REMEMBRANCE OF SUPERCONDUCTIVITY PAST

Leon N Cooper

Department of Physics and Institute for Brain and Neural Systems,
Brown University, Box 1843, Providence, RI 02912, USA
Leon_Cooper@Brown.edu

Recollections of events that led to BCS.

Memory fades. Is it possible to recreate the events that led to BCS? Can we recapture the great difficulty (even conjectured insolubility) superconductivity presented more than half a century ago? Perhaps not. But yellowed notes, like tea-soaked crumbs of madeleine, have awakened for me, sometimes with startling clarity, images and memories from that somewhat distant past.

My interaction with superconductivity began when I met John Bardeen in April or May of 1955 at the Institute for Advanced Study in Princeton. John was on the East Coast looking for a postdoc to work with him on superconductivity. He had written to several physicists, among them T. D. Lee and Frank Yang, asking if they knew of some young fellow, skilled in the latest and most fashionable theoretical techniques (at that time, Feynman Diagrams, renormalization methods, and functional integrals) who might be diverted from the true religion of high energy physics (as it was then known) and convinced that it would be of interest to work on a problem of some importance in solid-state.

As far as I can recall, it was the first time I had even heard of superconductivity. (Columbia, where I earned my Ph.D. did not, to my memory, offer a course in modern solid-state physics.) Superconductivity is mentioned in the second edition of Zemansky's *Heat and Thermodynamics* (the text that was assigned for the course in Thermodynamics taught by Henry Boorse that I took in my last semester at Columbia College), but I suspect those pages

were not assigned reading. Although John, no doubt, explained something about the problem, it is unlikely that I absorbed very much at the time.

The long and very imposing list of physicists (among them Bohr, Heisenberg and Feynman) who had tried or were trying their hand at superconductivity should have given me pause. Even Einstein, in 1922 — before the quantum theory of metals was in place — had attempted to construct a theory of superconductivity. Fortunately, I was unaware of these many unsuccessful attempts. So when John invited me to join him (he, somehow, neglected to mention these previous efforts), I decided to take the plunge.

In September of 1955, I arrived in Champaign-Urbana. I was assigned a wooden desk in John's office, the same desk that had been previously occupied by David Pines and later by Gerald Rickayzen. The atmosphere at Illinois was friendly and collegial. Down the hall, one could find Joe Weneser and Arnold Feingold who shared an office. Geoffrey Chew and Francis Low shared another. Among the experimentalists were John Wheatley and Charlie Slichter.

The department was led by Wheeler Loomis and Gerald Almy. Bob Schrieffer was one of John's graduate students. His desk was in an attic office above the third floor of the Physics building with a sign on the door that read "Institute for Retarded Studies." John had assigned, or Bob had chosen, superconductivity as a thesis problem. We would spend considerable time in that office commiserating with each other.

John's first suggestion was that I read Schoenberg's book. So, early in September of 1955, I began to consume Schoenberg and other books and articles on the facts and the theoretical attempts to solve the problem of superconductivity. I went through many of John's calculations, some classical attempts going back to Heisenberg and the arguments of H. and F. London. John at that time was writing an article for the *Encyclopedia of Physics* reviewing the theory of superconductivity; one of my assignments was to go over the arguments and to proofread the article. Also, sometime during that year, I gave a series of seminars on current methods in quantum field theory.

As Brian Pippard was to emphasize in a lecture he gave at Illinois, the facts of superconductivity appear to be simple. It seemed clear that superconductivity must be a fairly general phenomenon that does not depend on the details of metal structure, that there must be a qualitative change in the nature of the electron wave function that occurs in a wide variety of conditions in many metals and many crystal structures.

In many lectures we presented these simple facts more or less as follows:

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

And it has been recently discovered that the transition temperature varies with the mass of the ionic lattice as

$$\sqrt{M}\ T_c = \text{constant}$$

This is known as the isotope effect and indicates that the electron–phonon interaction is implicated in the transition into the superconducting state.

Fig. 5.
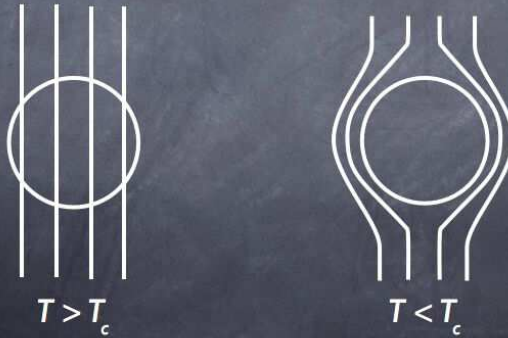
I became convinced early that the essence of the problem was an energy gap in the single particle excitation spectrum. This, of course, was the prevailing view at Illinois and seemed very reasonable — almost necessitated by the exponentially decreasing electronic specific heat near $T = 0$. John had shown that such a gap would be likely to yield the Meissner effect. His argument had been criticized on grounds of lack of gauge invariance, but John's opinion was th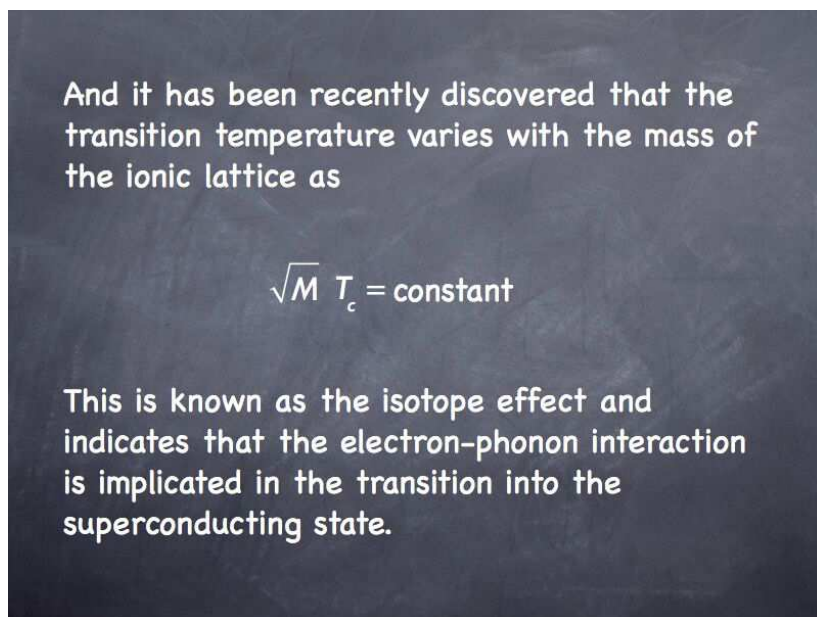at longitudinal excitations that would be of higher energy due to long-range Coulomb forces would take care of such objections. This also seemed reasonable. In any case, a single particle energy spectrum so greatly reduced from that of a normal metal would no doubt lead to a state with radically different properties.

So I started to attack the many-electron system to see how such an energy gap might come about. I attempted to sum various sets of diagrams: ladders, bubbles and many others. I tried low energy theorems, Breuckner's method, functional integrals and so on. After more attempts than I care to mention, during the fall of 1955, I was no longer feeling so clever. So, when John left for a trip in December, I put my calculations aside and tried to think things through anew.

Paradoxically, the problem seemed conceptually simple. The Sommerfeld–Bloch individual particle model (refined as Landau's Fermi liquid

theory) gives a fairly good description of normal metals but no hint of super-conductivity. Superconductivity was thought to occur due to an interaction between electrons. The Coulomb interaction contributes an average energy of 1 eV/atom. However, the average energy in the superconducting transition as estimated from T_c is about 10^{-8} eV/atom. Thus, the enormous qualitative changes that occur at the superconducting transition involve an energy change orders of magnitude smaller than the Coulomb energy. This huge energy difference contributed to the great difficulty of the problem. Also, the Coulomb interaction is present in all metals, but only some metals become superconductors.

After the work of Fröhlich, Bardeen and Pines, it was realized that an electron–electron interaction, due to phonon exchange, under some conditions could produce an attractive interaction between electrons near the Fermi surface. But, so far, there was no suggestion of how this would give rise to the superconducting state. In total frustration, I decided to shelve diagrams and functional integrals and step through the problem from the beginning.

This began to come together for me on a trip to New York for a family reunion and Christmas party. The train ride took about seventeen hours. Johnson once remarked that the prospect of hanging powerfully concentrates the intellect. He might have added seventeen sleepless hours on a train.

I began to pose the question in a highly simplified fashion, introducing complications one by one. Where does the problem become insoluble? Consider first N electrons in a container of volume Ω. This would give a Fermi sphere and, at least qualitatively, some of the properties of normal metals. Now turn on an attractive electron–electron interaction, even one that is very weak (so weak that interacting electrons would be limited to a small region about the Fermi surface). Immediately one is faced with an extraordinarily difficult problem in which vast numbers of degenerate quantum states interact with each other. What would the properties of the solutions be? The more I thought about this the clearer it became that, for all of the sophistication of various approaches, we did not yet know the answer to what seemed to be a fairly elementary question.

On my return to Illinois in January of 1956, I recall asking Joe Weneser, who worked in an office down the hall and who was always kind enough to listen sympathetically, "How do you solve problems in quantum mechanics for very degenerate systems?" Joe thought a while and said, "Why don't you look it up in Schiff?" Well I looked it up in Schiff and realized that Professor Yukawa, from whom I learned quantum mechanics at

Columbia, had already taught me what was there and that it didn't help very much.

So I embarked on an analysis of problems in quantum mechanics involving degeneracy. This led me to think about all types of matrices that might result from highly degenerate quantum systems. Piled on my desk, I may have had every book related to this subject in the physics library. I learned obscure matrix theorems, studied properties of stochastic and other special matrices and began a variety of theoretical experiments.

I was no longer doing what John expected me to do, and was unable to communicate what it was that I was doing. (I was not sure myself.) So, for a time we worked separately.

It was early in the Spring of 1956 that I realized that matrices, of a type that were certain to arise as sub-matrices of the Hamiltonian of the many-electron system I was considering, characteristically would have among their solutions states that split from the rest which were linear combinations of large numbers of the original states. These new states had properties qualitatively different from the original states of which they were formed. They were typically separated from the rest by NV (the number of states multiplied by the interaction energy) and therefore, since $N \sim \Omega$ and $V \sim \Omega^{-1}$, the separation energy would be independent of the volume. It was soon clear that this was a natural, almost inevitable, property of the degenerate systems I was considering.

Among the easiest such sub-matrices to pick, because of two body interactions, conservation of momentum, symmetry of the wave function, and all of the arguments that have been made many times since, were those that came about due to transitions of zero-spin electron pairs of given total momentum. I then focused my attention on such pairs.

I should note that I came about these pairs in my attempt to solve the degeneracy problem. Schafroth, Butler and Blatt had considered a system of charged electron pair molecules whose size was less than the average distance between them so they could be treated as a charged Bose–Einstein gas. They had shown that such a system displayed a Meissner effect and a critical temperature condensation. Schafroth, as I recall, gave a colloquium at Illinois presenting his ideas. I am not sure when that colloquium was given: whether it was before or after my own pair idea. However I was aware of Schafroth's argument by the time I submitted my letter to *Physical Review* in September 1956. As far as I was concerned, pairs spreading over distances of the order of 10^{-4} cm so that 10^6 to 10^7 occupy the same volume bore little relation to what Schafroth had proposed. These extended pairs might have

some Bose properties but it seemed unlikely that they would undergo a Bose
condensation in the Schafroth sense.

It is mildly ironic that at present we can go continuously from BCS
extended pairs to Schafroth molecule-like pairs in the BCS/Bose–Einstein
crossover. I thought, from time to time, that it would be extraordinarily
interesting if one could vary the strength of the interaction in order to ob-
serve this transition and had hoped for a while that the interaction would be
strong enough in high temperature superconductors so that this transition
might be seen as the temperature was lowered, a possibility I mentioned at
a roundtable discussion in Stockholm at the time of the Nobel award for
high T_c superconductivity. That has turned out, at least so far, not to be
possible, but the Feshbach resonance has made it possible to vary the inter-
action strength between fermions continuously so that we now can actually
see this transition. This is explained in more detail in Wolfgang Ketterle
and Gordon Baym's chapters in this volume.

A page from my notes (Fig. 6) from that period shows the pair solutions
as they first appeared to me. The last crossing on the left is the surprising
coherent state, split from the continuum by an energy proportional to $\hbar\omega$
multiplied by the exponential factor displaying the now well-known essential
singularity.

The energy of a ground state composed of such 'bound' pair states would
be proportional to $(\hbar\omega)^2$ and therefore inversely proportional to the isotopic
mass as expected. In addition, the exponential factor seemed to give a
natural explanation of why the transition energy into the superconducting
state was so small.

It seemed clear that if somehow the ground state could be composed of
such pairs, one would have a state with qualitatively different properties from
the normal state, with the ground state probably separated by an energy gap
from single particle excited states and thus likely, following arguments that
had already been given by Bardeen, to produce the qualitative properties of
superconductors.

In addition, all of this could be accomplished in what was close to a varia-
tional solution of the many-electron Schrödinger equation with demonstrably
lower energy than the state of independent particles from which the pairs
had been formed. I could show, using one stochastic matrix of which I was
particularly fond, a matrix with zero diagonal elements (zero kinetic energy
— what I called the strong coupling limit, $N(0)V \gg 1$) that $N(0)\hbar\omega$ non-
interacting pairs with average available phase space $N(0)\hbar\omega$ for transitions
(this would result if we chose pairs and pair states in a shell $\hbar\omega$ about the
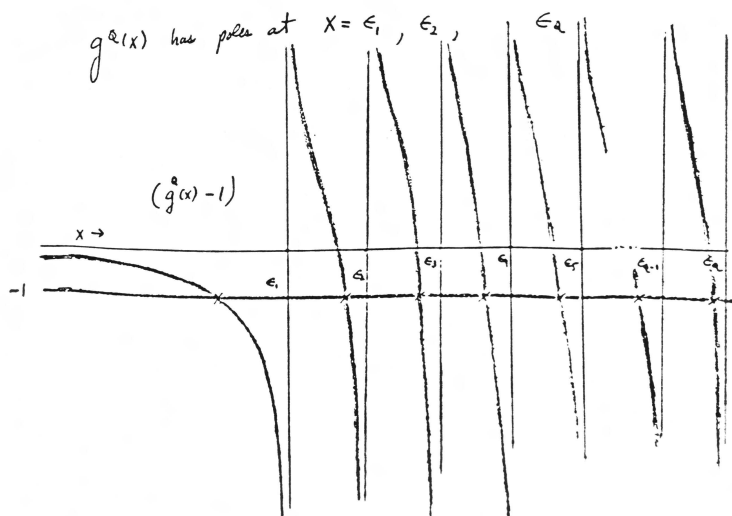
Fig. 6.   Handwritten notes from Spring 1956.

Fermi surface) would lead to a condensation energy $-N(0)^2(\hbar\omega)^2 V$. This, amusingly, turns out to be the BCS ground state energy difference in the same strong coupling limit. I did not know this last fact, of course, but these results were enough to convince me that this was the way to go. The solution of the next problem, to produce a wave function that incorporated the pairs and satisfied the exclusion principle with which one could calculate, took the rest of the year.

I was reasonably excited by these results but became aware, painfully, in the next months, how long the road still was. The way ahead was not as clear to everyone else as it appeared to me. It was not easy to see what the solution meant and it was not evident how to use it in calculations. I was asked over and over how one could have a bound state whose energy was larger than zero. (Going over my notes from that period, I read that

I was then calling it the "strange bound state"). At a seminar that I gave on the subject at Illinois some time during the spring of 1956, Francis Low asked how one could get a volume-independent energy shift if the interaction matrix elements went as Ω^{-1}.

In addition to trying to construct and calculate with a wave function composed of non-interacting pairs that satisfied the exclusion principle, I spent too much of the next six months answering questions of this type, showing that there could be developed a theory of Green's functions of electrons interacting above a Fermi sea, working on a pair algebra, making many attempts to prove that qualitative features revealed in the pair solutions would not be lost in the full many-body system. (I continued such efforts for several years afterward, long after everyone else regarded the problem as solved.) I thus spent much of my time engaged in missionary work: lecturing, trying to convince blank-eyed listeners and proving theorems. If I had been as mature then as I perhaps am today, I would have used this precious time to calculate and forge ahead. (As I now tell my graduate students: "Stop thinking and start working.")

Richard Feynman has related that at a meeting of the American Physical Society, likely sometime in 1956, he was chatting with Onsager when a wild-eyed young man came up to them and said that he had solved the problem of superconductivity. (There were, at the time, quite a few wild-eyed young — and not so young — men who were convinced they had solved the problem.) As Feynman relates, he could not understand what the young man was saying and concluded that the fellow was probably crazy. Onsager, on the other hand, according to Feynman, thought for a while and said, "I think he's right." Feynman believed that the wild-eyed young man was me. I am not at all sure whether or not this meeting actually occurred, but it might have. I certainly approached many people — trying not to be wild-eyed — while attempting to explain my ideas. But if it did happen, I sure wish Onsager had said something to me. I could have used the encouragement.

I spent the summer months of 1956 in California working at Ramo-Wooldridge (later Space Technology Laboratories). I do not remember what it was that I worked on there, but I did give a series of lectures on superconductivity — including, of course, the strange bound state.

Among other news items that greeted us in the Fall of 1956 when I returned to Illinois from California was a report of a talk by Feynman that was given at the Low Temperature Physics conference in Seattle. He concluded with the statement, "And when one works on it [the problem of

superconductivity], I warn you before you start, one comes up finally to a terrible shock; one discovers that he is too stupid to solve the problem."

I had been working on a rather long paper describing possible methods for putting the pairs into a many-body wave function. Hearing that Feynman was hard at work on superconductivity, jolted me to move faster. So, I hurriedly put together the letter that described my pair results. This was submitted to *Physical Review* on September 18, 1956.

I continued working trying to find a way to calculate with some kind of a many-body pair wave function, but went sideways as much as forward. I kept wandering back to the full many-body problem, the problem of the changing signs of general matrix elements — just the problem that restricting oneself to pair transitions would avoid.

Then came the news that John had won the Nobel Prize for his part in the invention of the transistor. Before John left for Stockholm, Bob, nervous about the lack of progress towards his thesis, spoke to him about possibly changing his thesis subject. John offered Bob his now famous advice, "Give it [superconductivity] another month . . . keep working . . . maybe something will happen . . ."

Then it happened. Bob finally made the crucial step of embodying the pairs in a wave function that satisfied the Pauli principle with which we could easily calculate — which is what we had been trying to do for the past six months. I believe he obtained this result in December 1956 or January 1957. It may have been while we were on the East Coast at the Stevens Institute of Technology for a conference on many body systems. If I recall correctly, Bob once told me that he worked this out on a New York City subway car. If so, he found more virtue in the New York subway system in this short sojourn than I discovered in all of the years that I rode it.

We met by accident in the Champaign airport, both on our way back from the East Coast. It was here that Bob first showed me his results. We were jumping up and down with excitement, oblivious to what others, who were in that airport on that cold winter evening, might have thought.

Bob's results finally convinced John. (As Bob said to me at the time: "We've turned the battleship around.") So one morning, late in January or early February of 1957, John asked me if I would agree to write a paper with Bob and him on the theory of superconductivity. I guess I must have agreed.

The next five months were a period of the most concentrated, intense and incredibly fruitful work I have experienced. We divided our efforts: Bob would focus on thermodynamic properties, I would focus on electro-dynamic

TABLE II. Matrix elements of single-particle scattering operator.

Wave functions[a]				Ground (+) or excited (−)		Energy difference $W_i - W_f$	Probability of initial state	Matrix elements $c_{k'\uparrow}{}^* c_{k\uparrow}$ or $c_{-k\downarrow}{}^* c_{k\uparrow}$	$c_{-k\downarrow}{}^* c_{-k'\downarrow}$ or $-c_{-k\downarrow}{}^* c_{k'\uparrow}$
Initial, Ψ_i $(k\uparrow, (k'\uparrow, -k\downarrow), -k'\downarrow)$		Final, Ψ_f $(k\uparrow, (k'\uparrow, -k\downarrow), -k'\downarrow)$		k	k'				
(a)				+	+	$E-E'$	$\frac{1}{2}s(1-s'-p')$	$[(1-h)(1-h')]^{\frac{1}{2}}$	$-(hh')^{\frac{1}{2}}$
$X0$	00	00	$X0$	−	−	$E'-E$	$\frac{1}{2}sp'$	$(hh')^{\frac{1}{2}}$	$-[(1-h)(1-h')]^{\frac{1}{2}}$
$X0$	XX	XX	$X0$	+	−	$E+E$	$\frac{1}{2}sp'$	$-[(1-h)h']^{\frac{1}{2}}$	$-[h(1-h')]^{\frac{1}{2}}$
				−	+	$-(E+E')$	$\frac{1}{2}s(1-s'-p')$	$-[h(1-h')]^{\frac{1}{2}}$	$-[(1-h)h']^{\frac{1}{2}}$
(b)				+	+	$E'-E$	$\frac{1}{2}s'(1-s-p)$	$(hh')^{\frac{1}{2}}$	$-[(1-h)(1-h')]^{\frac{1}{2}}$
XX	$0X$	$0X$	XX	−	−	$E-E'$	$\frac{1}{2}s'p$	$[(1-h)(1-h')]^{\frac{1}{2}}$	$-(hh')^{\frac{1}{2}}$
00	$0X$	$0X$	00	+	−	$-(E+E')$	$\frac{1}{2}s'(1-s-p)$	$[h(1-h')]^{\frac{1}{2}}$	$[h'(1-h)]^{\frac{1}{2}}$
				−	+	$E+E'$	$\frac{1}{2}s'p$	$[(1-h)h']^{\frac{1}{2}}$	$[h(1-h')]^{\frac{1}{2}}$
(c)				+	+	$E+E'$	$\frac{1}{4}ss'$	$[(1-h)h']^{\frac{1}{2}}$	$[h(1-h')]^{\frac{1}{2}}$
$X0$	$0X$	00	XX	−	−	$-(E+E')$	$\frac{1}{4}ss'$	$-[h(1-h')]^{\frac{1}{2}}$	$-[h'(1-h)]^{\frac{1}{2}}$
		XX	00	+	−	$E-E'$	$\frac{1}{4}ss'$	$[(1-h)(1-h')]^{\frac{1}{2}}$	$-(hh')^{\frac{1}{2}}$
				−	+	$E'-E$	$\frac{1}{4}ss'$	$-(hh')^{\frac{1}{2}}$	$[(1-h)(1-h')]^{\frac{1}{2}}$
(d)				+	+	$-(E+E')$	$(1-s-p)(1-s'-p')$	$[h(1-h')]^{\frac{1}{2}}$	$[(1-h)h']^{\frac{1}{2}}$
XX	00	$0X$	$X0$	−	−	$E+E'$	pp'	$-[(1-h)h']^{\frac{1}{2}}$	$-[h(1-h')]^{\frac{1}{2}}$
00	XX			+	−	$E'-E$	$(1-s-p)p'$	$-(hh')^{\frac{1}{2}}$	$[(1-h)(1-h')]^{\frac{1}{2}}$
				−	+	$E-E'$	$p(1-s'-p')$	$[(1-h)(1-h')]^{\frac{1}{2}}$	$-(hh')^{\frac{1}{2}}$

[a] For transitions which change spin, reverse designations of $(k'\uparrow, -k'\downarrow)$ in the initial and in the final states.

Fig. 7. Matrix elements as presented in *Physical Review*, 1957.

properties, with John focusing on transport and non-equilibrium properties. New results appeared almost every day. John's vast experience came to the fore. All of the calculations done previously with a normal metal (these John had in his head) could be turned on this new theory of superconductivity if one put in the appropriate matrix elements.

It is hard to believe that with the notation and techniques we used at that time that we could have ever obtained the correct results. As some may recall, we had excited singles, excited pairs, etc. How simple things seem now.

In less than a month, February 18, 1957, we communicated some of our earliest results in a letter to *Physical Review*. This letter describes the utility of the pairing approximation in solving the sign change problem that occurs for general many body matrix elements mentioned above and presents some very preliminary results; but there was enough to excite interest.

We communicated further results in two post-deadline papers to the American Physical Society meeting in March of 1957. John generously allowed Bob and me to present the papers. Bob made the trip to Philadelphia via New Hampshire (as I recall, to visit a friend) and I had the responsibility for bringing the slides for both of our talks to Philadelphia. It turned out that, for some reason I do not recall, Bob could not make it to Philadelphia on time. So, at the last minute, I had to give both of our talks. Although I missed having Bob there to support me, this was no particular problem;

either of us could have given either talk. The session audience overflowed the lecture room; the level of interest was incredible. Yet, I do not remember feeling overwhelmed. I was too inexperienced to fully appreciate what was happening, to appreciate how rare such an event was. In any case, even at that early presentation with only partial results, enthusiasm was enormous and acceptance was almost immediate.

In the course of working out the electromagnetic properties of our super-conductor, I had many discussions with John and Bob on questions related to gauge invariance. There were two problems:

The first arose due to the momentum dependence of the electron–electron interaction. This approximation, taken from my pair argument, simplified the calculations. We could easily have employed a momentum independent interaction, but this would have made the calculations much more cumber-some. Our feeling that this was not very important was confirmed by Phil Anderson who communicated his result sometime that spring and is mentioned in a note added in proof in our paper.

The second problem arose due to the fact that our calculation of the Meissner effect was done in the radiation gauge. This calculation was modeled on the one done previously by John assuming an energy gap in the single particle energy spectrum and was based on an early calculation by O. Klein. In a general gauge, we would have to include longitudinal excitations. John and I had talked about this on and off since I first looked over his calculations in the fall of 1955. His opinion, as mentioned above, was that the longitudinal excitations would be more or less unaltered from what they were in the normal metal and that due to the long-range Coulomb interaction would be the very high energy plasma modes and so would not affect our results. Further, since we all believed that our theory was gauge invariant, we could calculate in whatever gauge we chose. This view (also mentioned in the footnote added in proof) was corroborated by Anderson and almost at the same time, in an elegant fashion, using Green's function methods first introduced by Gor'kov and Nambu. Further, Gor'kov was able to derive the Ginsburg–Landau equation from his formulation of BCS and this equation is explicitly gauge invariant.

On my own, at this point, I might have gone off on a side-track to prove that our theory was gauge invariant. Here John's experience played a crucial role. He encouraged me not to divert my attention and to continue my calculations of the electrodynamics. This time I took his advice.

We had some real problems. In my calculations of the Meissner effect, I was obtaining a penetration depth a factor of two too small. John and I

went over these calculations and could not find anything wrong. We were almost ready to accept that, somehow, this might, in fact, be the case.

It was at a concert (probably in April of 1957) featuring the music of Harry Parch, brother of the *New Yorker* cartoonist Virgil Parch (a somewhat idiosyncratic composer who orchestrated his pieces with instruments of his own design — some of them huge wooden instruments — so that his music which, as I recall was not bad at all, was not playable except with the instruments shipped from his studio that required special training to play) thinking through the Meissner effect matrix elements for the thousandth time, I realized that between the initial and final state there existed another path, a path that did not occur in normal metal calculations. In the relevant limit the additional term would be of equal magnitude to the usual term but would possibly differ by a sign. The way we did the calculations then, there were about a half a dozen creation and annihilation operators to be manipulated. It was with some anxiety that I realized I could never, in the course of that concert, determine whether we would get the expected penetration depth or whether the Meissner effect would disappear. The family fortune was on the roulette table: double or nothing.

You will believe that I redid the calculations several times that weekend; by Monday morning there was no longer any doubt. Rushing to the office, possibly somewhat earlier than usual, I recall a vivid image: John in his chair, listening intently and absorbing every word. When I was finished I turned from the blackboard and said, "You see it comes out right." John was unusually loquacious that morning. He nodded in agreement and said: "Hmmm."

This new effect was immediately included in all of our calculations. It led to the now-famous coherence factors, with their surprising dependence on the behavior of the interaction under time reversal. Charlie Slichter, who was then doing the experiment with Chuck Hebel on the temperature dependence of the relaxation time for nuclear spins in superconductors, a quick study indeed, very soon was working along with us calculating the theoretical spin relaxation rate.

Somewhat later, Bob Morse communicated the results he had obtained with Bohm at Brown University on the temperature dependence of ultrasonic attenuation in superconductors. Nuclear spin relaxation and ultrasonic attenuation were compared in a note we added in proof. Our theory explained the remarkable and counterintuitive increase in the relaxation rate of the nuclear spins, which contrasted markedly with the unexpected sharp decrease in ultrasonic attenuation just below the transition temperature (see

Figs. 16 and 18 in Charlie Slichter's chapter in this volume). As we remarked in the note, this provided experimental confirmation of the effect of the coherence factors.

By July, exhausted after months of non-stop calculation, we knew that we had solved the problem. Our paper was submitted to *Physical Review* on July 8, 1957.

With a few exceptions, acceptance of our theory was almost immediate. There were some complaints (as expected, supposed lack of gauge invariance was one). As I have said, we had discussed this but, wisely, had not spent too much time on it.

There were also some regrets. One rather well known low temperature physicist (possibly hoping for a new law of nature) expressed his disappointment that "... such a striking phenomenon as superconductivity [was] ... nothing more exciting than a footling small interaction between atoms and lattice vibrations."

Sometime the next fall I received a preprint from Valatin, describing his method for constructing a much easier to use set of orthogonal excitations. What a relief. Gone were excited singles and pairs. We now could calculate more easily and much more rapidly. Some time later I received a preprint from Bogoliubov who, building on his previous liquid helium work, had arrived at the same excitations. And very soon thereafter I learned of the Green's function methods of Gor'kov and Nambu which made issues such as gauge invariance much more transparent. This is discussed in Nambu's chapter in this volume and the part of the story that occurred in the Soviet Union is related in Gor'kov's chapter.

As this volume makes evident, the simple facts of superconductivity are no longer so simple. Among other varieties, we now have superconductors that are resistive, that are gapless, and that display no Meissner effect. Superconductors now come in more flavors than Baskin-Robbins ice cream. We could not, in 1957, have been aware of all of these variations. But, more important, had we known all of this and worried about it, we might never have constructed anything. This is an example of what I believe is a general principle for the investigation of difficult scientific questions. One must make things as simple as possible ("but no simpler" as Einstein, supposedly, once said). In the case of superconductors, the key was to extract at least one qualitative feature of the superconducting state that was strikingly different from the normal state. For us it was the energy gap. By concentrating our attention on how such a gap could arise in a degenerate system of interacting electrons, we discovered the new ground state. An ironic

consequence of the full theory is that a variation exists in which the energy gap may disappear.

Among the consequences of our theory is the startling non-zero vacuum expectation value of two-electron creation or annihilation operators: what is now known as spontaneously broken gauge symmetry. It has become fashionable, in some circles, to call superconductivity a manifestation of broken symmetry and to assert that once gauge symmetry is broken the properties of superconductors follow. For example, Steve Weinberg writes in this volume that "A superconductor of any kind is nothing more or less than a material in which . . . electromagnetic gauge is spontaneously broken . . . All of the dramatic exact properties of superconductors . . . follow from the assumption that electromagnetic gauge invariance is broken . . . with no need to inquire into the mechanism by which the symmetry is broken." This is not — strictly speaking — true, since broken gauge symmetry might lead to molecule-like pairs and a Bose–Einstein rather than a BCS condensation. But, more important, such statements turn history on its head. Although we would not have used these words in 1957, we were aware that what is now called broken gauge symmetry would, under some circumstances (an energy gap or an order parameter), lead to many of the qualitative features of superconductivity. This had been well-known since the Gorter–Casimir two fluid model and the work of the Londons. The major problem was to show how an energy gap, an order parameter or "condensation in momentum space" could come about — to show how, in modern terms, gauge symmetry could be broken spontaneously. We demonstrated — I believe for the first time, and again using current language — how the gauge-invariant symmetry of the Lagrangian could be spontaneously broken due to interactions which were themselves gauge invariant. It was as though we set out to build a car and, along the way, invented the wheel.

It is true that in 1957 we never mentioned this very important symmetry breaking property of our theory, or that it was analogous to the symmetry breaking that occurs in the ferromagnetic transition. Though even I was aware of the properties of the ferromagnetic transition (not to mention Bob or John), we never explicitly pointed out the connection. Perhaps, as consolation for our oversight, we might remind ourselves that the great James Clerk Maxwell, to my knowledge, never mentioned that his equations were invariant under Lorentz or gauge transformations.

In the early sixties I returned for a while to the effort to prove that the pairing wave function was, in fact, a good solution of the many-body Hamiltonian, that the terms in the interaction we had omitted would not

Fig. 8.   Bardeen, Cooper and Schrieffer (BCS). Courtesy: AIP Emilio Segrè Visual Archives.

change the qualitative results. These efforts, I must say, met with very limited success. I recall that after a seminar on this subject, a young man asked me, "Why are you interested in this problem? Everyone knows there is a pair condensation."

How quickly impossibly difficult had become obvious.

This article is based in part on an account I gave of the origins of the BCS Theory at the University of Illinois at Urbana-Champaign's BCS@50 Conference, as well as various talks celebrating the 50th anniversary of the BCS Theory.