Oral History of Hans Queisser

Interviewed by:
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Craig Addison: I am Craig Addison from SEMI. I am with Hans Queisser. We're at the Computer History Museum in Mountain View, California, and today's date is February 27, 2006. Hans, could we start off at the beginning and have you talk about where you were brought up and your educational background.

Hans Queisser: Yes. I was born in Berlin, Germany, in 1931. My father was an engineer with the Siemen's Company. And at that time, Siemen's Town in Berlin was really a center of high technology. So I got very much influenced by my father, by our neighbors, in technical things. And I survived the Dresden air raid just barely in 1945 when I was 14 years old. And the end of the war was not easy, you can imagine. My father had to work for a Russian government agency. Times were tough. And my belief that I should enter a scientific field was strengthened by the fact that I could see that people who had a technical background and who had something to say, were treated very nicely by both the Russians and the Americans. But people who had nothing of value were treated rather badly. So that was my belief that in order to survive you had to have something that was a skill. And I wanted to go into physics, which I did. Wasn't easy to get a place at the University in Berlin, because the University was in rubbles and ruins, and the people who came back from the war were the first ones to be admitted. But I did an apprenticeship and a workshop and then found a job as a technician in one of the research institutes in Berlin. And my boss was Max von Laue for a time, famous Nobel Laureate who did the Laue Diagrams, who found out that one can use X-rays to find out about crystals. And I write about this in my book which is in English by Harvard University Press, "The Conquest of the Micro Chip", and the German version is a little bit more detailed.

And then I applied for a scholarship in the United States. At that time there was "reeducation to democracy" and people wanted journalists, teachers, preachers, lawyers, but not technical and medical people. But no matter, I won a scholarship to go to the University of Kansas in Lawrence, Kansas. This was in my second year of studies in 1951 and 1952, so I was a student at Lawrence. I very much appreciated and still love the friendliness of the people in the American Middle West. And I got quite familiar, of course, with the United States. After coming back, I went to the University of Goettingen, still a very, very important place in physics, mathematics. And while most of my student friends went into nuclear physics and neutron physics, high energy physics, and so on, I wanted to go into the field of solids, because I found out that this was a field, a discipline of physics, where you could so something as an individual, whereas in nuclear physics or high energy physics, there were huge teams of people and you just were one little part of that. So I did my PhD work in Goettingen in an institute where defective metals were looked at. We thought at that time that maybe the superconductivity, that is the loss of electrical resistance, was somehow geared to defects in the crystal order of these materials, and I finished my diploma in '56 in Goettingen, and my PhD in 1958.

From that time on, 1958, I read the American publications and tried to find a job in the United States. I applied, but most companies replied, "No. You have to be an American citizen because of security reasons and so on." And one of my professors knew that, and he received a letter from William Shockley here in Mountain View, [which said] "Do you not have a student who might be interested to come over and somebody who is a little bit familiar with defects?" So he said, "Why don't you apply?" And so I did. And Shockley wrote me a letter asking me if I wanted to become a member of his staff.

Addison: Let's just back up a little bit and fill in some of the gaps. So your father, he was an engineer?

Queisser: Yes. My father was a mechanical engineer on power plants. And he had been in the United States as a young engineer in one exchange program. Was not easy in 1928. But he eventually made it, and asked his fiancé to come over, too. But she didn't want to so he returned to Germany. So I was almost born in Ohio, but in actual fact I was born in Berlin.

Addison: And when you were studying in the 1951-52 period, were you aware of the invention of the transistor and its implications?

Queisser: In 1951-'52, no. I took basic courses in physics. The transistor, yes, I knew there was something going on there, but the transistor in 1952 was not really very strongly in equipment and so on. It was still the vacuum tube. The transistor had a much more difficult time to really overcome the vacuum tube system than one nowadays thinks about it. It was not easy for the transistor, especially the very simple point contact transistor. It was a very flimsy and shaky and non-reliable type of thing.

Addison: Now your PhD work in defective metals?

Queisser: Yes.

Addison: What application was that? Was that to be applied in electronic engineering?

Queisser: At that time we did not know why there was superconductivity. And my boss had the incorrect hypothesis that it was not an effect of the metal itself, but it had to do with a defect. It is very much like in semiconductors. Electric conductivity is not given by the material itself, the pure silicon, but by the very small amount of admixtures of other atoms. So that was the hypothesis, and we could make very defective bismuth films, and they were super conducting while the single crystal was not. So that was the idea. So we did a lot of work on that with X-rays and with other means of analysis to look in this effect. And we had some contacts with the U.S. at that time; people from Berkeley and from MIT came over. And at one time John Bardeen was visiting our institute in Goettingen. And my boss showed him around, and then he said, "Oh, by the way. This young man is going to California soon. He will go to Shockley Transistor." John Bardeen looked at me closely, slowly, and then he said, "Well, we'll see how you like Shockley." And of course, John Bardeen, together with Brattain and Shockley, they got the Nobel Prize [in 1956] for the invention of the transistor.

Addison: Before you received the letter from William Shockley, you had not worked for a company. You were still studying up until that stage?

Queisser: I was. I did my PhD in 1958, and after that I was appointed an assistant, an assistant professor you would say in this country, and continued working there. And I could have stayed on, and most of my friends said, "Oh, you're crazy taking such a risk going to the United States. Who knows what's going on? You can be a civil servant if you'd just wait another two years and so on." But I didn't want that. I wanted to go out and participate in the new activities, because by that time, of course, semiconductors, which I had not heard about in German university, were really beginning to be very important and that was the time of the Cold War, so there was lots of activity. And I really wanted to at least spend what is now called a post-doctoral period. We did not have that at that time. So Shockley, via my professor, made me this offer.

Addison: You are sort of an academic.

Queisser: Yes.

Addison: But you did not want to pursue a career as an academic. You knew that.

Queisser: No, not necessarily, because somehow this academic career, the older people in the Goettingen University, who were trying to get an academic position there, oh, they were in such bad shape. And I said, "I'm not going to wait there, and just sit it out. No. I want to do something new." And for me, since I had been in the U.S. for one year studying, and also I froze peas in Washington State and in Oregon to make a little bit of money and I hitchhiked through 40 different states, and since my father had always instilled a certain love for this country, I wanted to go to the U.S., which I did.

Addison: Now the letter from William Shockley, he wanted your specific skill in the area of defective metals?

Queisser: He didn't state that. It was a standard letter, and said that my salary will be not less than $800 U.S. per month, which was good for that time. And that I would be a senior researcher, senior member of staff, although I was a young guy. That was the title he gave me. But then, of course, when I came, he told me exactly what he wanted of me and he sent me material, reprints, and so on, before I came. And I bought his book, "Electrons and Holes and Semiconductors", which at that time and certainly still is a little bit, was the bible of semiconductor research, very important, nicely written book. And I tried to read that and get myself prepared, because in Germany semiconductors had not gotten a breakthrough in public recognition.

Addison: Now you had heard of William Shockley before the letter?

Queisser: Yes, of course. I knew that. People were always asked who won Nobel prizes in PhD exams and so on, and you had to explain this. And since I had no problem with the English language as compared to most of my co-students, I used to go into a library and read the Physical Review and Physics Today, the typical publications. So I knew a little bit more of what's going on. And superconductivity, well, could also have been something that I wanted to do. But going to a Nobel Laureate, Shockley had got the prize in 1956, and I looked around in 1958, so this seemed like a very good idea.

Addison: Did you meet Shockley here [in California] first, or did he go to Germany?

Queisser: I met Shockley here first. In a way, I am an exceptional case because Shockley never hired anybody who had not undergone a more or less psychological test by his wife, Emily, his second wife. She is or was a psychologist, and she talked to people. And Shockley also did something very much like an oral PhD examination. He tested people. But he was very much in need of somebody to finish two government contracts, and I was assigned to these two government contracts as soon as I arrived here.

Addison: And what contracts were those?

Queisser: These two contracts concerned silicon solar batteries, solar cells. Shockley took those contracts from the Army and Fort Monmouth Signal Corp and from the United States Air Force, in Dayton, Ohio, not because he wanted to make and sell solar batteries. There were other companies, especially in Southern California who did this. But he wanted to learn about silicon. And at that time, it was reasonable already to use silicon as the material for solar cells. So we had one contract of which most of the money was spent when I came. Shockley told me, "You are now in charge of this project. Most of the money is gone and we only have about 15% of the results. So get to work." Which I did. And we made a multi cell solar battery, which was a good idea at that time, to have a little chip with series connections of different solar cells, so that you get a higher voltage out and not just 0.6 volts that you get off an ordinary solar cell. So what I then really did was one of the first, or maybe the first, integrated circuit, but with the technology that was ridiculous. I had to make diffusions into a very thin 50 micron thick silicon wafer for both sides to meet and to create the series connection. I had to go into the lab Sundays and during the night. Take it out of the diffusion furnace. Really work very hard. I was single at that time, and this was new for me. And I kind of liked it. But there was quite a bit of pressure there. And I finished a battery that gave us over 10 volts. That was then the end of this project.

And then we started on a more theoretical thing, because the Air Force at that time in 1957-58, 1959, wanted to know what was the best material to take, the best semi-conducting material to take for a solar cell. And what was the optimum efficiency that you could get out, because if you plan for a satellite or anything else, you want to know these things. What is the maximum you can get out, and what's the best material? Because at that time the RCA people in Princeton, New Jersey, they were very much in favor of compound semiconductors such a gallium arsenide and so on. Other people, like Clevite in Cleveland, they said, "No. We do so-called two six [II-VI] semiconductors, one element from the second row two, and one element from the sixth row six, like cadmium sulfite and these things, or cadmium selenide. And they said this is the best material, so the military people wanted to know, now what is the best material? So we did a lot of calculations. By the way, at that time not with a computer but with a slide rule, a special slide rule. And I calculated. We got a very broad maximum, and silicon was in this broad maximum. So silicon was a good material. It was available. It was relatively cheap. And Shockley, of course, was interested in silicon because his product, the four-layer diode, we will come to that, can only be made with silicon. So I think we did a pretty good job there, did all the calculations. Found out silicon is among the optimal materials. And the best efficiency one can get out with single solar cell with the sun and atmosphere and all that was about 30%.

So we wrote a paper, submitted it to the Journal of Applied Physics in 1960. Was rejected. Shockley came steaming mad. He said, "Hans, look at this. They rejected our paper. It's nothing new." So we rewrote it again, added some things to it, made it a little bit more legible. And then it was published. But very few people at that time quoted it. It was still the engineering idea, just like in the old times of the steam engine, you have the input of energy and then you have certain losses, and what's left is then the useable remaining energy. And they estimated the possible maximum efficiency. But we did what then thermodynamics did for the steam engine, really made a solid theory of solar cell efficiency. And now the paper is being quoted all the time, and the present program of solar energy conversion is in this country beat the Shockley-Queisser limit. That makes us very proud, and the last time I spoke to Shockley was here at Stanford at the Faculty Club. He said, "Well, Hans, I guess we did a reasonable thing with the solar battery theory." So he was quite happy. But it was a tough start. People at first did not even recognize him, did not even read it. But now they do.

Addison: Solar power is now finally becoming big, especially in Germany and in Japan.

Queisser: But also in this country.

Addison: Here in California.

Queisser: Yes.

Addison: So you were way ahead of the time.

Queisser: That is correct. And, of course, the military people wanted to know solar power, what they could use. When the solar cell was first invented by Pearson and Fuller at Bell Labs, I knew both people very well. With Fuller in particular I did some joint publications later on when I was at Bell Labs. The military looked at the solar cell. Shockley had to go to the Pentagon and said, "We, at Bell Labs, have found that there is a solar cell which supplies power." The military looked at it and they said, "Ah, no. It's much too weak. We cannot give this to a soldier for his battery pack. No, no, no. Forget about it. You can use the patent. You can publish, and for awhile the solar cell was used for radios and portable things. So it had a difficult time, but eventually now with the price of oil going up, it will come. But it is, and I must say this in 2006, it is still quite expensive. It still is a factor of three, maybe five, more expensive than making electricity out of conventional fuel.

Addison: Let's go back to the challenges of growing the crystals and so forth. When you joined Shockley Labs, is there anything you could buy off the shelf? Did you still have to make the equipment yourself, or were there sources to buy?

Queisser: We made quite a bit of the equipment ourselves. I grew my own silicon crystals because my next project was to do diffusion on grain boundaries where you get very, very sharp profiles. We wanted to make a fast transistor utilizing these things. So I had to grow my own silicon crystals and the equipment that we used this was in Palo Alto at the Spinco building in Stanford Industrial Park. We made our own [crystal] pulling machine. The guy who did this eventually then started his own company, typical Silicon Valley thing. You make your own equipment first. But then there's more demand. The people then got out of that company, formed their own company, and became suppliers of semiconductor electronics manufacturing equipment. Very important. The chemicals we had…you could buy high purity silicon already, not nearly as pure as it is now. You could buy some photo-resist equipment that you put on your slides and made structures out of, but those were really from the printing business, not yet geared to the semiconductor industry. But at that time, our structures were very, very coarse. Nowadays it is nano electronics. But at that time there was millimeter electronics and we used to spray black wax, horrible, just the stone age of semiconductors. We used to spray black wax on. But we had good microscopes, optical microscopes, and one important thing of which I am particularly proud, I did the first experiments on transmission electron microscopy with semiconductors. When I was in Berlin before my studies, I was in a group that belonged to Ernst Ruska, who was the inventor of the electro microscope, later Nobel Laureate. And the University of California in Berkeley had such a microscope, and we were in the position for other reasons to make very, very thin pieces of semiconductors, so thin that the electrons would go through and we could look inside the crystal lattice defects. That was my job.

And so I took 14 samples, went up to Berkeley, talked to people and said, “Let’s have a look.” And they said, “Oh, semiconductors, they’re so perfect compared to metals. We’d hardly be able to find any defects in there. And, indeed, the first sample showed nothing. The second sample showed nothing. We were sitting in the basement of the old Hearst Building, and I began to really get a little bit uneasy. But Sample No. 13 showed a very nice defect, a stacking fault, which is something that you get when you oxidize silicon. People in the Valley, of course, know that nowadays. So from that time on, we continued to do some work. We looked at dislocations with the electron microscope. And now every decent semiconductor factory and development laboratory needs and uses transmission electro microscopes to really look at what’s going on. We had no scanning electron microscopes at that time. And the little outfit on San Antonio Road that we worked in with Shockley had lots of furnaces, furnaces for oxidation, furnaces for diffusion, furnaces for alloying, putting the contacts on. Evaporators where important. We had no clean rooms. It was just incredible what we had, but it was the beginning of something very nice and very big. It was wonderful for me as a young physicist, whatever you touched was new and you could get results. So I am very grateful I went into that business.

Addison: Could you talk a little bit more about the diffusion? I know that when Shockley first set up the company in 1956 it had a lot of trouble with the diffusion process.

Queisser: That is correct.

Addison: Had that problem been solved a little bit by the time you arrived?

Queisser: No, no. That was very difficult. Diffusion was studied at Bell Labs, and we from the Shockley Transistor Company, had a license. So I used to go to Bell Labs every two or three months and talk to the people. Of course, a whole army of very good people were doing work, particularly on diffusion, because previous to diffusion it was alloying, and the alloying is not a very good control, and cannot get you very fine structures. And diffusion, well, they had done the calculations, the mathematical formula that you would have. And when I did my first diffusion, I looked at these charts and people said, “Now, what are you doing?” Well, I said, “I want to find out for how long at what temperature I have to make this diffusion so the dopants or the phosphorous would go in there.” They said, “Ha, don’t trust this. Make a trial run.” It was trial and error how deep it would go. And it’s still a difficult thing. The basic principles of diffusion to do quantitative analysis of diffusion, that was rather difficult. And all we did at that time was to make test runs first, and then we made the real thing, especially from the four-layer diode which was the product that Shockley was interested in.

Addison: Why don't we talk about the four-layer diode? You talked about the solar cell, but did you have any involvement with the four-layer diode?

Queisser: Not directly. I became the expert and the one who had to do work on defects in silicon, like dislocations. Now looking back, I did some rather neat work on the influence of dislocations on diffusion, because any dislocation-diffusion goes faster, or the influence of doping and diffusion on dislocations. I found out that if you make a strong Boron diffusion, which you do for a four-layer diode or for a transistor, the misfit of the atoms of Boron and the silicon lattice creates dislocations. I could etch this and I found this. And what's interesting, I gave a talk at the American Physical Society Meeting in Monterey, California, and my boss Shockley was sitting in the last row and, of course, we had a rehearsal and everything, so the talk went well. And Shockley was sitting there with his friends from Bell Laboratories, directors and executive vice presidents and so on, and Shockley said, "Well, wasn't that a nice talk?" But the Bell Labs people said, "No. We don't believe it." Shockley, said, "What? You don't believe it? We have the proof." "No, no," they said. "This is so important for a transistor that we would have found it." And that made Shockley angry, and that made a much closer personal relation between the two of us. And that lasted and it was quite nice, and quite successful because Shockley, as you will know, was not an easy boss. So I had to work on the forward characteristic of a PN junction in silicon, which is not nearly as nice an ideal as it was with germanium. It is a non-ideal thing, and therefore the four-layer diode relies on the non-ideality of relation between current and applied voltage. So the forward characteristic going through defect states in the PN junction was my field and I contributed towards the understanding of the four-layer diode with my studies of forward characteristics, which you need for the solar cell. And the PN junction was not really under good control. Diffusion was difficult, and the hardness of a junction at the reverse current was very small. It was really very difficult at that time. But we slowly got there.

Addison: What is your view, Hans, on why Bill Shockley basically abandoned transistors and went to four-layer diodes?

Queisser: Well, he said, "I want to do it again. I want to do something again." And the four-layered diode, which he and others like Ebers and John Moll had found, maybe more or less by chance, just put four-layer diodes on, and found that it was like a switch. It's a bi-stable, two terminal device, two terminals being input and output and the transistor has a third one to control the current. But this was a diode, a simple thing. And it had two states, off and on, not carrying current, off, and carrying current, on. And it could be switched just electrically. Now, you must remember that Shockley was hired by Bell Telephone Labs right after World War II, in order to find a simple switch for the telephone system. His boss said, "The Bell system will grow very rapidly and we cannot do that with the old mechanical switches or with vacuum tubes who devour energy. Find out something new." And since people had done radar technology with semiconductors, they had a hunch that something could be done with these materials. So Shockley always was under the impression one needs a nice, swift, quick and simple switch, and the four-layer diode looked as if it would fill these conditions. What people really didn't know at that time was that a two-terminal device, a bi-stable thing that goes on and off, is very difficult to put into circuits, into large circuits. A transistor with three-terminals is much better, much more stable, and also the tunnel diode which, by the way, Bob Noyce found in the barn on San Antonio Road here in Mountain View, is also a bi-stable thing and a two-terminal thing, and never got anywhere. But this was not known at that time. And Shockley thought, "Well, we can do something new. I can do it again, not just a transistor. I will do the four-layer diode. It carries my name." There was a good deal of vanity involved in it. But also, the technique of conviction that such a thing which can only be done with silicon, because silicon is non-ideal, has these strange current depending amplification factors, Alpha. "I will do silicon. I will do my Shockley four-layer diode, and we'll sell it to many people, telephone business, control business, and so on." So that's why he came here not to southern California, because that's the smog. He didn't like that, although he had been going to high school in southern California, but he came to this area, which in 1959, still was wonderful, orchards around and the little apricot barn was unusual. So that was his idea. Technical conviction that this is a good switch and quite a bit of vanity with the name Shockley diode, and his company was called Shockley Transistor, so there was a strange misnomer in a way.

Addison: You talked about Shockley's work on silicon, and he's really given credit for pursuing silicon at a time when maybe germanium was still in use. Can you just talk about why?

Queisser: Germanium was so much easier. And Shockley insisted when he got that job at Bell Labs right after the war that he looked at germanium for two reasons. One, it had worked sort of, not understood as a radar diode. And two, Shockley's principle, try the simple things first. And germanium was simple. It was an elemental semiconductor. It was an element, chemical element, only one type of atoms, germanium atoms. The engineers, Shockley told me this very often, at Western Electric, said "This crazy guy at Murray Hill in the lab is using germanium. Why that? Germanium is expensive. It comes out of the Belgian Congo where there is civil war. In time there will not be enough. We already have selenium. We have cuprous oxide. We have other things, semiconductors that are useful like light exposure meters. Why do these crazy scientists do germanium? But he insisted that this be done. Germanium was easy to melt. It has no great affinity for oxygen, so you could really make it clean. Germanium is easily made very, very clean, and you could alloy it, because it was low temperature stuff. So the first transistor was made of germanium. But pretty soon people found out that germanium has a big drawback. It will not work if the temperature is elevated. If you leave your car radio in the sunshine of southern California or of Italy, it won't work anymore. So people knew they had to go to another material and that was silicon. Shockley for awhile thought that maybe even silicon wasn't enough. You have to go to still another material, silicon carbide. It's a very difficult material, abrasive in the true sense of the word. But we had a furnace there right next to the coffee urn, and where the donuts were delivered to at the back of this little old barn on San Antonio Road, but it never worked out. So silicon was a reasonable choice. Silicon carbide is too difficult. Germanium was nice and easy to make manufactured transistors and automated lines, but it wouldn't work. But for Shockley, silicon was particularly important because his diode switch could only be made with silicon. And, again, it was, of course, an elemental semiconductor, just one group of atoms.

Addison: What other kind of challenges were there with the four-layer diode in trying to get it produced?

Queisser: The four-layer diode my good Swiss friend, Kurt Huebner had to work on after the first string of people had left for Fairchild. You could make that for different switching voltages and switches currents and so on. So you needed to have a whole line of these diodes, and the yield of these things for reasons we didn't fully understand as yet, the yield was low, especially for, I think, the low voltage switches, which people thought they could use for applications in control units. We simply didn't understand the junctions. We couldn't make hard junctions, hard being the term that in the reverse direction there should be a very, very small current at all. These things had some currents in the forward junction, too. So we did not sufficiently well understand junctions. Diffusion was not under control. There were defects created by the diffusion, and so junction control was insufficient at that time. And the people who left Shockley and went to Fairchild, of course, worked on that very well. For a transistor, it wasn't all that important, but for the four-layer diode it was.

Addison: Now did you ever mass produce? I know they were sold. I have seen the catalogs. But were they produced in quantity that was sold?

Queisser: Oh, yes. Quantities, but small quantities, let's say, not the huge quantities that eventually came with the transistors and nowadays the integrated circuits. Yes. They were produced in quantity.

Addison: And who were the customers for the four-layer diode?

Queisser: The customers were some telephone people, some people who made control units, and so on. But the customer base was small. It was difficult.

Addison: And the solar cell project, that was never commercial, that was just a government program?

Queisser: The solar cell project…from the very beginning people asked me, now, why don't you make solar cells since you do such a good job in understanding the junctions? But Shockley didn't want to do that, just as he didn't want to do transistors when Bob Noyce came and said, "Dr. Shockley, we cannot sell the four-layer diode, but let's make transistors." But this was old stuff for Shockley and it was not his diode, so he didn't want to make any solar cells. We could have done that, oh, yes.

Addison: So you think there would have been a market for the solar cells at that time?

Queisser: Especially after the impact of the Sputnik shock, yes, there would have been a market, because people were really thinking about how to supply a satellite with power, and at that time the nuclear guys said, "Well, we make you a little nuclear reactor about the size of a pumpkin." But, of course, then the military soon found out that this wouldn't work, and they went to solar cells. And the Russians, of course, used solar cells. They got all the know-how through the United States, smuggled in diplomatic baggage. Some Russian guy told me the details of it. The military in Washington didn't want the solar cell, but the Russians said, "If we do that, if we shoot a satellite up and it should make beep beep, we need a power source." And they made solar cells, and as soon as that had happened, the interest of the United States in the Cold War, which was really something, and it influenced Shockley very much, then solar cells were important. We could have made some. But Shockley wanted to make the four-layer diode, no question. When we were bought by Clevite, we went bankrupt. Clevite bought us, a company in Cleveland and in Massachusetts. They said, "No, no. No four-layer diode. You make us a high frequency, high power transistor", which we could. And it was not a bad transistor. There was a better market for it. But, again, we went bankrupt and Clevite sold us off and we became part of IT&T. So that was California, Silicon Valley history. Yes.

Addison: Hans, earlier you said that you were one of the only ones who did not go through the psychological evaluation with Shockley. Can you just talk about your personal relationship with him, what kind of person you saw him as, and how you got along with him?

Queisser: A very unusual person. Could be very charming. For example, he was an amateur magician, and he did a standup, one man show of magic, very nice. He was very, very intense in everything he did, mountain climbing in New Jersey, sailing here in San Francisco Bay. Did all this with a scientific background. He was a firm believer in scientific principles. He was very unhappy with certain things in the United States, like the smog in southern California, and he spoke of the principle of equal unattractiveness that the Bay Area would soon also become as smoggy as southern California. And I got along with him in the beginning reasonably well, but later people told me that I was about to be fired. He fired 80% of his PhD staff within two years or so. Got immediately disenchanted with people. They made a slight mistake or something. I used a different formula for ionic radii when I did the diffusion thing, which he did not like. He construed that as criticism, which I was naïve in that case, but I think I was right.

And he was a tough teacher, also. Sometimes he would come and say, “Let’s go to Kirk’s”, which was a hamburger joint on El Camino. He took his yellow pad and a green pencil, and then he would buy a simple hamburger. He would have a hamburger, a small root beer, and a small coffee. That was his standard thing. And then after we had the hamburger, he would ask questions like an oral examination. He would test me on the things that I had to do. And he would say, “You have to go into this a little bit more deeply.” And apparently it worked out quite well with me. It didn’t with the people who had less of a background. I had a very good solid state background from Goettingen University, especially in chemistry. I had several technicians who were chemists, but I was aware of what was going on, so I had no problems there. Crystal growth was also done in Goettingen, not with these materials, so I quickly caught on there. And he appreciated that. And when we had rehearsals for talks, he liked that, and he said, “You go to the production facility, and I asked the leading people of the production facility that they listen to your lectures twice a week so that they learned what a PN junction is, what electrons and holes are, how the holes come in, and so on, how diffusion is done, and what the principles of PN junction in particular are, because there was the problem.” And that was kind of nice.

So I got along with him quite well, especially after this incident with the diffusion and the Bell Labs people saying, “No. If this is so important, we would have found out.” That irked him and that angered him. And from that time on, we were in pretty good shape. And he fostered my education and we had many trips together to the East Coast, and he told me of the development of the junction transistor, which is an interesting story for Shockley. Bardeen and Brattain had a transistor with these little needles, very unstable. And Shockley said, “This is not what we want. We want all the electronic activity in the inside of a solid state crystal, not with little contact needles.” And he did not become a co-author of the long paper by Bardeen and Brattain on the point contact transistor. He could easily have said that, because he was the boss of that group. He said, “No, no. That’s not what we want.” So he was in Chicago one winter, and there was a snowstorm, and they couldn’t get out of Chicago. So he was confined to a hotel room, and then he said, “Well, we must do this differently.” And then he developed the theory of the junction transistor, NPN, or PNP. Came back to Murray Hill…and he told me the story quite often. He said, “You wouldn’t believe it. These smart people, and I was giving a talk and I think I gave a reasonable talk on how everything can be done with an emitter base junction, base collector junction, and so on.” People did not grasp it. And he educated his people, and apparently he got into some difficulties, especially with Brattain who was a practical guy. That Bardeen wouldn't quickly understand his presentation of the junction, that was very difficult for him.

He was a man of science. Everything had to be scientific, like sailing on the Bay, you do it scientifically. And the business of increasing the intelligence of the United States, vis-à-vis the Soviet Bloc, also to be done scientifically. And he always said, "Think through." Remember at IBM at that time, every IBM employee had a little pad and it said, "Think!" afterwards. And Shockley, of course, was very aware that it was scientific things that ended the war in Europe, radar, and the war in the Far East, atomic bomb. So people were very, very much involved with scientific things. The U.S. population as a whole was a great believer in science and Shockley in particular. And he extrapolated and he said, "You must think to the end and do not get distracted by ideological things, by business considerations, by being just a nice guy and saying, 'Well, yes, that's how it is, but we don't do it.' You must think with all the consequences to the very end," which did not make him very popular with most of the people. So he then earned his strange reputation.

He often came to me. I remember one thing in particular he came to me and said, "Well, Hans, I will be away in the Midwest, will be given another honorary PhD degree. But I'll be back on Monday and then we continue our work." But he did come the next day. It was a Thursday or so. And I said, "What happened? Didn't you catch the plane?" And he said, "No. You wouldn't believe it. They called me on the phone in the evening and said, "Sorry, but we withdraw the offer to give you an honorary PhD." And, of course, Shockley knew that he was by then considered a racist, and they didn't want to have anything to do with him. This was not politically correct. And he said, "How did this happen in Germany that somebody couldn't utter his opinion?" Well, I did not want to get into a discussion there. Said I was just too young a kid. But he got very, very sad that in this democratic country you just couldn't stand up and present your opinion. Became more and more involved, stubborn. He was stubborn. And that got him deeper and deeper into these problems.

For example, he said, "We have to get good people. We have to get the intelligent people, and we have to sponsor them, be tough with them, demanding, and educate them," which he did himself [because] he wrote textbooks for high school physics. "And what do we do? We do welfare." This was the thing in the 1960s, mid-60s. All the money went into the inner cities. You supported young, unmarried, black mothers. And if they had three illegitimate children, they could live off the welfare they were paid. "And this", he said, "We cannot do. We must do something else." And also he was on the National Academy Committee to review the economic assistance to countries like India and so on. And, of course, the senators from Nebraska and Kansas and Oklahoma were very happy to sell wheat to the Federal Government, which was then shipped as foreign aid to India. And, again, Shockley extrapolated, and said, "If we continue to do that for another 10 years, we'll need the entire wheat harvest of the United States to support India. We should not send wheat to India. We should send condoms, contraceptives to India." Can you imagine what kind of stir that created? But, of course, he was right. Eventually India had to resort to a one-child policy and particular China had done. And just like the exhaust control laws of California came later and he was just too early for many of his times.

And he sort of enjoyed in a strange way to be at the center of attention, even though it was very negative. The professorial people said, "Well, Bill Shockley what are you doing here? You go into stuff which you don't understand. Do good physics." And he said, "Well, what's the interesting thing in physics?" And the people at Stanford said, "Well, the Dirac electron has such a problem." And he said, "OK, I'll look at that." And he presented a very good paper on the Dirac electron, just to show that he was just the genius which he thought he was, the man with the many publications, the man with the many patents. So he was certainly not easy. He insisted on his things scientifically, although they were very unpopular. He was a tough boss. Fired people. And whenever you had gained his confidence and you did good work, it was hard. He was very demanding, very impatient. He was a theorist. He would call you on the telephone in the afternoon and say, "Why don't you do such and such an experiment?" Next morning he would come in and said, "Have you done this experiment?" So he really didn't have the exact conception of what was going on, how difficult it was to do silica silicon junctions at that time. But he always told us, the other people and myself, "There is no law against making small, good junctions, and we can do it. We just have to do it the scientific way." And extrapolating absolutely all the way down, not get distracted by other things, by human affects, by economic things. He said, "If we all understand junctions, that's why I have to do the teaching, we will make a profit", which was naive in a way.

Addison: I have read one of his papers where he uses the term "the will to think."

Queisser: Yes.

Addison: And it seems as though he was tolerant of mistakes if it was towards finding a solution.

Queisser: Yes. Learning by mistakes, but if you made a mistake and it was a stupid mistake, or if you used vague language. He would shout at engineers who said, "No. We did this. There's a short circuit in here." He said, "I don't want to hear this. If you tell me that there is a short circuit, you must tell me is it one ohm or a 10^{th} of an ohm, or is it a kilo ohm, so that we can understand what's going behind it." Vague language, non-scientific language, he hated. And people were told that, and maybe they were fired.

Addison: I also understand that his car accident was in 1961 and that is when he kind of changed his views. Can you just talk about how he changed because of that accident?

Queisser: He mellowed. He was glad to have been alive, not getting killed. And the perspective of his life became a little bit different. He was easier to deal with. Maybe he also lost a little bit of his impetus. But the four-layer diode at that time was out already. And he knew that he couldn't achieve what he wanted to do here. And Fairchild, and then the other semiconductor companies, were very successful. So he changed the outlook of his life and also his relation to people, like myself and to other people. But he continued to be a patriotic American. The more I think about it, and when coming over here…I gave talks in Germany and I had a publication in Germany's most important daily newspaper on 50 years of Silicon Valley, and I thought a lot about it. And he was a patriotic American. He was almost embarrassed to have been born in London by accident.

He said, "The Second World War was won by scientific superiority, with lots of European people, Einstein, Fermi, having helped. And in the rather drastic Cold War with the Soviet Union and the excellence in Russia of science education, mathematics education, really very, very excellent education, we need this tool. Maybe our school system is not good enough, and we need to bring up the intelligent people with high intelligence quotients. But we do just the opposite with the welfare state." That made him very, very unhappy and he thought that if we continue along those lines, we will not be able to be victorious in the Cold War. Of course, he had all the access to the information clearances and so on [regarding] what the Russians were doing, and especially the shock of the Sputnik. You cannot imagine what a shock this was to this country, really. These guys sent up a satellite and you could hear it throughout the world. It makes beep beep. And Werner von Braun and the others had a rough time getting the rockets going and the electronics with it.

So a patriotic feeling for the scientific impact and the will to think scientifically in the Cold War times was the root of many of his doings, which were not seen as such by the public, especially by the politicians who wanted to get the vote of the inner cities, for example. And when he said we should send condoms to India, Catholic Senator Kennedy from Massachusetts said, “This is impossible. We cannot do that.” Abortion and condoms and all that. So he got deeply involved. He was sure that his convictions were right. But he made enemies. Tragic person. And you know that as far as Silicon Valley is concerned, people call him the Moses of Silicon Valley. He showed the way to the promised technical land, but he did not get there himself. A tragic figure. And also the success of the “Traitorous Eight”, the people he called, who wanted to make transistors at Shockley Transistor, and eventually did it at Fairchild, and then at Intel, was also like a Greek tragedy. Many of the top scientific people, very strongly focused on science, had a very unfortunate personal life. And Shockley was one of them.

Addison: Let’s just talk about the last few years of Shockley Labs, or Shockley Transistor, as it was called. Were you privy to any of the discussions with Arnold Beckman when he wanted to sell, and then Clevite buying?

Queisser: No. I met Arnold Beckman just once or twice when he came, but he didn’t talk to us very much. But he had somebody whom he confided in who came and then talked to us. He must have been somebody also putting money into Shockley Transistors, just like Beckman. And he wanted to sell us, yes. By that time the East Coast companies where the transistor had not originated really, although it was an East Coast invention…there were great companies like General Electric, like Westinghouse, RCA, and so on, but Silicon Valley was where there was land available and there was no conservative established industry like in Detroit or New York and so on. If you have something totally new, you must go to a new area which is open, in many ways open as far as land is concerned, open as far as the ideas are concerned, and people like Clevite and the Fairchild people realized soon enough that silicon was here to stay and it was very interesting, very important, and you needed it. But then many companies came. They looked at us. It was like on an old fashioned antique slave market. People came in and said, “Who are you?” Just like in the old days they would see you, and slaves had good muscles and so on. They ask you do you understand what’s going on here in silicon, and what kind of patents and what kind of publications and what do you know? So it was a very strange thing that we were on sale. Equipment, people, patents, and Shockley. So the Clevite people came. They utilized our know how. They had been making germanium diodes, which were lousy devices, and sold them to IBM. But eventually, of course, they caught on and took our silicon know how. But they found it very difficult to compete and we were sold again to IT&T, which also eventually abandoned all of the semiconductor activities. So there are not just glorious success stories in Silicon Valley, but also the other side, the dark side of the failures.

Addison: How did the company change after Clevite bought it?

Queisser: Well, we got a new building in Stanford Industrial Park, very nice new building. And we were quite successful. We did very well with Epitaxial. We made a very nice high frequency, high power transistor for them. Shockley lost more of his role and rule and the Clevite people were at the reigns. They came and said, "We'll do that", and transferred the know how to Waltham, Massachusetts. So I had to go there, Waltham, Massachusetts, together with my crystal grower. He told them how to grow silicon, which is much more difficult germanium. I had to go to Clevite in Cleveland. Made lectures on solar cells. We discussed whether their idea of say cadmium selenide solar cells versus silicon solar cells. So they [Clevite] utilized our know how. IT&T not so much, but Clevite really did. And Clevite had a German subsidiary. They sent the people over to us to stay for a week or two and learn about silicon technology in the new building. So it was quite different. It was more professional in many ways, but with decisive loss of control by Shockley himself.

Addison: Can you talk about how you left the company, the events that led up to your departure from the company.

Queisser: I was asked to become a visiting professor in Germany. And I thought it would be nice to do that. So in 1963 we had our first child here, Stanford born, little girl. We packed up and went to Germany. I taught and I thought I know so much now about semiconductors and this is going to happen in Europe, too, which it didn't. So I spent a year there at the University of Frankfurt, and then the recruiting people from Bell Labs who recruited in Europe came, talked to me, and said, "Why don't you come to Bell Labs?" So we packed up again. Went again over the Atlantic Ocean by ship, and I then did work on gallium arsenide and optoelectronics. I invented a very efficient infrared diode, which was used for remote control, television sets. That patent was very successful. I had to go to the consulate later on to sign. The patent was applied for in many countries, but AT&T didn't give you a bonus on that. You signed your right over to Western Electric.

So that was actually very nice, and then I got an offer as a full professor of physics in Germany, Frankfurt again. They must have liked me when I taught there. So it was great fun and, again, I had the illusionary thinking that Germany with its prior activities, Walter Schottky, who really understood what a PN junction rectifier was, and Siemens and other companies, and especially chemical companies, they still make some of the best silicon even nowadays, that this would come about. And [the attitude] was, again, with these German industries, no, no. If the Japanese are doing it, we cannot compete with them, which may have been correct. We just buy this stuff. German radio and television companies said, "Texas Instruments sells us all the necessary semiconductors. It's just like granulated plastics that we buy." They did not foresee what was coming on. Siemens for a long time thought silicon was just for power applications. Germanium transistors were very good. The boss of research of Siemens told me that in 1962 as a young man, yes, you do that for the rockets and so on. Totally wrong. The Japanese came to the Shockley Transistor barn very often. They realized very soon that it had to be silicon. So eventually the research boss of Sony, he came very often…and [he was] very sharp and realized what the potential of silicon microelectronics was, but not the Germans. No. They had their relations to the East Coast companies and, of course, they didn't do it either. There is no semiconductor at GE. Westinghouse semiconductors, no. RCA is gone.

Addison: So somebody from Sony came to visit Shockley Labs?

Queisser: Somebody from Sony…Dr. Kikuchu, who was very well known in Japan because he has television shows of science and so on, he then became the research boss of Sony, and they did it and they knew it. And the first portable radio was done at Sony, and almost at the same time at Texas Instruments. So the portability, the low energy consumption, of silicon integrated circuits, or even silicon transistors at that time…you could make it portable, not just for pacemaker or hearing aid, but also for other things. They saw that very, very soon. And the Japanese were desperate in a way. They had to get started. Germany still had a very effective and successful automotive industry. The [VW] Beetle was imported to the United States at quite a rate. Mercedes also. Machine tools. So they said, "Why go into a very difficult research needing, competitive field like semiconductors?" Big mistake. And, of course, there was also the drive by the military in Germany. Even the telephone systems, NATO, German companies could not get in. It was American telephone systems. We had to supply soldiers and tanks. So that was a totally different thing.

I was about to come back to the United States when they said, "Now, you have complained so much about semiconductor research development and so on, we'll make a Max Planck Institute in Stuttgart, and you be a director, and you better behave and so something reasonable." So, again, I had a big challenge, and we set up an institute. It was very tough at first, but eventually became a rather well known thing and one of my PhD students got the Nobel Prize for physics. One of my colleagues had a Nobel Prize. And we can compete on the scientific and with the best labs in the world, and that was very gratifying. So I do not complain that I did the wrong thing, that I went not into nuclear physics like most of my fellow students did. I went into solid state as it's called today. I liked the challenges and opportunities, with all the difficulties and all the toughness here in Silicon Valley, and later at Bell Labs. So I'm quite grateful and a wonderful thing to have been here at the very first time of the start of Silicon Valley. Of course, I came back later and I talked to Bob Noyce and Gordon Moore, and to other people. So I am very, very grateful.

Addison: That is kind of a nice way to end, but I just had a few follow up questions if I could ask. Did many other overseas visitors come to Shockley Transistor…you mentioned, Sony. Were there a lot of other overseas visitors coming to meet Shockley?

Queisser: Mostly the Sony guy who came regularly.

Addison: That was during the time you were there?

Queisser: Yes, yes. When I was there, and we always took this guy out for dinner when he came and visited. The French were interested. De Gaulle came to Stanford and he was shown some of the semiconductor factories. And he knew that this was important, and he tried to do something like that. Was a German visitor, the inventor of the III-V compound semiconductor from Siemens, Mr. Welker, whom I got to know very closely later on. But I think he went to San Francisco to Fisherman's Wharf and maybe to Yosemite, but to a little apricot barn where silicon was done, no. Hardly any Germans came. But then, of course, the German subsidiary of Clevite, these people came. And they really picked up and they really got all the information out to treat silicon. Koreans at that time, no, there was nothing going on in Korea. China was totally out at that time. So it was just a few Japanese. And in principal, Charles De Gaulle was there and looked at Stanford, a good university, but took a long time for the French to really see what was going on. It was germanium for some time in France, also.

Addison: Did Dr. Shockley recognize that the Japanese would be a future competitor?

Queisser: Oh, yes. Absolutely. And, again, I would say he was a patriotic American. He wanted to keep things here.

Addison: So he saw them as a threat.

Queisser: He saw them as a competitor, a threat, maybe a little bit too hard, but as a very viable competitor. He went to Europe quite often, Shockley did. There was a big conference in semiconductors in Prague in 1958, where he gave a major paper on PN junction of physics, of PN junction. He had good relations to the Siemens people and also to Britain. Hardly any British people came although, of course, radar, Royal Radar Establishment was quite a semiconductive thing. And in the 1960s and 1970s the British were not so bad with micro electronics. But not anymore.

Addison: Also you mentioned Bob Noyce and Gordon Moore. Did Dr. Shockley ever talk about them? Was he bitter about the fact that they left?

Queisser: He was bitter. In my first years with Shockley, we did not want to touch that subject. Bob Noyce came to my talk when I talked about the solar battery, because there was something like the principle of integrated circuits. He listened closely, smiled. Didn't talk to me. Went out, because he was afraid, of course, that we might have something against his patent. We could have, at that time. We really could have, because we made interconnects. We made metallic interconnects. Yes, we could have fought his patent. But at that time, we were sold again from Clevite maybe to IT&T, and so on. Shockley did not want to do that. Later on, I talked to Bob Noyce quite often. Showed me what he was doing. He consoled me on the activities at Siemens, and so on. He had very critical times at Intel. For awhile it looked very, very difficult, and he even came to Siemens to try to get some money there. Zilog, AMI and AMD and so on, came to Germany quite often. I was on the board of directors of Bosch Automotive Electronics. They had certain interest here with semiconductors. They did a good job now, both on very low cost rectifiers for automotive starters, but now especially for sensors. And I think they are the leading people for automotive sensors, exhaust, engine control, stability of the car, and so on. And the silicon chemistry people, I gave a talk once in Germany. It must have been in 1962, and they said, "What's going on here?" So the one man who had invited me to repeat the talk at the factory where they made the silicon, said "Do you think there's a future of silicon? Isn't it just germanium?" And I said, "No. I can put my head here and you can guillotine me, but silicon is coming." But he said, "My people, my bosses and my investors don't believe it. And the Wagner family goes to Bayreuth. We have Wacker festival all the time, and they say, "Oh, silicon is just a minor thing for high power." I said, "Don't believe it. Go into it." And for awhile Wacker was number one as a supplier of silicon. I said, "We have to have a factory on the West Coast." Ah, the people said, "No. We have such good relations to people in Georgia and we can easily do that." I said, "No. I am sorry. I insist that you look at possibilities on the West Coast, the same time zone as Intel and Zilog and National Semiconductor and so on." So eventually they opened a factory in Portland, Oregon.

Addison: Just a few brief words on the integrated circuit. When did you realize that was going to be something big? I know it was slow to take off.

Queisser: It was very slow to take off, and these people at Fairchild worked very, very hard. And we did, too. And when the first flip-flop came out with four devices on one chip, we said, “My God, these Fairchild people are gutsy and courageous.” The probability of getting a good PN junction for a transistor was so low that four together, you know, you get this to the 4^{th} power. But I realized that and one thing…in 1959, the winter meeting of the American Physical Society in Pasadena, California, I went there with Shockley. Shockley took me along, educational thing. I became a member in 1959. I am now a Fellow of the American Physical Society. And they said, “What are you doing tonight, Hans?” I said, “Well, I am here with my aunt, and she wants to take me out for dinner.” “No”, he said, “you cannot do that. You have to come to the after dinner talk.” Cost money. Banquet. I had no money, of course. I came with almost zero money and all I had was my salary. “No, no”, he said. “You have to go there. You have to come there because one of my very best students, a guy by the name of Richard Feynman will give the after dinner speech.” And that was the famous talk by Feynman, “There’s Plenty of Room at the Bottom.” And I had the great honor sitting even on the speaker’s table, and we had the chicken and peas, the usual thing, banquet thing. And that talk was extremely motivating and encouraging, because Richard Feynman said, “There is no law of making things very small.” He thought more about little lasers and so on. But, of course, we thought that we could make very small semiconductor amplifiers, rectifiers, and so on, putting things together. And Shockley always said, “Yes. I know how difficult it is for you to get a good junction, but there is no law of physics against it.” Just like Dick Feynman told us in his famous and charming and humorous, nice talk. And it has been reprinted several times, “There’s Plenty of Room at the Bottom.” So we knew it would be coming, but it was very, very difficult to do it. And eventually, of course, it came. But the amount of work that the people here, the early pioneers of Silicon Valley, put into it, should not be underestimated. And one man once told me, “You know, about capitalism, there’s one thing. It sure extracts energy out of people.” And that was done here at Silicon Valley. Remarkable. And the openness that people would go to diners or to the Wagon Wheel bar and you would meet people and there was no secrecy and there was not the hierarchical thing that you have say at IBM. IBM did a good job, too, on semiconductors. No question. But other hierarchical east coast or European-type companies, we didn’t have that here. People would go out. When Fairchild didn’t pay a bonus to Bob Noyce and the other seven, they said, “Okay. Let’s start our own company.” And then manufacturing equipment and chemicals and so on, the whole thing together, in an area where there was no conventional industry, which would control the labor market and would control politics and so on, it needed something like Silicon Valley, the Santa Clara Peninsula. Maybe even in southern California where the aircraft industry is dominating and domineering wouldn’t have worked.

Addison: Thank you very much, Hans.

END OF INTERVIEW

