THE NOBEL LAUREATE VERSUS THE GRADUATE STUDENT

John Bardeen, the leading condensed matter theorist of his day, was quite wrong when he dismissed a startling prediction by the unknown Brian Josephson.

Donald G. McDonald

In 1962, Brian Josephson, a 22-year-old research student at Cambridge University, suggested a new and surprising effect. A supercurrent, he argued, can tunnel through a thin insulating barrier.[1] University of Illinois theorist John Bardeen disagreed, and that mattered. At age 54, Bardeen was the most celebrated solid-state theorist of his time. He had shared the 1956 Nobel Prize in Physics with William Shockley and Walter Brattain for the invention of the transistor. He would share a second Nobel prize in 1972 with Leon Cooper and Robert Schrieffer for their 1957 solution (the BCS theory) of the long-standing riddle of superconductivity.

Bardeen publicly dismissed young Josephson's tunneling-supercurrent assertion in a "Note added in proof" to a 1962 article in *Physical Review Letters*:

> In a recent note, Josephson uses a somewhat similar formulation to discuss the possibility of superfluid flow across the tunneling region, in which no quasi-particles are created. However, as pointed out by the author [Bardeen, in a previous publication], pairing does not extend into the barrier, so that there can be no such superfluid flow.[2]

Bardeen's reproach led to a face-to-face debate in London that September. When I considered writing about this historic confrontation, I contacted Josephson and others who played roles in the drama. In an e-mail to me last year, Josephson offered the admonition: "Beware ye, all those bold of spirit who want to suggest new ideas."

The odds in the upcoming debate were stacked in Bardeen's favor. Superconductivity was his turf, BCS theory his grand creation. Nonetheless, Josephson insisted that Bardeen was wrong about superconductive tunneling. Josephson's insight into tunneling theory made the phase of the macroscopic wavefunction accessible to experimental control. That had profound consequences. It eventually raised metrology to extraordinary precision (1 part in 10^{19}), thus strengthening the foundations of physics.

I did not see the debate in London, but I learned of it in 1965 from Don Langenberg, who was then at the University of Pennsylvania. I have remained fascinated by the disparities between the antagonists, and by the outcome. As befits good science, the dispute was eventually resolved by experimental results, not by prestige. What would the sociologists who call for the deconstruction of science make of that?

The story of Josephson's discovery has been told by his mentor Philip Anderson (PHYSICS TODAY, November 1970, page 23), his thesis adviser Brian Pippard,[3] and by Josephson himself.[4] In those retellings, however, the role of Bardeen has been largely ignored. Bardeen had wide influence, and he was very much in the picture. He disagreed with a number of prominent theorists—among them, Anderson, Cooper, Morrel Cohen, Leo Falicov, James Phillips, Robert Parmenter, Vinay Ambegaokar, and Alexis Baratoff—regarding the theory of tunneling. Thus he was involved in private debate with these theorists, and then in public debate with young Josephson.

In the aftermath of the BCS theory came the discoveries of magnetic-flux quantization, the proximity effect, and—most prominently—the novelties of tunneling. These intertwined subjects provided the context for the debate.

Three discoveries

Bardeen's genius, Anderson once told me, was his "remarkable talent for picking the right problem to attack." Bardeen's interest in tunneling began with Ivar Giaever's experimental discovery, in 1960, of novel effects in single-particle tunneling. These exciting new results were the talk of the laboratory when Bardeen visited General Electric as a member of the laboratory's scientific review panel that summer.

Giaever's experiments—done while he was a graduate student at Rensselaer Polytechnic Institute—used thin-film tunneling structures that were "sandwiches" of aluminum, very thin aluminum oxide, and lead. He hoped to see a change in the electrical characteristics of these devices as the temperature was lowered to make the lead superconducting. BCS theory predicted an energy gap in superconducting lead, but its effect on tunneling was uncertain. Giaever's experiments showed a dramatic effect: The energy gap was displayed with beautiful resolution, much better than by other methods. No one had predicted this result. It was a major advance, a triumph of intuition.[5]

For Bardeen, it was new physics. What could he do with it? Standing on the threshold of a significant theoretical discovery, Bardeen submitted for publication a paper entitled "Tunneling from a Many-Particle Point of View," offering an explanation for Giaever's results. This paper contained an error, a mistake that was not important for Giaever's experiments. But it was decisive for Bardeen's later assessment of tunneling supercurrents. Bardeen wrote:

> If one looks at the [tunneling] problem more closely, from the viewpoint of the more gener-

DONALD MCDONALD *spent 29 years as a staff scientist at NIST in Boulder, Colorado, doing research on the Josephson effect and superconducting electronics. Now he is a founding partner of Boulder Metric Inc. (McDonald@indra.com).*

THOMAS REIS

> al Gor'kov equations, which allow for a variation of the energy gap parameter with position, one sees that D will drop to zero very rapidly in the barrier. In effect, electrons in this region are not paired and the wavefunction is essentially the same as in the normal state.[6]

Later, Ambegaokar and Baratoff would use Gor'kov's formalism and draw the opposite conclusion. Bardeen had not addressed tunneling with the comprehensiveness and vigor he had applied to the development of BCS theory. (See the article by Schrieffer in PHYSICS TODAY, April 1992, page 46).

The second discovery came in 1961, when Bascom Deaver and William Fairbank at Stanford University and, independently, Robert Doll and Martin Näbauer in Germany demonstrated the quantization of magnetic flux. These experiments confirmed Fritz London's 1948 theoretical prediction that a current in a superconductor could be described by the macroscopic wavefunction $\psi = |\psi| e^{i\varphi}$. Only the wavefunction's phase φ changes through the body of the superconductor. Consequently the flux threading a superconducting ring cannot have arbitrary values. It can only be an integral multiple of the flux quantum $\Phi_0 = h/2e$. The experimental confirmation of London's prediction provided the first direct demonstration of a macroscopic quantum effect.[7,8]

Just a month after this discovery, Bardeen wrote a paper entitled "Quantization of Flux in a Superconducting Cylinder,"[9] in which he used a macroscopic phase variable. But, apparently, he gave no further thought to the role of the phase. Meanwhile, Josephson was studying various different formulations of the theory.[4] His mantra became: The phase is "real enough to produce . . . flux quantization." How can I make it more explicit in experiments?

The third discovery was published in 1960 by Hans Meissner in a paper entitled "Superconductivity of Contacts with Interposed Barriers."[10] Meissner's experiments used superconducting wires of tin, coated with a thin layer of a normal metal such as copper. Bringing two such wires together, he found that the contact was superconducting despite the normal-metal overlay. This is called the "proximity effect"—the induction of superconductivity into normal metal. Meissner concluded that "the density of superconducting electrons in the normally conducting layer decreases relatively slowly with distance from the superconductor,"[10] with a range of about 300 nm into the normal metal. This empirical conclusion was supported by Parmenter's theoretical work within the framework of BCS theory.

Subsequent experiments by Paul Smith and coworkers Sidney Shapiro, John Miles, and James Nicol confirmed Meissner's results and addressed criticisms of his experiments. Smith and coworkers took an additional step. They asked for Cooper's conclusion of the proximity effect. At the IBM Conference on Fundamental Research in Superconductivity in June 1961, Cooper gave theoretical support to Meissner and Parmenter's conclusions.[11] Cooper's theoretical view contradicted Bardeen's statement of six months earlier.[6]

Was Bardeen aware of this difference of opinion? Cooper had been Bardeen's postdoc when they developed the BCS theory. Cooper told me he remembered their being together at the IBM conference. Either then or later, he tried to change Bardeen's mind, but to no avail. That was a critical error for Bardeen, because the physics of the proximity effect is similar to superconductive tunneling.

Cohen, Falicov, and Phillips

Following the IBM conference at which Cooper spoke, Bardeen had 15 months to rethink his ideas before his eventual confrontation with Josephson. In the meantime, at the University of Chicago, Cohen, Falicov, and Phillips would provide some fireworks. As Cohen tells the story, Phillips and Falicov wrote a qualitative paper on tunneling and brought it to his office for discussion. Cohen was not enthusiastic. He suggested instead that they begin with a transfer Hamiltonian, which he wrote:

$$H_T = \sum_{kq\sigma}[T_{kq} a^{\dagger}_{k\sigma} a_{q\sigma} + T_{qk} a^{\dagger}_{q\sigma} a_{k\sigma}] \; . \tag{1}$$

The important point here is not the details of this formulation but rather that one should use a precise Hamiltonian for tunneling. Bardeen had not taken that approach. Characteristically, he had focused on physical understanding, not mathematical elegance.

Phillips was initially not interested in this problem. So Cohen and Falicov met that evening and derived Giaever's empirical density-of-states result for tunneling. The next day, Cohen suggested that the group evaluate the tunneling between two superconductors, but his colleagues had little enthusiasm for that undertaking. It appeared to be substantially more complicated, and they thought they knew the answer anyway. Other ideas beckoned. The surprises of the two-superconductor problem were left to Josephson.

In February 1962, Cohen, Falicov, and Phillips wrote up the normal-superconductor case and mailed it to *Physical Review Letters*, with a copy to Bardeen. Phillips has told me that Bardeen urged Samuel Goudsmit, the journal's editor, not to publish the paper. Goudsmit suggested to Cohen that he and his coauthors discuss Bardeen's objections and try to reach a compromise. That led to the exchange of four letters between Bardeen and Cohen's group. Among other things, Bardeen wrote, "You take the Hamiltonian as more or less self-evident, and treat as the real problem that of calculating the transition rate. I feel that calculating the transition rate is trivial once the Hamiltonian is given."

As a compromise with their critic, the three authors modified their paper and included some of his comments in a footnote, which says in part. "Questions left open are whether the quasi-particle picture is valid, and if so the nature of the quasi-particle wavefunctions." With that, they went to press.[12]

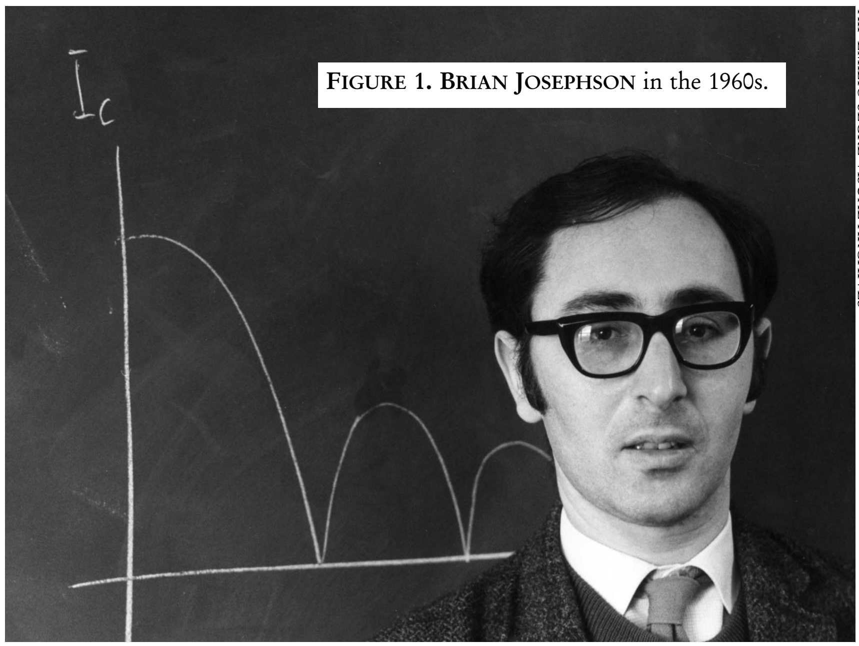

FIGURE 1. BRIAN JOSEPHSON in the 1960s.

AIP EMILIO SEGRÈ VISUAL ARCHIVES

Josephson's discovery

On the other side of the Atlantic, Pippard was a leading light of experimental superconductivity at Cambridge. He had followed the published work on the proximity effect—both Meissner's experimental results and Parmenter's theoretical interpretation. Pippard thought

> that it was perfectly possible for this superconductor to infect that normal metal with superconducting pairs so that supercurrent could pass from one side to the other. But our thought was strictly limited to the idea of a normal metal becoming a sort of dilute superconductor . . . passing current proportional to the gradient of the wavefunction.[3]

Josephson would take the novel view that the normal metal was a tunneling barrier. He concluded that, in a two-superconductor system, the phase difference $\Delta\varphi$ across the barrier obeys the relation:

$$\partial\,(\Delta\varphi)/\partial t = 2\,eV/\hbar, \tag{2}$$

where V is the potential difference between the two superconductors. Thus he "was led to expect a periodically varying current at a frequency $2eV/h$,"[13] which is now known as the Josephson frequency. But the magnitude of that current was unknown.

At about that time, Anderson arrived in Cambridge for a sabbatical year, 1961–62. Recalling the series of lectures he gave, Anderson has written (PHYSICS TODAY, November 1970, page 23), "Josephson had taken my course on solid-state and many-body theory. This was a disconcerting experience for a lecturer, I can assure you, because everything had to be right or he would come up and explain it to me after class."

Josephson, in his Nobel prize lecture, recalls, "The problem of how to calculate the barrier current was resolved when one day Anderson showed me a preprint he had just received from Chicago . . . in which Cohen, Falicov, and Phillips calculated the current flowing in a superconductor-barrier-normal metal system, confirming Giaever's formula."[13]

Josephson immediately set to work extending the calculation to a situation in which both sides of the barrier were superconducting. The expression obtained for the current contained a term of the form

$$I = I_1(V)\,\sin(\Delta\varphi). \tag{3}$$

That was completely unexpected. The coefficient $I_1(V)$ was an even function of V, and one wouldn't expect the current to vanish when V was set equal to zero. So the obvious interpretation was that this was a supercurrent. But Pippard had said that tunneling by pairs would not be observable, because the tunneling of a single electron had low probability, say 10^{-10}, and the tunneling of a pair would be the *square* of that small tiny probability. "It was some days before I was able to convince myself that I had not made an error in the calculation," Josephson recalls.[13] He had concluded that tunneling of coherent pairs was proportional to the matrix element, not to its square.

Josephson continues:

> I next considered the effect of superimposing an oscillatory voltage at frequency ν on a steady voltage V. By assuming the effect of the oscillatory voltage to be to modulate the frequency of the AC supercurrent, I concluded that constant-voltage steps would appear at voltages V for which the frequency of the unmodulated AC supercurrent was an integral multiple of ν, that is, when $V = nh\nu/2e$ for some integer n. The embarrassing feature of the theory at this point was that the effects predicted were too large! [13]

Anderson recalls:

> Probably because of the [lecture] course and some of the things I said, [Josephson] showed me his [tunneling] calculations within a day or two after first making them. . . . By the time I saw the calculations he had already worked out the rather sophisticated formalism he later published, in which he kept track of particle charges. By this time I knew Josephson well enough that I would have accepted anything else he said on faith. However, he himself seemed dubious. . . . We were all—Josephson, Pippard, and myself—very much puzzled by the meaning of the fact that the current depends on the phase.

Josephson's theory was received at *Physics Letters* on 8 June 1962.[1] On 25 July, Bardeen submitted to *Physical Review Letters* his challenge to Josephson.[2] An anecdote from Felix Bloch recalled the perplexity with which Josephson's theory was received: "[C. N.] Yang told me that he could not understand it, and asked whether I could. In all honesty I had to confess that I could not either, but we made a deal that whoever of us first understood the effect would explain it to the other."[8]

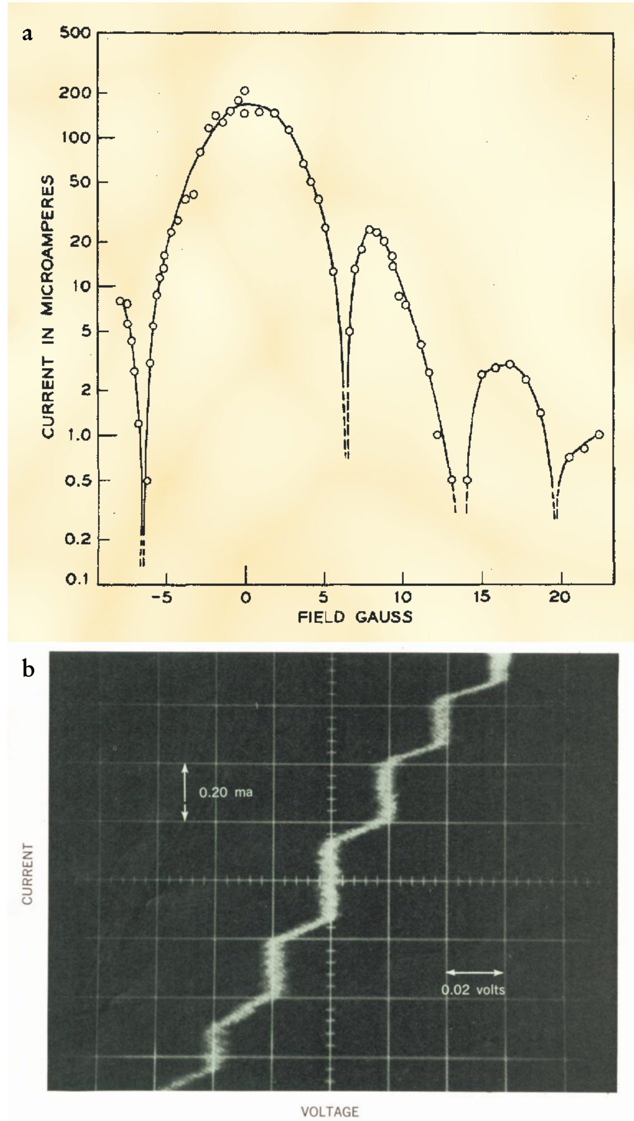

FIGURE 2. EXPERIMENTAL PROOFS of the Josephson effects. (**a**) Magnetic-field dependence of the supercurrent in the tunnel junction (from J. M. Rowell, ref. 16). (**b**) Sidney Shapiro's demonstration of the I–V characteristic of a Josephson junction irradiated at 9.75 GHz (from PHYSICS TODAY, October 1969, p. 45).

Face-to-face debate

The historic debate took place at the Eighth International Conference on Low Temperature Physics, which was held at Queen Mary College, London, in September 1962. Just before the debate, Giaever visited Cambridge, where he met Josephson for the first time. Giaever described that meeting to me:

> [Josephson] had already published his theory in *Physics Letters*, and one of my assignments was to find out if it was worth pursuing. . . . I did not understand the theory at that time, but the informal meeting in Cambridge, where Pippard was present, convinced me that Josephson was special. . . . Next, I remember that I introduced Josephson to Bardeen in London, when people were milling around in a big hall. Josephson tried to explain his theory to Bardeen. But Bardeen shook his head slightly and said "I don't think so," because he had carefully thought about the problem. I stood there during the short conversation. Then Bardeen left, and Josephson was quite upset. He could not understand that Bardeen was supposed to be a famous scientist.

Bardeen opened the conference at Queen Mary College with the Fritz London Memorial Lecture. Some light is shed on the intellectual ambiance of the conference by an observation by Derek Martin: "They wanted macroscopic quantum phenomena to be a major theme of the meeting," he told me.

Paul Martin, chairman of a session that included tunneling, thought it would be a good idea to have both Josephson and Bardeen speak at the end of that session. The two antagonists agreed, and that became the famous debate. The conference proceedings, however, make no mention of the debate, nor do they acknowledge that Josephson participated in the conference.

The conference room was crowded late that afternoon in anticipation of the debate. Among the 40 or so physicists present I have identified only a few: Cohen, Deaver, Pierre-Gilles de Gennes, William Little, Robert Powell, Geoffrey Sewell, and Harold Weinstock. Bardeen was seated near the back of the room.

Josephson has a few recollections of the conference. His task was to describe his theory of superconductive tunneling. He took the morning train to London and returned to Cambridge that evening, skipping the rest of the conference. Neither Pippard nor Anderson was present; Anderson had returned to the US.

Josephson had been fascinated by Anderson's newly introduced concept of "broken symmetry" in superconductors. He therefore spoke at the session about Anderson's pseudospin formulation of BCS theory (in which the angular orientation of the spins is a variable) and its relation to the phase of the F functions in Gor'kov's formulation of BCS theory. By modifying the Cohen-Falicov-Phillips Hamiltonian to include pair operators, and combining that with the Gor'kov formalism, Josephson predicted that pair tunneling would be a large effect.

After Josephson's talk, Bardeen rose to describe his theory of single-particle tunneling, including his previously published comment that pairing does not extend into the barrier. As Bardeen spoke, Josephson interrupted him. The exchanges went back and forth several times, with Josephson answering each criticism of his theory. The scene was quite civil, because both men were soft spoken, not given to the bluster of verbal combat, even though, as history would show, a Nobel Prize hung in the balance.

Cohen's summary of the event was that the debaters seemed to speak different languages. Josephson believes it was just a contest of ideas. But it was more than that. It was youth versus maturity, daring spirit versus depth of experience, and mathematics versus intuition. Bardeen had created the BCS theory, but Josephson believed that he understood it better than its creator did. The disagreement was not resolved.

Following the debate there was little discussion with the audience, one exception being an unrecorded comment by de Gennes in support of Josephson. Bill Little left the hall thinking that Stanford, his home institution, should hire Josephson.

Two years ago, I asked Josephson about the essential difference between him and Bardeen in those days. "My calculations," he answered, "presumed that high correlations would remain [for electrons] in the [tunneling] barrier. Bardeen assumed they would not."

AIP EMILIO SEGRÈ VISUAL ARCHIVES

FIGURE 3. JOHN BARDEEN (1908–89) in the mid-1930s.

Ambegaokar and Baratoff

Ambegaokar recalls,

> Bardeen was my hero. Not only because I had studied the BCS theory but also because, as a graduate student, I had been asked to give a few seminars on the background to the subject, and for that purpose had read a long review article written by him just before the new theory appeared. . . . His own reasoning was very hard to follow, and some of it, in hindsight, was clearly wrong. But it was wrong in a deep way: His wrong arguments had the seeds of how matters did, in fact, work out when the pieces were put together correctly. So, when I got back to Cornell, I said very grandly to my first graduate student, Alexis Baratoff, "Bardeen is having a disagreement with some Englishman (Welshman, in fact). Bardeen is always right. Let's find out why." . . . It was difficult for us to decide one way or the other about Bardeen's objection, because the key question of the distance over which two superconductors can affect each other was buried in the formulas. In the midst of this confusion, it occurred to me that it might be useful to translate Josephson's steps into another (equivalent) mathematical language—developed by L. P. Gor'kov and other Russian physicists—which I remembered as allowing for an easier visualization of spatial dependences. No sooner was this done than it became clear to us that Josephson was right![14]

Bardeen received a copy of these calculations and, in a February 1963 letter to Ambegaokar, he repeated what he had argued from the beginning:

> I feel that the use of your Hamiltonian . . . still contains the assumption implicit in Josephson's calculation that pairing extends across the oxide layer. In my view, virtual pair excitations in the superconductor ground state do not extend across the layer. The tail of the wavefunction is virtually identical with that of the normal ground state. . . .

This letter remains inexplicable to Ambegaokar, and suggests only that Bardeen had not thoroughly studied Gor'kov's work.

Bardeen was wrong. In Gor'kov's formulation of superconductivity, $F(\mathbf{x},\mathbf{x}')$ is the amplitude for two electrons, at $\mathbf{x}$ and $\mathbf{x}'$, to belong to a Cooper pair. The gap function $\Delta(\mathbf{x})$ is given by $V(\mathbf{x})\, F(\mathbf{x},\mathbf{x})$, where $V(\mathbf{x})$ is a local two-body interaction at the point $\mathbf{x}$. In the insulating barrier $V(\mathbf{x})$ is zero and thus $\Delta(\mathbf{x})$ is also zero.

The crucial point, however, is that vanishing Δ does not imply vanishing F. On the contrary, $F(\mathbf{x},\mathbf{x}')$ can have large amplitude for electrons separated by distances $|\mathbf{x}-\mathbf{x}'|$ up to the coherence length. Hence, for barriers thin compared with that length, two electrons on opposite sides of the barrier can still be correlated. Consequently, the pair current is substantial. A full evaluation[15] shows that, at zero temperature, the pair current is equal to the single-particle current at a voltage $\pi\Delta / 2e$.

As Josephson explained it to me, "Bardeen's basic error was to ignore the non-locality inherent in the Gor'kov theory, and to assume a local connection between the potential and the pairing." There remained, however, a point of contention. The Hamiltonians used by Josephson and by Ambegaokar and Baratoff predicted the Josephson effect. Nevertheless, if that physics is forbidden, as Bardeen thought, then the Hamiltonians must be wrong.

Confirmation and application

Anderson returned to Bell Labs and began a collaboration with John Rowell to observe the tunneling supercurrent. They succeeded, confirming that it was indeed the Josephson effect by testing, among other things, the magnetic-field dependence of the supercurrent (see figure 2a.) In January 1963, Anderson and Rowell submitted their paper "Probable Observation of the Josephson Superconducting Tunneling Effect" for publication.[16] After that experimental confirmation, Bardeen graciously withdrew his objections to Josephson's theory.

Five months later, Shapiro reported his observations of a second effect predicted by Josephson: the constant-voltage steps produced by microwaves, illustrated in figure 2b. Thus Josephson's main predictions, the DC and AC supercurrents, were experimentally confirmed within a year after his publication.

Josephson's Nobel Prize in Physics was awarded in 1973, eleven years after his discovery. He shared the prize with Giaever and Leo Esaki. Esaki's prize was for observing tunneling in p–n junctions. Josephson's legacy to physics—the ability to control macroscopic quantum phase—greatly enhanced the sensitivity of experiments using superconductivity. (See my article in PHYSICS TODAY, February 1981, page 36).

Twenty-five years after its discovery, the Josephson effect became the basis for an extraordinary advance in the precision of physics.[17] In such high-precision measurements, the basic idea is that a Josephson junction with bias voltage V has an oscillating current at a frequency ν_J proportional to that voltage:

$$\nu_J = K_J V, \tag{4}$$

where the proportionality constant $K_J = 2e/h$ comes from Josephson's theory. If one applies radiation to the junction, one gets the current steps at constant voltage illustrated in figure 2b. Langenberg, William Parker, and Barry Taylor, in celebrated experiments, used these steps to provide a more accurate value of e/h in the 1960s.

Later work emphasized the reproducibility of the voltage–frequency relation of equation 4, which could be tested independently of how well the SI units are known. The trick was to apply the same radiation to two junctions and independently bias each junction to the same step, and then look for minute differences in their voltages in a null experiment. The critical questions became: Do junctions fabricated with different materials, and with different dimensions, impedances, and bias conditions have the same K_J? And there was also, to my mind, the more profound question: How precisely can mathematics describe nature?[17]

The most refined work of this type was done by Ashok Jain, James Lukens, and Jaw-Shen Tsai at Stony Brook.[18] Assuming that general-relativistic corrections (gravitational red shift and gravito-electrochemical potential) are known, the final result of Jain and coworkers was impressive. They demonstrated that K_J, roughly 312 μV, was in fact the same for their two Josephson junctions to a precision of 1 part in 10^{19}. This stringent test of equation 4 goes well beyond the accuracy to which we know the SI units

The imperfect stability of the microwave frequency was not an issue in such a null experiment because both junctions, irradiated by the same source, change voltage together as the source frequency slowly drifts. This precision for the reproducibility of K_J in the two devices is equivalent to knowing the distance from New York to Los Angeles with an uncertainty of a millionth of a wavelength of visible light. I am not aware of any physical equality that we know with higher precision, except for the equality of the magnitudes of the electron and proton charges.

Anderson's response to these results was definite and immediate: "it shows that gauge invariance is exact," he told me. And gauge invariance is a fundamental assumption of quantum field theory. Cohen suggests that such Josephson-junction results may be the best test we have of the accuracy of quantum mechanics. In retrospect, then, the Bardeen–Josephson debate has had fundamental implications that no one foresaw. When we find agreement between theory and experiment at a part in 10^{19}, Nature seems to be revealing a deep truth.

I express my appreciation to those who shared their knowledge of this subject with me: John Adkins, Vinay Ambegaokar, Philip Anderson, John Clarke, P. E. Clegg, Morrel Cohen, Leon Cooper, Pierre-Gilles de Gennes, Charles Duke, Ivar Giaever, Walter Harrison, Brian Josephson, Leo Kadanoff, R. L. Kautz, Donald Langenberg, William Little, James Lukens, Derek Martin, Paul Martin, Berndt Mueller, Robert Parmenter, James Phillips, David Pines, Brian Pippard, Robert L. Powell, Leo Radzihovsky, Robert Schrieffer, Geoffrey Sewell, Sidney Shapiro, John Waldram, Harold Weinstock, and John Ziman. I am grateful to Lillian Hoddeson, Bardeen's biographer, for encouragement and copies of letters from Bardeen's archives. Special thanks to the staff of the Department of Commerce Library in Boulder, Colorado, for many reprints.

References

1. B. D. Josephson, *Phys. Lett.* **1**, 251 (1962).
2. J. Bardeen, *Phys. Rev. Lett.* **9**, 147 (1962).
3. A. B. Pippard, in *Proc. NATO Advanced Study Institute on Small-Scale Applications of Superconductivity, Gardone, 1976,* B. B. Schwartz, S. Foner, eds., Plenum, New York (1997).
4. B. D. Josephson, in *Physicists Look Back: Studies in the History of Physics*, J. Roche, ed., Adam Hilger, Bristol, England (1990).
5. I. Giaever [Nobel lecture], *Rev. Mod. Phys.*, **46**, 245 (1974).
6. J. Bardeen, *Phys. Rev. Lett.* **6**, 57 (1961).
7. See B. Deaver in *Near Zero: New Frontiers of Physics*, J. D. Fairbank et al., eds., Freeman, New York (1988).
8. F. Bloch, in *Near Zero: New Frontiers of Physics*, J. D. Fairbank et al., eds., Freeman, New York (1988).
9. J. Bardeen, *Phys. Rev. Lett.* **7**, 162 (1961).
10. H. Meissner, *Phys. Rev.* **117**, 672 (1962). For a modern review see G. Deutscher, P.-G. de Gennes, in *Superconductivity*, R. D. Parks, ed., Marcel Dekker, New York (1969).
11. L. N. Cooper, *IBM J. Res. Dev.* **6**, 75 (1962).
12. M. H. Cohen, L. M. Falicov, J. C. Phillips, *Phys. Rev. Lett.* **8**, 316 (1962).
13. B. D. Josephson, *Rev. Modern Phys.* **46**, 251 (1974).
14. V. Ambegaokar, *Cornell Arts and Sciences Newsletter* **18** (1) (1996), also at http://www.arts.cornell.edu/newsletr/fall96/ambegao.htm.
15. V. Ambegaokar, A. Baratoff, *Phys. Rev. Lett.* **10**, 486 (1963); erratum **11**, 104 (1963).
16. P. W. Anderson, J. M. Rowell, *Phys. Rev. Lett.* **10**, 230 (1963). J. M. Rowell, *Phys. Rev. Lett.* **11**, 200 (1963).
17. See D. G. McDonald, *Science* **247**, 177 (1990), for an extensive discussion.
18. A. K. Jain, J. E. Lukens, J.-S. Tsai, *Phys. Rev. Lett.* **58**, 1165 (1987). ■