Oral History of Shockley Semiconductor Laboratory

Participants:
James Gibbons
Jay Last
Hans Queisser
Harry Sello

Moderated by:
Michael Riordan

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Michael Riordan: We are interviewing Jay Last, Hans Queisser, Jim Gibbons, and Harry Sello at the Computer History Museum. It's the 27th of February, 2006. I brought you here together to address a few questions I feel we cannot address this evening at the public panel, either because they're too discursive given the time constraints or too technical for that general audience. <camera pans over panel> So let me begin by asking you each to tell me how you were recruited by William Shockley to work at Shockley Semiconductor Lab or Shockley Transistor Corporation, starting in the order of time, Jay Last.

Jay Last: I was working at MIT, finishing my doctoral thesis and was using a Beckman Instruments spectrophotometer in order to do my work. This was an instrument that was a little beyond Beckman's capabilities, so I worked with the Beckman people making it workable. So by the time I got my degree, a lot of people in Beckman knew me. And Arnold Beckman, when he signed his agreement with Shockley in about September of 1955, told Bill Shockley about me, and Shockley flew up to Boston to MIT to talk to me. So I was extremely impressed with him, and had a couple more talks with him. After that, I came to work for him.

Riordan: Do you have any particular remembrances of that encounter?

Last: I have very strong remembrances.

Riordan: Could you share them with us?

Last: Just seeing how Shockley was changing his life from the Bell Labs days, changing his life both personally and technically and wanted to start this company—and the nature of the work I would have with him. I was very impressed with what a quick study he was with my thesis. I was mentioning some problems I had and he immediately answered them. I was just flabbergasted! We talked in my lab at MIT for awhile, then a couple weeks later, I flew to Washington to meet him for breakfast at the Cosmos Club, and every famous guy I'd ever heard was having breakfast with Shockley—Vannevar Bush, and all these admirals and generals. It was a very impressive sight.

Riordan: Was he coming by to say "Hi" as you sat there at the Cosmos Club?

Last: No, I sat down and had breakfast with all these guys, and Shockley was having breakfast, too. After that, he and I talked for a couple hours, and I got more and more impressed with him and eventually came to work for him.

Riordan: I think you mentioned you were one of the earliest employees of the Shockley Laboratory?

Last: Yes, I accepted in December, and by then he had hired four people. This is a part of what made me get into it a bit this evening.

Riordan: Harry Sello, you're the next in the sequence.

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Harry Sello: I was thinking about the question, and I don't know how I answer it, because I never really did find out how Shockley got ahold of me. ,<camera pans over to Sello> I really don't know as of this day. If I were to place a bet, I couldn't place it. I was working at the Shell Development Company, which was in Emeryville, California, which is a little bit upstream or north of where the Shockley Laboratory was, and I just got this phone call. After asking me my name, he said, "Would you like to come to work with me," and I said, "Who are you?" <laughter> What else could I say? There was a pause after [he replied,] "William Shockley," and he said, "Do you know who I am?" I said, "Yes, Dr. Shockley, I know exactly who you are." He said, "Would you consider working in my laboratory?" <laughs> I was just flabbergasted.! Of course I would consider it. I didn't even assume that it was an offer. But to this day, I think [his interest] had some bearing on the fact that my physical chemistry, unlike Gordon Moore's physical chemistry--had a lot of organic componentry in its background: Shell Development, oil products. I think he was looking for that kind of exposure, which was something he didn't have in the laboratory at that particular time. But I really can't answer the question clearly.

Riordan: Was he considering you because he was looking at using black wax as a masking agent?

Sello: Very good question, Michael. He was considering getting rid of black wax <laughs>. That was why he was looking at me, it turns out.

Riordan: Now the other thought I had, I know that at a certain point Walter Brattain's brother, Robert, was working at Shell in Emeryville.

Sello: Yes, I knew Bob Brattain.

Riordan: Was he contemporaneous with you?

Sello: He was contemporaneous. He was in the physics department, so I pestered the hell out of him and I got him to confess that there was no connection with what he was doing. But he did like to offer [me his opinions], a couple of months later. He said look at it very carefully; there seemed to be some difficulties with that company. I paid no attention to that.

Riordan: So you were forewarned before you stepped in.

Sello: Yes, but it had no meaning to me. This was the Nobel Prize winner, and that was number one.

Riordan: This would have been early 1957.

Sello: Right, about February of 1957, and I came to work in April.

Riordan: Jim, you're next in line.

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Jim Gibbons: Whether Shockley hired me or was stuck with me, I'm not sure—probably the latter. <camera pans to Gibbons> My association with him actually began at Stanford because Fred Terman, {Ed. Note: Stanford Provost and Dean of Engineering] wishing to transistorize a curriculum, had brought John Linville from Bell Labs to do that. I had been John's first Ph.D. student, and when I graduated, I went to Bell Labs and got an immediate release from my employment to go to Cambridge for a Fulbright Fellowship. While I was there, Bill formed his company, actually slightly before that. But Linville then got interested in what the possibilities might be for Stanford to have some kind of a laboratory. Was it even possible for this to happen? Would it be reasonable for electrical engineering students or engineering students to build semiconductor devices, since that hadn't been part of the previous curriculum?. When it was vacuum tube technology, universities didn't build vacuum tubes, and there was a pretty negative reaction to even trying this at universities around the country—in Boston, our colleagues at MIT, and elsewhere. They thought this was a guaranteed failure, and you're going to kill people, which we might well have done, after all. But anyway, John and Bill had this meeting and Bill said "Oh yeah. The right thing to do is to set up an experimental lab. And if you want to do it, I can bring somebody here, apprentice them to me, and I can teach them how to do this."

So then I got a letter in England from John asking me if I wanted to do it, and I thought, "Well, it's worth a couple years to see what happens." They'd arranged for me to have a two-year leave. I was on the Stanford faculty from the beginning, but I had a half-time leave for two years to work with Shockley. So then, when I wrote back and said I'd be interested, I got a letter from Shockley, which was my first interaction with him. The thing I remember most about the letter is the fact that—it was a two-page letter—most of it was consumed with talking about the fact that he thought he had some rather smart people that were working with him at the time, but that they thought they had Ph.D. post-docs. They weren't working the way he liked to work. I wrote a letter back to him saying, "I'd be happy to try to do this, and if I don't put my shoulder to the wheel and make it turn, then I expect you to fire me." Then he wrote back and said "Okay, that's acceptable. I'll see you when you get here." I first met him on my first day, not knowing that his wife was also his secretary. So I showed up at that point in time. . .

Riordan: What time was that?

Gibbons: Nine o'clock in the morning, August 1, 1957. So I introduced myself to Mrs. Shockley, or to his secretary, and she said "Well, Dr. Shockley will see you shortly." And it was a long time. I figured he was in there talking to somebody, but I couldn't hear anything, and finally she said "He'll see you now." I walked in, and I got Shockley's idea of what an IQ test was, which I guess I did well enough on that he decided not to throw me out right there. So then he took me out and introduced me to Bob Noyce and said, "Here's the guy you're going to learn device design from, and he'll introduce you to other people who can teach you about diffusion and technicians who can help you with equipment design and setup. If you need people to come to Stanford to help you with that stuff, let me know." So we were off and running.

Riordan: And this was right in the midst of the time when the Eight were leaving or preparing to leave for a Fairchild.

Gibbons: If my recollection is correct, there's some stuff on a plaque--what did we call that thing yesterday? There's a display that has been created by the Computer History Museum, and the date that Gordon Moore went into Shockley's office to announce that they were leaving, I believe was September

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18, 1957, and then Beckman came up on September 19. And they finally left October 1. I believe it was their first day of employment [at Fairchild]. So I had been there two months when my coaches left.

Last: But Shockley was well aware [of the problems] long before that.

Gibbons: I didn't know anything about it. He didn't tell me anything about this when I was being interviewed.

Sello: I never came to the laboratory before going through the Saturday and Sunday full of psychological tests in San Francisco. It was only then that I got to meet the people at the laboratory <camera pans to Sello>.

Gibbons: I didn't have the same relationship to him as somebody who was working full time. I never did. I think Hans said once "Well, but you've got a different relationship to him, right? You don't have to live with this all the time." You did say that.

Riordan: I think it's particularly valuable that you had this degree of independence, one foot in Shockley Semiconductor Laboratory.

Gibbons: But I must say that after the Fairchild Eight left, it was a major blow for Shockley. <camera pans back to Gibbons>. He didn't have anybody left except me and for awhile Harry and Tom Sah, and then they left. Here we were. I was all he had to focus on for awhile, so I'd show up, and my assignment was to help move things forward by helping with circuit design for *pnpn* devices. But then I got a chance to ask him anything I wanted about the lab. He was *fabulous!* "Here, you need to read this. Here's some ideas. Try these and let me know how they work. You got any problem, give me a call." Constantly encouraging. Of course, he'd just been through this other deal . . . And then he was also continuing to write scientific papers, and he was looking for co-authors, so I co-authored a couple of papers [with him]. It was an exhilarating time for me. It took six months from a cold start to get our device lab up, making low-loss *pn*-junction diodes for parametric amplifiers, instead of a year and a half. So that was something else that couldn't have happened without him.

Sello: You didn't make those at the Shockley Semiconductor. . .

Gibbons: No, we made them at Stanford. I made them in the lab at Stanford. We started . . . T=0 was August 1 and six months later, we had those devices up, and that meant the lab was running.

Riordan: There were just graduate students in the lab.

Gibbons: Yeah, just graduate students, just electrical engineering graduate students who wanted to build these things for parametric amplifiers. So that was the idea. Can you build us an <unintelligible> device that would help you do your research? That was what we were trying to prove and we proved it.

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Riordan: Okay, and then for new staff at Shockley Semiconductor Labs, after the departure of the Eight, he turned largely to recruiting in Europe. I can recall several names. One was Adolph Gutsburger . . .

Gibbons: Goetzberger. He was first I think, wasn't he? The first to show up.

Hans Queisser: <camera zooms out to full panel> It could be Kurt Huebner, who came from Virginia, where he was with GE (General Electric), and he hired him first.

Sello: I think he came after Goetzberger, but it's close.

Riordan: So why don't I direct the next question to Hans. How were you approached by Shockley?

Queisser: I did my studies in Germany, my native country. <camera zooms in on Queisser> But I was fortunate enough in 1951 to get a scholarship to come to the United States. I applied and got that scholarship, although it was re-education to democracy for Germany, and they didn't want any medical or any engineering or scientific people, but mostly journalists and preachers. Somehow, I got it and I went to the great state of Kansas.

Gibbons: It *is* a great state. I'm from there. That doesn't make it great, of course.

Queisser: I had a very nice time there. No problem with the English language, which most of my fellow students had, so I used to go to the library and read *Physical Review*, a journal for physics and so on, but also the ads in *Physics Today* because I wanted, as soon as I got my Ph.D. in solid-state physics in 1958, to come to the United States. Most of the companies I wrote to said, "Sorry, we can only take US citizens." One of my professors, a theorist on phonons, defects, dislocations and so on, knew about that, and one day he came up and said, "I have here a letter from Shockley," (or maybe he met him at a conference in England) "and he is looking for a guy who would like to do some work on defects in solid-state materials." I said, "Yes, oh yes, I'm very much interested in that." Without any psychological testing or entrance exam, I was then hired. I had a little bit of difficulty getting a visa so Shockley wrote to the Secretary of the Air Force that I was needed, of course, with all the nice adjectives. So immediately I got a green card and visa at the consulate in Hamburg.

In I think October 1959, I came and saw Shockley for the first time on my first day, and he said "Well, we have to do something on solar batteries. We have two contracts. On one we've spent most of the money, something like 75 percent of the money, but with very little results to be shown, and the other one was more a theoretical thing." So I asked, "Well, are we going to make and sell, solar cells?" "No, no," he said. "No, but silicon seems to be the ideal material to make solar cells." You must remember this was the time of the *Sputnik* shock. and suddenly the Air Force wanted to know what was going on. So I started there. It was a lot of work. I went to the labs very often at night and on weekends, made a multi-cell solar battery, which was in a way the first integrated circuit but with a terrible technology. I mean, I had to make the fuses from either side <he motions with his arms> of a 50-micron slice of silicon, but I got a few samples which gave me an output voltage of over 10 volts. Remarkable. Not bad.

Last: No psychological tests?

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Queisser: No psychological tests at all, although, somehow, Mrs. Shockley seemed to have gotten me after I had breakfast at the motel, still jet-lagged. So she picked me up. The test was not much more than a nice little conversation.

Last: "How do you enjoy breakfast . . ."

Queisser: Something like that and how the weather was in Germany. She talked of non-committal topics of work. But then, of course, the famous thing that Shockley would take people to lunch at Kirk's. He took the yellow pad and his pencil, and this was like another oral Ph.D. examination. It was quite, quite tough and stringent, but useful I tell you.

Gibbons: If he didn't have his yellow pad, he wrote on the napkins <camera pans to Gibbons>.

Queisser: That's right.

Gibbons: Around hamburger drippings. The thing I most miss about this is not having saved those napkins. I wish I had those napkins.

Riordan: Did you say in your letter . . .

Gibbons: I think I did, but I can't find it.

Riordan: Do you have an archive at Stanford?

Gibbons: Yeah, but it's not in there.

Riordan: There are some wonderful things in the Stanford archive. I'll say a few things about it. In fact, some of them I found there I have here, and I'd like to move on to the next subject. <camera zooms out to full panel> One of the subjects that really intrigues me is this, why did Shockley choose the Santa Clara Valley? Why in the vicinity of Stanford? And I think the comments of everyone here is that Shockley grew up there, and his mother lived there. But I think there's much more to it than that. I can pinpoint when he made the choice. It was between October and November of 1955. Beckman himself would've preferred a location closer to his home offices in Fullerton, but I think Shockley convinced him otherwise. There's a couple of letters exchanged between the two of them, between October 31st and November 11th, 1955. <passes letters to Last, who begins reading one> Why do you think he chose the Santa Clara Valley? It has had an *enormous* impact on the technological, industrial, and economic landscape of America that he chose to come here. It's one of the things historians call contingencies, that could've gone some other way but went this way, that I'd like to learn more about.

Last: He was thinking about this question. In my first interview with him, he said "Where I'd really like to put this plant, put my plant, is in Hawaii, but the logistics of working in Hawaii would be horrible." <camera

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zooms in to Last> He couldn't have done that. I think the benevolent climate entered into this decision, and certainly the presence of his home and the presence of his mother were factors that entered into it strongly.

Sello: There was another factor that crept in just a couple times in conversations. <camera pans to Sello> I think he was certainly very bright, enough to realize that the guys that he recruited wanted to go and be in this area. I was in Berkeley, and Berkeley is—to some people—heaven, but I wanted to come on the other side [of San Francisco Bay] where the sun was. And there were some guys who liked that part very much. So I think that played a role.

Queisser: He told me that he seriously considered going to Southern California. <camera pans to Queisser> California definitely—away from the conservative industrial situation on the East Coast. And of course the East Coast, except maybe IBM, did not succeed in modern semiconductor electronics. RCA is no longer in existence. I don't want to talk about Bell Labs; it's too sad. General Electric, Westinghouse . . . So he wanted to go west and to Southern California—he went to Hollywood High School, remember—it was definitely his first choice. But he told me, "It is so bad there, the smog—you can't even see the hand before your eyes—that I decided against it." And you know he was very active—he often talked to me about that—[on behalf of] the Clean Air Act for Northern California. And he used the principle of equal unattractiveness, was afraid that the same smog would come to Northern California. The vicinity of Stanford was not that significant. He didn't say that "I want to go to Stanford," but he wanted to go to Northern California, and a wonderful choice it was. Relatively open country. No conservative industry around that would sort of control the policy, the labor market, and everything. So he came here.

Gibbons: I want to . . .

Riordan: Say a word for Stanford.

Gibbons: <camera pans to Gibbons> No, because as I've traveled around the country and the world to some extent talking about Stanford and the Silicon Valley, one of the points I try to make first is that for silicon technology, Stanford had relatively little to do with it. My source for this is usually Gordon Moore, talking about how long it was before anything that happened at Stanford actually found its way into . . .

Queisser: The physics department, horrible.

Gibbons: Well, and electrical engineering, whatever it was. Ion implantation, according to Gordon, was the first thing that was done at a university that actually found its way into semiconductor technology. And part of that was because there was this huge backlog of know-how everywhere else but where Stanford's concerned. I think both John Linville and Bill worked at Bell Labs, and when we got the lab up and running, all of a sudden we got to be very interesting to people like Gerald Pearson and John Moll. And I'm not so sure whether somewhere in their thinking, both Linville and Shockley thought that if we make this lab go, we might be a lot more attractive for people from Bell Labs. I could see Shockley thinking about this, that far in advance, and imagining that he would like to have here as consultants maybe Ian Ross, maybe Jim Early, who would be attracted to the area because there was a [good] university around. Maybe that's where they would have their appointment and they would be consultants. You and I

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have talked about this briefly. Shockley never said anything like that to me directly. He said the same thing to me that you just heard: “My mother lives here (She liked to talk about herself as the grandmother of the transistor), and this is a great place to live and work.”

Queisser: The closeness San Francisco, the city.

Gibbons: And close to San Francisco. If he had visitors here, and he didn’t want to go out with them, you could go anywhere you wanted up there, take these guys to dinner. I went to some of the best restaurants I’ve ever been to because Shockley didn’t want to go. But I think where Stanford’s concerned, he didn’t think he needed Stanford, but he did think Stanford might need him, and that he would be an attractive force that Stanford could use. And he was quite happy to see that happen, I believe.

Sello: I remember a conversation at lunch after a swim in the Stanford pool, <camera pans to Sello> where Shockley clearly said he’d never be a Southern Californian. That was Hollywood. That was not his métier. And he left it here.

Riordan: And he was completely familiar with it from Caltech [California Institute of Technology] and Hollywood High.

Sello: He just left it there, to say that Stanford was a [good] place, or Berkeley, but it wasn’t going to be Southern California.

Riordan: Critique the following working hypothesis I have. I feel that initially, his plan for building this company was to attract a number of his colleagues from Bell Labs. <camera zooms out to full panel> You can see it in one of those letters from Beckman saying “Is it true that Messrs. Sparks and Wallace are not going to come?” So they were certainly on the table, and I know for a fact that he tried to recruit Morry Tannenbaum, Ian Ross and Jim Early.

Gibbons: And they wouldn’t come.

Riordan: I think he had to offer these people something special, and I think Jim [Gibbons] is the best example of this, that if he offered them a chance also to teach at one of the emerging universities, one of the leading, emerging, technically-oriented universities in the country, that would be the something extra that a Bell Labs person would insist on. Critique that. Why don’t we start with you, Jim.

Gibbons: Well, it’s certainly true for Gerald Pearson. He knew that by coming here, he could really help Stanford. He knew he could work with Shockley, but he didn’t want to be employed by Shockley. Same for John Moll. There was an uneasy relationship between those two for a long, long time, which started . . .

Sello: And absolutely the same for Morry Tannenbaum. There was no question.

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Gibbons: But two of John's most famous papers, first one was written with Jim Ebers and the second one by himself, and the one that he and Ebers wrote [Ed. Note: J. J. Ebers and J. L. Moll, "Large-Signal Behavior of Junction Transistors," *Proc. IRE*, Vol 42 (December 1954), 1762-72] became the text for most electrical engineering undergraduate introductions to this subject. Shockley came around saying "I've thought of those kinds of things," and wanted to write this paper with him, and Moll said, "Get the heck out of here, man. We're not working with you." And there was a very uneasy relationship with John, even after they got out here. They wrote some papers together. Moll was perfectly happy talking with anybody about anything, but he knew what he had done, and he wanted to write up what he had done, and if somebody wasn't a co-author, he wasn't a co-author. He was pretty clear about this, and so he and Shockley had a working relationship, but it wasn't . . .

Sello: But your thesis is well taken. <camera pans to Gibbons and Sello> If you assume he was trying to attract the people from Bell Labs that he wanted and didn't get, that was the largest single fraction of any kind of a definable bunch that didn't want to come.

Last: I was destined to go to Bell Labs, and they had assumed I was coming there. <camera pans to Last and Queisser> Then I said I wanted to go to work for Shockley, and Ian Ross and some of the others really gave me a story that it was not going to be a bed of roses at all <laughter>. I could see then that Shockley wasn't going to get any of these people. There were very strong feelings about Shockley.

Gibbons: And you chose to come anyway!

Queisser: Just a few days before I left, going into Palo Alto, Mountain View, John Bardeen came to visit our university. My boss toured him around. <camera zooms in on Queisser> They came to my lab, and he said, "Oh, by the way this young man is soon going to join Shockley Transistor Corporation." John Bardeen got closer to me, looked at me, <Queisser turns and looks at Last> and said, "Well, we'll see how you like Shockley." <laughter> Oh my gosh! Later, I went very often to Bell Labs because we had a very nice licensing agreement with them during my times; I traveled to Murray Hill every two or three months. So we learned about diffusions, and we talked to all these people. And it was interesting whether I was with Shockley visiting, or just by myself. I heard comments about Jim Early: "Jim Early would never have come to work under Shockley. Absolutely not."

But then, after awhile, Bell Labs people used to come and listen to our stuff. And one other short story, which is interesting: I gave a talk on the creation of dislocations when I did a heavy-boron emitter-type diffusion. I gave a talk at an American Physical Society meeting in Monterey. Shockley was sitting in the last row, together with his old friends, Chenoweth and I think also Jim Early and a few others. And Shockley said, "Well, how do you like that? Nice paper isn't it?" The Bell Labs people said "No." "What do you mean 'No'?" [replied Shockley,] "We have the proof that this is so." The Bell Labs people said, "No, if this is so important, we would have found it first." This is the creative arrogance of Bell Labs, and I was at Bell Labs for some time. <camera zooms out to full panel> But that's the same of course with Gordon Teal at Texas Instruments, who probably wanted to hire a few Bell Labs people, too.

Last: Shockley still maintained close relationships with the top people at Bell Labs. We were getting all of their reports and silicon crystals and everything.

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Queisser: Abolutely. Everything!

Riordan: That brings me to my next set of questions. The next area I'd like to get into is how did the silicon technology, which was very new, especially the diffused-based silicon technology. How did it come across the country to Shockley Semiconductor Labs? Were there reports? Were there notes taken at the diffusion conference [Ed note: The third Transistor Technology Symposium, in 1956]?

Sello: I can give you a couple of good examples.

Riordan: One document that sends chills down my back, is a copy of a pre-print—this is back in the days before the web, of course—where the [Carl] Frosch and [Lincoln] Derick paper, "Surface Protection and Selective Masking During Diffusion in Silicon" [Ed. Note: eventually published in J. Electrochem. Soc., Vol. 107, No. 9 (September 1957), 547-552], arrived as a pre-print in December of 1956, because Shockley Labs was probably the last company to actually *buy* the Bell Labs' transistor patents, license the patents. They became public domain after a consent decree in 1956. But Shockley Labs got a lot of hand-holding from the patent license engineer at Western Electric. <Riordan passes paper to Last, who begins reading it.> Here's another document that came out of the Shockley Labs, and it has a routing list, a signed routing list. So I *know*, as a historian, that Gordon Moore [and the other Shockley technical staff] read this paper, probably in January of 1957.

Last: And there were a lot of these things flowing through there.

Queisser: We had the black book with the diffusion data, and others. Bell Labs had calculated the curves, the Gaussian distribution and so on. The information channel from Murray Hill into San Antonio Road was very open.

Sello: There was another aspect to this. I was lucky enough to be the recipient of some of this [important information]. Since I was then, accordingly to Shockley, some sort of a mysterious chemist with 'physical' attached to his name, he encouraged me and he pushed me to make several trips back to Bell Labs that he set up. Every one of them, Frosch and Derick, Ligenza (sp?), [William] Pfann, growing crystals, building furnaces—go see how they make them there. He encouraged all of this. He just pushed in that direction, and I felt very exhilarated about all of that because it wasn't a two-way street. The Bell Labs guys weren't coming the other way with their information, but Sah and I both got a ton of information from Frosch and Derick, both of these papers.

Riordan: Did people from Bell Labs come out to consult?

Gibbons: On the *pnpn* diode, yes and Ian Ross, since we were a licensee. Ian was out regularly to just make licensee visits, and we all had to talk about what we were doing.

Queisser: In the little conference room there.

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Gibbons: In the little conference room, yes. However much progress we made in building cross-point switches [Ed. Note: four-layer *pnpn* diodes] for telephone switching systems, it was more of "That's better than less of it," from Shockley's point of view. So there was a lot of "See, this thing really does work," and "Here's a good application for the Bell System." He saw his future in that application, his first major economic, commercial success, he thought.

Queisser: Absolutely.

Riordan: This was a part of the thrust of the whole Bell System toward electronic switching, I presume.

Gibbons: Yes, and this was a device which could do it. At the Kellogg switchboard in Chicago. I made lots of visits there to work with them on these circuits. I'd come back, and Bill would say, "Well, did you persuade them?" I'd reply, "Well, it didn't work exactly the way it was supposed to." He'd say, "Well, let's work on it."

Sello: This is a very anomalous paper, a very anomalous situation <referring to the Frosch and Derick paper, which he holds in his hands>. On the one hand, Shockley encouraged a number of us—Jay, a lot of them—to go back there and talk to the Bell people, but if you look at the signup sheet and finally ask yourself, "How many guys were anxious to work on this aspect of mesa transistors or just of diffusion transistors?" You'd think that there would have been big support of that by Shockley in his own laboratories, but there was absolutely none. Absolutely none.

Queisser: It was the four-layer diode, no question about it.

Sello: In fact, I was asked a couple of times if I was trying to bootleg something that I had learned at Bell without telling him about it.

Riordan: So you're saying Shockley himself didn't have any particular interest in his employees learning this [i.e., the Frosch and Derick paper and diffusion processes].

Sello: No, he wanted us to know what it was, but he wasn't interested in making devices. Not those devices.

Queisser: Even if we wanted to make solar cells. These were the first two projects I worked on.

Sello: Yes, but we were not involved in that one.

Riordan: We have about five more minutes on this tape, so I want to move on. We've established some of the channels for information transfer. On the lab floor, did Shockley Semiconductor Labs, prior to the departure of the Eight, push the silicon technology into any new areas? Or was it simply a matter of learning the existing industry know-how?

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Sello: We pushed it into support areas, but that's all. Support areas like crystal pulling, diffusion furnaces for silicon. All of the mechanics that go into working with silicon, but not devices.

Last: It was getting up to Bell's speed.

Gibbons: My recollection's a little different from that, because when the Fairchild Eight were leaving, there were seminars [in which], if I remember correctly, I was sitting there writing and asked a lot of questions. My recollection is that Bob Noyce was talking about the fact that they couldn't get photoresist to work. It wouldn't stick. It kept lifting [off the silicon surface], and they couldn't figure out why. He was genuinely involved in this. I can't remember the dates exactly, but I do remember listening to him in our little conference room, talking about the fact that this just didn't seem like it was going to work. I don't know how much later it was that it did actually work at Fairchild or elsewhere.

Queisser: Kodak Photo Resist—KPR.

Gibbons: Yes, but it wouldn't stick, and nobody could make it stick.

Riordan: This would be August or September of 1957.

Gibbons: Yes, it was August or September, because that's the only time I was there that they were.

Riordan: Pretty narrowly defined time.

Last: I don't have any recollection of using photoresist [at Shockley Labs].

Gibbons: Bob was trying to use it, but he couldn't make it work. He was just painting it on and trying to heat it. It would lift [off] and curl, and all that kind of stuff.

Sello: But that's exactly my point. The support work, not the devices.

Gibbons: Well, but it was aimed at being able to make a transistor eventually. That was why he was doing it. It was support work okay, but if you had it, you might be able to make a transistor.

Sello: Jim, I remember Shockley walking through the laboratory, and it was fresh in my mind because I was just a very new arrival. Victor Grinich and Gordon Moore were looking at a transistor on a curve tracer, and Shockley wanted to know what was the device. <Gibbons laughs> He knew damned well what the device was. It was a transistor, and it was a diffused transistor made according to Frosch and Derick and Morry Tannenbaum's information, but Shockley turned his back on it.

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Queisser: He wanted a new start. The transistor was old stuff for him, and he was looking into the future. One thing was very important, I think, the better understanding of junctions, like hardness of the reverse-biased junctions in the four-layer diode, and a proper understanding of the alpha, transistor alpha, as a function of current. So the very famous paper of Sah, Noyce and Shockley [Ed. Note: C. T. Sah, R. N. Noyce, and W. Shockley, "Carrier Generation and Recombination in P-N Junctions and P-N Junction Characteristics." Proc. IRE, Vol. 45, no. 9 (September 1957), 1228-43] That was a major contribution which was realized, recognized, and celebrated.

Riordan: Actually, I think I'll ask you about that in the evening proceedings, too.

Gibbons: But that aims you toward *pnpn* devices. There were transistor companies, [but] they weren't silicon transistor companies. People were making germanium transistors.

Sello: A world of difference.

Gibbons: Absolutely. This was a device where there was more physics in the device, in a way, and it was difficult, but simple to make, relatively speaking, compared to all the stuff you had to do to make a good transistor, and people were making transistors. <motions with hands> He didn't want to follow anybody in anything; he wanted to lead, and this was sort of a leadership position, to make the *pnpn* diode.

Queisser: With his name attached, the Shockley Four-Layer Diode.

Last: One thing that's key here is that Shockley did not encourage us to interact with each other. It had to come from Shockley on down. So a lot of stuff that went on there, I didn't learn about until times like today. <laughter> It was just a way of working, and when we went to Fairchild, it was just the opposite. We worked together extremely closely. You could see the power that comes from interaction of people, rather than one person channeling it all.

Riordan: I think that's a good way to end this discussion.

END OF INTERVIEW

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