JOHN BARDEEN

This is an interview on 29 May 1984 with John Bardeen at the University of Illinois. The interviewer is William Aspray.

Aspray: Would you begin by telling me how you came to Princeton?

Bardeen: I was interested in math as a student in high school, and even earlier. In college I studied electrical engineering, because I was told that that requires a lot of mathematics. I did not think there was much of a career in mathematics or physics or some other academic area. My father was a dean at the medical school at Wisconsin, and I didn't think I wanted to follow his footsteps into an academic career. I took, though, a lot of extra courses in mathematics and in physics while I was a student in electrical engineering. I think I stayed on for two years of graduate work. I was in the class of 1927, but did not graduate until a year later because I stayed out for a term and did not have some of the required courses. Between my junior and senior years, in the summer of 1926, I worked for Western Electric in Chicago. I liked the work, so I stayed on for another semester and thus missed some of the required courses to graduate in electrical engineering. I graduated at the end of the academic year '27-'28.

That fall, 1928, I stayed on as a graduate student. I took a course from J.H. Van Vleck, who had recently arrived from the University of Minnesota, in quantum theory. I had my introduction to quantum theory even earlier, from a very interesting course with Peter Debye, who was a visiting professor. Wisconsin was very strong in applied mathematics, and I took many courses in boundary-value problems, differential equations, and things of that sort—19th-century applied mathematics.

By the time I left in 1930 to work with the Gulf Oil Company in geophysics, I'd taken most of the graduate courses that were offered in mathematics and many of those offered in physics, including the lectures Van Vleck gave in quantum theory. This was one of the first courses in quantum theory given in this country. I had six-week courses of lectures from Paul Dirac, who was a visitor in the spring of 1929. There were other distinguished visitors from Europe, so Wisconsin was by no means isolated from the great discoveries in quantum physics which were being made in Europe in the mid to late '20s.

For my own M.S. thesis I solved a problem in applied geophysics. There was a great deal of interest in applied geophysics among people in physics and electrical engineering at Wisconsin. The professor under whom I did a master's thesis on electrical methods of geophysical prospecting took a position with the Gulf Oil Company in Pittsburgh, which had just started a new research laboratory with emphasis on applied geophysics. Applied geophysics was proving to be a great help in finding oil.

Aspray: Who was this professor?

Bardeen: Leo J. Peters was his name. He left in the spring of 1929. During the next year, '29-'30, I worked on a problem of antenna design with the head of the electrical engineering department, Edward Bennett. I was not enthusiastic about the problem, and I decided not to stay on to complete a Ph.D., but to take a position with Gulf to work with Leo Peters and others who were forming the new research organization.

It was an exciting time to be there, because geophysics was just opening up and new prospecting methods were being developed. But after two or three years of it, I decided that I didn't want to make a career in geophysics. For one thing, I didn't think my background in geology was strong enough. In the meantime I had been attending a seminar in modern physics which was run at the University of Pittsburgh by Arthur Ruark and Elmer Hutchisson. Talks were given occasionally by people from outside the area, but more often those in the seminar took turns reviewing published papers. In that way I kept in contact with developments in modern physics. In the spring of 1933 I decided to leave geophysics, and I applied and was accepted at Princeton. I chose Princeton between the Institute for Advanced Study was just getting started and there were many notable professors joining the already strong mathematics department at Princeton.

Aspray: Had you spoken with anyone at work or at Wisconsin or at Pittsburgh about your career?

Bardeen: I undoubtedly did at Pittsburgh. I don't think I did at Madison, aside from my father and my immediate family. At that time Van Vleck, who had been probably my most important contact in math and physics, had left to go to Harvard. He had been at Harvard for a year or two, so there was really no one at Wisconsin who had an influence on my decision. I think it was more because I had long had an interest in mathematics and thought this would be a wonderful opportunity. By the time I left Gulf Oil, where I was earning a good salary, I didn't know if I would ever be able to get another job. I had a little money saved up, so I decided to go to graduate school.

Aspray: I understand that you chose to go to graduate school in mathematics rather than physics. Why was that?

Bardeen: I felt that my background in mathematics was stronger than my background in physics. I had taken a number of courses in physics, and also had attended the seminar at Penn. But physics was more of a hobby; my central interest was more in mathematics. I did feel, though, that because of my background I probably would go into applied mathematics rather than pure mathematics.

Aspray: Did you consider any other schools at the time?

Bardeen: No. The only place I applied was Princeton, and if they had not let me in I probably would not have left Gulf. It was a wonderful opportunity to be at Princeton. For the two years I was there I was closely associated not only with the Princeton faculty, but with those at the Institute for Advanced Study, which was getting started at the time. At Princeton interactions between physics and mathematics were close. Princeton was an exciting place to be. Whenever visitors came from Europe, they generally would choose the Institute as one of their first stops. It was also an exciting period in the development of quantum theory. Von Neumann and others were trying to put quantum theory, which had been developed in an intuitive way, on a firm mathematical basis. The first year I was there, I attended lectures which von Neumann gave on Hilbert spaces, which are the mathematical background for quantum theory.

Aspray: Who in the Princeton community was interested in that problem?

Bardeen: There was a strong interest among some of the pure mathematicians, particularly Veblen, during the first year. Also Hermann Weyl, who came, I think, the second year I was there, '34-'35, was also much interested. Von Neumann's lectures, which were given in '33-'34, were attended by quite a few of the professors in the mathematics department. It is probably rare to have the sort of close interaction between pure mathematicians and people whose backgrounds were more in physics that was present at the time.

Aspray: Was H.P. Robertson interested in these kinds of problems?

Bardeen: Yes, he was one who was very much interested. Also E.U. Condon.

Aspray: From the physics department?

Bardeen: Robertson gave a course in relativity, relativity as applied to cosmology, which I took and enjoyed very much. Condon's main interest at that time was in atomic physics; he and George Shortley were just finishing up their classic book on atomic spectra. It has been the classic in the field for many years since it was written.

I didn't know what area I might work in. I first talked with Condon about a thesis problem. He suggested a problem in atomic physics. He had a number of small problems that he recognized in writing the book. They needed to be cleaned up, and he suggested that I work on one of those. It did not sound too exciting to me, so I did not follow up in that vein. I then became involved with Eugene Wigner. First, more or less on my own, I tried to see if I could do anything about the singularities or infinities which arose in quantum electrodynamics that I learned about in lectures, but I was not successful, although I spent a lot of time studying the problem.

I don't remember the time sequence, but I do know that soon after I arrived I became acquainted with Fred Seitz, who was a fellow graduate-student. I learned about his interest in the structure of sodium, which was the first time quantum theory was applied to a real metal, rather than to an idealized metal. He and Wigner tried to understand the nature of the metallic bond. This was a start of the development of what became called band theory, calculating wave functions for electrons in real crystals. The general theories that had been developed earlier did not apply to any specific materials. This was the first application of quantum mechanics to determine the electronic structure of real solids. It has become an important field. Work is still going on after all these years.

After the first problem that I attempted didn't pan out (trying to resolve singularities in quantum electrodynamics), I worked on a problem suggested by Wigner. It was on the theory of the work function, the energy required to remove an electron from a metal and place it just outside one of the crystal faces of the metal. This required an extension of the band-theory methods to the surface and a calculation of the electronic structure of the surface of the metal. It was in the second year that I was at Princeton that I got involved in this problem. Wigner had a half-time appointment at Princeton, and he was gone to Europe—I am not sure just where—the second semester of the academic year, '34-'35.

Aspray: May I interrupt your story for a minute to ask about Wigner. Al Tucker and I have been trying to figure out Wigner's appointments at Princeton. Can you tell us what kind of position he held and why he left Princeton a little while later to go to Wisconsin?

Bardeen: Originally (from 1930) he and von Neumann shared a full-time appointment; each had half-time appointments.

Aspray: That is, half in Princeton at the University ...

Bardeen: And half in Berlin. That went on for about three years until Hitler came in, and then they were without jobs in Berlin. I think that even if he could have continued at Berlin, von Neumann would have chosen the position at the Institute for Advanced Study. Wigner had only the half-time appointment at Princeton. I guess because of financial stringencies at the time, the University did not feel it could offer him a full-time appointment. So he had to find some other position in Europe for the second half of the academic year. I cannot remember now, but a year or two after I left Princeton, Wigner left to go to the University of Wisconsin because they did offer him a full-time job. It was only a couple of years later that Princeton found that they could come up with a full-time position, so he went back to Princeton. You can fill in the years, no doubt.

Aspray: We have the information on the years; our main question was why he left, and you have answered that.

Bardeen: There were a number of people in the group around Wigner who got started in solid-state theory. Wigner was one of the three pioneers in solid-state theory in the U.S., the other two being John Slater, who at that time had just been appointed a young department-head at MIT, and John Van Vleck, under whom I studied at Wisconsin and who later went to Harvard. It was in 1932, I think, that he went to Harvard. Van Vleck's interest was mainly in magnetism. Wigner was interested in a variety of problems, so the work that initiated a tremendous development in solid-state physics was more of a side-line for him. He initiated calculation of the electronic structure of real crystals. Slater recognized the importance of band structure calculations and carried it much further. These three, Slater, Van Vleck, and Wigner, were the ones who trained the next generation in solid-state theorists in this country.

Aspray: Yourself among them?

Bardeen: I was Wigner's second student. The first was Fred Seitz, and the third was Conyers Herring. Wigner had some other students—in particular post-docs—in a period of the few years during the '30s, when he had interest in these problems. I think by the time he went to Wisconsin his interest had shifted more to nuclear physics and other areas. It was only a very few years that he was involved in solid-state theory, but they were very important years.

Aspray: What was the problem that he got you working on?

Bardeen: That was the problem on the theory of the work function, which was an extension of what Wigner and Seitz had done for the bulk of the metal to the surface. In the spring of 1935 I was offered a position as a Junior Fellow of the Society of Fellows at Harvard. The Society had started just a couple of years earlier, and they hoped to build up to about 20 Junior Fellows from all different fields of study. They appointed about six or seven each year, and I was in the third group, which is the one that brought it up to the steady-state level of around 20 or so. I think that I was very fortunate to begin at the beginning, when there was lots of enthusiasm and excitement, both at the combination of the IAS and the math department at Princeton and at the Society of Fellows at Harvard.

Aspray: I believe you told me before this interview that you had some reluctance to leave Princeton to go to Harvard. What was the reason for your decision?

Bardeen: At the time my thesis was not finished. Wigner was away the second semester, when I normally would have finished up. According to the rules of the Society of Fellows, you were not supposed to work on a Ph.D. thesis during your tenure as a Fellow. But they gave me a special dispensation to finish up my thesis during the first few months of my appointment, which I did.

Aspray: How would you compare the two schools for the kind of work you were doing at the time?

Bardeen: Well, for the work I was doing, Princeton was certainly the more exciting place, especially the group around Wigner. There was a group around John Slater, which was developing at MIT. I had considerable interaction with him and also with some of his students. Bill Shockley was one of his students. He finished in '36 and went to Bell Labs. So the next generation of solid-state physicists can be traced back to Van Vleck, Slater, and Wigner. In the next generations practically all descendents can be traced back, one way or another, to those three. Certainly Princeton was the most exciting place to be for that field. Van Vleck's group was exciting for the fields he was working on, which centered around magnetism. They were not my own interests. While I worked closely with Van Vleck, I picked my own problems.

Aspray: You also did some work with Percy Bridgman while you were at Harvard, didn't you?

Bardeen: Yes. I wanted to see how far you could push the methods developed by Wigner and Seitz for calculating the binding energy of metal as a function of volume, and other problems of that sort.

Aspray: Let's turn, if we may, to the educational program at Princeton. How did the department treat the kind of research that you were doing? Was it an acceptable subject to study?

Bardeen: Well, I was in the program in mathematical physics. You could get a degree in mathematical physics either in the mathematics department or in the physics department. There was little difference between the two options; I think those who got a degree in physicshad to take a laboratory course. Students in math and physics worked closely together. In fact, we took the same prelim exam. I took my prelim at the same time that Fred Seitz did. We had identical exams although he was in physics and I was in mathematics. We were both aiming for a degree in mathematical physics. The common room of Fine Hall, with tea every afternoon around 4:30, attracted people from both physics and mathematics. It was a meeting ground for people from both departments.

Aspray: Was there much interaction between the mathematicians and the physicists at this tea?

Bardeen: There was more interaction with the physics theorists than with the experimentalists, but even the experimentalists used the common room to meet students in mathematics. It was a social center for both departments.

Aspray: If we may return to the prelim. Do you remember what subjects you were examined in?

Bardeen: The oral part was given mostly by mathematicians. It was partly on differential equations. I had been attending von Neumann's course and was asked some questions about Hilbert spaces. I don't remember other questions.

Aspray: Do you remember who examined you?

Bardeen: Let's see. Howard P. Robertson was the chairman.

Aspray: Eisenhart?

Bardeen: Eisenhart and T.Y. Thomas.

Aspray: It was typical for three people to be on the examining committee, so that may have been it.

Bardeen: I remember Robertson asked me a question about what electrodynamics would look like if there were magnetic poles. I got the right answer, but he started asking me the reason for it. I was using too much intuition and could not give a convincing argument of the sort he wanted, one based on meeting requirements of relativity. There was also a written examination with longer questions and problems you had to spend some time working on.

Aspray: Was that only in physics, or in mathematics also?

Bardeen: That was mathematical physics. I guess the oral was given by the mathematics department. The oral that Fred Seitz took was probably given by members of the physics department, but the written exam was the same.

Aspray: The math department for years and years resisted giving any kind of written examination to most of its students. There was only a short period in which they did that.

Bardeen: I think the written exam was just for mathematical physics. It could be because physics gave it to their students the mathematics department thought that their students should take it too. Some of the questions were more mathematics than physics.

Aspray: How much did the mathematics department expect the students in mathematical physics to be good, well-rounded mathematicians? Did you have to, or were you encouraged to, take a wide range of mathematics courses?

Bardeen: I did anyway. I took a course in differential geometry from Eisenhart, and a course in real variables.

Aspray: From Einar Hille perhaps?

Bardeen: I don't know if he offered that.

Aspray: Or Bohnenblust?

Bardeen: Bohnenblust, I think it was. There were very close relations among the graduate students, at least while I was learning, between students in the mathematics and physics departments. I think that that played an important role. It was easy to talk with people like Robertson. Wigner was a little more distant, a little more polite and reserved. People like Robertson and Ed Condon were very friendly. They met with students, heard the students talk, and entertained the students occasionally at social events.

Aspray: On a social level? I heard that the Robertsons were ones for giving grand parties.

Bardeen: Yes, the von Neumanns did too.

Aspray: Did you have much to do with von Neumann?

Bardeen: Not on a really personal basis, the way I did with Robertson or Condon. Or Walker Bleakney in physics. I knew von Neumann as a student in his seminar.

Aspray: Did you have the same sort of relationship with Weyl that you did with von Neumann?

Bardeen: Well, I probably knew von Neumann better, but I talked with both of them. They were both accessible. I think I had some feeling that there minds were so far ahead of mine that it was difficult to follow their thoughts.

Aspray: A lot of people felt that way.

Bardeen: I didn't get that feeling with Robertson or Condon or Wigner.

Aspray: What role did experimental physics play in the training of the mathematical physicists?

Bardeen: Well, I considered myself a mathematician, and I was not particularly interested in taking courses in physics for the sake of physics.

Aspray: Would someone like Veblen or Robertson or von Neumann encourage you?

Bardeen: Yes, they provided encouragement and inspiration.

Aspray: Were there disagreements along these lines between the mathematics and physics communities? It would seem that different philosophies might come out in deciding the points of an educational program.

Bardeen: I am not sure about that. I think those in mathematics had to take the oral in math, thus satisfying the usual requirements for a mathematics degree. Probably similarly in physics. There was a different orientation in the course work, but as far as research is concerned, there was no difference at all. I did not know until looking at the list that Fred Seitz was in the physics department.

Aspray: You mentioned Wigner's first few students. Were there other students in mathematical physics coming through at that time who were students of someone else?

Bardeen: Joe Hirschfelder worked with Wigner. He was in chemistry, and he went on to a distinguished career in theoretical chemistry. This was in the period when there were still many pioneer problems to do in applying quantum theory to chemical structure—chemical reactions as well as the solid state. It was particularly through the Graduate College that you got to know students in other departments. Most of us were single and lived at the Graduate College. I met a number of people in chemistry that way. I think Wigner's interests in solid-state physics did not last for more than for four years, from '33 to '37, when he had probably four or five graduate students and an equal number of post-docs. They left to start a whole new field of research.

Aspray: Coming through about the same time as you was Abraham Taub. Is that right?

Bardeen: I knew him well.

Aspray: With whom did he work?

Bardeen: He worked with Robertson, doing a problem in relativity. He was here at the University of Illinois at the time I arrived. Later he went out to Berkeley.

Aspray: How do you think your Princeton training affected your later career? Can you say what you got out of your Princeton years?

Bardeen: I got the foundations on which I built my subsequent career in solid-state physics. At the time I did not consider myself a solid-state theorist. I think people were more general in their interests. I considered myself a theoretical physicist and looked for any problem which might be of interest. But I got involved, not only at Princeton but also at Harvard, in solid-state physics. When I went out to Minnesota I worked a little more widely in other areas. It was only after the war, when I went to work at Bell Labs, that I committed myself to working on solid-state theory.

Aspray: You mentioned the Institute for Advanced Study as a reason for coming to Princeton in the first place. In hindsight how do you view the proximity of the Institute as affecting the kind of training you wanted? What was its importance in the intellectual environment?

Bardeen: I think that it was partly because the Institute was new at the time, so there was more excitement and enthusiasm than in a steady-state situation. It was not only being able to listen to seminars by people like von Neumann or Weyl; there were also visitors coming through. I did go most of the time, even though the talks were not in areas which affected my work directly. By listening one could get a broader knowledge of what sorts of problems people were interested in. Most of the seminars concerned fundamental problems, rather than applications of the sort I was working on. But I enjoyed going to those seminars anyway to see what was going on, to meet interesting people, and to see what major hurdles were being faced. I would say the common room was where all of the social interaction took place, including, as you've probably heard, games such as Kriegspiel and go. They both got started about the time I was there. I think of John Vanderslice as one of the enthusiasts for games and Bohnenblust as a frequent participant.

Aspray: Is it right to see a difference between American and European traditions in mathematical physics and to see the large number of European immigrants in the Princeton community as providing a core group for new kinds of research?

Bardeen: I think this was of great importance, since quantum mechanics was initiated in Europe, and aside from Van Vleck, who was trained in this country, most Americans did spend some time abroad. I think that Hitler's coming into power produced a large exodus from Germany, which was the center of activity. This made an enormous difference in quantum theory in this country. I think we were already becoming reasonably good before that as Van Vleck was first rate without having had the experience of studying in Europe for any great amount of time.

Aspray: Are there others you can point to, either individuals or centers where there was interest in research in quantum physics prior to the influx of immigrant scientists?

Bardeen: Well, only a few places. Harvard was a fine place, with Edwin C. Kemble the pioneer. An early graduate who became outstanding in solid-state theory is Clarence Zener. Eugene Feenberg studied at Harvard and later went to Princeton. Those were probably the two major centers in the '30s, in addition to students of Oppenheimer at Berkeley and Cal Tech. Of the immigrant scientists, Wigner had certainly the most influence in solid-state physics.

Aspray: Did Eisenhart himself have an interest in relativity or quantum mechanics?

Bardeen: I am sure he had an interest in relativity, probably not in quantum mechanics. He was not, for example, one of those closely involved, as was Veblen. This was probably a unique period, having not only the professors, but also post-docs and instructors interested in the mathematical formulation of quantum mechanics. There was such an outstanding group of people that it was bound to rub off on others.

Aspray: Is it fair to say that the graduate students were thought of as junior researchers, that they were given quite free range? That they were not closely watched in a set of courses and carefully encouraged in their work, that they were given freedom if they could handle it.

Bardeen: As a student you did pretty much what you wanted by picking problems.

Aspray: Were there rigorous course requirements?

Bardeen: No, I don't think so. This was partly because the graduate program was small; only a few courses were offered at the graduate level so most of the students took what was offered. There was not much of a choice. Now the number of courses is probably enormous, and the student has a difficult time deciding what to take.

Even though I was only there for two years, I consider the period at Princeton the two most influential years I spent anywhere. The period at Harvard was also important because that was the first time I was in the physics department. I didn't have any particular duties, so I had the opportunity to read broadly in many areas of physics and get the background I didn't have before. So that was an important period too. It was also the time that I initiated my own research efforts. I course I did this out at Gulf too, though that was applied work in geophysics. Probably the two years at Princeton and the three years at Harvard are what made my career.

Aspray: One of the things we have trouble getting is just impressions of people, their personalities or significance. Do you have any comments on some of the people you were associated with?

Bardeen: Fred Seitz was a very well organized person. He never wasted any time. He put great demands on himself and worked hard in getting his work done.

Aspray: Can you tell me something about Robertson?

Bardeen: He was an easy-going individual, a very likable personality. A sort of person who would not intimidate anyone from asking questions about anything. He was certainly one of those who made the department in terms of social interactions, creating a friendly atmosphere.

Aspray: How did he hold up in a lecture where there were all those luminaries around him, like von Neumann and Weyl?

Bardeen: He stuck pretty much to his own interests in general relativity, so his interests were not as broad as many people's. Having met Fred Seitz was one of the reasons I came here to Illinois; he'd already come here and established a strong group. Conyers Herring's career was much like my own. After he got his degree at Princeton, he was a post-doc at MIT. I had a bit of interaction with him there. He even joined Bell Labs after the war, shortly after I did, so the association has been a lasting one.

Aspray: What can you tell me about Wigner? What were your interactions with him as an advisor, to the extent that anyone at Princeton was an advisor for your Ph.D. work?

Bardeen: As I say, he was very encouraging about the attempts I was making towards electrodynamics that didn't go anywhere. Then he suggested the problem on the work function, and the first paper was a joint paper where he actually did most of the work.

Aspray: Was it common for graduate students to write their own papers at Princeton at that time?

Bardeen: Yes, I think it was.

Aspray: Was Wigner an easy man to get along with?

Bardeen: Very easy to get along with. He always had penetrating questions. I learned from him an important method of attack: to reduce anything to the simplest possible case so you can understand that before you go on to something complicated. You reduce a problem to its bare essentials, so that it contains just as much of the physics as necessary. I think that was a good lesson to learn. Others shared the same attitude. I know Bill Shockley did. With problems in group theory Wigner would take the simplest possible group which exhibited the sort of things he wanted, and he would look at that before he went on to anything more complicated. He had very good insight with his background in chemistry. Even though he spent only a few years—even then only part-time—on solid-state physics, he really made his mark because he could see what was essential and what the important problems were.

Aspray: Can you think of any anecdotes to indicate the social or intellectual environment of the period?

Bardeen: I am really not sure that I can. I met Walter Brattain at the time, but I was not associated with him until after the war when I was involved in the research leading to the transistor. His brother [Robert Brattain] was a grad student in physics at that time. Like Walter, he came from the West and was proud of his Western heritage. He had an outgoing personality. It was through him that I met Walter in New York; I went with him to New York where Walter lived with his wife in an apartment.

Aspray: Did you do any teaching while you were at Princeton?

Bardeen: No. While I was at Harvard I did some. I taught a course in relativity, based on the one I had taken one from Robertson, and I we went through Eddington's book.

Aspray: Were you, in your second year at Princeton, looking for a job?

Bardeen: That was something I was concerned about. One of my main considerations in taking the position at Harvard was that it was for a three-year term. I was offered the Procter Fellowship at Princeton, but that was for only one year. I would be able to finish my degree, but then I would have to look around for a job the next year. The Harvard position was a fine opportunity. It was obvious that I was going in the direction of physics and there was an opportunity to spend three years getting a background in physics. I had never been in a physics department before.

Aspray: Were you still not interested in an academic career? You mentioned earlier that you did not want to follow in your father's footsteps.

Bardeen: By the time I went to Princeton, I decided that I could not resist any longer. I was on leave from the University of Minnesota working for the Navy at Washington during the war. After the war I probably would have gone back to Minnesota except for the salary differential. That was an important consideration aside from the fact that there was a very good opportunity at Bell Labs. And in view of what happened there, that was lucky for my career. But I probably would have gone back to Minnesota if they had offered me a livable wage. I liked it at Minnesota.

I see on your list many familiar names, students and others. They were there for various periods of time. You see what an inspiring time it was there. The contacts were with a wide variety of outstanding people. Students learned from each other and from the post-docs. They also had outstanding professors. This was also extremely important too, even though contacts were not that close between students and professors. Some of the professors, like Robertson and H.F. Bohnenblust, were exceptions. Others, like Veblen and Eisenhart, were on a different level.

Did you see the papers which were published in the *Proceedings of the Royal Society* a few years ago. Conyers Herring and Fred Seitz and I all made contributions, like in the Princeton days. This was the start of the solid-state physics history project. It was organized by Sir Neville Mott in London in 1979. The papers were published in *Proceedings of the Royal Society (London)*, 371A, pp. 1-177 (1980).