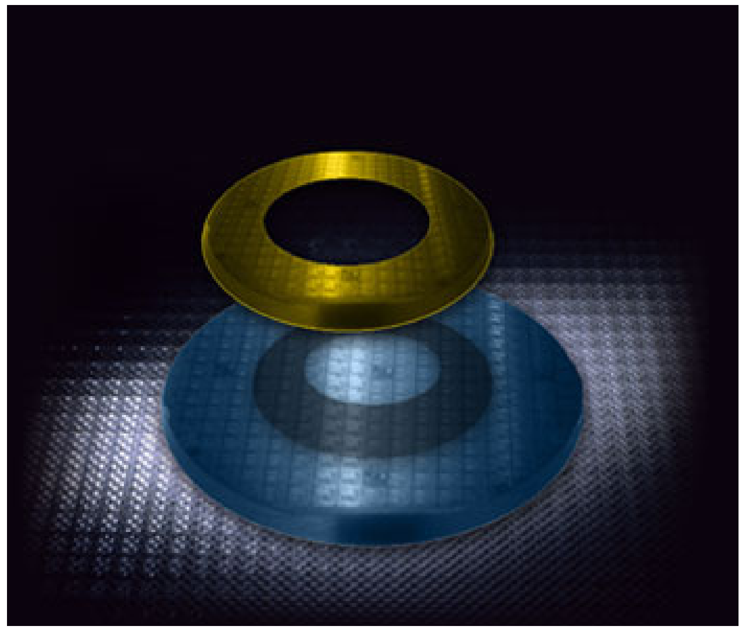
Moments of Discovery

The breakthroughs as they happened

Superconductivity

Superconductivity: So simple, yet so hard to explain!

For half a century the world's most brilliant physics theorists tried scribbling equations, only to crumple the paper and hurl it at a wastebasket. Bend a metal wire into a circle, make it as cold as you possibly can, and set an electric current moving around it. The current can persist. Put the circle of wire above a magnet, and it will float there until the end of the world.



In the decades after this strange discovery, physicists figured out the laws of relativity and quantum mechanics. They worked out equations to calculate all the colors and chemistry of the natural world, they cracked open the atomic nucleus, they uncovered the forces that light the stars... and still nobody had explained that little floating wire.

This exhibit tells how three extraordinary minds worked together to finally solve the puzzle. You will see that getting to a new theory may take not just one "Moment of Discovery" but a string of dozens of such moments among many people. For a personal account, listen to Bob Schrieffer, the youngest of the team, tell what happened in his own words. To get the full background, you can read or listen to how a noted physicist saw the story from an outside perspective. You can also read a detailed account by a historian of physics, and explore other supplementary materials.

Physics students and scientists can start with an Introduction, the story seen from outside

Everyone else may want to skip to Bob Schrieffer's story in his own words

Supplementary materials:

- Explaining superconductivity: A dance analogy (Schrieffer)
- John Bardeen talks about his early years
- A science historian's account (by Lillian Hoddeson)
- Further Reading and Links
- Exhibit Credits
- A note for teachers

Introduction (Slichter) >

Introduction to the History of Superconductivity

(for physics students and scientists)

by Charles Slichter

Download complete audio file (9.54MB) (Does not correspond exactly to the edited text below); or listen and read along using the embedded player below.

Charlie Slichter

Before you hear Bob Schrieffer tell about some of the highlights of the work he did with John Bardeen and Leon Cooper - work that led to an explanation of superconductivity — I want to tell you a bit about superconductivity and why their achievement was one of the major scientific events of the 20th century.

First of all: what is superconductivity? It's an absolutely remarkable phenomenon discovered in 1911 by a student working with the famous Dutch scientist, Kamerlingh-Onnes. Kamerlingh-Onnes pioneered work at very low temperatures — temperatures just a few degrees above the absolute zero of temperature. He succeeded in reaching temperatures much colder than anyone before him, and thus opened a new frontier for science — a field of science previously unexplored, the field of low temperature physics.

He and his students set to work to study what happened to various properties of materials when they were that cold. One of his students was studying the electrical resistance of wires. He found that as he cooled mercury wire the electrical resistance of the wire took a precipitous drop when he got to about 3.6 degrees above absolute zero. The drop was enormous - the resistance became at least twenty thousand times smaller. The drop took place over a temperature interval too small for them to measure. As far as they could tell, the electrical resistance completely vanished.

To test for the complete vanishing of the electrical resistance, Kamerlingh-Onnes devised an ingenious experiment. He took a closed circle of mercury wire and caused a current to flow around the circle. With his experimental arrangement one would expect ordinarily that resistance would cause the current to die out quickly, much as friction and air resistance cause a bicycle coasting on a level road to come to a stop. He found that for a loop of mercury wire the current, once started, would persist for as long as the wire was kept cold. The persistence of the electrical current in the circuit is a kind of perpetual motion — it's a totally startling phenomenon for physicists.

H. Kamerlingh-Onnes

Physicists understand quite well why an ordinary metal resisted the flow of electric current — why, so to speak, the electrons experienced friction in flowing through a conductor — yet something must go wrong with those ideas when the metal becomes superconducting, in order to allow the persistent seemingly-frictionless flow of current in the superconductor.

The general picture scientists had was that the resistance arises because moving electrons — which are what produce the electric current — from time to time bump into the atoms of the metal and are deflected. Thus, though they may be given an initial motion through the crystal, that motion does not persist. It's like trying to throw a baseball through a grove of trees. It bounces off the trees and comes to rest. The vanishing of electrical resistance seems analogous to requiring that the grove of trees vanish — and explaining superconductivity is like explaining why the grove appears to vanish. Remember, it's not fair for physicists to take magic as a reason! The fact is that below a certain temperature many metals enter into a *new* state of matter: the superconducting state. Just suppose you knew water only as a liquid - how curious you'd be when you discovered its transition into a new state of matter: ice.

You can well imagine that the explanation of this phenomenon of persistent current challenged the very best theoretical minds. Yet superconductivity remained an enigma for decades. Many of the world's greatest scientists tried to solve the mystery of the perpetual motion, but without success - at least five Nobel Prize winners. John Bardeen tried unsuccessfully shortly after finishing graduate school. [*Bardeen talks about this here*.] Even while working on semiconductors - and sharing in the discovery of the transistor - the challenge of superconductivity kept nagging at him in the back of his mind.

There was really no chance for any of the theorists to solve this problem at the time of discovery because before one could explain it, one had to have the quantum theory in the form that Schrödinger and Heisenberg developed, which didn't take place until the 1920's. For a long time the phenomenon of superconductivity was characterized by the statement that the electrical resistance vanished completely.


Walther Meissner

However, in 1933 Meissner and Ochsenfeld discovered another property of superconductors, which is in fact believed by many to be an even more basic characterization. This phenomenon, which is popularly called the Meissner effect, has to do with the magnetism of a superconductor. You're no doubt familiar with the fact that iron has remarkable magnetic properties. Iron tends to draw to it the lines of magnetic force of a magnet. That's why iron is often used to make electromagnets. It helps to guide the magnetic lines of force around in space where you wish to have them. The superconductor is just the opposite. It's what is called a perfect diamagnet. A superconductor excludes the lines of magnetic force. If you bring a small bar magnet up to a superconductor, the superconductor bends the lines of force away from it and doesn't allow them to penetrate.

Around 1935 another important theoretical advance in understanding superconductivity was made by Fritz London and his brother Heinz. In an ordinary metal we describe the phenomenon of electrical resistance by the famous Ohm's law. What the London brothers did was to show that there was another mathematical relationship which should be used in place of Ohm's law to describe superconductors. From this other relationship which they developed, they were able to explain both the Meissner-Ochsenfeld experiment as well as the persistent current of Kamerlingh-Onnes as two manifestations of the same thing.

I suppose in some ways the single most important experiment which directly played a role in guiding the way to an explanation of superconductivity was the experiment on the "isotope effect." This occurred in 1950 and, as so often happens in science, papers from two laboratories simultaneously revealed the same results. One paper told of the work of Reynolds, Serin, Wright and Nesbitt at Rutgers [University]. The other was by Maxwell working at the [National] Bureau of Standards.

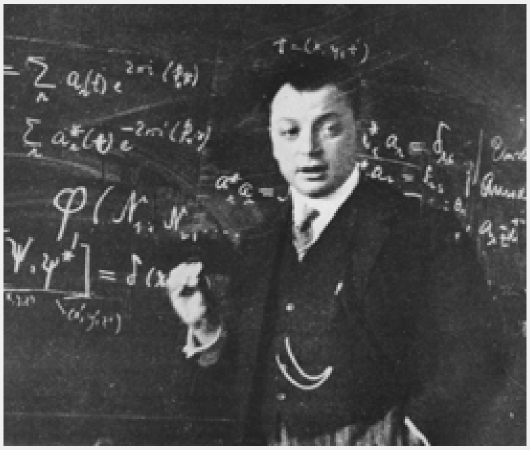
You know that the same chemical element may come with different nuclear masses - so-called isotopes. What these workers did was to prepare samples of material - in this case mercury - with their isotopic masses varying by a few percent between different samples. They found that the critical temperature for the superconducting transition was lower in the sample which had the higher isotopic mass. In fact the critical temperature was inversely proportional to the square root of the average isotopic mass of the substance. Well, this tells you that the mass of the nuclei was playing some role in the phenomenon of superconductivity.

Physicists are quite familiar with expressions which involve the square root of a mass. Suppose you think of a spring with a mass attached to it. If you give the mass a little push it will vibrate and the frequency of that vibration goes inversely with the square root of the mass. You may wonder what a mass and a spring has to do with superconductivity. Well, the connection is simple. If you want to have a simple picture of a solid you might think of it as a regular array of atoms - for example, think of a jungle gym and think of the intersections of the points on the jungle gym as representing the positions of the atom. In a jungle gym the joining points of course are rigidly spaced apart by the rods of the jungle gym, but in a solid it's probably a better approximation to think that the atoms which are joined together are not rigidly attached, and in fact the distance between them can be varied a little bit if you squeeze on the solid or pull on it. It's probably a good approximation to think of the solid as consisting of a bunch of masses - the masses of the atom - joined together by a set of springs. If you think of the solid in this manner then you realize that if you gave the solid a little poke you would set all those masses jiggling and all the springs vibrating. In any ordinary solid, this kind of jiggling phenomenon is always present unless you are at absolute zero. It goes by the name of the lattice vibrations.

So what the isotopic experiments we've just talked about showed was that although the electrical conductivity was known to arise because of the motion of the electrons, there's some role of these lattice vibrations. They enable the electrons suddenly to move through the lattice, evidently without hindrance, when the sample is cooled to the critical superconducting temperature.

The next big experimental discovery was done by two groups: Goodman, who was making thermal conductivity experiments, and Brown, Zemansky and Boorse, who were making specific heat measurements. They discovered what is called the energy gap. I must confess that I find explaining the energy gap the most difficult part of the explanation of superconductivity. You have to be patient with me while I back up a bit to get us all together in our concepts.

You're all familiar with the way in which one builds up the periodic table by adding electrons to an atom. As one does this, one thinks of orbits of an atom which one fills with electrons. The unique chemical properties are associated with the extent to which an orbit is full or empty. Here the electrons can go only into certain orbits and only one electron can go into any given orbit. This exclusive property of electrons was first noted by Wolfgang Pauli, after whom the phenomenon is named: the Pauli Exclusion Principle. It's of great importance throughout all of physics and it plays an important role in understanding superconductors.



Wolfgang Pauli

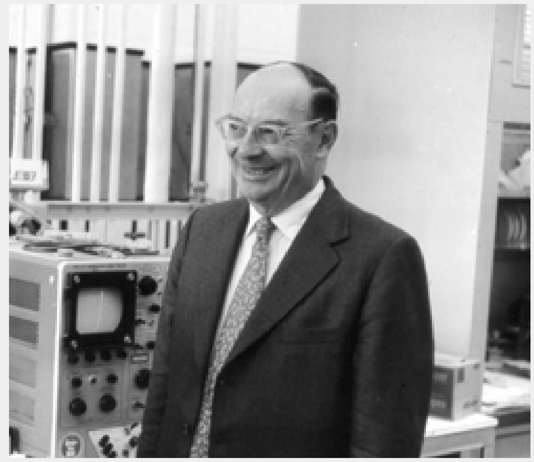
Now, when we talk about a metal and we think of putting the electrons in it, we can get a pretty good picture if we think of those electrons as bouncing around inside the metal - the metal being, so to speak, like a box. We can think of the electrons very much as we think of the atoms of a gas which are bouncing around inside whatever container the gas is in. In an ordinary classical view of the world - the way people thought before the quantum theory was discovered - we would say that if we got the metal very cold, the electrons would be moving around rather slowly, and in fact they'd come to rest when one got to absolute zero. That would be a proper description of things were it not for the Pauli exclusion principal. The fact is that when electrons are in a metal, they can possess certain orbits in much the same way as electrons in an atom can possess only certain orbits. One way of thinking about these orbits is that some electrons move slowly, some move somewhat faster and some move even faster. The orbits which are possible can be specified by the speed and the direction in which the electrons are allowed to move. If we then start putting electrons into a metal to achieve the situation at absolute zero, the first electron we would put in would go into the lowest energy orbit, the next would go into a somewhat higher energy orbit, and so on until we had put in the proper number of electrons. Those last ones we put in have a good deal more energy than the first ones. The energy which they have relative to the first one is commonly called the "Fermi energy," after Enrico Fermi who first calculated its value.

Now, suppose we think about what happens in this metal if we were to heat it a bit above the absolute zero. When you heat something, you give a little more energy to all its parts. The electrons are no exception. Think of one of those electrons which initially has a rather low amount of energy. Of course, if you try to give it more energy it has a problem, because the orbits of somewhat higher energy are already occupied by other electrons, and the Pauli Principle does not let this electron switch over into an orbit which is already occupied. This same argument applies to most of the electrons. But now let's talk about those electrons which have the Fermi energy - that is to say, they were the last ones to get added into the energy states and the ones therefore which are moving around most rapidly. Those electrons have nearby energy orbits which are *not* occupied by electrons. So if you heat the metal, they are free to speed up a little bit and thus go into an orbit which is just a bit above the orbit in which they used to be. There is in fact a continuous set of energies available to those electrons at the Fermi energy, so they can gradually add energy as the metal is warmed.

This brings us to the point of the energy gap. Suppose instead of having the situation I've just described in which one could give those electrons of the Fermi energy just a little bit more energy, suppose they had to pay, so to speak, an entrance fee to gain energy. Suppose there weren't any states which were available close by. Suppose you had to give them a really large chunk of energy before their motion could change. Then one has described what is called a gap in the spectrum of the possible energy states. This is the situation which exists in superconductors. This energy gap was discovered in the experiments of Goodman on thermal conductivity and Brown, Zemansky and Boorse on specific heat. The experimental evidence was clear.

It was in the early 1950's when John Bardeen decided to work again on the problem of superconductivity. By then some more clues had been found, but I won't explain them since it would take too long. But, although scientists had accumulated a number of facts about the new state of matter, no one had been able to put it all together and provide a theoretical explanation for it.

Now, let's go back a bit to the discovery of the isotope effect in 1950. When John Bardeen heard about it, he was stimulated to work again on the problem of superconductivity. He had in fact worked on it at various earlier times and always kept it in the back of his mind. Meanwhile, the British physicist Fröhlich was very interested in superconductivity. He'd not known about the isotope effect, but he guessed that lattice vibrations might play a similar role. At the same time, Bardeen and Fröhlich independently put forward theories of superconductivity which later on turned out to be incorrect. However, both of them said they thought an essential portion of the problem had to do with what happened to the electrons whose energy was equal to the Fermi energy. Bardeen is such a great physicist that even when he's wrong, to some extent, he is still right for the most part. That's what we mean when we say a physicist has great physical intuition.



John Bardeen

After these theories proved to be unsuccessful, Bardeen went back to work on further aspects of superconductivity to try to take the problem apart more thoroughly. At this point Bardeen had come to the University of Illinois from Bell Laboratories, where he, William Shockley and Walter Brattain had invented the transistor. By the way, these three men won the Nobel Prize for that invention in 1956, and in Bob Schrieffer's tape you'll notice the reference to Bardeen going off to Stockholm to receive the Prize. Bardeen was jointly a professor of physics and a professor of electrical engineering at the University of Illinois at Urbana. There, with David Pines, he studied the details of the interactions of the electrons with the lattice vibrations and with one another.

At this point Bardeen proved a tremendously important theorem. He said: suppose we consider that a superconductor is nothing but a normal metal in which we have introduced an energy gap. That is to say - instead of having some states which we know are present in a normal metal, slightly higher in energy than the energy of the electron with the Fermi energy, one would simply omit those orbits from the calculations and proceed from there. Bardeen then analyzed what happened to that metal when a magnetic field was applied to it. He succeeded in showing that he was able to derive an equation very similar to the London equation, describing the exclusion of a magnetic field by a superconductor. Bardeen made the statement at that time that if one could find the reason for the energy gap, one would very likely have the explanation of superconductivity. It's clear when one considers the papers Bardeen was writing and the thinking he was doing at that time that he was very close to the solution of the problem of superconductivity.

He had a very deep and intuitive feeling for exactly what was taking place. He knew it involved an energy gap. He knew it involved the interactions of the electrons with the lattice. There was one other thing which he knew as well, and that is that superconductivity is a phase transition of a very special kind, a type which is called a condensation in velocity or momentum space.

So now I want to explain just what we mean by such a condensation. I can't help but thinking, however, that you are sitting there scratching your head and feeling, oh boy, there sure are a lot of explanations involved in superconductivity! The fact is that this is a tough subject and you're beginning to get a feel for why it is that people like Niels Bohr, Felix Bloch, Richard Feynman, Werner Heisenberg, Lev Landau, the Londons, and others who are just brilliant physicists worked on this problem all those years and in fact didn't succeed in cracking the problem. Well, it's difficult, and that's why we find that even an effort at an elementary description of what happened is a pretty tough thing to listen to as well.

Phase transitions are something that we're all familiar with. I suppose the most common ones we all know are the melting of snow or ice or the vaporizing of water into steam. Now, usually when we think about phase transitions, we think about condensation in real space. To illustrate that - in the summertime we've all had the experience that if you have a drink with ice in it, water condenses on the outside of the glass. That's why people use coasters. What's involved is simply that the water vapor which is always present in the air is no longer permitted to remain there when it's in contact with something that is as cold as the surface of the glass, and the water molecules prefer to gather together to form the droplets of liquid on the cold surface of the glass. That's an example of a condensation - condensation in real space - that is to say, the molecules physically come together to form little droplets of water. Almost all the time when physicists think about condensation, they naturally revert to condensations in real space.

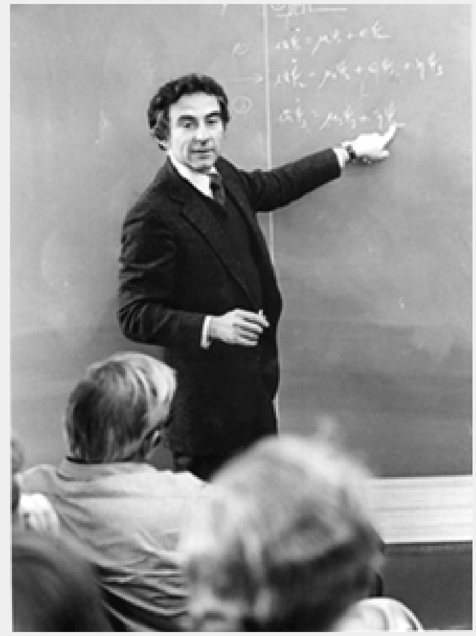
But the kind of condensation which is important for superconductivity is the condensation in another sort of space; it is the condensation in what one could call "velocity space." This is a slightly abstract idea, but there are very concrete ways to illustrate it. What is meant by a condensation in velocity space is that a whole bunch of objects assume nearly the same velocities. Contrast that with a condensation in real space where they assume nearly the same positions. To visualize a condensation of velocities think for example of a playground with a whole bunch of children playing in all different portions of it, running back and forth and scattered around on the playground. Suppose the bell rings signifying that recess is over. Then all the children suddenly start running towards the doorway from all over the playground. What happens is that, although their positions are scattered, suddenly they're all running in the same direction. They have nearly the same velocity. Note that you can condense their velocities without condensing their positions. Think of another example - think of the cars driving along a superhighway, which by and large they do at the speed limit. All up and down the superhighway there are automobiles miles apart all going the same direction at the same speed, and as different cars enter the superhighway from the entrance ramps, they pick up speed until they're driving at the speed limit. And thus we see a condensation of velocity even though the positions are widely separated. When you think about an east-west highway you might say that the velocity of the cars are condensed at two particular points in velocity space, namely the speed limit going east and the speed limit going west. That is the kind of condensation that takes place in superconductivity. The electrons condense in velocity space. The fact that this condensation takes place in velocity space was first recognized by Fritz London, who pointed out that this was almost surely involved in superconductivity.

That then represented the situation in the knowledge of superconductivity when Bardeen, Cooper and Schrieffer got together at the University of Illinois. There were three critical elements — 1: there was a condensation in velocity space; 2: there was an energy gap; and 3: the interactions of the conduction. electrons with the lattice vibrations were evidently critical in making the phenomenon occur.

I have to count it as one of the luckiest things in my life that I happened to be working as an experimenter in the field of superconductivity here at the University of Illinois back in 1955 to '57, just at the time that Bardeen, Cooper, and Schrieffer were working on the explanation of superconductivity. Bardeen's theoretical work in the early 1950's had stimulated my student Chuck Hebel and me to undertake a new kind of experiment on superconductivity. Our results were surprising, and we'd been to John to talk about them. And then, one day early in 1957, John Bardeen stopped me in the hall of the physics building. And it was clear he wanted to talk. He seemed to stand there almost for hours before he spoke. He's a quiet and a modest man. And then he said, "Well, I think we've finally figured out superconductivity." This was one of the great scientific announcements of the century. I may have been the first person apart from the three of them to know they'd actually solved it. You can imagine how excited I was!

I'd like to tell you just a few things about this trio that cracked the problem of superconductivity. John Bardeen came to Illinois in the early 1950's from Bell Laboratories where he, Walter Brattain, and

William Shockley invented the transistor. Leon Cooper had recently completed his PhD, in a totally different area of physics, and in the process he'd learned a set of mathematical techniques for what is called quantum field theory. He'd become quite expert in this and was viewed as one of the best young men in that general area. Bardeen felt it might be important to know these techniques in order to tackle the problem of superconductivity. So he invited Cooper to come to Urbana from the Institute for Advanced Study in Princeton. You could say Bardeen called in a quantum mechanic from the East.



Leon Cooper

Now, the first major breakthrough this trio made in superconductivity came from Leon Cooper. Scientists often crack a tough problem by judiciously unraveling one part of the mystery at a time. Here's where the role of judgment and physical intuition is paramount. You've got to decide which piece to tackle. Experiments on thermal conductivity and heat capacity had shown that superconductors had what is known as an energy gap. Bardeen's own work had shown that if one could understand why there was an energy gap, one would most likely be close to the heart of the explanation of superconductivity. Cooper set about trying to explain the existence of the energy gap.

J. Robert Schrieffer

Meanwhile, Bob Schrieffer was at the University of Illinois as a graduate student. He had completed his undergraduate studies at MIT, working in a group of solid state physicists. When he reached graduation time, he decided that the man he'd most like to do his graduate work with was John Bardeen. Schrieffer began to work with Bardeen. As a warm-up, Bardeen suggested some work on semiconductors. When it came time for a thesis, Schrieffer chose to work on superconductivity. Bardeen suggested he familiarize himself with the theoretical work Keith Brueckner had recently done on the nuclei of atoms. Nuclei, like metals, consist of many particles close together interacting strongly, so Bardeen hoped that theoretical methods helpful to nuclear physicists might help on the problem of superconductivity. Actually this tack did not lead Schrieffer to anything useful.

Leon Cooper was making an effort to find out why there was an energy gap. Now, I should point out to you that if you start with a system which represents a normal metal, and you introduce some sort of interaction which is going to cause the transition to a superconductor, it's not easy to find a situation in which a gap of energy occurs. So Cooper studied the general theories of quantum mechanics to see under what circumstances gaps arose. He decided to pick a highly simplified physical model of the system, because a real metal has many electrons in it, and the fact there are so many particles interacting presents overwhelming complexities. He found a very clever way of simplifying this problem. He said: let's just consider the interactions of two electrons. Now all the other electrons are present in the problem and we have to take account of them, but he said the most important thing which they did was to occupy all the low energy states - that is to say the states which were filled up to the Fermi energy. Since those electrons are occupying those states, those states were not available in any way for the two electrons whose interactions he wished to study.

He then examined what happened to these two electrons, taking into account two aspects which Pines and Bardeen had delineated. The first was that the electrons repelled one another, because they're particles of the same charge. The second thing was that the electrons are moving through this lattice, which contained the positive nuclei with their masses kept apart by their springs. As we mentioned previously, it's not proper to think of the lattice as being totally rigid. Instead, when you bring an electron in between two of the positive ions, these ions are attracted to the electron and thus pull somewhat closer together than they would be if the electrons were not there. When one electron was there and the ions pulled together, it made that point in space somewhat more favorable for a second electron to be there also. Since the pulling together was due to one electron, one could say that in this way one had an interaction of one electron with another by means of the lattice, and that the interaction was energetically favorable - that is to say, it was an attraction.

Cooper succeeded in solving the problem of the electrons interacting in the two ways I've described. He found that there was a delicate balance between repulsion of the electrons because they are of the same electrical charge, and the attraction brought about by the lattice distortions we've just described, and that when the lattice distortion term was somewhat larger, the two electrons had a net attraction and an energy gap was formed. Thus he was led to the conclusion that superconductivity arose when the attractive interaction of one electron for the other, through the lattice, was larger than the direct repulsion. This then became a criterion for superconductivity. This paper was published in 1956 and is one of the famous papers in the history of superconductivity. The interacting pairs of electrons have since been known as "Cooper pairs" after their discoverer.

Important as the step was which Cooper made, one must understand that a large amount of the problem yet remained unsolved, because he had considered the interaction of only a single pair of electrons, whereas in a real metal there are many interacting electrons - something like 10^{23}.

At this point Bardeen, Cooper and Schrieffer set about trying to generalize Cooper's results to the problem of many interacting electrons. That is - to make a many-body theory out of it. Now, the trouble was that when they tried to put together solutions, they would find that although they could make two electrons interact favorably with one another, quite typically what that did was to make them interact with a third or fourth electron unfavorably. The problem is somewhat analogous to one of those complicated three-dimensional puzzles which one attempts to assemble. When you try to put the puzzle together you may find you may have a couple of pieces that fit, but then when you try to get the third piece in, it won't fit, and the two pieces you already have interfere with adding it. What you have to do is find how to fit those pieces together in just such a way that they all simultaneously go together in a favorable manner. That was exactly the problem that was posed to Bardeen, Cooper and Schrieffer.

The break came when Bob Schrieffer succeeded in guessing, in essence, the nature of the solution at absolute zero. The form of the solution which he found turned out to be especially simple when expressed in a highly ingenious mathematical form. You can well imagine the feverish activity which then followed as the three attempted to generalize the solution to the higher temperatures, and to show that in fact they could account for all of the facts of superconductivity. Schrieffer in his description tells about their work carrying through to the final solution. The theory was published in the spring of 1957. This theory accounted for essentially all of the known experimental facts of superconductivity.

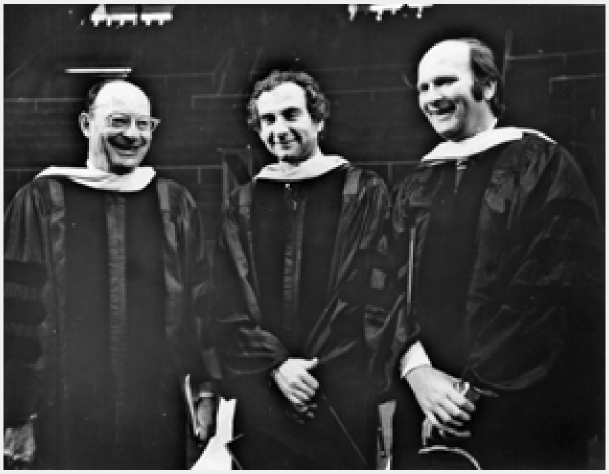
The trio then felt they were hot on the trail. But they still had lots to do. It's like assembling one of those three-dimensional puzzles. They knew how to handle any two electrons — but a metal has many more than just two. But within one year they were successful. They understood that a single Cooper pair was unstable. That is, the other electrons in the neighborhood would want to pair off, too. At the critical temperature — in their full explanation — the other electrons all do so. And that constitutes the phase transition from a normal metal into a superconductor.

How was it greeted? Experimenters greeted the theory with great acclaim, because it had such success in explaining their experimental results. The theory created a great flurry among the theorists as well. It's interesting however, and I think it illustrates the true nature of science, that many of the theorists felt a surge of disappointment that someone else solved this exciting problem. I remember, in fact, riding in an automobile from New Hampshire to Boston, returning from a conference on solid state physics. In the car with us was one of the truly great physicists of the time who'd worked on the problem, and he told of the enormous disappointment he felt when he found that someone else solved the problem. That led to a conversation in the car in which various people recollected their reactions. One member of the group told about a confession made to him by another truly great scientist, who said that when he first saw the account that Bardeen, Cooper and Schrieffer published saying they had solved the problem, he looked at it just closely enough to be able to see that it looked right, but couldn't bring himself to read the paper. He had to wait until one day when he himself had solved a particularly tough problem, and felt in a real mood of elation, to be able to bring himself to the point where he could sit down and really seriously study the Bardeen, Cooper, Schrieffer solution.

But the resistance to the theory was not solely because people were disappointed in not having solved the problem. When the theory first came out, it had some aspects which people questioned. But in a short period of time theorists were able to straighten those matters out and become satisfied that indeed the theory was correct. There's a marvelous quote which illustrates this, when David Schonberg remarked at the Cambridge [England] conference on superconductivity in 1959, "Let us see to what extent the experiments fit the theoretical facts."

One might have supposed that a theory which was as successful as this one would have closed the field and allowed physics to move on to other things. That was not the case. In fact the original work of Bardeen, Cooper and Schrieffer has been an enormous stimulus to work on superconductivity.

In 1972 John Bardeen, Leon Cooper and Bob Schrieffer got the Nobel Prize in physics for their theory of superconductivity.



Bardeen, Cooper & Schrieffer at the Nobel ceremony

Schrieffer's Story >

Schrieffer

Schrieffer's Story

Theorists at work:
How we got an explanation of superconductivity

Excerpts from an interview with J. Robert Schreiffer

Questions by Joan N. Warnow

Q: Bob, everybody knows you were involved in what turned out to be the explanation of superconductivity. How did that come about?

I recall the second year I was at Urbana, that was '54, '55.... and I had really hoped all the time when I went there that I would get to work on superconductivity....I came and asked Bardeen for a real thesis problem, and I'm sure he had this in mind. And he said, "Come in and see me." Exactly how the discussion came, I don't quite recall, but he traditionally kept in his bottom drawer a list of problems. And I remember there were ten problems on this particular list and the tenth was superconductivity. He said, "Well, why don't you think about it?"

Bardeen

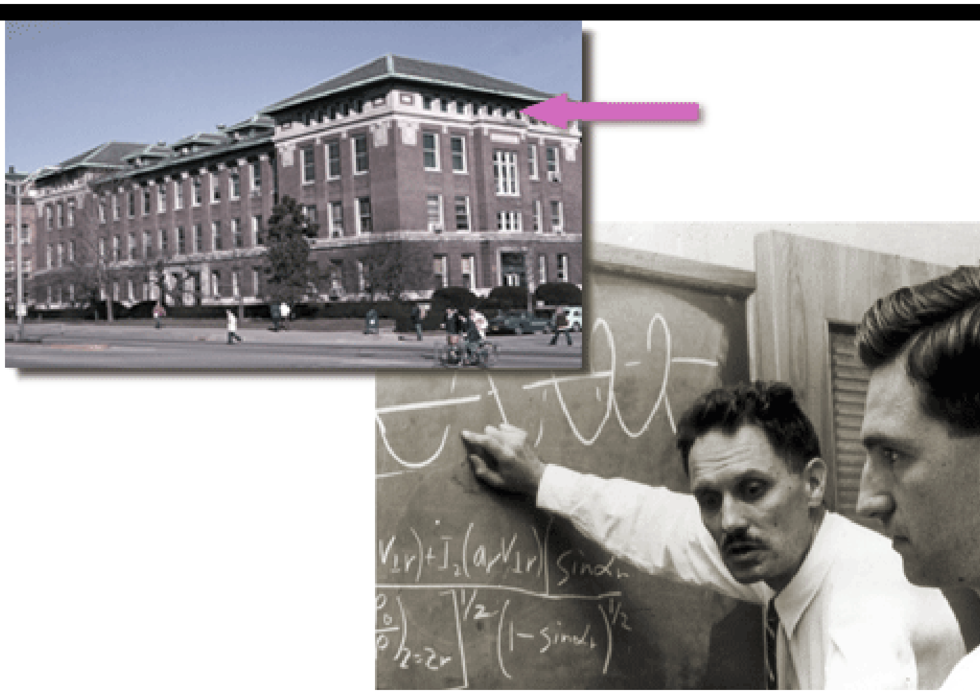
Q: Well, what did you do?

I went and chatted with Francis Low about this, because I felt that I could chat with him. He was very open. And I asked him what he thought about it, should I try this? He, I recall, asked, "How old are you?" and I told him. And he said, "Well, you can waste a year of your life and see how it goes."

The program really had been worked out in John's mind, I don't know, ten years before or what have you....He had this thing so nailed down on every corner: he understood the experiments, he understood the general requirements of the theory. The whole thing was more or less jelled in his mind. And then there was this stumbling block, and that was, you know, how to write down the wave function.

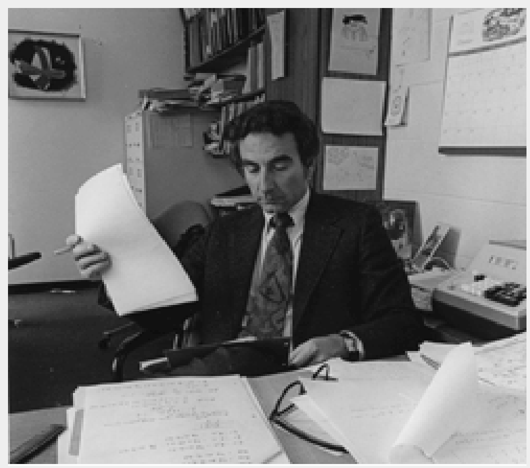
Q: Now, just where were you located — I mean, physically — at the University?

I was at what was called the "Institute for Retarded Study" — affectionately known — and it was on the third and a half floor of the building... It was again a wonderful format. There were people all together in one large area. There were field theorists, there were nuclear physicists — all theorists came there. And if somehow you were able to move to the Institute for Retarded Study, you had made it. That was considered the greatest. And when there was a place open, a desk open, then everyone would sort of scramble around to see who could get in there.... There was a great blackboard, and there were always two or three people at the blackboard, arguing and discussing. So that was fun. They were all students there.



Q: That seems marvelous! Now — let's see — John Bardeen and Leon Cooper were in the physics building and you were at the "Institute." How did you all interact?

Bardeen



Cooper

Bardeen and Cooper shared an office, and that was very important. They could wheel around their chairs and talk to each other continually. But I would come down and say something. to John and then Leon was there and we'd get together into a three way discussion — or if Leon was out, John and I would chat. But it was sort of a round robin where I think John and Leon probably didn't talk too much more than I chatted, but they were always together — and when they had a question, it would come up and they would discuss it. So, that was a very happy relationship which largely came about because there weren't enough offices for everyone. They were just squeezed in.

I'd been working on the Brueckner theory, and Leon took very seriously the energy gap aspect and focussed on that.

Cooper

Leon's discovery, that a pair is unstable, suggested a direction we should move to understand which hunk of the Hamiltonian we should look at — which piece of the total interaction was important.... So we started thinking about how we could make a many-body theory which took into account many pairs at the same time....We said, "OK, let's write down the problem where all electrons are treated, but we treat them in the second quantization formalism corresponding to pairs of zero momentum, and try and solve that problem".... And the fact that we concentrated on the pairs of zero momentum, rather than trying to treat all momentum pairs simultaneously, was to a certain extent out of simplicity: do the simplest thing first, if it doesn't work, then go on to the next most complicated. That seemed obvious. But then the problem came, we couldn't even solve that simplest of problems.

[**What is he trying to say?**]

We wrote down the Hamiltonian and looked at it and couldn't make any progress on it. We didn't know how to approach it — various ideas about variational methods, we thought — tried all sorts of approximate schemes....It was very exciting, but it was very frustrating, needless to say. And it wasn't clear what was going to happen....We also felt we were really hot. It was sort of this mixed feeling. We were really on the trail, and it was sort of almost a schizophrenia, you know &mdash we're going to do it, and we're not.

Q: You all knew you were looking for a wave function. Am I right?

That's right — in garbage cans and whatever

Q: And this went on for several months?

Yeah.

Q: Weren't you all feeling somewhat discouraged?

I personally had become somewhat discouraged at being able to make significant progress taking Leon's beautiful result and making a many-body theory out of it....I had started to quietly work on ferromagnetism. And I had mentioned to Bardeen that I thought perhaps I would like to change the thesis topic, because I didn't quite see that we were going anywhere.

Q: And what was Bardeen's reaction?

Well, I remember, just before John left for Stockholm, he said, "Give it another month or a month and a half, wait 'til I get back, and keep working, and maybe something will happen and then we can discuss it a little later."

In any event, we proceeded on, and then there was this meeting at Stevens and the New York meeting. And that was in the middle to end — I guess the end of January. And, somehow, during that couple of days in New York — whether it was at the Stevens part of it or the APS meeting part, it was some time during that week — I started to think about the variational scheme associated with this Tomonaga wave function....I wanted to use a variational scheme because there didn't seem to be any other scheme that was appropriate. One had to guess the answer, if you like, and then use some sort of a variational approach.

Schrieffer as a student

And I said, "Well, lookit, there're so many pairs around, that some sort of a statistical approach would be appropriate." That was sort of floating around in *my* mind — that there are so many pairs, they're overlapping — some sort of a statistical approach is appropriate. And, then the other one was this Tomonaga wave function — all sort of crystallized in saying, "Well, suppose I put an amplitude," I think I called it the square root of H, "that the state is occupied, and a square root of 1 minus H, that it's unoccupied — the amplitude — and then let's product these over all states k." And that's just what Tomonaga did for that problem. I said, "Well, at least that allows the electrons to hop from — or the pairs to hop from — state to state, and that seemed like a reasonable guess." We were futzing around and that was one try.

[**What is he trying to say?**]

So I set that down and then I looked at it, and I realized that that didn't conserve the number of electrons. It was a variable number of electrons and that had worried me, I remember. And so I decided, "Well, what I should do is multiply that wave function by a term involving e to the minus the number of particles and —just like in the grand canonical ensemble in statistical mechanics — sort of extend that idea to the wave function in quantum mechanics." And I said, "Gee, I don't know if it's going to work, but it seems to me a reasonable approach. Let me try it."

So I guess it was on the subway, I scribbled down the wave function and I calculated the beginning of that expectation value and I realized that the algebra *was* very simple. I think it was somehow in the afternoon and that night at this friend's house I worked on it. And the next morning, as I recall, I did the variational calculation to get the gap equation and I solved the gap equation for the cutoff potential.

It was just a few hours work. It was really exciting, it was fun. it was sort of beautiful and elegant — things worked out. It was all algebraic and I didn't have to go to a computer, or you know, there weren't terms I just threw away because I just couldn't handle them, but the whole thing was analytic. There were certain beauties, a simplicity, which — you might call it esthetics. I think that's — to my mind, that's a phony word, it implies more than that. But, it was sort of nice and I liked it.

Q: So, now you had it — that wave function. Did you feel that things were falling into place?

The consequences, you know, weren't clear to me or weren't important.

Q: And you were also very young. 25?

Right. I'd seen a certain amount of physics. And I didn't have perspective....I didn't have any basis to judge right or wrong, so I assumed that this was, perhaps not wrong, but it was a beginning of another interesting idea. Like Leon had a very good idea and it worked to a certain extent. I assumed that this was perhaps a good idea and it would move one along, but this wasn't the solution to the problem.

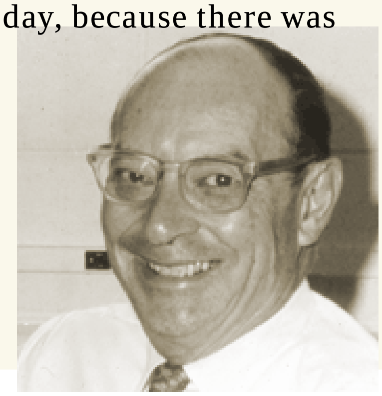
So people keep saying, "Nature is ultimately simple." I guess in some sense it depends upon the eyes. But this was so simple I didn't believe it. And that was sort of the other side.

It was an intuitive leap. And any intuitive leap, you have to justify it through a lot of tie points to experiment, and ultimately you hope there's a theoretical deductive way of getting there. But it was certainly far from that and I think even today we're not there....But I guess the main point I wanted to make was, I thought it was too simple and this just can't be the answer. It was exciting because it was fun to do, it worked out.

Cooper (ca. 1972)

And I met Leon then at the Champaign airport. Apparently he'd come in also from New York. Why we came there — I don't know — at the same time, but we appeared. I showed him this and he seemed very interested. He said, "Great, looks terrific," and "Let's go and talk to John in the morning"....You know, we really worked as a team, and I can't imagine of any more cooperative feeling....So the next morning we went and chatted with Bardeen and very quickly, as I recall, he looked at it and he said he thought that there was something really there.

It was so fantastically exciting that we sort of worked 18 hours a day, because there was just so much to do.... So we were working on two levels. One was saying, "Isn't it fantastic? It's all breaking open." But on the other level we were having mechanical difficulties of doing all the calculations and working and checking, etc. So it was an intensive period of intellectual activity, but also just hard work.



Q: What seemed to be the biggest problem at this point?

We did the low temperature thermodynamics and we

tried very hard to get the second order of phase transition — the jump in the specific heat — and that just didn't come out.... Then I think it was about three weeks to a month later — I'd been working very hard on it and Bardeen had — and I remember it was a Wednesday I thought I'd broken the problem. And I had made a slip of a sign....But I think that. Friday night, a distinguished Swedish scientist — Berelius, I believe — was visiting the Bardeens. And so, as I recall — again, my memory may not be accurate here — that John was somehow off on Cloud 7 that night. And there were long gaps in the conversation where John was staring into space, and the conversation was going on, but in a very strange sort of way. And it was clear that John was thinking hard about something. And what he was thinking about was how to get the second order phase transition and exactly how to write the wave function down.

So the next morning — apparently that night he had cracked the problem and called up the next morning. He woke me up early in the morning....and sort of said, "I've got it, I've got it. The whole thing's worked out."

But I had to write the thesis. So I went off to New Hampshire in — what? — the beginning or the middle of March, quietly getting the thing written out.

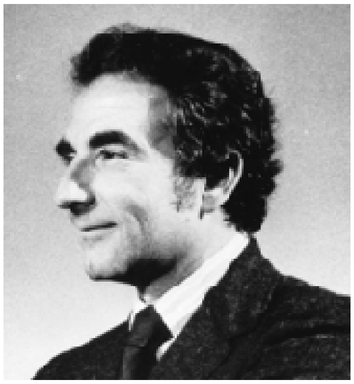
An American Physical Society meeting

Then came — let's see — then Fred Seitz had called Eli Burstein (who was somehow in charge of, or at least related with, the March meeting of the American Physical Society here in Philadelphia, the solid state physics meeting) and said that a major break in the theory of superconductivity had occurred — or at least John believed so — and was it possible to have two post-deadline papers?

So those were arranged, and John refused himself to come to speak about the theory because he wanted to make sure that the young people got the credit. And, you know, that's unbelievable, fantastic. So Leon was able to come and I got the word so late that I couldn't get on the plane to come. So he gave both papers together: he gave the one I was to give and the one he gave. This was a particularly interesting event not only because it was announcing the theory fairly early after its inception, if you like, and in a very raw form. It had only been — what? — a month and a half old, and the system responded to provide a possibility or a vehicle to get this out.

But much more so, it was to my mind a remarkable insight into the personal character of John Bardeen, who, I think, in many ways has felt the intrinsic intellectual contribution he made through superconductivity in some ways superseded that which was made in the invention of the transistor. He's said this on various occasions. And yet, for him, after struggling with the problem with a great amount of success, and having finally come to the pinnacle of achievement in his professional life, in a sense, steps aside for two young people — one of whom was a graduate student just sort of began in the field a year and a half before; the other wasn't from the field at all but was a post-doc brought in — and says, "OK, you go out and tell the world and I will stay here in Urbana." It's just beyond belief.

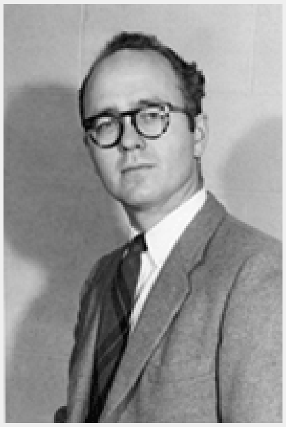
So, I think, to my mind, that's probably the most exciting message of the whole thing.



A Dance Analogy >

What’s he trying to say?

Schrieffer is dropping into the technical language of physics, the only way he can describe what the theorists were trying to do. But we don’t need to understand every word* to get a feeling for how the work was going. As Schrieffer tells it, he and Cooper were trying to simplify their problem. They were looking for some little piece of the physics that they could hope to take aside and solve. With the powerful mathematical tools they had learned, couldn’t they at least work out what happens between a single pair of electrons, among the countless electrons in a superconductor?



Keith Brueckner helped invent special techniques for solving quantum mechanics problems.

*(In case you were wondering — the “Brueckner theory” and “second quantization” are techniques for dealing with the hairy mathematical problems that arise in quantum theory when you try to deal with a bunch of electrons, the “Hamiltonian” is a particular mathematical way of describing energy, which can display all the forces that influence an electron, and a “wave function” is another way of capturing all the main physics in a single equation.)

Close Window

What's he trying to say?

Again Schrieffer can describe what happened only by dropping into the technical language of physics. But we don't need to understand every word* to get a feeling for how he was thinking. As he tells it, Schrieffer was rummaging through a tool kit that he kept in his head, a bunch of mathematical techniques and special ways of thinking. As a student he had learned how to apply these tricks to all sorts of physics problems. At the same time, he was trying to imagine how actual electrons might push each other around within the superconductor. His moment of discovery came when one particular set of abstract mathematical tools clicked with his vision of the physics of the electrons

Shin'ichiro Tomanaga got a Nobel Prize (along with Richard Feynman and Julian Schwinger) for showing how to solve the most difficult equations of quantum mechanics.

*(In case you were wondering — the "Tomonaga wave function" is one way to describe in mathemtics how electrons behave in the strange world of the quantum, a "variational scheme" is a technique for solving equations for particular types of problems, and H is shorthand for the Hamiltonian, that is, the energy in Tomanaga's equations.)

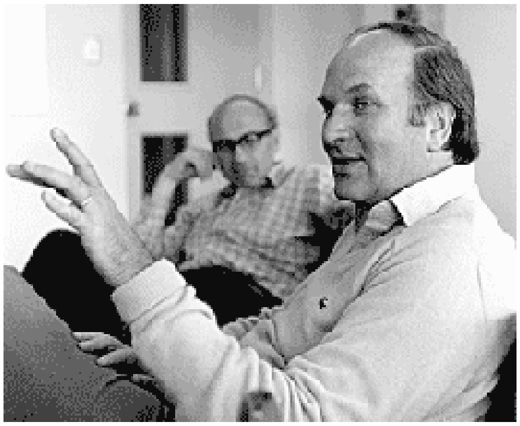
Close Window

Superconductivity: A Dance Analogy

J. Robert Schrieffer

Introduction by Charles Slichter: Let's now hear Bob Schrieffer explain how they put together lots of Cooper pairs to explain superconductivity, in terms of an analogy to many couples dancing on a crowded dance floor. In it he tries to give you a feel for why the assembly of Cooper pairs (or dancing couples), acting together, inhibit the scattering of electrons which ordinarily produce electrical resistance, and thus why a current can persist.

Download complete audio file (4.30MB)



J. Robert Schrieffer

SCHRIEFFER: Suppose that you say that you have a large number of couples on a dance floor, and every male has an up spin and a female has a down spin, so they're up and down spin electrons. They're doing a frug, or whatever, where they never touch each other and are very far apart and dancing around this dance floor. Okay. They may be, say, a couple of hundred feet apart. But they always know exactly to whom they are mated, who's their partner, and yet there are roughly one million other pairs dancing in the area corresponding to the space in between those two areas — the cube root, or the two-thirds root of that — about 10,000 people.

Now, these dancing couples are essentially totally covering the dance floor. There is very little space not covered by people. So when they dance they have to do a highly intricate step of moving into a space that, at that instant, happens to be vacant. And this is enormously complicated choreography, so that one doesn't trip, if you like, or hit someone else. And the electrons can't hit each other, or at least they can't occupy the same space at the same time. Fine. So they're all dancing together. By dancing, if you like, they lower their energy or make themselves happier or whatever analogy you like to make.

Now, suppose that the dance floor is tipped. Or, another way of saying it, somebody starts pushing on one end of the dancing group and the dancing group starts to drift across the dance floor. Everybody still doing the same choreographed step, not in the rest frame, but in the moving frame.

Suppose, however, that there happen to be some wood chips or nails or what have you sticking up from the floor — and these correspond to the impurities or defects in the superconductor, or lattice vibrations that are thermally excited — then, say, a given mate of a pair would tend to be tripped. But, unfortunately, there's no space for that mate to go into because it's occupied by another one. Or, if it does go into the wrong space where it shouldn't have gone, it gets out of synchronism, or dance pattern, choreography, with its mate and can no longer dance. Ergo, its energy goes up discontinuously.

Dancers in the Burnside Ballroom during the late 1950's;

Courtesy: City of Burnside, www.burnside.sa.gov.au

The only way, to slow down the entire ensemble is not differentially, pair by pair by slowing down, because that increases the energy. The only way to slow it down and decrease the energy is for the entire dancing ensemble to slow down. And that's very unlikely if they're just random bumps around the floor.

So the choreographic notes, if you like, or that thing which is written down by the choreographer to tell everybody how to dance, or at least describe how they do dance — you know, it may be that God created all these people beautifully choreographed and all we did is figure out what the dance pattern is and we wrote it down. And instead of taking 200 volumes, it turns out to take two lines. And, if you get the right language it appears enormously simple. If you have the wrong language, you probably couldn't write it down — I'm sure you couldn't write it down in all the volumes in the entire world. If you write it down in coordinate space, there are 10^{23} electrons. And to write down even 10^{23} symbols would take more than all the paper in the universe. So you write it in a symbolic way which is enormously simple, allows you to calculate with it and make predictions without ever writing the thing down in its gory details.

The wave function is just... symbols which record the dance the electrons are making.

Now, we didn't invent the dance. Kammerlingh-Onnes discovered that the dance was going on and we were the choreographers that recorded what the dance was.

Still confused? Exit this exhibit to see other ways to explain the theory (from superconductors.org)

Bardeen Reminisces >

John Bardeen Speaks About His Early Years

From a 1977 interview by Lillian Hoddeson

Early Scientific Interests

In high school, Bardeen was interested mainly in mathematics and chemistry:

Q: Did you also do some experimental work, or any work with radio or things like that?

Bardeen

Not so much with radio, though I built a "cat's whisker" detector radio, that most boys were doing at that stage. They were easy to build. Some of them went as far as putting in vacuum tube amplifiers but I never got that far. My main project, I guess, in high school days was doing chemistry projects in the basement laboratory... basement at home. I got interested in that from reading a book on "Creative Chemistry" by Slosson. During the First World War we were shut off from importing dyes from Germany, so the organic chemists in this country had to learn how to produce the dyes. And that was described in this book. So I got interested in how dyes are made, and I made some. I dyed materials with it, and also made experiments on injecting dyes in eggs, seeing how you get colored chickens, and things of that sort. Nothing too elaborate..

A First Try at Superconductivity

Bardeen's first attempt at a theory was never published.

Q: What were you thinking about most deeply in that period?

I was working on superconductivity primarily. The only thing published was just an abstract. I sent around a few preprints for comments. I wrote a paper and sent it around for comments. It looked like quantitatively, it was off at least by a factor of ten or so. And so I never published the full paper.

About that time I left to go to Washington to work for the Navy, so that got stopped. But some of the ideas are carried over into the present theory, that there is a small energy gap covering the entire Fermi Surface, and that was the basis for this sort of a model. But the way the energy gap was obtained was different than it was at that time.

Bardeen as a young man

Q: Was this a subject that lots of people were very interested in at the time?

I sent around preprints to people who were interested, like [Frederick] Seitz and others, and got comments from them.

Bardeen left university life to work on military projects in 1941 as American entry into the Second World War looked increasingly likely.

Historical Study (Hoddeson) >

John Bardeen and the Theory of Superconductivity

by Lillian Hoddeson (1)

Introduction

Every theory of superconductivity can be disproved! This tongue-in-cheek theorem struck a chord when Felix Bloch announced it in the early 1930s. Virtually every major physicist then working on theory — including, besides Bloch, Niels Bohr, Wolfgang Pauli, Werner Heisenberg, Lev Landau, Leon Brillouin, W. Elsasser, Yakov Frenkel, and Ralph Kronig — had tried and failed to explain the mysterious phenomenon in which below a few degrees Kelvin certain metals and alloys lose *all* their electrical resistance.(2) The frequency with which Bloch's theorem was quoted suggests the frustration of the many physicists who were struggling to explain superconductivity.

Neither the tools nor the evidence were yet adequate for solving the problem. These would gradually be created during the 1940s and 50s, but bringing them to bear on superconductivity and solving the long-standing riddle required a special set of talents and abilities: a deep understanding of quantum mechanics and solid state physics, confidence in the solubility of the problem, intuition about the phenomenon, a practical approach to problem-solving, patience, teamwork, and above all refusal to give up in the face of repeated failures. When John Bardeen took on the problem of superconductivity in the late 1930s, he held it like a bulldog holds a piece of meat, until he, his student J. Robert Schrieffer, and his postdoc Leon Cooper solved it in 1957.

Princeton and Harvard

Bardeen probably first encountered the problem of explaining superconductivity between 1933 and 1935, when he was a graduate student at Princeton. He was entering the new field of the quantum theory of solids and avidly reading its pioneering papers. In their comprehensive review published in the 1933 *Handbuch der Physik*, Hans Bethe and Arnold Sommerfeld identified superconductivity as the only solid state problem that still resisted treatment by the quantum theory.(3) While we have no evidence Bardeen even attempted to attack the problem in that period, he likely entertained the thought, for he was amply endowed with competitive spirit.

Arriving at Princeton in the fall of 1933, in the depths of the Great Depression, Bardeen boldy turned his back on the secure engineering post he had held for the last three years at Gulf Research Laboratory in Pittsburgh. He enrolled in Princeton's graduate program in mathematics. Abandoning his initial idea of working with Einstein, who also arrived in Princeton that fall, Bardeen became the second graduate student of the young, but already quite eminent, mathematical physicist, Eugene Wigner.

Just then, Wigner was excited about employing quantum mechanics to explain the multitude of behaviors and properties of real materials. He was working with his first graduate student, Frederick Seitz, on developing a simple approximation method for calculating the energy bands of sodium, the first real (i.e., nonideal) material to which the quantum theory of metals was applied. Wigner was bothered by the fact that his work with Seitz failed to account for the interactions between electrons. He recognized that his own attempts to add an electron interaction term in a study of the cohesive energy of metals was only the beginning of the development of a "many-body" theory, in which the interactions between electrons, as well as between the electrons and lattice are properly dealt with.(4)

Wigner posed the fundamental question to Bardeen: How do the electrons inside metals interact? The problem so enticed the student that he never let go of it throughout his physics career of almost 60 years. He returned to it, for example, in his doctoral thesis, in which he calculated a metal's "work function" (the energy needed to remove an electron from the metal),(5) in his study of semiconductor surface states in 1946, a major step in the invention of the transistor;(6) and in the numerous many-body problems he addressed from the 1950s on, including charge density waves and superconductivity.

During Bardeen's period as a Harvard Junior Fellow from 1935 to 1938, he often found himself frustrated by problems that required a many-body theory. For instance, he was unable to explain the experimental finding that the "Fermi surface" (the surface of the Fermi-Dirac distribution in wave vector space) is sharp, despite exchange and correlation effects, as suggested by the recent experiments at MIT of Henry O'Bryan and Herbert Skinner.(7) While Bardeen recognized that correlation effects had to be taken into account to avoid having an infinite velocity at the Fermi surface, he did not know how to correctly include them in the calculation.(8) The process of working on many-body problems that could not yet be solved within the existing theoretical framework helped Bardeen prepare for the major challenge of his career.

While he did not yet take a real stab at explaining superconductivity while he was at Harvard, Bardeen later claimed that he became interested in the problem there in the course of studying the new phenomenological theory published in 1935 by the London brothers, Fritz and Heinz, who had resettled at Oxford after fleeing Hitler's Germany.(9) Bardeen was powerfully drawn to this theory, particularly to its idea that superconductivity exists as a macroscopic quantum state — "the superconductor become characterized as a single large diagmagetic atom."(10) Bardeen believed this intuitively: although determined by an ordering of electrons extending over substantial distances (10^{-4} cm), the state of superconductivity required a quantum-mechanical description. To fully establish this intuition would take Bardeen approximately two decades.

Minnesota

Bardeen began his work on superconductivity at the University of Minnesota, where he held his first academic post from 1938 to 1941. To get a "feel" for the phenomenon, he read David Shoenberg's new book reviewing the experimental situation.(11) Experiments established that the transition to superconductivity is reversible and can therefore be described using thermodynamics. Most shocking was Walther Meissner's and Robert Ochsenfeld's experimental finding in 1933 that superconductors expel magnetic fields. Ever since Heike Kamerlingh Onnes's discovery of superconductivity in 1911, zero resistance had been considered the essential feature of superconductivity. Now it appeared that diamagnetism might be more basic. The vanishing of the resistivity followed mathematically from the London theory, which had been modeled phenomenologically to account for the expulsion of magnetic field. Bardeen felt it would be possible to derive the London theory from first principles.

He tried viewing the experiment of Meissner and Ochsenfeld from the point of view of the electrons in the lattice, asking whether the Meissner effect could mean that electron orbits are much larger in superconductors than anyone had realized. Seeking to explain in a quantum-mechanical framework how gaps appear in the electronic structure, as stressed by the Londons, he drew on the Pauli exclusion principle and guessed that because the energy scale of superconductivity is low (about 10^{-4}

eV) the only electrons likely to be involved are those at the edge of the Fermi surface. (Electrons further in would not have states to receive them.) Like an engineer testing his apparatus, he tapped his theoretical model and explored introducing a small periodic distortion of the crystal lattice.

In one of his more important works at Harvard, a first-principles calculation of the electron-phonon interaction in metals, Bardeen had assumed (unlike earlier calculations) that the unscreened potential moves along with the ion. Applying ideas he developed there to superconductivity, Bardeen tried to show that a periodic disturbance introduced into a superconductor causes the electrons to gain an amount of energy which more than compensates for losses due to ionic displacement. From the disparity he hoped to explain how the gaps form.(12) Unfortunately the numbers were off by more than a factor of ten; he did not commit his calculation to print (other than as an abstract). Bardeen could not help but recognize that his work was only a beginning.

He would have to wait almost a decade to continue the study, for in March 1941 he was suddenly called to Washington D.C. to work during World War II at the Naval Ordnance Laboratory on magnetic mines. But, he later confessed, "The concept of somehow getting a small energy gap at the Fermi surface remained in the back of my mind."(13)

Bell Labs

Bardeen moved in October 1945 to Bell Telephone Laboratories, where he joined a new semiconductor group directed by William Shockley. Bardeen initially enjoyed working in this group, until the atmospher changed in late December 1947, after he and Walter Brattain invented the first transistor, a point-contact device. Chagrined not to have been directly involved in the discovery, Shockley now began feverishly to pursue the original transistor's sequel, the junction transistor, excluding Bardeen and Brattain and generally ruining the quality of their research life.(14)For two years, Bardeen tried to work in this frustrating environment. By early 1950, he knew he was wasting his time. "Bardeen was fed up with Bell Labs — with a particular person at Bell Labs," Brattain reflected.(15) Bardeen's efforts to separate himself from the pain of working under Shockley brought him to the most important work of his life.

Bardeen pulled out his old notes on superconductivity. Reviewing the experimental progress made since he last worked on the problem, he noticed that much new evidence was supporting the London theory.(16) But what riveted him to the problem was a phone call he received on May 15, 1950 from

Bernard Serin. The Rutgers experimentalist wanted to speak with Bardeen about his new findings studying mercury isotopes, available as a consequence of the wartime atomic bomb program. Examining isotopes made at Oak Ridge having mass numbers between 198 and 202, Serin and his students had found an "isotope effect," the lighter the mass, the higher the temperature at which the materials turn superconducting. Emanuel Maxwell at the National Bureau of Standards found the same effect independently studying isotopes made at Los Alamos.

Bardeen instantly understood the new clue these results offered, noting to himself on May 16th, "electron-lattice interactions are important in determining superconductivity." He spent the next several days trying lattice fluctuations in place of the periodic lattice distortion in his Minnesota theory. The effort failed, but he was sure he was on the right path. To secure priority, he dashed off a letter to the *Physical Review* outlining the idea.(17)

As it happened, Bardeen was not the only theorist to connect superconductivity with the electron-lattice interaction. Earlier in 1950, before Maxwell and Serin found the isotope effect experimentally, Herbert Fröhlich had set forth a theory predicting it. When Fröhlich learned of the expermental results a day or two after they appeared in the *Physical Review*, he sent a letter to the *Proceedings of the Royal Society* to claim priority for his theory.(18) The competition was on.

Neither Fröhlich nor Bardeen could calculate all the relevant quantities, such as the superconducting wave function, the energy of the superconducting state, or the effective mass of the electrons. Their mathematical formalism was too limited. While both theories could explain the isotope effect, they could not explain superconductivity because they focused on individual electron energies rather than the energy that arises from the interaction of many electrons. The basic problem on which both got stuck was to find an interaction that made the total energy of the superconducting state lower than that of the normal state. The energy from the electron-phonon interaction had to dominate that arising from the ordinary Coulomb repulsion of electrons. More than a year later, Bardeen confessed to Rudolf Peierls that all the methods he had tried could not treat this problem. Even so, he wrote,"I believe that the explanation of the superconducting properties is to be found along the lines suggested by F. London." The hint that bolstered Bardeen's confidence was, "The wave functions for the electrons are not altered very much by a magnetic field."(19) This "rigidity" of the wave functions, assumed by the Londons, offered a basis for the long-range ordering.

Meanwhile Bardeen increasingly felt like an outcast at Bell Labs. He longed for greater contact with colleagues, students, and especially experimentalists, not to mention institutional support for his research on superconductivity. Shockley was a continuing source of irritation. During a fall conference in the Pocono Mountains, Bardeen sat down with his old Princeton friend and colleague, Frederick Seitz, for a heart-to-heart talk. He told Seitz about his problems with Shockley and about his exciting work on superconductivity. "I'm really planning to leave the Bell Labs, can you advise me of any jobs?"(20)

Seitz was the perfect confidant. Not only had he known Shockley for many years, but he was just then building a solid state group at the University of Illinois. Seitz spoke with administrators and soon Illinois extended an offer to Bardeen, who responded, "well Illinois would be perfect, it's the kind of place I'd like to be at."(21)

Illinois

After the move to Illinois, Bardeen prepared to finally crack the riddle of superconductivity. Starting over, he approached the problem in the way Wigner taught him, separating it into smaller parts, examining all manageable pieces, later trying to reassemble the parts to get a handle on the larger issue.(22) He soon encountered the old hurdle of the many-body interactions. He was aware that in using the standard (Hartree) approximation, he might be eliminating the most critical aspect.

Bardeen also made another move of a kind that had served him well in previous projects, including his work on the transistor. He engaged collaborators who had knowledge, talents, or experiences that he judged possibly relevant and that he himself lacked. He thought David Bohm's new many-body formalism for treating the electron plasma might be useful in modeling the electron-electron interactions. Bohm's interest in electron plasmas grew out of his wartime work on electromagnetic separation of isotopes.(23) Bardeen was particularly interested in the way Bohm and his student David Pines had mathematically separated the troublesome long-range Coulomb interactions from the single-particle excitations, which interact short-range. Offering Pines a postdoctoral position at Illinois, Bardeen hoped to extend his own repertoire with Pines' experience.

When Pines arrived in July 1952, Bardeen asked him to look at a problem Fröhlich had recently studied, the motion of an electron in a polar crystal. Simpler than superconductivity, this "polaron" problem, had a number of the same features. One could study in a less complex system how the electrons are strongly coupled to the lattice vibrations (phonons). Working with Tsung-Dao Lee, a young theorist then spending the summer in Urbana as Bardeen's postdoc, Pines realized that a method Lee had recently used in his field theory studies (the "intermediate coupling method") could be adapted for the polaron problem.(24) Also bringing in Francis Low, then on the Illinois faculty, Lee, Low, and Pines arrived at a formulation that would be useful in the development of the BCS theory.

Then Bardeen worked with Pines to adapt the Bohm-Pines theory to treat the combined influence of all the electron interactions in a metal. In a calculation comparing the size of the attractive phonon-induced interaction with that of the repulsive Coulomb interaction, they found that for cases where the energy transfer is small, the attractive interaction is stronger.(25) Bardeen immediately recognized the importance of this finding: for pairs of electrons near to the Fermi surface, the net electron-electron interaction is attractive!

In the same period, Bardeen also undertook an extensive literature study of superconductivity while writing a review article on the theory for the 1956 *Handbuch der Physik*. In the review he argued for London's notion of superconductivity as an "ordered phase in which quantum effects extend over large distances in space" and ventured that superconductors are "probably characterized by some sort of order parameter which goes to zero at the transition." But, he admitted, "we do not have any understanding at all of what the order parameter represents in physical terms."(26) He emphasized the diamagnetic origin of supercurrents, and discussed the second-order phase transition between the normal and superconducting state. Following London, he stressed the role of the energy gap caused by the rigidity of the wave function with respect to magnetic perturbation. While he could not yet derive the gap, by assuming it, he could show how to develop both the electrodynamic properties of superconductors and a generalization of the London equations similar to the non-local formulation of superconductor electrodynamics recently put forth by Pippard.

Another focus was the machinery for computing both the electron-electron and electron-phonon interactions. He stressed the importance of considering the electrons as electrically "screened," and he commented on the promise offered by recently developed field theoretical techniques, such as Sin-itiro Tomonaga's strong-coupling approach and the Bohm-Pines theory. He concluded: "A framework for an adequate theory of superconductivity exists, but the problem is an exceedingly difficult one. Some radically new ideas are required."(27)

Painfully aware of Fröhlich's advantage in field theory, Bardeen telephoned Chen Ning Yang at the

Princeton Institute for Advanced Study during the Spring of 1955 and asked whether he could send to Urbana someone "versed in field theory who might be willing to work on superconductivity."(28) Yang recommended Leon Cooper, who had recently taken his Ph.D. After arriving in September, the young theorist offered a series of seminars on field theory. The third member of the team, J. Robert Schrieffer, was a Bardeen graduate student who selected superconductivity for his thesis after proofreading Bardeen's *Handbuch* article because superconductivity "looked like the most exciting thing."(29)

Bardeen was unquestionably the leader who set the problems, motivated the members, organized the approach, and planted theoretical seeds by making appropriate assignments. He asked Schrieffer to look into the "*t*-matrix methods" that Keith Brueckner recently developed in studying nuclei. He asked Cooper to examine the Bohm-Pines theory, as well as his 1954 work with Pines on the electron-electron interaction. Bardeen continued to look out for other useful leads, while nurturing the team's work in frequent discussions.

The collaboration was family-style. Bardeen and Cooper shared an office. And when Schrieffer came to speak with either, both would "wheel around their chairs" and join in. Schrieffer claims that he and Cooper absorbed Bardeen's taste in physics, his experiment-based methodology, his habit of breaking down problems, and his simple style of using as little theoretical machinery as possible, "the smallest weapon in your arsenal to kill a monster."(30)

As the team grappled with the difficult many-body problem, Bardeen held to his belief that the key to solution was in the London theory, which Fritz London had recently reformulated in a book that explained better how the rigidity of the wave function and the long-range ordering brought about "a quantum structure on a macroscopic scale ... a kind of solidification or condensation of the average momentum distribution,"(31) Another guiding idea was that there is only one stable current distribution, and in thermal equilibrium there is no persistent current in an isolated superconductor, unless the system is in the presence of a magnetic field. Bardeen further stressed that these currents "differ for every variation of the strength or direction of the applied field." Schrieffer recalls Bardeen pressing them to clarify the notion of long-range order using a "phase coherence" parameter of the size (the order of a micron) of typical correlations between the particles.

Bardeen also helped the team strke out into the unknown by offering a principle that formed a bridge between the known theory of the normal state and the unknown theory of superconductivity. The principle stated that the superconducting energy states should correspond one-to-one with the normal states. Thus it should be possible to express the wave function of the superconducting state as a linear sum of the normal state functions as defined in quantum field theory. That way of thinking helped them concretize their meditations and concentrate on the small energy difference between the normal and superconducting states.(32)

Cooper had a breakthough in September 1956. Examining the simple case of only two electrons just outside the Fermi surface, and making certain other assumptions, he showed that if the net force between them is attractive, when their energies lie within a certain range of one another the two electrons form a bound state below the continuum states that is separated from them by an energy gap.(33) But the group got stuck trying to go from a single "Cooper pair" to a many-electron theory. A major difficulty was coping with the fact that many pairs would overlap. Schrieffer later portrayed the problem using an analogy with couples dancing the Frug on a crowded floor. Even though partners dance apart for considerable periods, and even though other dancers come between, each pair remains a couple. The problem was to represent that situation mathematically.(34)

They worried about their approximations. The energy change in the transition from normal to superconducting (about 10^{-8} eV per electron) was much smaller than the accuracy with which they could calculate the energy of either state. In working only with the part of the system responsible for pairing, they knew they might be ignoring another part important enough to invalidate the whole analysis.

They still were stuck in November when the exciting news broke that Bardeen, Brattain, and Shockley had won the 1956 physics Nobel Prize for the invention of the transistor. This was a most confusing time for Schrieffer. Now a fourth-year student, he had recently been offered an attractive NSF fellowship that he wanted to accept for study in Europe. But a condition was that he be done with his doctorate. Schrieffer was pleased about Bardeen's prize, but he had his own future to consider. He met with Bardeen shortly before the latter's trip to Sweden and asked, since the group was at an impasse, whether it might make sense for him to switch his thesis problem.(35)

Bardeen did not want to slow his student's career, but he truly read the situation differently. Having worked on superconductivity for almost two decades, he could sense what Schrieffer could not: that they were very close to breakthough, so close that he could not let him give up. "Give it another month, or a month and a half," he muttered. "Wait 'til I get back and keep working. Maybe something'll happen and we can discuss it a little later."(36)

The timing of the Nobel was in fact poor for Bardeen too. Richard Feynman had spoken on superfluidity and superconductivity that September. Bardeen was well aware of Feynman's advantage in field theory. And on some deep level, he felt that from a physics point of view the transistor, although important technologically, was only a gadget.(37)

Bardeen went right back to work after Stockholm. His daughter Betsy, then 13, recalled that during Christmas her father was in another world.(38) Yet the problem did not break in December, nor through most of January. But in the last days of January the turn came. Schrieffer and Cooper were attending meetings on the many-body problem on the East Coast, one in Hoboken and another in New York City. As Schrieffer was commuting between the meetings, and also to Summit, New Jersey, where he was staying with a friend, something clicked.

The process, as Schrieffer remembered, was a sort of intellectual tinkering. Having listened to talks on the nuclear interaction (between pi-mesons, protons and neutrons) and thinking constantly about superconductivity, he ventured to guess a possible form of the wave function for the superconducting ground state, one that took the Cooper pairs into account. Then he tuned up the expression using a variational approach like the one Tomonaga had used in the pion-nucleon problem. Knowing that a conventional (Hartree) product, where the state k is either occupied or unoccupied, does not lead to an energy lowering, he made sure his wave function didn't require any given state to be definitely occupied or unoccupied. "I wanted to have some flexibility, so the electrons could scatter around and lower their energy."

He called on Bardeen's bridging principle, "to form the wave function as a coherent super-position of normal state-like configurations." Tinkering on, he realized, "so many pairs, they're overlapping — some sort of a statistical approach is appropriate." Following Tomonaga, he tried forming a product, thinking, "Well at least that allows the pairs to hop from state to state, and that seems like a reasonable guess." He noticed that what he had constructed didn't conserve the number of electrons, and when he tried to fix that problem, "I decided what I should do is multiply that wave function by a term involving E to the minus the number of particles," in effect employing what in statistical mechanics is known as the grand canonical ensemble.(39)

Then "it all sort of crystallized" while he was on the subway. "I scribbled down the wave function and calculated the begining of that expectation value, and I realized that the algebra was very simple." He worked more on the expression that night at his friend's house, and in the morning did a variational calculation to determine the gap equation. "I solved the gap equation for the cutoff potential. It was just a few hours work." Expanding the product, he found he had written down a product of mathematical operators on the vacuum that expressed the creation of electrons. In his sum of a series of terms, each one corresponded to a different total numbers of pairs. He was completely astonished to find that his expression "was really ordered in momentum space" and that the ground state energy "was exponentially lower in energy," as required for the state to be stable.(40)

Schrieffer could hardly wait to tell Bardeen and Cooper. By chance he and Cooper flew into Champaign at the same time, and he could not resist showing the expression to Cooper right there in the airport. "Great, looks terrific," Cooper said. "Let's go and talk to John in the morning."(41) And when Bardeen saw the wave function, he calmly drawled that "he thought that there was something really there." Then, after "we chatted around about that for a few hours," Bardeen set out to try to use the wave function to compute the energy gap. Schrieffer remembered that Bardeen was very confident and that it took him only a few days. The magnitude with the gap parameter in the ground state energy!(42)

The most exciting moment occurred several days later, when Bardeen calculated the condensation energy in terms of both the energy gap and the critical field, obtaining a relationship between these experimentally determined quantities. At first Bardeen had trouble converting units. He "was very upset that he couldn't get the numbers to work out." But eventually they did work and turned out "something like 9 compared to 11 in the appropriate units. And we were really overjoyed, and sort of hit the roof. Things looked like pay dirt."(43) All the pieces were fitting together.

The three began to race. Bardeen divided the tasks asing Schrieffer to work on thermodynamic properties, Cooper to explore the Meissner effect and other electrodynamic properties, while he took on the transport and non-equilibrium properties. Bardeen's colleagues knew that something was up when they asked him a question and were told, apologetically, that he was too busy to think about anything else just then.(44)

Two weeks after Schrieffer's breakthough, they were ready to publish. But Bardeen had not succeeded in deriving the second-order phase transition. He finally decided not to let this hold up their publication any longer. When they sent their historic letter on BCS to the *Physical Review* on

February 15th, Bardeen requested immediate publication: "I know that you object to letters, but we feel that this work represents a major breakthrough in the theory of superconductivity and this warrants special handling."(45) And shortly after sending off the letter, Bardeen succeeded in computing the second-order phase transition.

The letter explained how superconductivity arises from the coupling between electrons and phonons, an interaction in whose presence the system forms a coherent superconducting ground state in which individual particle states are occupied in pairs, "such that if one of the pair is occupied, the other is also."(46) The letter summarized the advantages of the theory:

1. It leads to an energy-gap model of the sort that may be expected to account for the electromagnetic properties.
2. It gives the isotope effect.
3. An order parameter, which might be taken as the fraction of electrons above the Fermi surface in virtual pair states, comes in a natural way.
4. An exponential factor in the energy may account for the fact that kT_c is very much smaller than $\hbar\omega$.
5. The theory is simple enough so that it should be possible to make calculations of thermal, transport, and electromagnetic properties of the superconducting state.

Bardeen announced the breakthough to his Illinois colleagues in a characteristic way. Bumping into Charles Slichter in the hall, he momentarily struggled for words, and then offered, "Well, I think we've figured out superconductivity." Slichter remembers that instant as "the most exciting moment of science that I've ever experienced."(47)

Slichter and his student Charles Hebel were among the first to confirm the BCS theory experimentally. Measuring the rate at which nuclear spins relax in aluminum as a function of temperature, they found that as they lowered the temperature and the aluminum makes its transition to superconductivity the nuclear magnetic resonance rate *increases*, instead of decreasing, to more than twice its value in the normal state. Then as the temperature is further reduced, the rate begins to decrease again. While the effect was contrary to the predictions of the prevailing (two-fluid) model of superconductivity, BCS could explain it in terms of an increased density of states below the transition temperature. Soon many experiments at many institutions were confirming the theory.(48)

The team announced the theory at the annual solid state meeting of the American Physical Society in March, held that year in Philadelphia. Concerned that Schrieffer and Cooper receive their due credit, Bardeen decided not to attend and arranged for two post-deadline papers to be delivered by his younger teammates. Schrieffer got word too late to attend, so Cooper had to deliver both papers. One week later, their historic letter on the BCS theory appeared in the *Physical Review*.

Their full-length article, sent to the *Physical Review* four months later, showed in more detail how the theory explains: (1) the infinite conductivity discovered by Kamerlingh Onnes; (2) the diamagnetic effect found by Meissner and Ochsenfeld; (3) the second-order phase transition at the critical temperature; (4) the isotope effect; and (5) the energy gap. It also showed how the theory gives quantitative agreement for other experimentally determined quantities including the specific heat and penetration depth.(49)

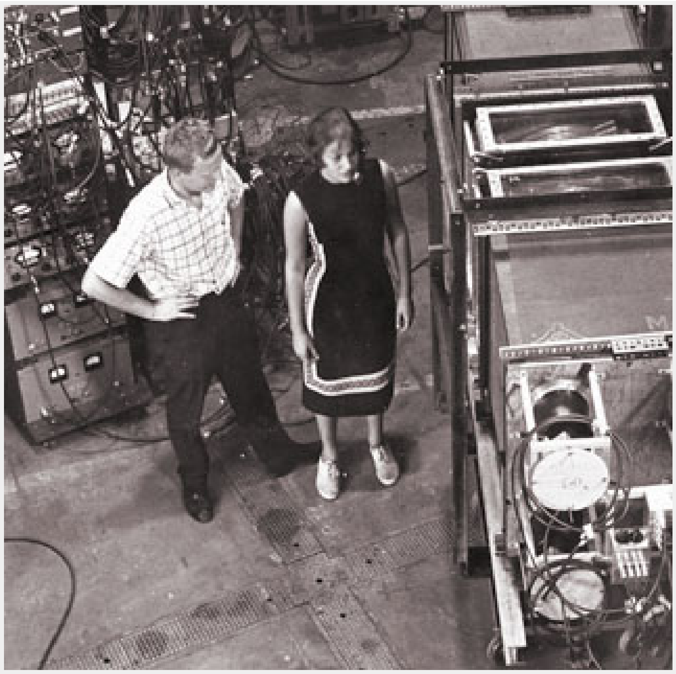
Many theorists met the theory with criticisms or questions. One objection concerned the apparent lack of gauge invariance. When Philip Anderson, Pines, Schrieffer, and others dealt with this issue, their work had an important by-product, the idea of "broken symmetry." One of the original objectors to BCS, Yoichiro Nambu, then introduced the notion into particle physics, where it helped build the Standard Model of particles and fields.(50)

Bardeen worried that the Swedish Academy of Sciences would keep to its tradition of not awarding any individual two Nobel Prizes in the same field, thus preventing Schrieffer and Cooper from receiving an award they had earned. But to his relief and joy, the Academy broke with precedent and honored all three with the 1972 Nobel Prize for Physics. Bardeen has the distinction of being the first person to win two Nobel Prizes in the same field.

Acknowledgements

Quin Luttinger, my doctoral advisor, did not share my passion for history of physics. Although he was amused by some of the details, he was painfully aware of the human complexities behind the scene and doubted anyone could get them right. For a physics graduate student, he considered history a frivolity and a distraction. As my advisor, it must have been frustrating to recognize that my interest in physics was driven by a desire to understand its history. Quin did not hesitate to apply this insight. He knew I would work harder to grasp the physics of any assignment that included history. So in 1963 he devised an exercise to teach me something about superconductivity. He called for a paper

discussing progress in the field during the last three decades. It was a great problem for me, one I thoroughly enjoyed. Unfortunately, when I recently stumbled across that old student work in a dusty carton, I was embarrased by its poor historical quality — which for Quin could only have confirmed his disapproval of history. I'd like to take this opportunity to resubmit a new draft, one more narrowly focused on the work of a single major actor and based on materials not available to me when I was a graduate student. While the account does not begin to represent the whole story, I think it is an improvement over what I did with that old assignment Quin was kind enough to design for me.



Lillian Hoddeson (with David Bartlett) inspecting the particle detector for his high-energy physics experiment at Columbia University's Nevis Laboratory in 1962, when she was a graduate student.

I would like to thank Gordon Baym for a helpful critical reading of an earlier version of this article, and the Richard Lounsbery Foundation, the Alfred P. Sloan Foundation, the Dibner Fund, and the University of Illinois' Center for Advanced Study for their generous support of this historical work.

Notes

1. Reprinted from *Journal of Statistical Physics* vol. **103**, nos. 3/4, 2001, pp. 625-640. This article draws in part on a manuscript by L. Hoddeson and V. Daitch, *True Genius: the Life and Physics of John Bardeen*, and Hoddeson, G. Baym, H. Schubert, S. Heims, "Chapter 8: Collective Phenomena," in *Out of the Crystal Maze: A History of Solid State Physics, 1900-1960*, edited by L. Hoddeson, E. Braun, J. Teichmann and S. Weart (New York: Oxford Univ. Press, 1992), pp. 489-616. An earlier

version of this article was the basis of my article "John Bardeen and the BCS Theory of Superconductivity," in *Materials Research Society Bulletin*, Vol. 24, No. 1, pp. 50-55 (Jan. 1999). BACK

2. L. Hoddeson, G. Baym, and M. Eckert, "The Development of the Quantum Mechanical Electron Theory of Metals, 1926-1933," in *Crystal Maze*, op. cit., pp. 88-181, see esp. pp. 140-153.BACK

3. A. Sommerfeld and H. Bethe, "Elektronentheorie der Metalle," in H. Geiger and K. Sheel, eds., *Handbuch der Physik*, ser. 2, (Berlin: Springer, 1933), *24*, pp. 333-622, quote on p. 555. BACK

4. See "Collective Phenomena," in *Crystal Maze*, p. 491. BACK

5. E. P. Wigner and J. Bardeen, "Theory of the Work Functions of Monovalent Metals," *Physical Review 48* (1935), pp. 84- 87; J. Bardeen, "Theory of the Work Function, II The Surface Double Layer," *Physical Review 49* (1936), pp. 653-663. J. Bardeen, "Reminiscences of Early Days in Solid State Physics," in *The Beginnings of Solid State Physics*, edited by Sir Nevill Mott (London: The Royal Society, 1980), pp. 77-83. BACK

6. See M. Riordan and L. Hoddeson, *Crystal Fire: the Birth of the Information Age* (New York: W. W. Norton, 1997). BACK

7. H. Jones, N.F. Mott, and H. W. B. Skinner, "A Theory of the Form of the X-ray Emission Bands and Metals, *Physical Review*, 45 (1934), 379-84. BACK

8. J. Bardeen, "Reminiscences," *op. cit*.BACK

9. Bardeen, "Reminiscences" *op. cit.*; F. London and H. London, "Supraleitung und Diamagnetismus," *Physica 2* (1935), pp. 341-354; F. London, "Macroscopic Interpretation of Supraconductivity," *Proc. Roy. Soc. A152* (1935), pp. 24-34. For a summary of research on superconductivity up to the Second World War, see P. F. Dahl, *Superconductivity: Its Historical Roots and Development from Mercury to the Ceramic Oxides* (New York: American Institute of Physics, 1992). For insight into the work of the London brothers, especially Fritz, see K. Gavroglu, *Fritz London: a scientific biography* (New York: Cambridge University Press, 1995). For further references see *Crystal Maze*, p. 588, n 21, pp. 141-53, and sec. IV. "Superconductivity, 1929-1933," in L. Hoddeson, G. Baym and M. Eckert, "The development of the quantum-mechanical theory of metals, 1928-1933," *Rev. Mod. Phys. 59* (1987),

287-327. BACK

10. London and London, "Supraleitung und Diamagnetismus," p. 348. BACK

11. D. Shoenberg, *Superconductivity* (Cambridge: Cambridge University Press, 1938). In the mid-50s, Bardeen would recommend the 1952 edition of this book to his postdoc Leon Cooper. BACK

12. Bardeen, "Theory of Superconductivity," Abstract, *Physical Review 59* (1941), 928. Bardeen would employ a similar approach in his work during the 1980s on charge density waves. BACK

13. J. Bardeen, "Reminiscences," *op. cit.*. BACK

14. This story is told in *Crystal Fire*, pp. 185-192. BACK

15. Brattain interview by C. Weiner, May 28, 1974, AIP, quoted in *Crystal Fire*, p. 190. BACK

16. The evidence came from work by a group of experimenters based in different labs including A. Brian Pippard, William Fairbank, Emanuel Maxwell, Paul Marcus, J. C. Daunt, Kurt Mendelssohn, B. Goodman, A. Brown, Mark Zemansky, Henry Boorse, W. Corak, Michael Tinkham, and R. E. Glover. BACK

17. Bardeen notes written between May 15 and May 18, 1950, Bardeen papers, Univ. of Illinois Archives; J. Bardeen, "Zero-Point Vibrations and Superconductivity," *Physical Review, 79* (1950), pp. 167-68; Chapter 8, *Out of the Crystal Maze*, pp. 549-50. BACK

18. H. Fröhlich, "Isotope Effect in Superconductivity," (letter) *Proceedings of the Physical Society (London) A63* (1950), p.778. BACK

19. Bardeen to Peierls, July 17, 1951, Peierls Papers, Oxford. BACK

20. F. Seitz interview by L. Hoddeson, V. Daitch, and I. Elichirigoity, April 22, 1993. BACK

21. Ibid. BACK

22. D. Pines interview with Lillian Hoddeson and Vicki Daitch, December 3, 1993, p. 8. BACK

23. D. Bohm interview by L. Hoddeson, May 8, 1981; see Crystal Maze p. 536. BACK

24. D. Pines interview by L. Hoddeson, August 13 and 16, 1981. BACK

25. J. Bardeen and D. Pines, "Electron-Phonon Interaction in Metals," *Physical Review 99* (1955), pp. 1140-1150. BACK

26. J. Bardeen, "Theory of Superconductivity. Theoretical Part," in *Handbuch der Physik* (Berlin: Springer, 1956), Vol. 15, 274-369. BACK

27. Bardeen, "Theory of Superconductivity. Theoretical Part." BACK

28. J. Bardeen, "History of Superconductivity Research," in *Impact of Basic Research on Technology*, edited by B. Kursonoglu and A. Perlmutter (New York: Plenum, 1973), 15-57. BACK

29. Schrieffer interview with J. Warnow and R. M. Williams, Sept. 26, 1974, hereafter cited as SWW. BACK

30. SWW. BACK

31. F. London, *Superfluids, vol. 1: Macroscopic theory of Superconductivity*. (New York: Wiley, 1950), esp. 142-155. BACK

32. SWW. BACK

33. L. Cooper, "Bound Electron Pairs in a Degenerate Fermi Gas," *Physical Review 104* (1956), 1189-1190. BACK

34. SSW. BACK

35. SWW. BACK

36. SWW. BACK

37. D. Lazarus interview with I. Elichirigoity, 24 February 1992. BACK

38. Betsy Bardeen Greytak, Bardeen family interview with L. Hoddeson and M. Riordan, March 15, 1992. BACK

39. SWW. BACK

40. SWW. BACK

41. SWW. BACK

42. SWW.BACK

43. SWW. BACK

44. D. Lazarus interview with I. Elichirigoity, 24 February 1992. BACK

45. J. Bardeen to S. A. Goudsmit, Feb. 15, 1957, Bardeen Papers, UIUC. BACK

46. J. Bardeen, L. N. Coooper, and J. R. Schrieffer, "Microscopic Theory of Superconductivity," *Physical Review 106* (1957), 162-164. BACK

47. Charles Slichter, John Bardeen Memorial talk, February 8, 1991. BACK

48. Among the first were R. W. Morse and H. V. Bohm at Brown and Glover and Tinkham at Berkeley BACK

49. J. Bardeen, L.N. Cooper, and J. R. Schrieffer, "Theory of Superconductivity," *Physical Review*, Vol. 108 (1957), 1175-1204. BACK

50. L. Brown, R. Brout, T. Y. Cao, Peter Higgs, and Y. Nambu, "Panel Session: Spontaneous Breaking of Symmetry, L. Hoddeson, L. M. Brown, M. Dresden, and M. Riordan (eds), *The Rise of the Standard Model: Particle Physics in the 1960s and 70s* (New York: Cambridge Univ. Press, 1997), pp. 478-522.

BACK

Further Reading and Links

Further reading

- Lillian Hoddeson and Vicki Daitch, *True Genius: The Life and Science of John Bardeen*. (Washington, DC: National Academies Press, 2002)
- Tom Shachtman, *Absolute Zero and the Conquest of Cold*. (Boston: Houghton Mifflin, 1999)

Links

- Wikipedia on superconductivity and its history.
- A Teacher's guide to superconductivity for high school students.
- Superconductors for beginners from superconductors.org, with many links and some more ways to explain the theory.
- Superconductivity finds practical use in "SQUID" (superconducting quantum interference device) detectors. Here are brief videos on their use in pinpointing brain problems and sports injuries.
- The History Center's Exhibit Hall - explore the history of physics, astronomy and geophysics.

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Schrieffer's recollections are from interviews conducted in 1974 and 1976 by Warnow and Robert M. Williams, in the AIP's Niels Bohr Library & Archives.

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Page 2 (Schrieffer)

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- (box) Tomonaga: Drawing by Geoffrey Cook, courtesy AIP Emilio Segrè Visual Archives, Weber Collection.
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- Cooper ca. 1972: AIP Emilio Segrè Visual Archives, gift of Leon Cooper.
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- Bardeen (walking): Courtesy University of Illinois at Urbana-Champaign.
- Cooper (with name badge): AIP Emilio Segrè Visual Archives, Segrè Collection.
- Schrieffer (bottom of page, left): AIP Emilio Segrè Visual Archives, *Physics Today* Collection.
- Bardeen (bottom of page, center): Courtesy Bell Telephone Laboratories.

Page 3 (Dance Analogy)

- Schrieffer: AIP Emilio Segrè Visual Archives, *Physics Today* Collection.
- Dancers in 1950's: Courtesy City of Burnside, http://www.burnside.sa.gov.au.

Page 4 (Bardeen)

- Bardeen photos: AIP Emilio Segrè Visual Archives, *Physics Today* Collection.

Page 5 (Hoddeson)

- Article reprinted from *Journal of Statistical Physics* vol. **103**, nos. 3/4, 2001, pp. 625-640.
- Lillian Hoddeson with David Bartlett: Courtesy Lillian Hoddeson.