivity by Bardeen, his new postdoc, Leon Cooper (who succeeded me in the fall of 1955), and his graduate student, Bob Schrieffer.

Bardeen suggested that Cooper begin by learning about earlier work on superconductivity, especially by reading the treatise by David Shoenberg and discussing at length with Bardeen the draft of his *Handbuch der Physik* review. Cooper then began to explore the use of multiple scattering matrix approaches to study the behavior of a pair of electrons excited above the ground state, under the assumption that their interaction with the ground state electrons was similar to the one Bardeen and I had derived. By the early fall of 1956, he found that for such an effective attractive interaction, the ground state might be unstable against the formation of bound electron pairs. This was a quite important result: it confirmed the Bardeen-Pines-Frohlich conjecture that the role of the phonons in superconductivity was to bring about an attractive interaction between electrons; and it showed that low-lying pairs of electrons of opposite spin and momentum were especially susceptible to this interaction. But a microscopic theory was still not at hand, because it was not at all clear how to go from the anomalous behavior of an excited single pair to the kind of coherent behavior, including the rigidity of the ground state against external disturbances predicted by London, that was required to explain superconductivity.

In the fall of 1956, Bardeen received word that, together with Walter Brattain and William Shockley, he had received the 1956 Nobel Prize for the invention of the transistor. This meant that Bardeen had to take time off from superconductivity to put together his Nobel lecture, and then travel to Sweden to receive the prize. For many Nobel laureates, receiving the prize represents a culmination of their life's work, and they spend the succeeding years on the "Nobel circuit"—responding to the many, many requests for lectures to audiences of every stripe, from learned societies to elementary school students. For Bardeen, while the prize was a wonderful recognition of his invention, it was also a major distraction, as time spent on Nobel-related activities was time away from his search for a microscopic theory of superconductivity, so immediately on his return from Stockholm he resumed his work with Cooper and Schrieffer.

That search was soon rewarded. As he describes in detail in his 1964 book, *The Theory of Superconductivity*, Schrieffer had been working with Bardeen to find a variational wavefunction that might describe the ground and excited states of a superconductor. He was aware, through its application to the polaron problem, of the intermediate coupling approach developed by Tomonaga to deal with the coupled meson-nucleon problem. In late January 1957, while riding an NYC subway train following a meeting in Hoboken, New Jersey, on "Many-Body Theory," Schrieffer had an "Aha! moment." He realized that he could generate a candidate wave function by assuming that the key physics was the formation of a macroscopically occupied coherent quantum state made up of pairs of electrons (of opposite spin and momentum) and then adapting the form of the Lee-Low-Pines intermediate-coupling ground-state wave function to describe it. He found that he could calculate in this way an energy gap and a condensation energy—the gain in energy for the system when it becomes superconducting. On his return to Urbana, he showed his results to Cooper and then to Bardeen. Bardeen quickly recognized that this was the correct basis for finding a solution to superconductivity, and with great excitement he, Schrieffer, and Cooper began to work out its consequences for the ground and excited states of a superconductor.

Within some two weeks, they had developed the microscopic theory that soon became known as BCS, and on 16 February 1957 sent a brief account of their results for publication in *Physical Review Letters*. Their results included the microscopic description of the two fluids that characterize superconducting behavior: the superfluid, a single macroscopic quantum state, formed by the condensation of pairs whose average spacing is large compared with the inter-electron spacing, that flows without resistance and acts to screen out external magnetic fields; and a normal fluid that is made up of the "pair-breaking" elementary excitations that a finite amount of energy—the energy gap—is required to excite. These quasiparticles scatter against each other and impurities much as normal electrons do. The new theory was also able to explain the quite surprising results of a measurement that had just been completed in Urbana by Charlie Slichter and his student Chuck Hebel, on the change in the nuclear spin-lattice relaxation rate when a material becomes superconducting.

In using Schrieffer's wave function to calculate various properties of the superconducting state, Bardeen, Cooper, and Schrieffer were guided at every stage by the phenomenological description Bardeen had enunciated two years earlier, with what quickly became recognized as remarkable success.

It was typical of Bardeen that he wanted his young collaborators to receive significant credit for the theory. To this end, he arranged for post-deadline papers on the theory to be given by Cooper and Schrieffer at the forthcoming "March" meeting of the American Physical Society in Philadelphia, while he would stay home. Following that meeting, at which Cooper presented their results to an enthusiastic overflow audience (Schrieffer was not present because of a missed train/plane connection), the three collaborators then worked intensively on developing the theory in further detail during the spring and early summer, and submitted a full account of their work to the *Physical Review* in early July 1957.

So how was it that Bardeen and his young collaborators were able to solve the riddle of superconductivity? The solution had, after all, eluded the many distinguished theoretical colleagues who were working on the problem at that time—notably Feynman, Landau, London, Frohlich, Ginzburg, Gor'kov, Blatt, and Schafroth. The answer is, in part, to be found in Bardeen's emphasis on understanding the experimental facts and developing a phenomenological description of these while simultaneously pursuing a number of different theoretical scenarios with his younger colleagues. Equally important was his total dedication to cracking the problem, and the encouragement, support, and freedom to pursue their own ideas that he gave to his younger colleagues, who played such key roles in the development of the theory.

The story of BCS is now a high point in the history of physics in the twentieth century. The theory, for which its authors received the 1972 Nobel Prize in Physics, not only explained all existing experiments on superconductors, but made a number of predictions that were subsequently verified. It quickly had an impact on other fields of physics. In the summer of 1957 the key BCS idea—that a net attractive interaction between fermions (particles of intrinsic spin ½) would always lead to a pairing state that was macroscopically occupied—was applied to atomic nuclei by Aage Bohr, Ben Mottelson, and the author, and soon thereafter was taken into the realm of particle theory by Yoichiro Nambu and Gianni Jona-Lasinio. Indeed, within two years it had become so clear that the BCS theory was successful that David Shoenberg, in his introductory remarks at a 1959 superconductivity conference at Cambridge, was led to make his classic remark, “Let us see to what extent experiment can explain the theoretical facts.”

In the years following BCS, while continuing to pursue applications of the theory to a variety of challenging problems in superconductivity, Bardeen became increasingly involved with Haloid, soon to become Xerox, and with advising the government. John was a highly respected adviser to John Dessauer, Haloid’s vice president for research and development. During his frequent visits to Rochester, as George Pake writes,

> John [Bardeen] urged a strong research effort and suggested that a judicious use of government contracts could aid the fledgling company in supporting its research effort, which was proportionately large for a company of its (then) small size. . . . [I]n 1958, [its] total cash flow was less than $5 Million, and it budgeted almost $2 Million for basic research. John was in some respects the scientific mastermind of the effort to develop xerographic technology, . . . and influenced the nomination and selection of new hires for the rapidly expanding research team.
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> In 1960 Xerox’s Model 914 plain-paper copier burst upon the scene. The machine used a selenium photoreceptor, a result of pioneering R&D that drew heavily on Bardeen’s fundamental insights and his encouragement of Haloid-Xerox scientists, engineers and top management. Little wonder, then, that in 1961, the year the corporation shortened its name to Xerox, Bardeen was elected to the board of directors, a position he held until he reached the age of board retirement in the 1970s. Bardeen served in a dual role. He was not only a member of the Board of Directors, but he also served on the corporation’s technical advisory committee, a small group of distinguished external scientists and engineers who occasionally reviewed Xerox’s overall research program and advised senior management on scientific opportunities and overall R&D philosophy. In this latter role, Bardeen was highly valued not only by Dessauer, but also by the senior corporate management and by Dessauer’s successors—Jacob E. Goldman and [George Pake]. . . .
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> By 1972 Xerox PARC had been established and was already well into its pioneering of laser xerographic printing, an application for which the prospect of solid-state lasers with low power consumption and long life offered high promise. Bardeen’s advice was instrumental in PARC’s assembling a small group of University of Illinois PhDs (including students of Holonyak’s and thus, in a PhD-progeny sense, “grandchildren” of Bardeen’s). This little group was later to establish world leadership in high-power-output solid-state laser structures, another example of how Xerox benefited from John’s advice to research management on strategic technological investments. In his activities

> as a director and technical advisory committee member, Bardeen found many occasions over the years to visit the research centers by himself. His visit to a scientist's laboratory was often an impressive event. Although John was a man of few words, he was a superb listener. His comments and advice about the experimental program drew upon a massive base of understanding and scientific vision unique in the scientific world. Imagine the effect of having this powerhouse, first-ever two-time physics Nobel laureate focusing intently on your research right there in your own laboratory and joining with you to assess likely next steps!
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> Not only were John's advice and counsel of great value, but his presence as a board member and consultant was of incalculable value in recruiting new scientists to join Xerox research. I myself [George Pake] was strongly influenced by this factor as I struggled over whether to leave academia, where I had spent most of my life. And in recruiting scientists and engineers to the new PARC, I found that John's high-level presence in Xerox provided almost instant validation of the corporation's serious intent in research—an especially important factor given the episodic histories of other corporations that have tended to turn research on and off with the ups and downs of the business cycle. John knew that successful industrial research requires consistent investment, and his steadfast embodiment of this view at the board level may have had much to do with the enviable Xerox track record of steady investment in basic and upstream research.

Bardeen's influence on industry was not confined to Xerox. For a number of years he was a major consultant for General Electric and he served as a key expert witness for Texas Instruments when it filed successfully for a Japanese patent on aspects of Jack Kilby's basic integrated circuit invention. In addition, as Pake writes, "he helped the start-up companies that some of his former students occasionally launched. An example is Supertex, incorporated in 1975, for which John served on its Board of Directors (from 1983 to 1991) . . . and influenced [its] strategic decision to emphasize high voltage integrated circuits."

Significantly, although not a formal consultant, Bardeen had a close relationship with the Sony Corporation of Japan that began with his initial visit there in 1953. He formed a close relationship with George Hatoyama, the first director of the Sony research center, with his successor, Makato Kikuchi, and with the company's founder, Akio Morita. In the years that followed, Sony celebrated the importance it attached to the "Bardeen" connection in a number of ways. Hatoyama came to the 1968 UIUC Symposium honoring Bardeen on his sixtieth birthday with the perfect birthday gift for John: a golf ball containing a transistorized radio, which became one of his prized possessions. Some twenty years later, in 1989, Sony made what was at the time the largest single gift ever made by an industrial corporation to a university, a gift of $3 million to establish the John Bardeen Chair in Physics and Engineering at UIUC. The following year, it gave an early eighty-second birthday party for him on what turned out to be his last visit to Tokyo; a photograph of John, with Jane at his side, blowing out the candles on a cake made in the shape of the first radio transistorized by Sony, may be found in George Pake's article in *Physics Today*.

John Bardeen also became a trusted adviser to the U.S. government. He was a founding member in 1959 of the first major Presidential Science Advisory Committee, appointed to advise President Dwight Eisenhower, and remained on PSAC until 1962, a period during which it exerted a powerful influence on decisions made by Eisenhower and his successor, John F. Kennedy. He then served on President Johnson's Commission on the Patent System, and in 1982 was again a presidential adviser, as a member of President Reagan's White House Science Advisory Council. In the latter capacity, he produced with David Packard an important report on the scientific output and potential usefulness of governmental laboratories, ere resigning from the council in 1983 to protest Reagan's decision to go ahead with "Star Wars," the Strategic Defense Initiative, about which Bardeen correctly felt that the council had been insufficiently consulted. A measure of Bardeen's strong feeling on the issue is that he subsequently wrote with Hans Bethe a letter to the *New York Times* asserting that the SDI offered little prospect of success while, as Pake writes, "deflecting the nation's limited scientific manpower away from pursuits that would have strengthened the competitiveness of the US civilian economy." Bardeen continued to have strong views on this topic, and subsequently wrote about SDI in 1986, "At a time when our civilian economy needs all the help it can get to remain competitive in world markets, the best scientific and technical brains in the country may be drawn off to work on a project of dubious value."

In much the same way as with Xerox, Bardeen's high-level presence in Urbana made it possible for the UIUC physics department to develop a world-leading group in condensed matter theory. The opportunity to interact with John on a daily basis was the reason I rejoined the UIUC faculty in 1959. Together we persuaded Bob Schrieffer to join us there. After Bob left for Pennsylvania in 1962, we persuaded Leo Kadanoff and Gordon Baym to come to Urbana; they were later joined by Christopher Pethick, Charles Duke, and Bill McMillan, who had received his UIUC Ph.D. under John's supervision. As a group, with John as our senior and most distinguished member, we had continuing support provided by government grants (from the Army Research Office, the Office of Naval Research, and the National Science Foundation, some initially obtained by Fred Seitz before he left Urbana to head the National Academy of Sciences) during the period 1959–69; we were able to use that support to attract a remarkable group of postdoctoral fellows, among whom were John Hubbard, Ludwig Tewordt, Franco Bassani, Massimo Altarelli, Brian Josephson, Anthony Leggett, Christopher Pethick, Wolfgang Goetze, and Michael Moore.

In addition to advising industry and government, Bardeen was generous of his time in giving talks to a remarkably broad spectrum of audiences eager to hear his views, audiences that spanned the gamut from elementary school children to medical societies. But throughout the two decades following BCS his central interest remained in physics, beginning with an authoritative review of BCS and its applications written with Bob Schrieffer in 1961, and including his study of applications of BCS theory to a number of superconducting phenomena, such as the motion of flux lines in type-II superconductors.

One of the most remarkable consequences of the BCS theory was worked out in 1961 by a graduate student at Cambridge University, Brian Josephson, who argued that if two superconductors were separated by a thin insulating barrier, it would be possible for pairs of electrons to tunnel through that barrier from one superconductor to another. Surprisingly, Bardeen, who had worked extensively on developing a microscopic description of quantum tunneling, did not immediately accept this proposal, and a substantive debate over its correctness ensued. It was an almost unique example of Bardeen's being wrong on a major issue in physics, as during the following year a number of experiments showed that Josephson was correct. Bardeen not only graciously conceded that he had been wrong, but soon thereafter invited Josephson to spend a year with him as a visiting postdoctoral fellow in Urbana.

During the 1960s, Bardeen, in common with many of his colleagues, also began considering the possible existence of a superfluid state of liquid ^{3}He brought about by a net attraction between its fermionic quasiparticles. One of the leaders of the search for that new state of quantum matter was Bardeen's colleague John Wheatley, who was carrying out experiments two floors down from John's office on the behavior of liquid ^{3}He at increasingly low temperatures and on a new kind of quantum liquid, dilute mixtures of liquid ^{3}He immersed in a bath of superfluid ^{4}He. John's interest in the ^{3}He-^{4}He liquid mixtures was twofold: he hoped to sort out their transport properties, and he wanted to see whether the ^{3}He subsystem might undergo a transition to the superfluid state in the ^{4}He background. That seemed at the time to be as likely as the superfluidity of pure ^{3}He, which was proving elusive and which remained undiscovered for some fourteen years following BCS.

Bardeen followed Wheatley's experiments on an almost daily basis. His basic insight into their behavior was vintage Bardeen: he recognized from the outset that to first approximation, there was no macroscopic way to distinguish ^{3}He from ^{4}He, apart from the greater zero-point energy of the former. Therefore, he reasoned that the key physical parameter would be the fractional increase in the molar volume that accompanied the introduction of ^{3}He atoms into liquid ^{4}He, and that the effective interaction between the ^{3}He atoms would be proportional to the square of this fractional increase. John told Gordon Baym and me about his result, and we decided to see if we could derive it from first principles by using response functions to characterize the response of the background liquid to the introduction of ^{3}He. After some time, we obtained John's result, which we found came about as a result of a cancellation between the direct interaction between ^{3}He atoms and their induced interaction produced by the exchange of virtual phonons in the background ^{4}He. Given the rather subtle interplay of many-body effects responsible for this cancellation, Gordon and I marveled that John had known intuitively, from the beginning, what the correct answer would be. The three of us then worked out in some detail the transport properties of the mixture. Unfortunately, the transition temperature for the weakly interacting ^{3}He quasiparticles to become a superfluid turned out to be so low that one could not have the hoped-for opportunity to study the many fascinating new phenomena found in a mixture of two superfluids.

In the decade after BCS, although many new superconductors were discovered, the transition temperature for the appearance of superconductivity never exceeded a temperature ~25K, while a calculation by M. Cohen and P.W. Anderson suggested that might indeed be a "phonon ceiling"—a maximum transition temperature for superconductivity originating in a phonon exchange between electrons. A number of theorists, including Bardeen, began to explore the possibility of other ways to achieve a net attraction between electrons, in the hope of finding a purely electronic mechanism that could yield materials with higher superconducting transition temperatures. Bardeen's entry in this electronic sweepstakes was developed with his last Ph.D. students, David Allender and Jim Bray; they proposed in 1973 that in materials containing excitons (bound electron–hole pairs), an exchange of excitons between electrons might lead to a net attraction and give us higher-temperature superconductors, a possibility that has yet to manifest itself.

In working on these and other problems with students, postdocs, or colleagues, John did not take an Olympian stance. He was there in the trenches, carrying out detailed calculations, checking factors of two, and being intimately involved in writing up the results for publication.

Instead of basking in the glory brought his way by a continuing output of remarkable, seminal contributions, he preferred always to work on the next scientific challenge. There was almost no area of condensed matter physics about which he had not thought deeply, from many-body theory to the motion of dislocations, and because of his willingness to take on new challenges, he was much sought after for advice and counsel when a new puzzle turned up in a paper or in the laboratory. For example, in 1953, when Keith Brueckner was developing his theory of nuclear matter, he sent his preliminary results to Bardeen, and put forth the argument that his approach might indeed provide an exact solution to a many-body problem. Bardeen went through his calculations in detail, and then pointed out to Brueckner that there were a number of higher order terms in the strength of the interaction that he had not been able to take into account, so that his theory could not be considered exact.

On the day in 1972 when John received word that he, Leon, and Bob had been awarded the Nobel Prize for the BCS theory, he was late getting to the office. His garage door opener had malfunctioned (not, John informed me, because of any problem with the transistors involved). He arrived, finally, eager to get on with the scheduled major activity for the day: attending a lunch seminar by David Lee from Cornell, who was to describe the results of his experiments with Doug Osheroff and Bob Richardson on the very-low-temperature properties of liquid ^{3}He that Tony Leggett had interpreted as providing evidence for a superfluid phase. So at the seminar we celebrated with two liquids: Champagne and superfluid ^{3}He. The properties of that finally discovered quantum liquid turned out to depend in an essential way on the fifteen-year-old BCS theory being honored that day with the bubbly liquid.

John retired in 1975 from his professorship, some three years before the statutory date. He wanted to make it possible for the departments to appoint younger people. But he also wanted more time for travel, and for his grandchildren. However, when John was in Urbana, his routine was essentially unchanged from his pre-retirement days. He would come to the office early and leave late—unless the day was unusually promising for golf and he had an early afternoon golf date. His office door was always open, to signal his availability for a scientific discussion. His interest in physics, and in creating new physics, never flagged. It was only during the last year of his life that he began work on the archival task of putting his scientific papers and correspondence in order, and I suspect he started that project only because his eyesight had become so poor (as a result of macular degeneration) that it was very hard for him to spend the whole day calculating and reading the literature.

Bardeen's main scientific drive following his retirement, and extending through the 1980s, was to understand the transport of electrons by moving charge-density waves in quasi-one-dimensional metals. John wrote about it in what turned out to be his last scientific article, a "popular" account of superconductivity and other macroscopic quantum phenomena that appeared in the December 1990 issue of *Physics Today*, just a month before his death. He had concluded that the depinning of such charge-density waves was a macroscopic phenomenon associated with tunneling from one discrete quantum state to another. Among theorists working on this problem, John was almost alone in believing that quantum rather than quasiclassical calculations were essential to understanding the data. Although his theory accounted in a simple way for the essential experimental facts, John had great difficulty getting his initial paper on the subject published in *Physical Review Letters*. His oft-expressed fury at the prejudice and ineptitude of its referees was almost unbounded. It was a source of continuing frustration to him that his approach to quasi-one-dimensional metals was not yet widely accepted more than a decade after its development. John confided to me that his principal motivation for writing the *Physics Today* article was to set the record straight. In it he wrote, "In spite of the remarkable success of the tunneling model over more than a decade, many theorists . . . still try to account for the data with classical theories that ignore the tunnel step."

Bardeen was delighted by the 1986 discovery of the high-temperature superconductors by Georg Bednorz and Alex Mueller. He followed the subsequent experimental work closely, and he frequently discussed candidate theories with his colleagues in Urbana and elsewhere. It was characteristic of John that when he was considering possible mechanisms for high-temperature superconductivity, he didn't focus on the exciton mechanism he had proposed. He preferred to let the experiments decide. (For the record, as the experimental results started coming in, John became certain that a purely electronic mechanism was at work and came to think that a spin-fluctuation mechanism stood the best chance.)

John Bardeen was a remarkably self-contained and self-confident individual, so much so that he was sometimes referred to as "Silent John," as a way of capturing his habit, when talking with someone, of saying nothing for what seemed to be an extraordinarily long time—maybe fifteen to thirty seconds—before making what was often an understated response. While very much interested in what others had to say, he felt no great need to talk about himself or his accomplishments or to express his views at length. His persona in this respect was clearly formed early on. Walter Brattain liked to tell 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. This was perhaps the first of innumerable Bardeen stories that focused on his laconic tendencies. I include two others here.

His wife, Jane, told my wife, Suzy, that on the December 1947 day of his discovery of the transistor, John came home with an unusual air of excitement, telling her, "We discovered something quite interesting in the lab today." Jane, who was in the midst of stringing beans, replied, "Please wait until later to tell me about it, as I am quite busy fixing dinner." As Jane tells it, the moment then passed. John went on to other activities, and she lost her chance to learn what had happened that day in the laboratory, as Bardeen understood the implications of Walter Brattain's latest experiment.

Charlie Slichter, John's longtime UIUC physics colleague, tells of meeting John almost a decade later: "Then one morning in early February [1957] I walked out of my office on the second floor of the Physics Building and encountered John. He clearly had something on his mind and wanted to say something to me. John was not much of a talker. I realized that if I spoke up, I would preempt what he wanted to say. So we just stood there while I waited. It seemed like forever, but was probably just a few seconds. Then he said 'Well, I think we have figured out superconductivity.' What a message! That was no doubt the most exciting event in my life as a scientist. Evidently the night before John, Bob, and Leon had become convinced that they had the solution in hand."

John loved to travel. His first trip to Europe was with Bill Shockley in 1947. On his return he wrote an extensive account of that trip, during which he had met leading figures in condensed matter physics such as Neville Mott and Louis Neel.

He particularly valued the scientific friendships he had formed, and he welcomed the opportunities that travel to scientific conferences abroad provided for renewing old relationships and making new ones. When going to Europe, he would prepare for the time change days in advance, getting up an hour earlier each day, so that his biological clock was almost completely reset upon his arrival.

Bardeen's travels to Japan, China, and the Soviet Union were especially meaningful to him. John was perhaps the first theorist in the West to appreciate the importance of the work of Vitaly Ginzburg and Lev Landau on the superconducting phase transition, its application to type-II superconductors by Alexei Abrikosov, and the microscopic derivation of the Ginzburg-Landau equations from the BCS theory by Lev Gor'kov in 1958. John first went to Moscow in 1959, following a meeting in Prague on semiconductors. His trip was arranged on rather short notice by Abram Ioffe, a pioneer of solid-state physics in the USSR, who with his wife had met Bardeen at an international conference in Ottawa in 1956. Ioffe had been unsuccessful in getting permission for Landau to come to that meeting or to visit the U.S. So on meeting Bardeen again in Prague, Ioffe felt it especially important that John meet Landau and the theoretical group around him, and arranged a visit on very short notice. John was deeply impressed with the physicists he met in Moscow—not only with their scientific capabilities, but also with their remarkable personal qualities, formed in no small part by the need to survive under a repressive regime.

In December 1963 John made his second trip to Moscow, to attend an All-Union Conference on Theoretical Physics. Together with the other members of the U.S. community invited there (Leo Kadanoff, Paul Martin, Walter Kohn, Pierre Hohenberg, and the author), John began to develop what became close scientific and personal relationships with the members of Landau's school of theoretical physics (Arkady Migdal, Evgeny Lifshitz, Ginzburg, Ilya Lifshitz, Isaac Khalatnikov, Gor'kov, Igor Dzyaloshinskii, and Abrikosov, among others). They were still very much feeling the pain of losing Landau in a freak car accident the previous year, and we were all still suffering from the tragic assassination of John F. Kennedy a few weeks earlier. Our relationships were enhanced by a series of joint U.S.-USSR conferences on theoretical physics during the following decade, in which John was an active participant. With Jane, he traveled to the USSR on several other occasions, the last of which was in 1986 to receive the Lomonosov Gold Medal of the Soviet Academy of Sciences, which had been awarded to him in 1985. In 1990, as it became possible for members of the Landau school to travel abroad for extended periods, John and I arranged for Lev Gor'kov to spend a year in Urbana as a visiting faculty member, and John was instrumental in nominating Gor'kov for the honorary degree he received at the 1991 UIUC commencement.

John went to China not long after Nixon, as a member of a 1975 NAS-sponsored delegation in solid-state physics, while his first trip to Japan had been much earlier, when he and a small group of American theorists were invited to an international conference in Kyoto in 1953. He returned to Japan often, forming a number of close relationships with Japanese scientists (especially with the theorists Sadao Nakajima, whom he first met in 1953, and Toshihiko Tsuneto, who had been John's student in Urbana in the early 1960s) and, as noted earlier, with leading members of the Japanese corporate community. He became increasingly impressed with Japanese technology and the use the Japanese had made of the transistor and related electronic devices. John especially admired the long-term view that leading Japanese corporations had adopted, as evidenced by their willingness to make substantive commitments to projects that might not pay off for a decade. He often contrasted that view with the much shorter attention spans of their U.S. counterparts.

John was not only loved and admired and very deeply respected by his students and colleagues, but he was also much honored and celebrated by the broader community. He remains the only scientist to have won two Nobel Prizes in Physics. He was a founding member of the UIUC Center for Advanced Study, and was elected to the National Academy of Sciences, the American Philosophical Society, and the American Academy of Arts and Sciences, and to foreign membership in the Royal Society, the Soviet Academy of Sciences, and the Hungarian Academy of Sciences. In addition to the 1985 Lomonosov Medal, he received the 1952 Stuart Ballentine Award of the Franklin Institute, the 1955 John Scott Medal of the City of Philadelphia, the 1955 Buckley Prize of the American Physical Society, the 1962 Fritz London Award for low temperature physics, the 1964 Vincent Bendix Award of the American Society for Engineering Education, the U.S. National Medal of Science in 1965, the Medal of Honor of the Institute of Electrical and Electronic Engineers (1971), the James Madison Medal of Princeton University in 1973, and honorary degrees from Cambridge University, the University of Glasgow, the University of St. Andrews, the Indian Institute of Technology, Clarkson College of Technology, Georgetown University, Harvard University, the University of Illinois, the University of Michigan, the University of Minnesota, Notre Dame, Princeton University, the University of Pennsylvania, Rensselaer Polytechnical Institute, the University of Wisconsin, Union College, and Western Reserve University. He was inducted into the Inventors Hall of Fame in 1974, and received an Alexander von Humboldt Award in 1977, the University of Illinois Distinguished Medallion in 1978, and the Michaelson Morley Award from Case Western Reserve University. In 1990, Bardeen was one of eleven recipients of the Third Century Award honoring exceptional contributions to American creativity. He was also named by *Life* magazine as one of the one hundred most influential people of the century.

Bardeen also played a leading role in the organization and governance of scientific activities. With Frederick Seitz and Roman Smoluchowski, he was instrumental in the formation in 1947 of the division of solid-state physics (now condensed matter physics) of the American Physical Society, and served as the American Physical Society's president in 1968–69. He was a founding member of the Commission on Very Low Temperatures of the International Union of Pure and Applied Physics in 1968 and served as the commission's chairman from 1969 to 1972.

John's devotion to friends and family extended to his students and postdocs, who became, through John and Jane, part of the extended Bardeen family.

Bardeen's sixtieth birthday was the occasion for a major celebration in Urbana that brought together his former students, his mentors Eugene Wigner and John Van Vleck, his former colleagues from Bell Laboratories, and his admirers at GE, Xerox, Ford, and Sony for a day of talks and a banquet celebrating science and John. One of its highlights was the presentation to John by Bob Schrieffer of a large poster that depicted John as a (super)conductor, waving a baton, and his students as members of his orchestra. From then on, the poster occupied a position of pride in John's office. On the occasion of his early retirement in 1975 from UIUC, his colleagues organized in his honor a second major symposium that focused on the problems not yet solved in physics and in the semiconductor industry, while his eightieth birthday provided an excellent occasion for bringing his former students, colleagues, and family together. In keeping with the best of family reunions, it offered a chance for John to express his deep affection for his extended family, and for that family to express its love and admiration for John. That respect and admiration was shared by all those who came into contact with him, and continues to this day; at the banquet held as part of the UIUC 2007 celebration of fifty years of BCS theory, very nearly the entire evening was spent exchanging stories about encounters with John, in which his quiet demeanor, marked by rare occasional bursts of public emotion, played a starring role.

John's passion for science was almost matched by his passion for golf. At moments of triumph on the golf course he gave vent to more exuberant expressions of pleasure than he permitted himself to display in science. Once, when he had pitched out of the rough directly into the cup, his golfing companions watched him jump up and down with excitement, shouting, "Did you see that shot go in?" "I bet he didn't do that when he won the Nobel Prize," one of them remarked. In another match John hit a hole in one. When he was asked how it felt, he replied after careful thought, "Well I guess two Nobel Prizes are better than one hole in one."

John took special pleasure in the company of young children. When his children and grandchildren were young he delighted in taking them swimming on his back and in playing all sorts of games with them. His interest in children was not confined to his immediate family. Conyers Herring fondly recalls asking John in the course of his visit to Bell in the summer of 1955 to step in to help with a family emergency:

> My wife, Louise, and I had invited [John] 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.

My wife, Suzy, and I had a chance to confirm Bardeen's delight in young children when, in June 1958, he accompanied us and our then twenty-two-month-old daughter, Catherine, on a six-hour drive from Paris to an international meeting in Holland. John began the trip by sitting in the back seat with Catherine; she started talking to him as we left the outskirts of Paris, and, as best we could determine, scarcely stopped for breath as she discussed her life and times with John. After the first hour or two, we felt that surely John was ready for a change of scene, but he was very clear in telling us that he was having a marvelous time and continued his visit with Catherine for the entire trip. It was apparently a memorable trip for John, since ten years later, in showing some slides toward the conclusion of his sixtieth birthday celebration, he included one of his seatmate on that trip.

And children loved John. John's grandson Andrew Greytak, when asked in the fifth grade to write about "My Hero," wrote about his grandfather. "A hero is a person who does great deeds . . . at great cost to him or herself," wrote young Andrew. "The deeds may involve leading groups of people . . . in a war [or] doing something great for the world, such as inventing a very important thing. . . . I have two heroes. The first one is the inventor of the transistor and the man who wrote the theory of superconductivity. . . . He is also my grandfather. He has won two shared Nobel Prizes for the things I just mentioned. The main reason he is my hero, of course, is that he is my grandfather. The other reason is the transistor. My other hero is George Washington."

John was also a truly generous man. He never hesitated to give others credit for their contributions to science. He particularly admired Fritz London, whose views on superfluidity as a macroscopic quantum phenomenon significantly influenced John's thinking. In recognition of his intellectual debt to London, John endowed the Fritz London Award and the Fritz London Memorial Lectures at Duke University with his share of the 1972 Nobel Prize money.

John Bardeen died on 30 January 1991. He was survived by his wife, Jane, their children, Jim, Bill, and Betsy, and their grandchildren, Charles G. Bardeen, Karen G. Bardeen, William T. Bardeen, David P. Bardeen, Andrew B. Greytak, and Matthew B. Greytak. In an editorial published on 3 February 1991, the *Chicago Tribune* wrote, "Near the end of this decade, when they begin enumerating the names of the people who had the greatest impact on the 20th century, the name of John Bardeen, who died last week, has to be near, or perhaps even arguably at, the top of the list. . . . Mr. Bardeen shared two Nobel Prizes and won numerous other honors. But what greater honor can there be when each of us can look all around us and everywhere see the reminders of a man whose genius has made our lives longer, healthier and better."

In 1994, the *New York Times* introduced what became an annual compilation entitled "Lives Well Lived" with these words:

> When John Bardeen died in 1991, a scholar said, "There are very few people who had a greater impact on the whole of the 20th century," and he was right. Bardeen was a co-inventor of the transistor, heart of the electronics revolution. He was a pioneer in superconductivity physics. He was the first person to win two Nobel prizes in the same field. Reading all that in his obituary made a film director named David Frankel wonder how someone could be so important yet not be better known. Shouldn't there be some way, he asked himself, to recognize lives that have had special impact on the world?
>
> The question wouldn't go away. Ultimately, Frankel shared it with friends at The Times Magazine; this issue is our answer. . . .

There are several tantalizing "what if" questions in Bardeen's career. What if—rather than continuing with engineering at Madison and then going into industry for three years—he had been successful in his 1929 application to Trinity College, Cambridge, for one of their coveted fellowships? What if—as a consequence of incorrect experimental results—Eugene Wigner had not become discouraged about understanding nuclear physics in 1932 and therefore not spent three critical years studying condensed matter? In either case, would Bardeen's scientific interests have gone in quite a different direction? And what if Bill Shockley, who was single-handedly responsible for Bardeen's leaving Bell Labs in 1951, had supported John's continuing interest in the fundamental physics of semiconductors and his desire to work on superconductivity? Would Bardeen have been able to solve superconductivity if he had stayed at Bell Labs? (My own answer to this last is "almost certainly not," for Bell could not offer to him the encouraging and nourishing research environment and the postdocs and students who played such an important role in his finding the solution at the University of Illinois.)

John Bardeen's scientific legacy is extraordinary for its breadth and depth, but in the long term its most important part may be his persona and scientific style, which so greatly influenced his students, postdocs, and colleagues, and which can serve as a beacon to so many future generations of scientists. His nine-fold way may be summarized as follows:

- Focus first on the experimental results via reading and personal contact.
- Develop a phenomenological description that ties different experimental results together.
- Explore alternative physical pictures and mathematical descriptions without becoming wedded to any particular one.
- Thermodynamic and other macroscopic arguments have precedence over microscopic calculations.
- Focus on physical understanding, not mathematical elegance, and use the simplest possible mathematical description of system behavior.
- Keep up with new developments in theoretical techniques—for one of these may prove useful.
- Decide on a specific model as the penultimate, not the first, step toward a solution.
- Choose the right collaborators.
- *Don't give up*: Stay with the problem until it is solved.

Elected 1958

David Pines
Distinguished Professor of Physics and Co-Director
Institute for Complex Adaptive Matter
University of California, Davis
Research Professor of Physics and
Center for Advanced Study Professor Emeritus
University of Illinois at Urbana–Champaign

Acknowledgments

In writing this memoir I have drawn heavily on the April 1992 issue of *Physics Today*, a review I wrote (of *True Genius* by Lillian Hoddeson and Vicki Daitch) that appeared in the May 2003 issue of *Physics World*, and the John Bardeen archives at the University of Illinois. I wish to thank the editors and publishers of both journals for their permission to quote liberally from these issues and to record once more my thanks to the many colleagues whom I consulted in the course of preparing the *Physics Today* article. My special thanks go to Jane Bardeen and her daughter, Betsy Bardeen Greytak, for sharing with me some of the memories of John that appeared in *Physics Today* and appear here, and to Bill Bardeen for his many helpful suggestions on the present manuscript and for providing a copy of the May 1951 Bardeen memo to M. Kelly.

For the reader who wishes to learn more about John Bardeen, there are available, in addition to the Hoddeson-Daitch book about Bardeen, *True Genius*, published by the Joseph Henry Press in 2001, videotapes of the talks given at the UIUC celebration of BCS at fifty years (http://www.conferences.uiuc.edu/bcs50/video/), a memoir by Nick Holonyak written for the National Academy of Engineering, and a videotape of a seminar about Bardeen given by Holonyak ("The Transistor: From Germanium to Silicon to Integrated Circuit [1947–1960]").