Like many of the previous speakers, I begin by recalling when the full beauty of the BCS theory struck me. Bob, John and Leon were too busy to cover all the places where they were wanted as speakers, so we at Bell and Princeton had to make do with a substitute, David Pines, whose colloquium in spring 1987, at Princeton, we drove down from Bell to hear. I was in a car with Harry Suhl and Larry Walker, and it is my recollection that our discussion of the algebraic properties of the Cooper pairs began on the drive home, which discussion developed into the pseudospin representation of BCS which I used in later work.

As an inveterate conclusion-jumper, I confess that I never had a moment's doubt that the theory was correct. Perhaps part of the reason was that I had not worked at all in the field before, so had no prejudices. My certainty was only strengthened by the fact that Chuck Hebel made a recruiting visit to Bell that summer, with news of the first striking experimental prediction of the new theory, the "Hebel-Slichter peak". Thus, when several of our visitors that busy summer expressed serious doubts I leapt automatically to the defense.

The doubts were expressed in papers by the Schafroth group in Australia as concerns about gauge invariance. They were taken very seriously by people as eminent as Gregor Wentzel and Walter Kohn.

As derived in the original paper, the London equation which expresses the Meissner effect comes out simply as

$J = KA$, with A being the vector potential and J the current; while of course the real London equation is the curl of that,

$$\nabla \times J = K \nabla \times A = KH.$$
which is gauge invariant. A itself, of course, is not, the addition of the gradient of any scalar to it leaves physics unchanged.

The way in which equation 1 is derived in the BCS paper is frustratingly indirect but, in fact, I have never seen a real improvement on it—I know, because in recent work I have had to go right back to understanding the electromagnetic response of a superconductor. What one does is to observe that there are two terms in the response

$$J = \sum_{electrons} e(p - eA/c)/m$$

the direct acceleration by the field A,

which seems always to give $J = (ne^2/m)A$. When we add to it the perturbative effect of the field on the wave functions. it cancels this term exactly in a normal metal. But the instant a gap opens up, the perturbative part drops out, and the electrons can accelerate as if free. But the question is, why does this never happen for any other kind of i gap? All previous "gap" theories had failed this test.

My first answer to these questions was embarrassingly clumsy, but, I believe, essentially correct. As the Russians remarked (after Gor'kov derived the explicitly gauge-invariant Ginzburg-Landau theory from BCS) the symmetry which is "broken" by the BCS theory is not only Gauge but simple Galilean invariance: The BCS ground state is made up of explicitly zero-momentum pairs, hence picks out a specific coordinate frame as special. But the translational symmetry remains valid for the original Hamiltonian, and is restored by the existence of what came to be called "Goldstone modes", whose frequency goes to zero at long wavelengths, and whose zero-point amplitude diverges. But, as I pointed out, such modes are purely longitudinal density waves, and the transverse Meissner response is unaffected by them. It is these modes which respond when a true gauge transformation is carried out.

This answer to the dilemma was developed independently and more or less simultaneously by myself, Y Nambu, and Bogoliubov and Shirkov in Russia—all of whom were proudly quoted by the three heroes of BCS. There was only one problem—that solution is wrong! There is a real energy gap and there are no Goldstone bosons!

I should not brag too much here—the Schafroth group had in fact discussed the similar problem of the charged Bose gas in 1955, and got it right. But of the three widely quoted "solutions" to the gauge problem only one—mine—was correct.

What happens is that the above solution ignores the effects of the long range and great strength of the Coulomb interaction, which is such that there can be no true charged density wave in a superconductor, since no wave can propagate below the plasma frequency of several ev. Solving the equations of motion using the random-phase approximation, I showed that the Goldstone mode became the longitudinal plasmon. The transverse behavior—the Meissner effect—was unaffected by any of this.

This discovery did not make a big stir at the time. But I was concerned that I should impart it to the Bogoliubov group, and by coincidence I was invited, courtesy of the thawing US –Soviet relations of the time as well as of a bit of manouvering by Charlie Kittel, to a meeting in Moscow in December 1958. The meeting was an excuse for the visit, not too serious, and the main scientific contacts I made were with the Landau group, who—especially Abrikosov—were very hospitable. In contrast, for nearly two weeks I tried to make some contact with Bogoliubov through our Intourist (read KGB) guides, and failed completely. The Intourist-KGB's were infinitely accommodating, taking us to ancient monasteries, helping Bernd Matthias shop for an icon, and one day treating me to a palatial lunch where my political opinions were tested. But the best they could do in terms of our meeting scientists outside the Landau group was a strictly touristic visit to the

accelerator lab at Dubna. While we were in the reception lounge listening to a presentation about what a great achievement Dubna was, a slight, young-appearing Russian sneaked in and led me down the hall to a classroom. He introduced himself as Shirkov, Bogoliubov's collaborator, and for perhaps 15 minutes we talked at a blackboard, and since he seemed very quick I believe he understood what I had to say to him. After that brief interlude we were discovered by a group of burly fellows with shiny black shoes, and hustled off in opposite directions. Otherwise the main interest of that trip to Dubna was the view of the gigantic Stalin statues along the Moscow canal.

In summer 1961 Nambu and his colleague introduced BCS and the world of particle physics to each other, as he has told you, and he visited us at Bell to explain it to us in the theoretical group. His paper makes elegant use of the Goldstone boson of a BCS model for the nucleons as a description of the pi meson in the presence of nearly chiral symmetry. But it wasn't until the next spring, during my year in Cambridge, that I began to hear gossip about the desperate search for theories with spontaneously broken symmetry which did NOT leave the world cluttered up with massless, unobserved Goldstone bosons. (Incidentally, Steve Weinberg was at the Cavendish that year, too, but I don't remember actually talking physics with him)This left me a bit puzzled, in that as I had shown, the original BCS theory had no Goldstone boson and a true energy (mass) gap. By this time, there was also plenty of evidence to this effect experimentally. So I began thinking along an amusing line: what would an animalcule, who lived in a superconductor and couldn't get out, think were the laws of physics? He might well see charge conservation, but no accompanying massless gauge field.

When I returned to Bell it happened that John Klauder was hosting John G Taylor for the summer, and between the two of them they filled me in on some of the dilemmas in field theory. I realized that the particle physicists really didn't understand the physics of BCS

theory, and so I wrote a short paper* explaining the real physics of the Meissner effect., and that the gap could be empty of zero-mass particles, in the nearest language I could manage to "particlese". Therefore it had extensive references to an incomprehensible (but correct, actually) Schwinger paper of 1962, which in very general and vague terms had already explained that zero mass was not a problem.

*PLASMONS, GAUGE INVARIANCE AND MASS, PR130,430,1963.(parenthetically, the next paper chronologically, in my bibliography is the discovery paper for the Josephson effect.)

Naturally, this paper did not exactly swamp the citation indexes—but it did the necessary, in that it caught the attention of one particle theorist, a very nice Scotsman named Peter Higgs. He managed to translate it even better, and his language eventually caught the attention of John Ward and the three Nobelists of the electroweak theory.

Perhaps as a postscript I could trace the history of the "Higgs" boson. As you will recognize from the above, none of these theories—not Schwinger, not Higgs I, not P, GI&M, is really a theory of the Higgs boson—they are theories of the massive vector bosons which become the Z and the W mesons. In my original RPA paper, I got lucky and did mention the one among the "Anderson-Bogoliubov modes" which was to turn out to be the Higgson—i e the Cooper pair amplitude mode. But that was strictly in passing and I certainly made nothing of it. MUCH later, Littlewood and Varma actually identified, in the spectrum of a real superconductor, a Raman-active resonance which they showed is indeed the Cooper pair amplitude mode, the equivalent of the Higgs particle.

I had hoped to have time also to describe a bit of the prehistory of "non-BCS" superconductivity, superconductivity for which the

cooper pair is not in the lowest singly-degenerate state. Again, my part of this at least was much stimulated by the remarkably open nature of our group during those few years. In this case the stimulus came from another irregular visitor, in this case from the nuclear physics world, Keith Brueckner.

In 1958 I had acquired a brilliant French student as a legacy from david Pines, Pierre Morel. One of the problems we were working on together—not at a very rapid rate, to be sure—was a notion I had had of a BCS theory with anisotropic Cooper pairs. I happened to mention this to Keith and he immediately suggested that it could apply to He-3—which substance was news to me, it had only become available as the decay product of Tritium produced for the h-bomb, and it existed only at Los Alamos, which was out of bounds for a determined civilian like me. It was characteristic of Keith that only a few weeks later came a preprint on the subject by Keith and his student Soda. Keith, I felt, was more or less welcome to my idea—it in fact turned out that several others, epecially Pitaevskii in Russia, had independently thought of it—but I was ferocious in defence of Pierre's thesis topic, and Keith was quite happy to submit his paper as Brueckner, Soda, and the two of us. Keith's estimates of Tc were wildly wrong, for reasons understood later, but they were the first. Pierre and I went on to discuss the physics in much more detail in later papers and pointed out a number of the crucial points—missing others, as well. But we did make the really crucial one, that this kind of superconductivity would usually be very sensitive to impurities.

As more and more exotic and unusual superconductors begin to be uncovered, it seems likely that this strange type of state actually may turn out to be even more common in Nature than proper BCS! I will leave you with that wild conjecture to discuss.