A Conversation About Solid State Physics

Hans A Bethe and N David Mermin

Hans Bethe reminisces about the first applications of quantum mechanics to the theory of solids in the late 1920s and early 1930s.

What follows is the transcript of a half hour videotape, recorded on 25 February 2003, in which I interview my Cornell University colleague Hans Bethe about the early days of solid state physics and the role he played in its development.

*Mermin**

Bethe and Mermin

MERMIN: When you arrived in Munich, Sommerfeld was just working out his quantum theory of free electrons.

BETHE: That's right. When I arrived in Munich it was 1926, and the big thing was that quantum theory for the first time was on a firm basis from Heisenberg and especially from Schrödinger. Sommerfeld loved Schrödinger's wave mechanics because that was the sort of thing, differential equations with eigenvalues, that Sommerfeld had loved for a decade or two. And he gave a special course for graduate students on the differential equations of physics, and Schrödinger fitted right in. So we all were asked to take one section of Schrödinger and give a seminar on it. So that's how I learned some of Schrödinger's perturbation theory, because that was the section I was to give in the seminar.

MERMIN: But then, when he came to applying it to solids, ...

---

* *David Mermin is a distinguished solid state physicist and a colleague of Hans Bethe at Cornell. He is well known as a commentator and a teacher of physics.*

BETHE: Now in '27, a year later, he applied it to solids.

MERMIN: And at that point, he decided the electrons could be treated as free.

BETHE: Well, he was very much impressed by papers by Drude, early in the century, in which Drude had used classical mechanics and free electrons, and Drude just assumed electrons must be free – if they are to conduct they'd better be free. And Sommerfeld assumed likewise in quantum mechanics. Neither of them had proved that the electrons indeed were free. That was only done by Bloch about a year later, and only in quantum mechanics. You cannot prove free electrons in classical mechanics. But in quantum mechanics, Bloch proved that the wave function of an electron was a plane wave multiplied by a function which was periodic with the period of the lattice, and Bloch, in a way, was the fundamental origin of the free electron picture.

MERMIN: So Bloch, in a sense, vindicated Sommerfeld.

BETHE: Right.

MERMIN: Were you suspicious of Sommerfeld until Bloch?

BETHE: Not at all.

MERMIN: Why not?

BETHE: I came from a slightly different direction. I was told by Sommerfeld, for my thesis, to make a theory of the experiments of Davisson and Germer which proved the wave nature of electrons, and proved that electrons behaved very much like x-rays. And so it was obvious to me that electrons were free.

MERMIN: Was Sommerfeld worried about, for example, the anomalous Hall effect, which is a real challenge for free electrons?

BETHE: I don't believe so. But he was happy to see it explained.

MERMIN: I found a nice quote from Peierls about Sommerfeld in the context of free electrons. He said, "It was characteristic of Sommerfeld's positive attitude that one learned more about the successful solution of difficulties than about the mysteries that remained. He was completely fair in listing the contradictions. It was just a matter of emphasis."

BETHE: This is a very good description. Peierls and I were simultaneously graduate students with Sommerfeld, and Sommerfeld indeed emphasized the positive.

MERMIN: Do you remember what Sommerfeld's reaction was to Bloch?

BETHE: I don't remember. I don't remember, in fact, that Sommerfeld was worried about electrons being free. But to me, Bloch was essential.

MERMIN: One of the things that always struck me about Bloch's work was that, first of all, he showed that electrons in a periodic lattice behaved like free particles with an altered energy-momentum relation.

BETHE: Yes.

MERMIN: And then, immediately, people studied the response of such electrons to applied electric and magnetic fields by treating them as classical particles with that altered energy-momentum relation. Now showing that you can do that is not so simple.

BETHE: Yes.

MERMIN: Was this controversial? Or was it regarded as obvious?

BETHE: Well, physics was simple in those days. And it was obvious you needed free electrons to have conductivity.

MERMIN: Right. Once Bloch had shown that electrons did propagate freely, it was clear there were free electrons. But then you want to know how they behave in magnetic fields, for example, and Peierls assumed that they would be acted on by the ordinary Lorentz force, the only difference being the velocity was not proportional to the wave vector.

BETHE: Yes.

MERMIN: And that in a sense is a big leap from Bloch's theory.

BETHE: It may be a big leap, but at that time it seemed obvious: Electrons are acted on by a Lorentz force.

MERMIN: So this funny use of quantum mechanics to get the altered energy-momentum relation and then you start being classical again, was fine.
BETHE: Right.

MERMIN: And of course it worked.

BETHE: It worked. It seemed obvious that that's the way to do it.

MERMIN: What about the question of electron-electron interactions?

BETHE: I understand that the Russians found that very difficult. But to me it seemed quite obvious, because after all, it happened in the atom, and we had the Hartree–Fock theory of electrons in an atom, so we obviously would have a Hartree–Fock theory of electrons in a crystal. And I think it is very strange that the Russians found it difficult, because, after all, Fock had told them how to use electron-electron interaction. Hartree already had told them. I found it quite natural to treat electrons by the Hartree–Fock approximation, and the Hartree–Fock approximation simply said what you said a moment ago, that they behave like ordinary electrons with a different relation between energy and momentum. And it was obvious that the Fermi surface had to be a sharp surface. Otherwise you wouldn't get the Fermi distribution.

MERMIN: Over the weekend, I found a statement from Bloch, who said it worried him, the neglect of electron-electron interactions. But he felt it was worth seeing how far you could get in a one-electron picture.

BETHE: Very good. It didn't worry Peierls and me. Peierls and I worked very closely together. We had been graduate students in Munich simultaneously, and we stayed together by letters and were friends. And we didn't find any difficulty.

MERMIN: Of course, eventually it did become important for questions like metal-insulator transitions.

BETHE: It did indeed, and the great person there is Neville Mott. But we did know that there was a Fermi surface in the reciprocal lattice and that Fermi surface would not necessarily fit into an elementary cell of the reciprocal lattice.

MERMIN: That brings us to the question of the distinction between metals and insulators.

BETHE: Right. It was perfectly clear to Peierls and me that insulators were substances in which the band was completely filled.

MERMIN: Apparently it wasn't obvious to Bloch as late as 1931, because he says in a 1980 retrospective article in *Proc. Roy. Soc.* that he did not understand the band theoretic basis for the distinction between metals and insulators, until it was pointed out by A H Wilson in 1931. And he actually refers to "my misconception of the essential difference between insulators and conductors, later pointed out by A. H. Wilson."

BETHE: Well, Peierls and I believed that we understood it perfectly well. The insulator had completely filled bands, and on the other hand, the elementary cell of the reciprocal lattice in general did not coincide with the Fermi surface, and that made it possible to have divalent metals where part of the elementary cell was empty and part of the next band was filled.

MERMIN: Apparently that had not occurred to Bloch as late as 1931, because both Bloch and Wilson independently tell this story of Bloch not believing Wilson when he made this point. Apparently Bloch believed that it was a continuous difference. It was a question of the tunneling probabilities getting very small.

BETHE: So it seems. We did not correspond with Bloch in those days. But Peierls and I were very impressed by Brillouin's reciprocal lattice, and there are pictures in my article with Sommerfeld which show that by 1933, certainly, we had understood it completely.

MERMIN: Peierls in that same issue of the *Proceedings of the Royal Society* asks himself the same question: When did we realize the band theoretic basis for the distinction. And he says that in his 1929 Hall effect paper, there is the statement that a filled band will not conduct. But he does not expand that into a discussion of the fundamental difference between metals and insulators.

BETHE: That's correct.

MERMIN: My guess is that the reason he didn't was that he was very strongly focused in that paper on conductors.

BETHE: Yes.

MERMIN: And then he went on to talk about nearly free electron systems, the opposite of tight binding, where of course everything is metallic. So it sounds as if in a sense Wilson may have been the first person explicitly to raise that question

BETHE: I think that's correct.
MERMIN: and say what is the difference.

BETHE: Yes. But it was clear to Peierls and me.

MERMIN: One of the things Peierls did do explicitly involving insulators was the theory of thermal conductivity, and the importance of umklapp processes.

BETHE: Umklapp processes were important already for metals, because after all, when you have resistance, you have an interaction of an electron with a lattice wave, and the sum of the wave vector of the electron and the lattice wave could very easily go beyond the limit of the band of the Brillouin zone. And therefore you needed umklapp processes already in metals.

MERMIN: To have a resistance?

BETHE: To have a resistance. Especially at lower temperature, but even at room temperature, the Fermi surface of sodium is almost a sphere which doesn't fit into the reciprocal lattice. So if you had resistance, you had to go on one side of the Fermi sphere to the other side, and you could do that only by a lattice wave of considerable wavenumber. So umklapp processes were immediately needed already in metallic resistance. Therefore they emphasized it.

MERMIN: As I remember, Peierls also made the point – maybe it's the same point in a different language – that without umklapps, the electrons and the phonons would come to equilibrium together but in a moving frame.

BETHE: Correct. Exactly right.

MERMIN: Which Bloch had avoided in his early paper by putting in by hand that the phonons were already in equilibrium in the stationary frame. So the notion of umklapps was also not controversial; people accepted it.

BETHE: Yes. But the application to heat conduction in insulators was extremely important and gave a very remarkable result. Insulators conduct heat better than metal at low temperature.

MERMIN: I don't think that was actually observed for another couple of decades.

BETHE: You're right.
MERMIN: I think it wasn't until the early '50s that people made crystals pure enough.

BETHE: It was observed only 20 years later because only then could people make pure enough substances so that umklapp processes had any meaning.

MERMIN: In fact I found an amusing remark of Peierls to the effect that he had failed to warn people that the crystals had to be not only chemically pure but isotopically pure as well, and that apparently caused some confusion.

BETHE: I don't remember that.

MERMIN: Pauli is said to have been fond of saying that he invented solid-state physics.

BETHE: Pauli in fact wrote the first paper in which quantum mechanics was applied to crystals, and he explained paramagnetism of alkalis by these means. And that was important. It may even have influenced Sommerfeld, but I don't know. The important part of solid state physics is that you have the whole solid state and not just one atom in the solid state. And Bloch had only considered one alkali atom in the solid state.

MERMIN: But aside from paramagnetism, did Pauli do any other work?

BETHE: No, and he despised solid-state theory. In fact, he said he had wanted to take me as an assistant, but I worked on solid state and he didn't like that.

MERMIN: I wonder why he didn't.

BETHE: I do not know. And I never asked him.

MERMIN: In thinking about this conversation, I'm ashamed to say for the first time in my life I actually looked at your long article with Sommerfeld on the electron theory of metals. And I was struck by how big it is. It makes a 300-page, small-print book. And the other thing that struck me was how much duplication there is between your 1933 book and my 1976 book with Neil Ashcroft on solid-state physics – how much was already known in 1933.

BETHE: Shows the stability of the theory.

MERMIN: Yes. The other thing that struck me was that it took me and Neil eight years to write our book, and I believe it took you.

BETHE: It took me one year.
MERMIN: Yes. And the question is how? Why did it only take you one year to write such a massive. ?

BETHE: Well, there was a lot of it known at the time, and it was very close to our daily work.

MERMIN: But that book is almost 300 pages of very small print with many figures, and I presume you were doing other things at the time.

BETHE: Yes, I was. By that time my real interest was nuclear physics, which started with Chadwick's discovery of the neutron in '32.

MERMIN: Are you the kind of writer who can just write things once and it's perfect?

BETHE: I was at that time. A friend of mine, George Placzek, claimed that I just had two stacks of papers – one was white and the other I had written on – and I wrote continuously and turned it over to the other pile which had the finished product. Fermi worked the same way.

MERMIN: And in addition, I assume you didn't sleep very much?

BETHE: Oh, I slept a great deal.

MERMIN: Really? I'm very impressed.

BETHE: Eight hours a day.

MERMIN: I noticed that there's a footnote to chapter 1 by Sommerfeld saying that he wrote the first chapter on free electrons, which is 35 pages or so, and that you wrote the rest of the book. Nevertheless, the book is Sommerfeld and Bethe, and not Bethe and Sommerfeld.

BETHE: Well, Sommerfeld was a famous physicist who could do almost anything he wanted, and he was on very good terms and very well regarded by foreign physicists. They all invited him, he was lecturer at Ann Arbor in the summer school, he got the premium of the American Association for the Teaching of Physics.

MERMIN: For those wonderful books of his.

BETHE: Yes. And I was very proud 20 years later when I got it. Sommerfeld was known all over the world, so in order to show that this was a serious book, Sommerfeld had to come first.

MERMIN: I think things have changed somewhat. I think these days the student's name tends to come first, if at all possible, if it's a collaboration.

BETHE: To advertise him.

MERMIN: Exactly. It's been terrific talking to you about this. I've learned a lot.

BETHE: It was fun.

MERMIN: It was indeed.

It stands to the everlasting credit of science that by acting on the human mind it has overcome man's insecurity before himself and before nature.

Albert Einstein

Not until the creation and maintenance of decent conditions of life for all people are recognized and accepted as a common obligation of all people and all countries - not until then shall we, with a certain degree of justification, be able to speak of humankind as civilized.

Albert Einstein