INTERVIEWEE: Kurt Lehovec (session 2)

INTERVIEWER: David C. Brock

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BROCK: This is an interview with Kurt Lehovec by David Brock on August 31, 2007 in Williamstown, Massachusetts. And Kurt, I wanted to talk to you about—go back into the early period of your work at Sprague to talk to you a little bit more about the resources for getting your work going at Sprague. For example, personnel: when you were hired to get Sprague into semiconductor electronics, who did you then bring in? How big was your group? Who were the people? That sort of thing.

Well, when I came to Sprague, I was very happy because they gave me exactly what I had discussed with them—namely, a totally free hand to hire whomever I wanted to hire; no exception. Nobody had to approve the people I hired. I took only one man with me from the Signal Corps where I had worked. His name was Robert Bob Koch, K-O-C-H. And he had this _____ [00:01:43] at the Signal Corps developed. He vacuumed silicon. He vacuumed germanium-pulling equipment.

And of course, I attended for Sprague Electric the big conference by Bell Lab that they gave to all licensees, and I really didn't have to attend it because I knew all the hypothesis already. But of course, the first thing they told people is how to pull germanium crystals, and so

it was interesting to see everybody of these executives who attended with their technical underlings running to the next telephone and ordering an RF generator for pulling germanium crystals. A line in the telephone.

The first—these poor guys didn't realize that there's a long road of losing money before money comes in for transistors. They all jump on the goldmine. Well anyway, so Bob Koch came in to set up the germanium pulling, but I immediately decided that silicon is a surface, not germanium. So in the same room where the germanium pulling was, I set up a vacuum equipment to pull silicon crystals.

Now the problem there was the course of a year to hold liquid silicon, and I don't know how I decided it, but I jumped on board one night. I even saw that as a problem with the boron being a dopant agent to the silicon, but apparently it does not for silicon.

BROCK: So you were pulling these crystals within a vacuumed environment?

LEHOVEC: Within a vacuumed environment.

BROCK: So then you—I guess you were also using RF heating.

LEHOVEC: Yeah, right; RF heating coil into one.

BROCK: And so would the RF coupled to that boron-nitride crucible?

LEHOVEC: No, it may have been _____ [00:04:46]. I've forgotten now how I simply coils around the heating coils around the boron-nitride, but it may have been because _____ [00:05:02]. And it was interesting that the company named Pacific Semiconductors, and the man in charge of it was Mr. R.C. Sprague together. There must have been some possibility of buying his outfit by Sprague or the other way around.

And this was 1953 that he came to Sprague Electric, and he mentioned to us at Sprague, which pleased me to no end, that he is so astonished what I have achieved within one year at Sprague Electric that it would take others three or four years. Of course, R.C. Sprague was very happy to hear that.

Of the other people I hired, I wanted a practical mechanical engineer and I interviewed—we advertised, and I interviewed a young man named Armen Fermenian. And he came from Western Electric, and he told me he leaves Western Electric because he cannot stand the bureaucracy. And this right away say, "I hire you." Alone this statement, "He hates bureaucracy," say, "I hire you." And he was a real genius.

BROCK: He was a mechanical engineer?

LEHOVEC: He was a mechanical engineer. By using a sheet of metal strip which is sheered simply bent on a frame and there you have it rather than the pins with wires and coils adjusting them under a microscope.

BROCK: Right.

LEHOVEC: And then I hired another man named Belmont.

BROCK: What was his background and responsibility?

LEHOVEC: I had him on the silicon pulling crystals, and then making transistors by surface melting, where I coated as slice with a dopant and fused it in by radiant heat and made this. He used the course of some of my papers. And then there were two people—I don't want to give their names because they didn't work out.

One person was interesting why he didn't work out. He came also from Western Electric. If I tell them what I know, what is my position, so to speak? So he told me he has to hide his own knowledge in order to justify his job. And this, of course, is—but I still didn't fire him, but I wanted to fire him. But I never fired anybody. I don't do it because, in part, feeling for the families or for—I don't do it.

But I regretted having him. But then he was taken care of in a very interesting way. As I mentioned to you, they fired or disposed of the man who had hired me and hired a new man, Dr. Lazier [00:09:50], who had no background whatsoever, and he came from Pfizer from the drug industry. And it very soon became apparent in our group that he was hindering in us. He ordered us to stop silicon work and I, incredibly, agreed and gave up work on silicon.

Because he demanded we give up work on silicon, we must concentrate on one. In the future, we must concentrate on what everybody is doing.

BROCK: When was that in time?

LEHOVEC: That must have been—I wonder, but I'm not very good at timing. It must have been '54 or '55 in one of these things. And, Mr. Burly [00:10:52], who was—now I give his name. He's a guy from Western Electric who wanted to hoard his information. Otherwise, he

was a nice guy. And he noticed that Dr. Lazier is kind of opposing us, and he got so upset about it that he said, "I will go to the man and explain to him," and I said, "Don't do it. No, I will do it." He went into Dr. Lazier's office and he came out 15 minutes later, "I'm fired."

Well, so who were the others? Yeah. Then was the German guy named Rainer Zuleeg.

BROCK: What was the name? I'm sorry.

LEHOVEC: R-A-I-N-E-R Z-U-L-E-E-G. Got a degree in electrical engineering, and he came to the United States and to Washington D.C. and he wanted to marry a girl there whom he had known or whatever. I don't know how they got together. And this girl found a South American diplomat and married him and broke his heart.

And my brother wrote me, "There is this Mr. Zuleeg—Dr. Zuleeg—no, Mr. Zuleeg; he had no doctorate degree—in Washington D.C. with a degree in electrical engineering, and can you use him?" "Sure." I want diversified team. I had this Armen Fermenian as a practical guy. I had Robert Koch for big vacuum equipment pulling crystals. I needed a good high-level mechanical engineer and I needed for automation and I needed a chemist and I needed an electrical engineer.

So here was the electrical engineer. I got this Mr. Zuleeg up and he was our electrical engineer and he was also a very practical man in experimenting and so on, so he worked out well. Now I needed a mechanical engineer—a high-level automation engineer—because in the alloy junction transistors, you send this dot optically and automatically moves equipment in the right point, brings it down to the right point, heats it up the wire to diffuse it in this piece [00:14:07] of the wire and two wires stick out. But all this I wanted automatically was all done by hand. Microscope and girls [00:14:16] no good.

BROCK: This was to make the contact to the alloy?

LEHOVEC: Alloy dots to make the contact. And then, somebody from a German company _____ [00:14:41] for whom I made this invention of these aluminum magnifier [00:14:44]. He told me that one of the good mechanical engineers, Leo [00:14:56] Fedotowski—F-E-D-O-T-O-W-S-K-I—has immigrated to Canada and is in Montreal and has no job. So I traveled to Montreal and I found Leo Fedotowski with his wife, Molly, incredibly mechanically high-end: smallest dimension, moving, searching, all these things. And I saw great potential in him, but not Sprague, who didn't believe in mechanization and all this.

And so I had my mechanical engineer. Now I still looked for a chemist and I hired a man who was a professor in chemistry and wanted to go in industry, and he was another negative guy, but personally a very nice guy. He became the godfather of one of my children

and we had good family relation. A very nice guy, but technically a professor type of guy, not practical.

And he did things, for instance: since I couldn't use him technically, I thought I could use him as a personnel man for my group, and I asked him to make a time study of the making alloy junction transistors so that we can assess the pricing. So he came back with a time study and I looked at it and I said, "This can't be right. That is much too long." I went with him, I told _____ [00:17:14] time. He made in the time study for the technician who does it every day.

BROCK: To fabricate the alloy junction transistor?

LEHOVEC: Yeah right. So simple processes, I look at the processes, I do it unskilled. I don't expect then the skilled technician to do it faster. So I said, "How could you come up with this figure?" "Yes, but they get tired. They have to take breaks and stuff like that." So he was totally incapable as a personnel man.

BROCK: Can you share with me his name or do you feel uncomfortable about that?

LEHOVEC: Yes, the guys who didn't work out.

BROCK: What's that?

LEHOVEC: The guys who didn't work out I don't want to give their name.

BROCK: Can you tell me where he came from?

LEHOVEC: He came from a small college in New England. He had no industrial experience. He was just a—

BROCK: Was he a physical chemist?

LEHOVEC: No, just a—but actually, all the processes are not really chemistry in semiconductors. I mean, you can call doping chemistry, but it's really a physical process of diffusion of phase diagrams. Of course phase diagrams are chemistry, but phase diagram your problems are related to surface. And there I made the biggest mistake I made. The biggest

mistake I made, looking back, was an oversight where I had the right inkling and I didn't follow my inner voice.

And this was the following: There was a scientific meeting in Washington, D.C., and at this meeting a guy from Kodak—was it Kodak? Well, one of these companies in Rochester discussed photo resist technique.

BROCK: Probably Kodak.

LEHOVEC: Kodak, yeah. And in conjunction with germanium, and I was sold that chemistry on germanium is a contamination on the surface and needs to be avoided very clean and so on and putting organic gunk on the surface is going in the wrong direction. So I did not go to this meeting in Washington, and so my inner voice told me, "Go." Well, this technique, in conjunction with silicon, my God, I would have immediately had the whole money and everything in my hands. I didn't. I missed out on photo resist until too late.

BROCK: So in the mid-50s, was your group then those five or six people we talked about?

LEHOVEC: Yeah, only five or six people. And some very good ones: Armen Fermenian—creative, practical, fast; Rainer Zuleeg—good engineer, electrical engineering; Bob Koch—very good with vacuum equipment and crystal pulling.

BROCK: You talked about some of the informal connections that led you to find some of your staff. For people that you found through advertising, where did you advertise? Do you remember?

LEHOVEC: No, Sprague did it.

BROCK: Sprague did it? And so while you had these five, six people, did you have an equal number of technicians or was five to six people it?

LEHOVEC: No, we had a couple of technicians and they had titles and education and so on. And I had a very advanced style of management. With a small group, I discussed the project. I brought everybody in, including the technicians. Not, "This is a boss of the technician and I talk to the boss." Everybody is there. And I found resentment for that.

I should mention two other people who came in—very important people—came in later. And these were two people from Germany: one name was Dr. Carl Busen, B-U-S-E-N, and he came from Siemens in Germany. And I don't know how he got knowledge of me or how I got him. And the other one was Dr. Schier—I can't think of his first name—S-C-H-I-E-R. He also came from Germany.

Dr. Schier had gotten his degree in optics. He was very experienced in optics and he came and they kind of pedaled them in Washington at the big meeting. You go there and you can interview these people and offer them jobs. And that's where I found Dr. Schier. And I believed _____ [00:24:17] Leo Fedotowski in sending and placing buyers and so on.

Optics is a very important part and I wanted to hire Dr. Schier and Lazier, my boss, rejected. But I went to R.C. Sprague and I got it reversed and we hired Dr. Schier. And he was a top expert, and you know in the step and repeat, optics is everything.

BROCK: Sure.

LEHOVEC: And he is the top expert in it. So I had a mechanical engineer in Leo Fedotowski: top expert in handling, moving and so on. Top expert electrically. I had the best team together. And Carl Busen: excellent experimental in gaseous [00:25:16] diffusion. I put him on gaseous diffusion, so he was a gaseous diffusion man.

Schier was an optical man. Fedotowski was a mechanical man. Zuleeg was an electrical man. And Fermenian was my whenever I had an idea –

BROCK: Maybe ’52 to ’59 your group would then be 10 to 15 people?

LEHOVEC: Well, if you include technicians, about 15. Without them, I would say about 7 professionals and 7 technicians. A small group, but we could do better than anybody else. We didn’t need a big group.

BROCK: And we talked a minute ago about crystal growing. I was wondering if you could share your thoughts about the importance of crystal growing to getting activity going in semiconductors. How important was the crystal-growing function?

LEHOVEC: Well, it was a necessary function, but there was no great deal of innovation in it. The two directions of innovations in crystal growing were the following: One was the tendency of growing bigger and bigger and bigger crystals, simply for better efficiency in production. And now it’s amazed how big crystals are grown. I don’t even know how they do it because

part of the problem in crystal growth was a strain on the crystal which makes defects which are there for lifetime and so on.

So to go—strangely, if you could—and the bigger they are, the better. And this only other innovation I know of was _____ [00:27:31] of having double-doped melt and by changing the pulling grade from the melt, they can make it either an OP [00:27:44] type, which I used in my patent application as an example of P/N junctions. But otherwise, crystal growing is pretty straightforward.

BROCK: Do you remember from where you got your supplies of germanium and silicon?

LEHOVEC: No.

BROCK: So it sounds like you maintained your connections to that German firm that you had worked for in the –

LEHOVEC: _____ [00:28:35]

BROCK: Yeah.

LEHOVEC: No, I didn't. For some reason, they knew, of course, that I had got to America because of the Selenium and so on. So I don't know even who—oh yeah, I know now. He was not only important for what he did to me at Sprague. Even so, it was not appreciated at Sprague.

At Sprague, for instance, for the Philco transistor, they also brought wires down on the little contacts on the Philco transistor. And they had a production problem with the wires coming from the spool being kind of bent. And that made it difficult to find the little dot and so on. So they came to me and I gave Leo Fedotowski the job of doing that—finding this—and he built an equipment and he would not let go of it.

And Ernie Boss [00:30:09], the Executive Vice President, came in production and said, "We need the equipment. Where is it?" I said, "It's almost ready." "No, I need it right now." And Leo would not give it up because a knob for the puller on his equipment was not polished and nickel-plated. And he would not give an equipment out of his hands that's no good for the technician. It must be polished knobs. "Where is the equipment?" He wouldn't release it.

So they hated Leo; they didn't appreciate it. And therefore, as soon as I left Sprague, Leo was fired. Best man they had in this whole field. But Leo had a son—had several sons—and one of his sons graduated from Quebec University in optics, and he also had been at MIT

first. And when he graduated, I hired him at the University of California as my assistant. And his name was Andre Fedotowski.

And Andre Fedotowski was an absolute genius. I don't know what I would have done at USC without Andre. Andre Fedotowski is responsible for so many things which helped me—the whole Fedotowski family. Andre Fedotowski was not only a top computer expert. They had no teaching laboratory for semiconductors and they saw, of course, a need to teach students about all the processing and semiconductors. So no professor wanted it because they had no experience.

And I had experience, but I'm not a practical man in the sense I'm an idea man, and they needed a practical man to execute it. So they hired a new man from Yale—a PhD—and everybody was amazed in our interview of him how perfect the man is: very bright man; in every aspect he looked perfect. And they handed him the job to set up a teaching lab for semiconductors.

So the poor fellow whose experience was not semiconductors, so what did he do? He set up a _____ [00:33:35] measurement magnetic field, electric field for measuring mobilities together with conductivity. He set up a four-point [00:33:44] technique of four contacts, two carries of current and two measures of potential in the middle grade. So this thermoelectric effect: the simplest things. It's a time when they build all the computers and chips and so on.

Next time, two students signed up. No interest. So here came my assistant, Andre Fedotowski, and he said, "Why don't we set it up?" I said, "Andre, this is a big project; a lot of equipment setting up and processing and so on. I am an idea man. I know all these processes. I teach them." "I will set it up." And he set up the entire lab: _____ [00:34:46] crystal, a slice of silicon. And they had to process it to make transistors, resistors, capacitors by diffusion technique, by photo resist technique.

The only thing missing was ion implant. And I wanted ion implant, and by good luck I met a man who had worked at Westinghouse and had set up his own laboratory. He couldn't stand the bureaucracy at Westinghouse. And he had the Westinghouse work on silicon carbide, so he knew about silicon carbide light-emitting diodes.

So he contacted me. He came to California and he set up his own company. And since he was so reliable and did so well, the Japanese came to him and had him make their prototypes for their new circuits. They came to this private guy working all alone in Los Angeles. And he came to me and he had set up ion implant and everything.

And I said, "I haven't got ion implants." So he said, "Why don't you send your students down to me and I teach them the ion implant and they do it on my equipment." And he did it and he didn't charge a penny for it, and my students managed, in their semi-conducting courses and their slices, came to the point of needing ion implant, they went to him in South Los Angeles and did the ion implants there.

BROCK: What was his name? Do you recall?

LEHOVEC: Yeah, Leonard Kroko: K-R-O-K-O. And, it was a very sad story right now about him. A couple of weeks—I have a tendency not only of not _____ [00:37:05], also, when I meet somebody and work with somebody and so on, when it is over I don't follow up the relationship at all. So I left USC 15 years ago and I never followed up with him. So I suddenly saw from my two inventions for the solar cell and for the desalination [00:37:36].

Kroko is the man. He can set it up for me. So I tried to contact him. The poor guy is now in the hospital. He can hardly walk anymore. He had this disease where—

BROCK: Parkinson's?

LEHOVEC: Parkinson's Disease. And he can hardly talk anymore. He recognized me and he smiled, but.

BROCK: It's awful.

LEHOVEC: But Andre Fedotowski set up the semiconductor lab for me, and all I had to do is to teach him the processing, which I did in three hours Monday afternoon. It was the only thing I did with him. I didn't even sometimes set foot in the lab at all. It was handled by assistants and by Andre. And yet on my list of the USC, my name appeared all over. The semiconductor lab was a full success. It was three shifts and the students loved it and they wrote beautiful reports. I have them in Los Angeles.

BROCK: Going back to Sprague, we talked about the equipment for growing silicon and germanium crystals; what other sorts of processing equipment or electrical measurement equipment did your group need and where did you get it? Do you remember anything about that?

LEHOVEC: Well, really very simple. With the point contact transistor, there is a process which is called condoforming [00:39:34].

BROCK: Yeah.

LEHOVEC: And they put pulses through the contact in the point contact transistor to presumably indiffuse some impurities from the contact. And Rainer Zuleeg built for me the pulsing equipment so that we could put pulses of a given power through them and then switch immediately on our sildascope [00:40:06] see the effect on these transistor characteristics and pulse again and see the changes until it was perfect. That was what we wanted.

So we built this equipment: Rainer Zuleeg did that.

BROCK: Okay. Did you need furnaces or heating equipment to do any of the—obviously to do diffusion work.

LEHOVEC: Yes, of course.

BROCK: Surface melting.

LEHOVEC: Yeah, right. But surface melting was radiant heat with the RF coil. Of course I told you already about our improvement of the alloy junction process that we had the capillary in pumping the alloy from a capillary to it.

BROCK: Right.

LEHOVEC: And Hans Schier—Hans was his first name—Dr. Hans Schier, H-A-N-S. He was instrumental in building this equipment.

BROCK: We also talked before that your laboratory facility was in the mill building that today –

LEHOVEC: With all this cleanliness and so on, you would have think that this is the worst place, and when I first visited Sprague for my first interview and then I came a couple of months later to Sprague, I was aghast about this facility. But they did a very good job for the few rooms which I got. I essentially had only two rooms and they cleaned it up with new walls and so on. So this was adequate.

BROCK: Did Sprague own the mill? Were there other Sprague operations in there?

LEHOVEC: Yes, yes, yes. It was part of a big publication. And actually, the office of R.C. Sprague was in the same building, but they had also two other factories for capacitors in those atoms [00:42:30].

BROCK: Right. But not in that mill complex?

LEHOVEC: Not in the mill complex.

[END OF AUDIO, FILE 1]

BROCK: Okay.

LEHOVEC: Perhaps one comment that this guy, Bob Koch, whom I took with me from the Signal Corps, was very excellent man in setting up heavy equipment—vacuum equipment. He was a man who left me and joined what became National Semiconductor. They hired him.

Julian Sprague, who was a vice president, he took him to set up for his son National Semiconductor. So Bob Koch went there.

BROCK: Interesting. When did that happen? Was that also in the mid-50s?

LEHOVEC: Yeah, it was very early in. I was very disappointed that he left me because he was a very nice guy and a very good guy: both at Signal Corps and at Sprague. But Sprague was so stupid. They wouldn't give him adequate salary, and they said, "We have to keep the same salary level." They gave the same salaries to people who did paper capacitor holding [00:01:47] to people in semiconductors.

And I told them, "This is a highly competitive field. If anybody wants to hire," – "No, we cannot change it. One year it is semiconductors, the other year it is paper capacitors." The personnel man, John Shimmer III [00:02:09]; no idea for adjustment, just low salaries to everybody. And it was so incredible _____ [00:02:28]. We paid salaries which were 50 percent below what people get for the same jobs in other places.

BROCK: Wow.

LEHOVEC: And yet, all these people stayed with me. Nobody really left except this Bob Koch.

BROCK: Was National on the East Coast at that time?

LEHOVEC: No, they still were in the New York area. There was a branch of Spary [00:03:02]. Dr. Osleen split off and Spary went out of this and he set up his own company somewhere on the East Coast.

BROCK: I thought it was Raytheon.

LEHOVEC: No, no, no. Spary, yeah.

BROCK: So the job for your laboratory seemed to have been primarily to do research: device-development research.

LEHOVEC: The problem was—and this is a problem that I always strictly limited my activities to certain aspects, and I do it because when you work on this difficult aspects, you need full concentration. You cannot split your service. But as you know, to any business there are many aspects. One is a product, the other is sales.

And I—you might say stupidly, but it all depends what is objective. If my objective in life is to make contributions, then I did great. When my objective in life is to promote the contributions to make big things out of it, I failed miserably.

BROCK: In terms of product?

LEHOVEC: Right. So when I came to Sprague—and as I say now, stupidly, in a sense—I will not go into promotion and sales. What I should have done is to take my developed products and be my sales agent and not leave it to Sprague Electric field engineers who don't know anything about semiconductors and don't even want to sell it because they get paid.

BROCK: Was the role of you in your lab of getting semiconductor products made at Sprague? I mean, it sounds like you were –

LEHOVEC: We made the prototypes in the lab, and now it would need setting up a production line after getting orders for large orders. And here came the sales effort, and the sales effort was carried out in semiconductors by a man named George Norman. George Norman was assigned to me to sell my products, and George Norman was a Canadian. He was an officer in the

Canadian Army or Air Force and he was the utmost politician. He would speak of R.C. Sprague always as Mr. Bob, for indicating a great intimacy with him.

And he was a creature who Julian Sprague who wanted to kill me because of his son in National Semiconductors. So he was supposed to handle my product. So I gave him my point contact on this, for instance, and he wanted to pedal it for $6.00 apiece. And I told him, "There are already alloy junction transistors which sell for $1.00 apiece and nobody will buy your point contact. We have to sell it for 20 cents apiece. It costs us 10 cents to make this."

I don't know whether it was stupidity. I think it was deliberate, even.

BROCK: So, for example, with different devices that your group would make—a point contract—your improved method for making point contact transistors, your alloy junction and your surface melt transistors. So your lab would produce enough sample products for the salespeople to sort of sample it to customers?

LEHOVEC: They could have, but would not even be given to the customers. Nobody took them, and this was my fault, perhaps. Now in hindsight I would say, "So why the hell didn't I go to R.C. Sprague and say: You're organization is totally unsuitable for handling this new field. You cannot trust the ordinary field engineers. They don't get much money out of it in the beginning, so they're not interested. They usually don't understand at all that, so how can they

talk to semiconductor circuit people who are all young people in a new field and here comes an old fogey twice their age who doesn't understand the field and wants to sell them a product." It's ridiculous.

BROCK: So what were the –

LEHOVEC: But unfortunately, the field of alloy junction transistors disappeared just at the moment very early when _____ [00:08:54]. So it was not only blaming Sprague on it: point contact transistors does disappear when we came out with a good point transistor. Alloy junction transistor disappeared when just came out with a good alloy junction.

BROCK: So it sounds to me, then, from what you were just saying that your group was responsible for a lot of the work that some people would have in a wholly dedicated group like preproduction engineering and development, device development; that was all within your team.

LEHOVEC: We did everything, yeah.

BROCK: Did you have people from elsewhere in Sprague who worked on developing the new capacitors or manufacturing? Did you have any access to that?

LEHOVEC: Here came, of course now with hindsight, another thing which I missed. We, of course, with our alloying and diffusion capabilities, we were in the position to make many other semiconductor devises like diodes and capacitors and resistors. And for the future, you could say, "Why doesn't Sprague make, for instance, resistor capacitor combinations as a product and sell it?"

They sell the resistors, they sell the capacitors and they know they're used in RC combinations and they're not complicated combinations. So why doesn't Sprague say, "Make it in semiconductors and sell the integrated chip?" It would have been a cinch for us. We could have one it in three days is my hope.

I could have done them anything they want. But this was, again, marketing considerations and I was basically a scientist who was making the individual component. And I was not even participating in the integrated circuit until I went to this conference and I saw the people talking about this big circuit and say, "Ah-ha, how do we do P/N junctions?"

BROCK: It sounds like you were also working in isolation at Sprague, really.

LEHOVEC: Totally. As a matter of fact, two interesting stories about Sprague: When I came to Sprague, there were about half a dozen capacitors, and he told me when I came in—I think he may be dead now, so you could possibly quote him—he said, "The worst thing is to work for Sprague. If you go to,"—he went to a seminar at Williams—and I said, "Why did you go to the seminar? What is it about?"

He said, "I don't care. Anything to get away from Sprague. The worst thing is Sprague." That was the attitude to work at Sprague.

BROCK: Amongst the scientific staff?

LEHOVEC: The scientific staff. And there was another one—what is his name? I can't think of the name, but he published papers with me so I can find it out. He was a recent PhD who just was hired by Sprague. And John Shimmer, the personnel officer, came to me and said, "We have here this man who was just hired and he's unhappy and wants to leave. Could you find something? He's a good man; could you find some job for him?"

And I said, "Sure," and I put him on the solid electrolyte battery and I gave the job to do—what's his name? Young chemist; excellent man—I gave him _____ [00:14:11] the job of making solid electrolyte batteries at Sprague. And Fermenian designed a silver cup which was coated with silver chloride, and he stuffed into it graphite powder with iodine mixture and

then the silver chloride formed silver iodine and this skips the electrochemical reaction in these little cups _____ [00:15:04] which went into the carbon iodine mixture so it would be whole strings like sausages like this made on semiautomatic equipment designed by Armen Fermenian. And he could fold them into _____ [00:15:17] and _____ solid electrolyte batteries.

And Don Smyth was his name—Donald Smyth. This was another _____ [00:15:29] we did. But again, there was no marketing. And I even wonder nowadays when in the microchips—see, the power is so small right now they use in electronics, this is not the ideal battery. It lasts forever. Normal battery has a limited serve life. This has a serve lifetime estimated projected for 100 years.

BROCK: Are they rechargeable in any way?

LEHOVEC: No. You use up the sliver eventually, but it all depends how thick your silver cup is. And we have several patents on them, by the way.

BROCK: Right. Well, one of the questions I wanted to pose to you kind of touched on already by mentioning these six other scientists—six or so other scientists—who had worked for Sprague or were working at Sprague but when you were there. And the question was: Did

Sprague—before you came and set up your group—did they ever have any sort of research lab or research function before?

LEHOVEC: Yes. It was essentially what I did on semiconductors they did for these electrolytes.

BROCK: But that was one person with a very small group?

LEHOVEC: And then their main moneymaker were tantalum capacitors, and they set up, particularly under Dr. Lazier, a big _____ capacitors. And Dr. Bernard _____ research, I presume. But when I say, "I presume," I don't realize what's—I don't know what's really contributed because in the tantalum capacitor, there is no special trickery.

BROCK: Were they in a separate facility? Was that group on the tantalum capacitors in a separate building?

LEHOVEC: Same building I was, but in different part of the building. And Sid Ross [00:18:28] was also in the same building in a different part of the building. And I remember—

but you know that Dr. Lazier, who was the most stupid and evil—when he heard that the little group had set up on tantalum capacitors and produced a capacitor as good as Sprague Electric without any research or anybody. So he wondered, "Where is really our advantage? What does Sprague really have an advantage?"

Tantalum capacitors are made by centering a tantalum powder into kind of a porous plug. And the reason for this it to have a very large surface area and then energizing this tantalum to find the tantalum oxide and then impregnating through the pores. Somehow they use a context. But of course there are tricks involved in centering it properly that you get a very large surface area: how small the powder is.

So there are—I don't say it is completely simple, but there's not much in impurities or anything in it, or cleanliness or anything.

BROCK: It didn't mind the impurities so much?

LEHOVEC: No. There are no impurities affect nothing.

BROCK: So all told, if there's these three different groups—the tantalum capacitors, electrolytes and –

LEHOVEC: Capacitors. And this was under Dr. Fabricius. Fabricius is F-A-B-R-I-C-I-U-S. I think he had five daughters or six daughters; very nice man.

BROCK: So there were roughly, then, somewhere in the order of 30 sort of research professionals between all those groups? Something like that?

LEHOVEC: Yeah.

BROCK: And then maybe an equal number of technicians?

LEHOVEC: Yeah.

BROCK: So maybe 60 people, all told?

LEHOVEC: Could be.

BROCK: And did you ever have meetings between the directors of these different leaders of these four different groups?

LEHOVEC: No.

BROCK: No?

LEHOVEC: No. No. I grew up in a perfect vacuum there. And of course I met some at the dining table and in the same buildings, possibly, and screens for video microcircuits give optically the output images [00:22:24]. And I suggested to Sid Ross, the chemist, to look into liquid crystals. I was very interested in liquid crystals, but it was not my field of semiconductors so I wanted Sid Ross to interest him in liquid crystal displays on top of my semiconductor structures to the liquid crystal display.

He would always do the same thing his whole life; the same thing was more or less electrolytes.

BROCK: So, we talked about how your group would create sort of sample production of these different transistor devices but that they never really went anywhere with the salesmen. Was it really with the second source to Philco transistor that Sprague actually begin to sell semiconductor products?

LEHOVEC: Yeah, definitely.

BROCK: That was the first?

LEHOVEC: Yeah, definitely. But I think the main fault in Sprague in not promoting our devices deliberately and without the sales effort or promotion effort. It's the same as with my books. I have my poetry books and there is no sales or promotion effort; nobody buys them—nobody knows. Exactly the same thing.

And, of course, I could be blamed by not—even though I realized it—by not taking the steps to really—what steps could I have taken? One step: step out of Sprague Electric with my excellent goal—there was financing all over the place—and do it independently of Sprague. If Sprague doesn't contribute, it hinders me. The other way is to go to R.C. Sprague and convince him, but it would have been difficult to bypass his partner, who was Vice President in charge of sales.

And then, of course, when Philco started, then my group got distracted. Instead of developing faster in the semiconductor field, we were distracted in helping out the Philco process. So I got involved in diffusion, and I don't know whether I told you the last time, phase diagram of the alloying of the aluminum dots to the silicon.

BROCK: To the germanium?

LEHOVEC: No, I did it later for silicon, too. First germanium, then silicon. So this is a phase diagram. And the second is a diffusion in the solid state. Now, normally, diffusion can be mathematically assessed by temperature and time. And if you know the diffusion coefficient, then you can calculate what the _____ [00:26:17] is.

Well _____ time all they did in Concord, and the few who qualified didn't come out as I thought it should. And this was a big problem now in the design of these transistors. And I found out what it was, of course, and I published it. So I made a big contribution to diffusion technology, not _____ [00:26:57].

But he wouldn't even acknowledge in his book, I presume, even though I told him about my contribution. And the big discovery—my discovery, of course, if you study statistical

distribution of electrons and they are two statistics: one is a _____ distribution and the other the _____ [00:27:34] distribution.

BROCK: Right.

LEHOVEC: And when the firm level [00:27:37] moves into the conduction bend and it is below the conduction bend, the electrons in the conduction bend follow the _____ distribution and the firm level [00:27:52] moves into the conduction bend. That is a very high concentration of electrons. That is called a degenerate electron gas. It follows affirmative distribution [00:28:04].

And, I explained that if you have a degenerate electron gas, if you have very high dopant concentration, then the _____ distribution is valid. Then, an electric field is set up along the diffusion profile.

BROCK: That changes it.

LEHOVEC: So the remove of the dopant in the diffusion is not by diffusion but by combination of the diffusion and the electric field. And the interesting part is that it turns out that this

electric field different than otherwise, and that explained the anomaly in which I published on it. And actually, the man who publishes and worked with me on it is Fedotowski. No, wait a minute; not Fedotowski—Slobodskoy. Yeah, he was at Sprague. I forgot to mention him. He published with me several papers. Slobodskoy: S-L-O-B-O-D-S-K-O-Y.

He was essentially my assistant in my theoretical [00:30:00] work. He was not involved in any practical equipment design or transistor design or anything. He was simply—when I did my theoretical work I had too much to do because other aspects I need an assistant to do some of those things. And then I went into exile to Europe from Sprague. I took Slobodskoy with me, and he even wrote a big paper with me on the MOS capacitors method: Lehovec and Slobodskoy. It just mentions the Lehovec and Slobodskoy _____ [00:30:43] and this accelerated diffusion by high dopant concentrations.

BROCK: Did he go on to have a career in the semiconductor industry?

LEHOVEC: Yes. Well, I didn't really follow him too much. All I know is that when I came back from Europe and he left Sprague and when he sold the reorganization and he left Sprague. And he may have come to IBM; I think he did, but I never heard of him again.

BROCK: Let me just save this recoding file.

[END OF AUDIO, FILE 2]

BROCK: I wanted to talk to you about this patent that you filed in 1955 and just to talk about it briefly. I mentioned it last time we met but it didn't ring a bell. And it's called—was filed in June 1955 and it's called Multiple Junction Transistor Unit. And I brought a copy of it. I don't know if you can see that very clearly.

LEHOVEC: I should have brought my glasses.

BROCK: Oh, well –

LEHOVEC: All I see is it looks to be the alloy junction unit or liquid.

BROCK: Yeah, I think what—if you can see—you might not be able to make it out here, but this is like an NPN rate-grown structure. Then here you have these alloyed junctions. So you kind of had an NPN and a PNP sharing one.

LEHOVEC: I see how, yes. But what about it?

BROCK: Well, I just was wondering: it seemed like an interesting device to me, and I was just wondering: this structure looks kind of close to what you see in your later patent on the PN junction isolation. You know, just graphically, these series of rate-grown N and P regions and then an alloyed –

LEHOVEC: No, but it didn't come from this earlier, no. But incidentally, what baffles me is that on my PN junction patent, when I look at this part of it, I don't recall offhand. As I told you, I filed it, I sent it to Washington my own. I didn't recall this at all. And even I don't understand; I would have to read my patent, which I never did. Because I don't recall what this is all about.

BROCK: I think it was a different embodiment of the use of PN junctions for isolation.

LEHOVEC: Perhaps it is, but I didn't recall it; perhaps because all I know is this structure, but I should read my own patent.

BROCK: Well, I wanted to talk to you—so there was no real—okay, no real strong connection between the ’55 patent and the ’59 patent?

LEHOVEC: No.

BROCK: Just in terms of—I guess they were both embodiments that you were familiar with: the rate-grown and the alloy.

LEHOVEC: Yeah. I picked the right _____ [00:03:33] simplest way of doing it.

BROCK: Well, I was wondering: as you got into—to me, or it seems to me, that there was a lot of activity going on involving different sections of the military with different companies getting into microminiaturization and integration. And one of those, I saw that the Signal Corps Signal Research and Development Lab at Fort Monmouth had this program for the micromodule in ’57, ’58 and I guess they did a lot of work with RCA in this period—’57, ’58.

When you were here in North Adams, did you maintain connections to the Signal Corps?

LEHOVEC: Not at all.

BROCK: Not at all?

LEHOVEC: No. I never collected with them, I never talked to people there, I never went there applying for a contract or anything—not at all—to sell my transistors to them, nothing.

BROCK: Why?

LEHOVEC: That's me. I told you; I meet people like a passing star, like a comet, and they go out and I go my own way. I always have new ideas, new things, like building a garage here or moving this structure and building a very complicated project because tiny legs could break easily.

BROCK: Sure.

LEHOVEC: So I had to start orientation and motion and translation and motion to move it this was, and this is, to me, as important as the _____ [00:05:38]. And now looking back, of course, I'm not proud of it. I really am ashamed of it. I was a favorite son of the Signal Corps and I said goodbye without going to Colonel Young and saying goodbye to the commanding officer who took me on boating trips, and I just disappeared. I never went back.

And when I became a poet and then Cambridge wanted to call to make me the Scientist of the Year for the whole world, I didn't even answer.

BROCK: The University of Cambridge?

LEHOVEC: I was not a poet. It was a closed book. This is—call it extreme stupidity, and in a sense it is.

BROCK: It's just your personality?

LEHOVEC: It's my personality. I make a contribution and I never look back. And I want to mention—perhaps I mentioned it before—but this will turn out to be one of my biggest contributions. It is this Paper No. 17 in my list of papers on the surface of ionic crystals. I think

I mentioned the last time, perhaps only very briefly, that in the general review, of course, the _____ [00:07:14] defects—the crystals—and their concentration depends on the energy of its environment. And I looked at it and I thought, "Yeah, but at the surface of a crystal, the environment differs because it's cut off so it's not anymore the same they are missing here, so therefore, the energy of the defect near the surface will differ from the energy in the bulk. And therefore, the concentration of defects at the surface is thermodynamically automatically different from that of the bulk, and if there is a concentration gradient, then there is an electric field related to it in order to stabilize it.

So I pointed it out in a very brief paper and said it had a great consequences for the photographic process for _____ [00:08:18] electricity, to mention only two; it mentioned others. And I sent it in to The Journal of Chemical Physics of Physical Chemistry and the reviewer wrote all of it back. I never heard of it. It is the key paper of the next branch of electronics.

BROCK: Which is?

LEHOVEC: Making a communication _____ [00:08:57] as thin as a hair.

BROCK: Oh, the fiber optic?

LEHOVEC: Well, it's not fiber optics which conducts it, it is a surface conduction in this which is used for—and as I said to you the last time, one of the guys who is now the leader of it—I think his name is _____ Minkoff [00:09:21] from Russia. He sent me his paper and he mentioned that this is now a new branch and it will overtake everything. _____ 1954, I think, or 1955, and I didn't even want to publish it.

BROCK: Well to go back to that period, you said you didn't have ongoing connections to the Signal Corps. Were you aware of their micromodule stuff?

LEHOVEC: Not really. This is another thing with me: you can only do so much. And when I am creative in some respect, the whole world disappears for me. And so I had no time. One of the biggest mistakes perhaps was—and this was stupid mistake and Dr. Preston Robinson, whom I considered to meet.

I was one of perhaps 6 to 12 people in the United States who were in the semiconductor field when the transistor was discovered, and I was well recognized as such. When people at Nottingham gave an annual conference to invited people only on semiconductors, and I was one of the handful of people invite. Bob Noyce was one of his students whom I met there, but anyway.

And what was my focus of thought now?

BROCK: Concentration, not following the micromodule.

LEHOVEC: Right.

BROCK: Preston Robinson.

LEHOVEC: Right, it was all a mistake of Preston Robinson. And there was a conference by the I Triple E on semiconductor research: not a public one for it; a committee of it. And there were about a handful of people only on this committee and I was one of them. I was very well recognized as one of the leaders of 6 or 12 leaders in the country.

BROCK: Vice [00:12:05] Research Conference or whatever?

LEHOVEC: But that was kind of the guiding committee of the I Triple E, and this was the ideal venting spot to listen and hear what is going on in the field in a concentrated way. And Preston Robinson made me quit it.

BROCK: Why?

LEHOVEC: I should concentrate on my work and forget all these people talking and so on, but this would have kept me in touch with what's going on. I didn't keep in touch with all these programs—these different names of the Signal Corps and so on. And where did I find a good write-up on it? It may have been the Kilby paper. I think the Kilby paper was a very, very good write-up on it.

BROCK: Yeah, Kilby in '76. It is.

LEHOVEC: From it, I only found out the first time what was going on in the field.

BROCK: That's very interesting.

LEHOVEC: I was unaware of what was going on.

BROCK: So you didn't go to many meetings or conferences?

LEHOVEC: To conferences, yes, but no committee meetings.

BROCK: You would just attend the conferences?

LEHOVEC: Ordinary conference when I saw on the abstract something of interest, yes I went to conferences. But unfortunately, I'm at the conference and I missed it: Washington _____ [00:13:30] Photo Resist Technology. _____ with my Signal Corps work, which _____ transistor and everything.

BROCK: Well, your comment there, just because I had a series of questions, if you knew, about—there was also work at RCA that was naturization [00:13:53].

LEHOVEC: Yes. I mean, the Kilby paper has all these names.

BROCK: Yeah, but you didn't know about that stuff at the time?

LEHOVEC: No, I only knew it now when I wrote my paper. I read for the first time the Kilby paper _____ [00:14:11] because, obviously, I commented he would quote me correctly, so I must have glanced over the Kilby paper as far as I am concerned.

BROCK: Well, I was interested—last time we met—let's see, let me get back to my question list—you were telling me about how in the fall of 1958 you went down to Princeton and went to a conference, and there you saw a paper by a guy from RCA—a Swede—who talked about these large transistor arrays and that was the inspiration to think, "Let's do it differently, better—PN junction isolation—on the way home."

And I wanted to see if I could figure out—I haven't been able to figure out what meetings connected to the I Triple E were in Princeton in 1958, but I did—I think I determined that the guy you must have heard speaking was somebody named J.T. Wallmark.

LEHOVEC: Yeah, exactly.

BROCK: And, let's see –

LEHOVEC: Could it be that it was already '59 and the conference was '59?

BROCK: Well, I don't know how long it took between. I mean, obviously you had the conception right after the meeting. Do you recall at all how long it took you to prepare the –

LEHOVEC: No, I don't, but potentially it could have been very quick, so it could have been '59. But then I somehow remember, but I may be wrong, since I drove back with the roof of my convertible down. But perhaps it's only imagination that I figured myself driving with the roof down. But, I mean, there must have been some record of Princeton conference going on there.

BROCK: I'm sure there was. I haven't been able to find it. And I'm still looking, but I wanted to just—the fall of '58 would make sense to me for the timing, or early 1959, because in August of 1958—August 21, 1958—Wallmark filed a patent on what I think must have been part of what he was talking about: these large arrays shift register out of them. And he seemed to then have been talking about it because I found this publication. This is from June of 1959 where he

discussed what they made at RCA, which it's hard to see, but it was—here's a sort of shift register that they built all on one slice of—I think it was germanium.

But then they were getting into these big arrays with these field-effect devices, so they were contemplating these.

LEHOVEC: Yeah, this was exactly—this was definitely the guy, yeah.

BROCK: And then it seemed like—and you had mentioned that he was making –

LEHOVEC: Excuse me, is he perhaps, in some of his publications, mentioning presenting at the conference perhaps in his bio or references or so on?

BROCK: I haven't seen that. I haven't seen him make reference to, "As presented at such and such a meeting." I haven't found that. But as I say, I'll continue the search. One thing I did find was you had mentioned that he had also talked about analogies of—we did find a paper that he wrote in August 1959—so not far away—where he was drawing these sort of comparisons to the human brain. So he was talking about here—it's hard to see, but subminiature things like very small vacuum tubes or these micromodules, then what he called the microminiature realm

of integrated electronics and then it's extreme miniaturization where he's talking about the human brain and neurons.

And then there's another one where he's talking here about the size power relationship for direct coupled, or the sort of devices that he was making, and he has the brain and his large arrays. So he may have been—he clearly was talking about those brain comparisons.

LEHOVEC: Wallmark: W-A-L-L –

BROCK: M-A-R-K.

LEHOVEC: M-A-R-K. Right. He's the man, yeah.

BROCK: So that's good.

LEHOVEC: Well, my Alzheimer's Disease—I still remember.

BROCK: Well, and clearly you thought that the problem with—in my reading of these things, which is—I'm not that operation of these structures that he was proposing was precisely that of the intrinsic semiconductor was used to conduct electricity between the transistor structures. So the operation of these—his proposed devices, like the shift register, for instance, precisely involved the components not being electrically isolated from one another.

So I was wondering maybe was that the contrast that –

LEHOVEC: No, you are saying that he wants them not isolated from each other?

BROCK: I think so.

LEHOVEC: Not some of them—I understood them as separate devices wired together, not with him. But he may have, of course, mentioned—I mean, I don't recall it from his talk, but he may have—as other people—I think Kilby mentioned it somewhere that if the semiconductor is sufficiently pure like silicon or germanium, it's called intrinsic semiconductor. Then it has a very high—what I remember know, if I remember correctly, was the stability of intrinsic silicon is sent through the five centimeter, and that not high enough for _____ [00:22:30] isolation.

BROCK: But it could also have been that he was thinking of different ways in which—yeah, probably in these very large arrays, I think he was—Wallmark, that is—talking about making sort of this basic unit that involved, let's say, maybe two transistors on a slice and then interconnecting hundreds of these basic units. So maybe that was the problem that you were seeing.

LEHOVEC: Possibly, yeah.

BROCK: Well, then having cleared that up, the last—I guess there were maybe three other topics I wanted to talk to you about today. One was to really get into Sprague's use of your patent on P/N junction isolation, or lack of use.

LEHOVEC: Lack of, yeah.

BROCK: And then, to talk to you about what's explicit about the P/N junction isolation. So maybe we could talk about first the Sprague issue and then the patent issue. I guess they're both patent issues.

LEHOVEC: About the Sprague issue that—No. 1, as I mentioned the last time already, when I came back from Princeton with my stroke of genius or strike of genius.

BROCK: Stroke.

LEHOVEC: Stroke of genius, Hill Sweeney [00:24:38], who was still alive here in North Adams—the local patent attorney at Sprague—wouldn't file it immediately. He kept saying, "Docket number and many important patents." And so I sent it in directly on my own to Washington and when I came back from my momentary exile in Europe, my first question was about this patent application, and I was informed that I just had received the patent and I was very excited. I knew I had in my hands the whole future of electronics.

And then a couple of weeks _____ [00:25:37] not to defend my patent, but to get for it from Texas Instruments whatever we could get, as his formulation was. And I –

BROCK: Like a monetary payment?

LEHOVEC: Right, or whatever.

BROCK: Settle, then?

LEHOVEC: Not bother with it because he had no idea about this.

BROCK: Right.

LEHOVEC: And I objected vehemently and when I got the file back a couple of days later—because I wanted to copy his letter to Connolly—the letter had disappeared. And they decided to defend it after all. And I told you this last time: I went down there and then when I finally prevailed in the higher patent court, I suggested to him to get out of the semiconductor field and collect royalty.

And because—and getting out of the semiconductor field was a necessary, I believed, because, you know, there's so many patents floating around and there's always one patent licensing need or something else, so they'll all exchange it among each other big deals, and I wanted him not to make any deal, but stay 100 percent only on this patent and have no electronic field.

BROCK: Participating in the semiconductor field, then they could essentially collect rents from everybody else?

LEHOVEC: Exactly.

BROCK: But if they were going to keep making semiconductors, then the other companies would say, "Well, let's do a trade."

LEHOVEC: Yeah, exactly. They all traded with each other. I suggested they simply not to trade, but rest the case on this and get out of it. Well, he wouldn't do it and then they presumably they did it for how much and with whom I don't know, and he wrote me this letter which I have from the Board of Directors that it turned out to be a valuable asset—my patent—and they express their sincere thanks to me.

BROCK: In Bo Lojek's book, History of Semiconductor Engineering, on Page 205 he reproduces that letter that you must have given him a copy.

LEHOVEC: Yeah, I must have given him a copy, yeah.

BROCK: And what's interesting is that—well, what surprised me was the date is January 11, 1967 and it begins by saying, "We've just completed a license exchange agreement with Fairchild," and then it says, "We hope to do it," –

LEHOVEC: Now I'm astonished. I have no recollection of this letter because I recall the letter I refer to was presumably earlier and it was a very short letter where he—and I have a copy of this letter I refer to—I don't have a copy of this letter right now. I don't recall it.

BROCK: Well, it's from R.C. Sprague to you.

LEHOVEC: I know, I know. I heard you, but I don't know where he got it. He possibly didn't get it from me; I don't know. But the point was that in this letter, I remember it. I have a copy of—no particular deal is mentioned, only in general, "It has turned out to be a valuable asset," and they express their thanks.

BROCK: That's in the last paragraph of this letter that's reproduced here. It reads, "Your patent has turned out to be a valuable piece of property, and this brings for me and our entire management team hardiest congratulations on your invention."

LEHOVEC: This is what I recall exactly like it, but I don't recall the first part. I must have looked at the letter again which I have. Perhaps it is in it, but I'm interesting to see that. So that is what happened.

BROCK: That's Joe's—that took them an awfully long time to get those license agreements in place.

LEHOVEC: What year is that?

BROCK: January 11, 1966 is the date on the letter.

LEHOVEC: But actually, I think my patent was issued about '63 or '64.

BROCK: I can tell you. 1962.

LEHOVEC: Oh, ’62 was issued?

BROCK: Yeah, April 1962.

LEHOVEC: So then the decision of the patent office in the interference court was Kilby probably came out ’64.

BROCK: I see here March 1966.

LEHOVEC: Yeah, so –

BROCK: So it is right after the decision that then Sprague—yeah.

LEHOVEC: But now I wonder why it's so long to get the decision from the patent office because my recollection is that as soon after I came back from Europe—you say the patent was issued in '62, so we got into the interference right after that. I learned it's going on and I went to Texas, and why did it take so long from '62 to '66?

BROCK: It could have been—I can't remember when they granted the Kilby patent.

LEHOVEC: I don't know. This seems now to be an awful long time. Yeah, but this already explains something else to me now with hindsight when I hear that namely why I stayed so long with Sprague Electric. Because coming back from Europe in '62 I was very disappointed that they had dissolved my beautiful group, split it up to two people: one a surface tension expert.

BROCK: Pardon me just one second.

[END OF AUDIO, FILE 3]

BROCK: Okay, it's finally back on. And I thought before we talked about the P/N junction isolation and the different patents by Noyce and Kilby, etcetera, I mentioned about the Lojek

book, and in there, you know, we were referring to the letter reproduced in this book and there's a whole section about you and your work in this book, which doesn't have a lot of footnoting or referencing in that section. And we were talking about it before, without the recorder going, and I think you wanted to talk about where Lojek's –

LEHOVEC: Yeah, he got it all from me: everything.

BROCK: He did an interview with you and you shared documents with him?

LEHOVEC: Yes, he spent quite some time with me and I showed him documents, I'm sure, but the decision by the interference court is the main document. And also, it's interesting that he's from Czechoslovakia, so he's a compatriot of mine and he knows Dr. Gilmer Frank [00:01:26], who worked with me at the—he worked also, not with me—at The Physics Institute in Prague, where I worked.

And he remained in Czechoslovakia and became the big issue there in electronics. And Gilmer Grank came to visit me in Los Angeles with Mr. Lojek, so that was over 50 years to see the old time.

BROCK: Well, did you want to talk further at all about any of the work that's expressed in the Kilby and Noyce patents and how they relate to the use of—how in those patents, electrical isolation is treated or P/N junction isolation?

LEHOVEC: Well, I mean, as I mentioned in my write-up which I gave you that Kilby achievement is one of engineering, not one of invention. That he managed to build the first integrated circuit together with electrode with semi-conducting components, however using isolation by a slot, and that means by means which are not adjustable to _____ modern microcircuits [00:03:12]. And my friend, Mr. Shockner, who independently investigated that, came to the same conclusion that there is no invention for the microcircuits in Kilby's patent.

And as far as Noyce in concerned, he uses _____ [00:03:44] and just adds the monolithic isolation on the surface, which is mentioned by both already—by Kilby and by me—but not claimed. But Noyce did use my P/N junction isolation and doesn't claim it, which seems incredible, unless he knew that he couldn't get along with his claim. And Noyce expressed that he got the P/N junction isolation from me in his papers, so get gives me credit, but Kilby incredibly _____ [00:04:26] P/N junction isolation as a contribution even. It has not been proven in court and it is used.

It is not clear _____ everybody uses it already, so you must really question the sanity of this Nobel prize winner in making a claim like that.

BROCK: Did you know Jack Kilby?

LEHOVEC: I met him only in Texas at the interference hearing.

BROCK: You talked about meeting Bob Noyce last time we talked at that Nottingham.

LEHOVEC: I went skiing with him when he was a student in Vermont and I knew him later and I wanted him to hire him from Philco and work together and we knew each other because of Philco and so on. But he was kind of distant, and now I realize why—because he felt about his patent situation, perhaps; I don't know.

BROCK: You were talking about—we were talking before, when the recorder wasn't on—about what would you call it? Popular understanding of the integrated circuit story and many existing accounts. The attention really focuses on Kilby and Noyce. And in fact, there's a lot of going back and forth about, "What was Kilby? What was Noyce?" and a lot of conversation about that.

And we were talking about each of those men, Kilby and Noyce, being associated with the big company that was doing a lot with those patents and also doing a lot of promotion. Do you think that is why some of the other names, like your name, and other people who were involved in the 50s, in doing similar work aren't mentioned as often?

LEHOVEC: Definitely. Definitely. But the biggest propaganda machine has collapsed _____ [00:07:13] and they have members on all committees and they are reviewers of papers and so on, and so that is always Bell Labs, Bell Labs, and actually—but please don't quote me in a publication—in my opinion, Bell Lab is the worst-managed company in the world. They just win by pure size; that they hire so many people and put them that something good has to come out eventually.

But it was very clear it was a breakup of Bell Lab that they stupidly, in my opinion, assisted to get rid of the telephone companies for the right to stay in the computer field. And that is because of the stupid belief that they are so smart, and yet in the computer field they were the biggest disappointment of all companies. They hired on of the other companies that built up; I forgot which one now—one of the smaller companies—and they all failed and failed and failed, and even now, the rest of Bell Labs is little company. I forget what the name is.

BROCK: Oh yeah, Actel or –

LEHOVEC: Yeah, something like that. Nothing came out of it. Now some other company bought it. Even that is nothing comes out of it. They are just –

BROCK: Yeah, and I was wondering if you thought that—and you mentioned several times that your solitude and also –

LEHOVEC: Not to keep it up contact.

BROCK: Not keeping up contacts and moving to the next thing.

LEHOVEC: Exactly.

BROCK: Would you say that those three connected characteristics have been part of your personality for a long time?

LEHOVEC: Well, let's take the three again.

BROCK: Okay. So your being a solitary person.

LEHOVEC: Yes.

BROCK: Not sort of keeping up contacts with people.

LEHOVEC: Yes.

BROCK: And sort of a restlessness, I guess, of always going looking forward to the next thing, the next question, and once you have an answer, moving on to the next question.

LEHOVEC: Yes. Well, the first aspect of being a solitary person comes from my early upbringing. We could spend days talking about this. My mother—psychologically, I was totally screwed up. That made me to become 150 percent scientist because I felt there's nothing else for me in life: "I will never have a girlfriend."

But I'm grateful now to my mother for having introduced that because that's why I worked day and night as a scientist and I made the recent superficial study of important contributors in various fields. And I found all of them come from a failed family. This is a necessary ingredient to becoming a great contributor. Nobody will become a great contributor, according to my superficial study, who comes from a well-adjusted family.

But not everybody who comes from a not well-adjusted family becomes a great contributor, so it isn't necessary but is a sufficient condition. So that is one thing. My mother was a only child. She got divorced from my father and lived in total solitude with my brother and me. We never had friends invited or parties or anything, so I grew up totally un-at-ease in social settings. So that was the first thing that—what was the other one, keeping up contacts?

BROCK: Yeah.

LEHOVEC: To meet this complete concentration and solitude, how can I keep up contacts and call up. This fellow _____ [00:12:17] for instance, who came lately to visit me, he now constantly writes me and wishes me a happy birthday and sends me pictures of his family and all this sort of thing; it's totally alien to me because I am concentrating on one thing.

And as far as staying with a subject, I like to go to a subject of which I understand nothing and develop for my own pleasure like you solve a crossword puzzle for your own. You don't do it for others, you do it as a challenge. So I developed an understanding on a new field,

and when I have the basic understanding and now it comes to develop details of it that bores me already. It is not a big deal.

So that's why. And, for instance, this paper I tried to mention to you, I mentioned to you several times as No. 17. I wrote a poem on it, which is, "I dropped a seed and walked away and never looked back. Then years later, I came back." And that's what I do. I start a field, and as soon as I understand it, leave it to others.

BROCK: Well, it strikes me that that personal dimension might also be in part why your name doesn't have the same relevant prominence in the discussion of the integrated circuit: that is, if you're a solitary person who in social situations is that's not their favorite place to be, isn't a big networker, isn't a big self-promoter and doesn't stay in one area and kind of beat the drum. You know, that's, I think, a difference between somebody like a Kilby or a Noyce, who is highly networked, who in their own careers talked all the time about this stuff.

LEHOVEC: Exactly. And for this reason, I have made more contributions in larger diversity of scientific fields than anybody else ever has and ever will. I made the biggest contribution and so on; I don't want to discuss this, but anybody else who made contributions in another specter, and I made it all over the place: phase diagrams and you name it. It's all over the field. And always walk away and never look back and therefore, in the single field I am not the recognized person.

They always wonder, "There's this strange guy Lehovec who wrote a paper. What became of him? We never heard of him again." So that is eventually the attitude of all the people. I had my fun making contributions and like solving crossword puzzles. That's how I look at science. To me it's solving crossword puzzles. The latest is moving the structure up there, so that gives me pleasure and great achievements.

But of course, you pay a price for it, but you get your rewards because once you maintain all the contacts, that takes a lot away of your solitude and your isolation and your concentration, and that's why I—did I tell you that some people would consider me a monster as a grandfather because I had two grandchildren whom I didn't met until a couple of months ago when they were 4 years old: my own grandchildren. And they're in the United States.

BROCK: And you have four daughters, is that right?

LEHOVEC: Yes, I have four daughters. I met all them, of course. 'Til a couple of months ago when they traveled to Hawaii and came through Los Angeles and I met them at the airport. But fortunately, I was a little bit, of course, had a chip on my shoulder. Why did I, as grandfather, not meet them, not send them any birthday presents, not talk to them, and how would they feel? Would they resent it? But it was a very happy reunion, both with the grandchildren and my

daughter and her husband. They understand me. I have explained to them that's me and I can make my contributions only by being totally isolated from the world.

BROCK: And they've come to live with that?

LEHOVEC: They've come to accept it. That is strange, and I agree. I certainly am a strange man. Now show me another grandfather who doesn't meet his grandchildren.

BROCK: There's probably more than you think.

LEHOVEC: I don't know. There's some who are in disagreement with their children and they fall apart. No, we are never a fight, never a misunderstanding; I just walk away and live in isolation and do my thing.

BROCK: When you were at the University of Southern California, we talked about—before some of the—did you have any industrial contacts during that time or had you gone fully into academia and left industry behind?

LEHOVEC: No, no, no, I had—as a matter of fact, this work in _____ [00:19:11] was—all of it was done in conjunction with MacDonnell Douglas Corporation. And how did I get to MacDonnell Douglas Corporation? One of my workers at Sprague, Dr. Rainer Zuleeg, he left Sprague—oh, that's another interesting story—because I hired him like I hired Dr. Schier for people whom the United States brought to this country.

I went there fishing for interesting people and I found a young Hungarian electrical engineer, and I was impressed from Hungary by his personality and I hired him for Sprague. And I hired Rainer Zuleeg there as an electrical engineer, and not very long after I hired this man named Joseph Lindmayer[00:20:30], Rainer Zuleeg quit. And he quit and Rainer Zuleeg went to California to MacDonnell Douglas Corporation, and MacDonnell Douglas is all this big aerospace companies where had aspects of rockets and space and so on.

And part of their problems were always radiation hardening of electronic components, so they hired Rainer Zuleeg to set up a _____ [00:21:07] silicon and then _____. And when I came back from Europe and was disappointed about that they destroyed my group here at Sprague and didn't appreciate my P/N junction, I partially separated from Sprague, remaining as a consultant. And they were very nice. They gave me a free hand at Sprague to be a consultant 100 percent or 50 percent or 20 percent, as much as I want. I get paid my salary as a percentage of the time spent, but it is my own choosing how much time I want to spend. And so how could you ask for a better deal?

And I could consult other companies and I selected _____ [00:22:13] at USC and actually wrote our papers. He got his PhD from the former Japanese University. So yes, I was in contact in this respect. I also was providing consulting to Rockwell International and also to Westinghouse. But actual work at the university I did in conjunction with MacDonnell Douglas. Zuleeg did the experimental work and I did the _____ [00:23:00] interpretation and published several papers with it.

BROCK: What was that? Do you know any of the details about that government program that was bringing over these European technical people?

LEHOVEC: The original program under which I came over was called The Paperclip Program. It was when Brown and the others came over. I don't know the name of the other program; I only knew that we got fliers from the government that said, "Bring over people for an interview on certain days in New York City."

BROCK: But were these people, like, escaping from behind the Iron Curtain type thing?

LEHOVEC: No, not necessarily. They were good scientists who wanted to come to the United States. They made the initial contact. They were contacted on excellent personality? And while they were very smart men, they knew nothing about transistors but quickly learned.

And I thought, "I have to give them some period of adjustment in this country and in semiconductors. I had a man in my group named Chuck Wrigley [00:24:37], who was a junior man, having gotten his Bachelor Degree here at Williams and he was the son of a president of a local bank in North Adams. And I put him together with Chuck Wrigley, essentially to Chuck helping him introduce Lindmayer to the American way and so on.

And one day, the personnel manager, John Chambers III [00:25:08] at Sprague, came to me and said, "I see this Joseph Lindmayer and Chuck Wrigley always sitting together and talking to each other. What is going on? Do you think they are homosexual?" And I said, "No, I assigned Chuck Wrigley to Lindmayer as to introduce him to my semiconductors, which I liked, and wanted and needed somebody for ordinary electromagnetism. And I told him I didn't like to teach no research, nothing.

But anyway, guess what? Chuck Wrigley and Joseph Lindmayer wrote one of the first books on transistors. Perhaps you can find this book. Lindmayer and Wrigley: one of the first books on transistors, and it is a very book and they wrote it in my group without telling me. I knew nothing that they wrote a book, and they wrote it on my time at Sprague under my nose and published it. Now there I give you a story now to write up about Joseph Lindmayer.

BROCK: So when would that book have come out?

LEHOVEC: So this must have come out early 60s. It was one of the first books on transistors, and I heard it from other sources, "A very good book."

BROCK: I'll see what I can –

LEHOVEC: Now, of course, it goes back to Wrigley, in a sense, who took my course. But anyway, how can you help of not being a good book. But now, listen to that. On my desk, all kinds of notes and so on and so on working on many subjects. I came back and found out that Lindmayer had published several papers from my notes. He took my notes and published them as papers.

BROCK: Did he think they were just the property of the company?

LEHOVEC: No, no. So I said, "Joseph, how could you do that?" He said, "It's your fault. Why didn't you publish them?" "It's my fault?" Just like that.

And then, Sprague hired a high-level retired Bell Lab executive named Fred Lack. R.C. Sprague, with his leg of technical knowledge, he believed in hiring high-level retired people from companies, which is good public relations. So he brought in Fred Lack and Fred Lack took the book, Lindmayer, and brought it back to Bell Labs and in my absence the Bell Lab guy—can't think of his name now—to take over part of my group—the part leading the semiconductors and devices, and he had no detailed knowledge—and he leaned on Lindmayer as his source.

So Lindmayer became the man who run this whole thing. Johnny Sprague knew nothing in detail and so Lindmayer run it. What Lindmayer said was done. And for instance, Succina [00:29:45] came joined the company and Lindmayer didn't like Succina and Lindmayer said to the guy –

BROCK: This is in, like, now in the '63 to '66 –

LEHOVEC: In the middle or whatever. And Succina wanted to give a paper and Lindmayer said to—what is this guy? It will come to me. Said to him, "If Succina gives the paper, I don't present my paper. And Succina was presented the paper. This was the power of Lindmayer.

And Carl Busen wanted to publish a paper about a lateral P/N junction transistor, not laterally. The paper was in and a technical assistant developed a method of making MOS transistors, which by adding comume to the oxide, which makes them better hardening for radiation. And they were tested by another group at Sprague whom Johnny Sprague put in, and he said they were circuit people. They're very good. And Lindmayer forbade this man to work on this subject. The technician had developed it on his own. Lindmayer forbade it and filed a patent on it on his name. So this was Lindmayer.

And he stole from everybody in sight, and Carl Busen left the company because of it. And eventually, it got so bad it got to Johnny Sprague. And Johnny avoided me when I came back from Europe. He never came to me, consulted me for anything. And, of course, if I would have been a politician, I would have settled with Johnny, who was my friend, and advised him and I could be what Lindmayer was on a higher level, but I'm not a politician.

So I avoided Johnny and Johnny avoided me. Draw your conclusions. And Lindmayer essentially was fired by Johnny Sprague. And the next thing I heard of Lindmayer was from Eddie Ritner [00:32:33]. Eddie Ritner had worked for North American Phillips in New York City and I knew him from science field contacts. And Eddie called me and said, "Kurt, I got a guy here, Lindmayer, and he worked at Sprague and he doesn't give you as a reference. What is going on?"

I said, "Eddie, he is an extremely," – before I say that, I have to go back and tell you something else what happened at Sprague with Lindmayer. But I said, "Eddie, he's a very smart

guy, very personable, but don't turn your back on him," and he laughed and said, "Oh, I can take care of it." And what happened? He hired Lindmayer, put him on the NASA solar cell program.

Lindmayer made contact with senators in Washington, stepped out from NASA, stole their procedures –

BROCK: For making solar cells?

LEHOVEC: For space. And set up his own company.

BROCK: What was his company?

LEHOVEC: Solarex [00:34:00], which became the biggest solar cell company in the world. And NASA lost, and they lost because Lindmayer had taken all the prints from NASA to set up his company. But, NASA found out that he had taken the prints by illegal means, so they couldn't be kept in court. So this Lindmayer, who stole from everybody, became the head of the biggest solar cell company in the world.

And first I tell you things what happened in Sprague with Lindmayer. Lindmayer made two contributions to the field at Sprague. One was the metal-based transistor. The junction transistor speed is limited among others by the resistance of the intermediate layer in the P-layer resistance [00:35:17] _____ time constant and so on. The resistance plays a role in limiting the speed.

And if the P-layer resistance could be diminished, smaller resistance, speed would go in. So there was an idea of making metal-based. Nobody had made one. So Lindmayer applied for a contract on it. Sprague got the contract. Lindmayer put on this—Jim Casey, who was my godfather and no good technically. But, they made a metal-based transistor under this contract, and Lindmayer gave a paper in Toronto in a meeting of The Electrochemical Society.

And I went there to this meeting and was very impressed by Lindmayer's paper. He's an excellent talker. He showed characteristics that were astonishing. Everybody was amazed and applauded. And I say it, this is a great development.

I was at that time a consultant at Sprague and senior wise man, so I wanted to look at this transistor. They couldn't find it. I asked Jim Casey if they had made only one sample and it disappeared. I believe they never made the working transistor; they just fabricated characteristics they showed.

Then, there is one—there is no semi-conducting conductor. Lindmayer came up with the idea that Philco silicon transistor can be operated as an inductor and under certain

circumstances. And, he applied for a government contract on it. I don't know if they ever published it, but he applied for a government contract on it and got on.

And in his application, Lindmayer calculated the inductants as related to the structure, and he mentioned that he had actually measured it on a Philco silicon transistor. Sprague got this contract and they hired a new guy to work on the contract, and this new guy, of course, studied Lindmayer's papers on it and he found out that Lindmayer had made a mathematical mistake and it could never give the conductants Lindmayer derived of having the conductants. And now, he was—and he went to IBM in Germany and got a job there.

So Lindmayer lied, falsificated, stole and become the head of the biggest solar cell company of the world.

BROCK: Wow.

LEHOVEC: An American career. What takes it to make a career in America? To be a liar and thief and a nice public image.

BROCK: Well, there were two things that occurred to me to ask you about: One is about—it must have been in this period of, I don't know, the mid-60s, the ion implantation work started going at Sprague, or was that more in the late 60s?

LEHOVEC: No, no, quite early.

BROCK: How did it start?

LEHOVEC: Well, I'll tell you. While I was in Europe and Sprague—first of all, this Mr. Lech, who R.C. Sprague handled as a consultant—he told R.C. Sprague that the way to make progress is a Bell Lab way, and the Bell Lab way is to hire bunch of PhDs who can then charge the salary as 10 percent of their profit, so the more they hire, the greater their profit. But for a company like Sprague—and the events were in the period of my absence—while I kept a very limited, lean, excellent goal. When I came back, I found new faces all over the place.

BROCK: Was everyone in that mill building?

LEHOVEC: Yeah, in the new research building. No, no.

BROCK: They built a new building?

LEHOVEC: They built a new research building across the street. It still exists there, but now it is sold to some other outfit. And in my absence in Europe, they dedicated it under great _____ [00:41:36] and so on. And well, so they hired many people and some good people among them, and one was Ken Manchester. And there was another one and I can't remember now his name.

And these two people brought with them the ion implantation. And I had thought about ion implantation much earlier—ten years earlier almost, when I came to Sprague—and almost put Leo Fedotowski, my equipment designer, on it. But I believed erroneously that they had this ion implantation.

BROCK: When you came back from Europe?

LEHOVEC: From Europe. But as they didn't do anything with it. And Sprague hired them; was in a terrific expansion mood. And they hired two little outfits which had split from other companies. And one was an outfit which had split from Texas Instrument, and it was Comsat [00:43:00]. And they brought the Comsat people, who had really nothing to offer, especially,

but they looked at the ion implant that Sprague had and they immediately realized, “This is what we need in the MOS transistor field.”

BROCK: That’s Mostech [00:43:25].

LEHOVEC: Mostech. Comsat was the NASA company. They realized that this is the key now to control the surface states in the new field of field-effect transistors. And they did it, and very soon. Mostech was more valuable than the entire Sprague Electric Company. And then for one reason, and I only hear it from others, Mostech collapsed because it went into the memory field and over-expanded or something.

And incidentally, at USC I selected—when I came to USC, I selected three subjects to work on with my students. And one was theoretically on gallium arsenide [00:44:39], where MacDonnell Douglas did the experimental work. Another one was MOS memory systems, and particularly what’s going on in the MONS—what’s going on in the nitride—and I published several papers on it. And what was the third one?

BROCK: Silicon on sapphire?

LEHOVEC: Exactly. And silicon on sapphire was the third one. Now the silicon on sapphire got nowhere and eventually—practically got nowhere. So that is the Lindmayer story.

BROCK: I know that at the time that the ion implantation stuff was really –

LEHOVEC: I came back from Europe. And this is, again, my personality. I kept deliberately out of all the, "What is Sprague doing?" For instance, I never talked to Johnny Sprague at all about the programs and other work and so on, and he didn't talk to me.

So he gave me a very small group of people—two or three people—with whom I could do whatever I wanted to, and one of them was Hans Schier and the other was Leo Fedotowski. And I got them because they didn't know what to do with them. And they were two top experts in the fields. And the third one was a man I can't remember now,. a new one who they hired from the PhD from the University of Illinois.

And I worked with them on imaging: translating electronic information into visible things. And I did very strange things. In one case, I found out in the literature that clay, of all the ingredients—and I found out from the literature that the transparency of clay layers depends, changes with the electric field applied to them. So I made image _____ [00:48:03], so to speak, of clay. When I applied an electric field, the objective properties changes visibly.

BROCK: Interesting.

LEHOVEC: But I looked at different types of clays that behaved differently, but I couldn't identify the ingredients. And the other thing was they aged. When I did that over and over again, it changed. And I did that with Leo Fedotowski. So this got nowhere.

And then I revised something which was started during the war at Prague, but not by me but my somebody else in the group under Professor Gudden. And this is to a visible image, and particularly for infrared detection. And the way it works is as follows: You have a cathode in a vacuum cube and the cathode is coded with, let's say, a semiconductor.

And then, you have an electron beam scanning this surface.

BROCK: Of the cathode?

LEHOVEC: Of the cathode. And if the potential of the cathode, it depends whether the electron beam scanning it is deflected without impinge upon it or impinges on it and discharges through it. So it all depends on the surface potential of this thin film. And if, for instance, you

charge it and then you discharge it with an infrared beam photoelectrically, then the surface potential changes depending on where the infrared beam hits.

And I wanted to build this type of structure with a tantalum oxide and investigate by it, then, the surface structure of the tantalum oxide films. And I had Hans Schier build this equipment. We built it and it was amazing what we saw on the surface. The surface of the tantalum oxide film is not homogeneous at all, and then some patches, the beam is deflected and others it goes through, so you see cheesecake type of images on top of it, and we had great fun with it. It was the second project I had

And the third project was this guy from Illinois for the more conventional way. So this is what I did with my group of three people. And he then very soon left for Europe, doing back and became a professor there, and I left Sprague, so this was my last contribution there.

BROCK: So what was that operation of Sprague?

LEHOVEC: Well, Sprague—as I said, they had their _____ [00:52:32], nothing exceptional.

BROCK: So it was just like a factory to do that?

LEHOVEC: A factory, yes. Manchester was one of the four. He had one of the heads. I forgot the other man's name: Manchester and _____ [00:52:51] ion implantation.

BROCK: John McDougal?

LEHOVEC: McDougal, exactly. Manchester and McDougal. These were the two people who had arrived at Sprague during my absence in Europe, and I had nothing ever to do with them technically, except of course talking with them and knowing about their projects and this sort of thing. But I didn't contribute to them at all.

BROCK: I have two very last things that I wanted to talk to you about. One was—forgive me, but I've forgotten the name of the man who convinced you to come to Sprague the first –

LEHOVEC: Preston Robinson. A great man as a man and in his philosophical attitudes to research and so on. His knowledge of what was going on in the world.

BROCK: He had developed the tantalum oxide capacitor?

LEHOVEC: To my knowledge, yes. He had the patent on it and had developed it.

BROCK: Did he do that while he was at Sprague or did he bring that?

LEHOVEC: No, no, he was at Sprague.

BROCK: He did that work at Sprague?

LEHOVEC: He was one of the earliest employees of Sprague.

BROCK: What was his background? Do you know?

LEHOVEC: Not really. Now he had to use a bacteria in the human bodies to generate potentials as power sources for electronic devices in the human body, so totally creative ideas.

And also, his ideas about—he didn't contribute anything to my field and so on. Oh yeah, no, this was stupid _____ [00:54:50]. Who was the other guy? No. I can't think of his name. Well, it may come to me. He had a great idea.

BROCK: Robinson?

LEHOVEC: Yeah, and he came to me with this idea. And his idea was the following: semiconductors and insulators, they differ in the bend gap. And, hydrocarbons have a big bend gap and ion insulators. And graphite is the old bend gap through it is a conductor. And, so why don't you look at the transition form insulator?

You take paint, you paint on a glass slide and you have an insulator: an organic hydrocommon insulator. Then you put this glass slide on a heating plate, you turn the heat on and I starts fuming. And when you do it long enough, you end up as a carbon layer. So, you catch it in between and you make a transistor.

And he wanted me to do the experiment, and I—and this was, again, stupidity—I was not at that time, now I am a wise man—I said, "Absolutely not. I will not do it," because in a transistor, you need two types of electrons and holds and you need a certain lifetime for them to go from the emitter to the collector, and lifetime means ideal crystal structure and so on. And

what you end up is not a single crystal, first of all, and secondly, this cannot work. Of never forcing anything down the throat of a man.

So he went to another guy in the Sprague research lab, and I wish I could remember his name. And this other guy was the top politician, and he assigned the project to this top politician. And this top politician, of course, was smart; not what I was. He simply did it and reported it back to Preston Robinson, "It didn't work," and I argued with him and refused to do it. Why didn't I say, "Great idea, let's try it," and come back in a couple of hours later and say, "It doesn't work." But this is, again, a lesson in life.

BROCK: How old was Preston Robinson when he was moved out of the company would you say? Roughly.

LEHOVEC: I would say he was in his 60s probably—early 60s. And what I should have done is when Dr. Lazier came in, I should have gone—stepped out of Sprague Electric and do it with two people: one person from the receptor company who at Signal Corps got me to be his consultant at the Signal Corps. He wanted to set me up as an independent company wherever I wanted. I could have gotten to him and could have said, "Mr. Kohn, I have developed now silicon components ready for sale. I'd like to step up out of the company. I need financial backing. Preston Robinson, I would like to use your lab to set up the facility. With these two people lined up with me, it was perfect.

I didn't do it. I stayed with Sprague and tried to appease Dr. Lazier by stopping work on silicon.

BROCK: When you were at—during the 1960s during your association with Sprague—in that period, it seems like you had an incredible amount of autonomy: the deal you made to go to Europe. When you came back, before you became a consultant it seemed like you had free reign to do what you want, and then you got a very autonomous sort of consulting arrangement in the end period. And I was just wondering: Were they giving you all this autonomy to maintain the connection so they could –

LEHOVEC: Certainly, in part: my opinion. For instance, there was a memory type developed by, I think, Rockwell based on the barium type. And I was already in California and they wanted my opinion on it whether Sprague should buy into it and so on. That was the only one thing they wanted my opinion ever since then, so it was not to get my opinion.

There was a, I believe, a cognition that my PN junction patent had become very valuable and they simply thought, "Keep this guy and he will make another great invention." And this is where I came in and I deliberately decided not to make any contribution to Sprague which was commercially valuable. And for this reason, I closed my eyes to the very important development in semiconductors, which was kind of stupid and immoral almost.

So I was in a very psychological dump at that time. Coming back, my –

BROCK: The association, then, if you did make another real big invention, they would have it?

LEHOVEC: Yeah, exactly.

BROCK: And also, if you went somewhere else, somebody else would have it.

LEHOVEC: Well, I don't think they wanted to keep me out from companies. Perhaps they wanted to keep me from competition, but mainly I thought I would make other big contributions was the main thing. And didn't cost much, but they didn't take advantage of me in their procedures. I never visited the plant in Brewster [00:63:17] not a single time.

BROCK: They didn't take you around to other research groups?

LEHOVEC: No, they didn't take me. They left me alone. I could come and go whenever I wanted. I didn't have to punch in the time clock. And of course it made it a bit difficult to live. I had my family here, my nice home at the Green River and children went to school here and I got a reasonable salary for doing whatever I wanted, so many people kept saying, "What more can a man want in life?"

But of course, I wanted more. I wanted to continue in not only creating interesting ideas, but to actually execute them and come out with hardware. And this I was deprived of and never got into it again. But you know, typically for how strange I am that my university—the University of Southern California—doesn't know about my P/N junction invention. They don't know it.

BROCK: So they don't make any noise about using Ameritus?

LEHOVEC: Do you know that a retired faculty—they have a procedure where retired faculty get a one-time grant of $1,000.00. Do you know that I never got this grant. Everybody else got it.

BROCK: To support your research or whatever?

LEHOVEC: No, just as a thank you for service.

BROCK: Your service to the institution.

LEHOVEC: I set up the semi-conducting labs, which were very popular and so on. I made the research; wrote more research papers there than anybody else. I didn't get the $1,000.00.

BROCK: Why?

LEHOVEC: Because I have to apply for it and I have to list as other faculty members to support me, and I don't know where the other faculty members are now. They are retired.

BROCK: Yeah, it seems like they would want to make more of their association with you.

LEHOVEC: Exactly. They could use—I mean, the university depends a lot on getting grants and reputation. I think they could make a big splash out of it: "Here we have a man who

invented a key ingredient to the microcircuits and so on." Of course a point would be that didn't do it at the university but I did it before, but nevertheless, they could say something.

And I really became ashamed to talk about it. I never received—on all my contribution I made, I never received a reward from any outfit in the world.

BROCK: A prize, an award?

LEHOVEC: Never a prize; never. I received a Fellowship of the American Physical Society and Fellowship of the I Triple E very early in the early 50s, but never anything else for the PN junction. And I almost am ashamed because if for some reason—for contracts and so on—they have to write up about me and other people did anything.

BROCK: That seems peculiar to me. Maybe that also relates to moving quickly from one area to the next.

LEHOVEC: Definitely there. If I would have stayed in any one area, of course I would be the key man and so on; might even have gotten a Nobel Prize for light-emitting diodes or something. So that's how the cookie crumbles, as they say. But as I said, I don't feel regrets

about the whole thing because I know that in order to make these contributions, I need total isolation and concentration.

And all those other things require involvement with others, and so I have a choice: involvement with others and get a prize and become well-known in one given field or go happily own as an unknown and make contribution in new fields, and I prefer the second one hands down.

BROCK: That sounds like a good place to pause.

[END OF AUDIO, FILE 4]

BROCK: Okay, recording again.

LEHOVEC: One brief remark which comes now to my mind that one of the two mean whom Sprague hired to replace me was Dr. Link Vulgar [00:00:29] from formerly Bell Lab. And when you look up his contributions, you will find one paper I think he did, and this was all his knowledge on the field of semiconductor devices.

The other one was Dr. Fowkes, F-O-W-K-E-S, who came from Stanford and was the expert in surface tension. He got the basic part of my group and Vulgar got the device part of my group. Now, my involvement with the MOS situation—I'm trying to think of the year when that happened. It was soon after Johnny Sprague joined the company and worked under me.

I assigned him to the MOS field, which of course, at that time, MOS transistors had already come into being and the whole world started making them field-effect transistors, making them work. And the problem was that nobody could control the surface states with other works. If you apply on an ordinary capacitor a voltage, you know from the capacitants the charge from the thickness and dialectic constant of the insulating layer of the charge which you induce by this voltage on the other side.

And if this charge is all mobile, then you can move it laterally in a field-effect transistor. And this is the idea of the MOS transistor. The problem is when you do it with the MOS structure, the charge induced is not the one you would expect from the capacitants or thickness of the oxide _____ [00:03:22] applied voltage because some of the charge is not mobile but immobile in surface states.

And if you cannot control this, you can't control the turn on voltage of the field-effect transistor, so you cannot use it in circuits in manufacturing. And so the whole trying to contour the surface charge [00:03:54]. And eventually, it was solved by two steps: One, to determine the number of the surface states which are where the charges are mobile. And the second one is to compensate for this number once you have to establish it buy ion implant into the silicon.

And the question is: How do you know how much ions to implant in order to compensate the surface state charge? So you have to know what is the surface state charge. And there was a meeting of The Electrochemical Society in Dallas, Texas, and this happened soon after Johnny Sprague joined the company. Now, I don't know when it was, perhaps 1958; I don't know. It was in the late 1950s.

And I went with Johnny to this conference, and there was a scientist from England, a lady named Dr. Berz, B-E-R-Z. The capacitants as a functional voltage—possibly frequency; I don't know. And the analysis was highly theoretical and totally based it on the electrochemists, obviously. And it was not very practical.

And Johnny wanted to take notes and I waved him off. I said, "Don't take any notes. I know exactly what she talks about. I go back and I can help produce it immediately."

[END OF AUDIO, FILE 5]

LEHOVEC: So, then coming back to Sprague, I looked into it, and I thought I could reproduce it afternoon and I realized it's much, much more complicated, particularly when you look at the frequency aspects and so on. So I started a project on it, both theoretically and practically, and I

put Johnny Sprague on it to make this MOS structures and through the measurements. And I worked theoretically on it and used my assistant, Slobodskoy, to help me in the theoretical part.

And then there was—soon after—just before I left for Europe, I think in 1961, there was a meeting at Stanford of the I Triple E Research. And this was interesting because I was, I guess, the only man ever who presented three papers on different subjects at the same conference. And one was the subject of the MOS capacitor surface states. And in my list of references, I cite this conference and the title of my paper, namely, "How To Determine the Concentration of Surface States by the MOS Capacitor Analysis."

And I presented some data which we had and the paper, but essentially is the principle of the whole thing. And then I wrote this paper with Johnny Sprague and Slobodskoy and submitted it to the physical review and left for Europe. And now, again, something incredible happened. The paper was rejected by the physical review. Johnny Sprague got the rejection because I didn't have my European head. And, he didn't inform me about it. He didn't inform me in Europe about it—about our paper.

And when I finally got in contact with him, he told me, "It has been rejected." And I said, "Where is the rejection? What grounds?" So he sent me the rejection and it didn't make any sense.

They rejected it for two reasons: Reason No. 1, the review said, "Why is he rushing into publication? They also should first learn of how to control the surface states." There's a whole field. Thousands of scientists work on it.

And the second reason was: "And, by the way, the method was not completely new." So, they cited a British book which was not available. I was in Vienna at that time. I couldn't find it in the library in Vienna and I finally got it from England.

And at that time, they had existed already: so-called surface conductants method where they measure the electrical conductants in use by the applied voltage on moist transistor, and from this conductant, they tried to decide something going other. And there's already published papers on that. But in this book, which I finally got from England, somebody mentioned the inset of the electrical conductants method one could also use vertical capacitants method. So just mentioning the possibility to use this method was the only reference.

And these two stupid remarks: "First control it before you talk about it," and, "It is not new," eliminate my _____ [00:05:24]. One was—it may have been Bratton who dislikes me. Bratton dislikes m very much. And he dislikes me because he is a great experimenter and not a theoretical man at all, and I just splurge out ideas without the trouble of experimenting. And he has to work very hard at something and I just give an idea. And this is part of the reason he dislikes me. And I don't know what other reasons he may have.

It was even so bad that at one time at the I Triple E conference he said if I am the invited speaker, he will not speak.

BROCK: It was personal at that point?

LEHOVEC: Personal at that point, but I don't really know why. I never had any fight with him either on scientific level or otherwise. I have no idea. But more likely, it is the following: that at the Stanford was a professor who came from Bell Lab, and I just cannot remember his name. If I would know his name, you probably would have heard of him.

And he had a student, and this student—working on the same field—and this student and this professor attended to IBM and published on the same subject, "How to Determine Surface States by a Moist Capacity."

BROCK: Gibbons, would that be?

LEHOVEC: No. But in my paper—so what happened to my paper? Now it was too late to—I didn't know that this other guy published all of this. I didn't know that. But it was too much time elapsed between the rejection and now going back after a year later and seeing a written

rejection. So I took the paper as it is and sent it to a journal which had just started in Germany, Physical Statusology [00:07:59].

And I sent the paper to The Physical Review. I sent it to them and they printed it without question immediately. But when it came out, it was already second to the paper by this guy who had listened to the lecture and went to IBM, and so initially, there was some question about the principles of the method. But then in Europe, with my assistant, Slobodskoy, I worked out in great detail this method in the United States and I mentioned to him about, "I wrote this paper that is so long and I hate to publish long papers." And he said, "No, send it in anyway," and he published it.

And this is now very _____ [00:08:57] which has capacitors and resistors which relate to the surface states to transition of electrons into and out of surface states into the whole thing and gives a total thing [00:09:20]. And I later expanded that in two respects: One is the original papers referred to surface states of one energy level. And I made a very simple expansion to a continuum spectrum of energy levels, which is really a very simple theoretical step. And this paper is very much quoted, too.

And the second one is: I published a simplified method to determine the surface state. It is entitled, "Simplified Method for CV." And this is very, very much published, even today. Everybody uses this simplified method. But if you ask me, "What is a simplified method?" I don't recall. I would have to read up on it.

BROCK: And that has become—those CV measurements are just ubiquitous in process control and everything.

LEHOVEC: Exactly.

BROCK: Yeah.

LEHOVEC: So if you would ask me for my greatest contributions, according to my own feeling it is the explanation of the solar cell effect, which is not recognized widely at all. And No. 2, the explanation of the light-emitting diode, which is recognized widely. Not necessarily in sequence of importance.

The P/N junction isolation, which is achieved through all the microcircuits—the CV method, which is a key to all the microcircuits in processing. And last but not least, right now, this strange paper, No. 17, on the new supposing _____ [00:12:01] make me a billionaire.

BROCK: I can't wait.

LEHOVEC: Neither can I. But I don't know that I have the time to wait.

BROCK: Have you filed a patent for that?

LEHOVEC: For desalinization, yes. It is in the patent office.

BROCK: I'll have to –

LEHOVEC: I'll have to wait now, but my great problem is to file it internationally because it obviously is not restricted to the United States.

BROCK: Is there anything in that desalinization method that's connected to semiconductors or that is wholly different?

LEHOVEC: Nothing with semiconductors; nothing.

BROCK: I wouldn't see how.

LEHOVEC: Nothing. And I don't—I am tempted to tell you about it, but I will not. But I will tell you one interesting thing: It goes back to an observation I made at Prague in the 1940s in my lab there. A simple observation for no purpose, particularly, I just noticed something. It surprised me, in effect, but I didn't realize its importance at all and it was buried for 60 years in my mind.

And it suddenly came to my mind in Los Angeles by the government by using osmosis, and I thought, "Oh my God, what I observed 60 years ago could be useful." Isn't that strange?

BROCK: Very. Yeah. So when did you file that patent?

LEHOVEC: I would say about half a year ago or possibly one year ago. First I got the typical kind of rejection that the line spacing is not correct and I have to do it with a different line spacing or whatever it was. It's a formal—but then they wrote me, and again they give me: they have a now procedure to accelerate an investigation with respect to age for old people. It seems I qualify; I fall under the accelerated review process.

BROCK: Good.

LEHOVEC: Hopefully it comes out before I leave this earth and go somewhere else.

[END OF AUDIO, FILE 6]

[END OF INTERVIEW]