CHEMICAL HERITAGE FOUNDATION

HARRY SELLO

Transcript of Interviews
Conducted by

David C. Brock and Christophe Lécuyer

at

Menlo Park, California

on

4 November 2004, 7 January 2005, and 16 March 2005

(With Subsequent Corrections and Additions)

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Harry Sello, interviews by David C. Brock and Christophe Lécuyer at Menlo Park, California, 4 November 2004, 7 January, and 16 March 2005 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0300).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

HARRY SELLO

1921	Born in Chernihiv, Ukraine on 20 March

Education

1942	A.B., chemistry, University of Illinois
1944	M.A., physical chemistry, University of Missouri
1948	Ph.D., physical chemistry, University of Missouri

Professional Experience

	Shell Development Corporation
1948-1956	Research Chemist
	Shockley Semiconductor Laboratory
1956-1959	Senior Staff
	Fairchild Semiconductor Research and Development, Palo Alto, California
1959-1981	Head of pre-production engineering section, Semiconductor Research Laboratory
1962-1964	Operations Manager, Societa Generale Semiconduttore
1966-1967	Manager of Materials and Processes Department, Semiconductor Research Lab
1967-1968	Technical Planning Director, Research Laboratory
1968-1980	Technical Director, International Marketing Division
	Harry Sello and Associates, Menlo Park, California
1980-present	President

Honors

1944	Pi Mu Epsilon, Mathematics Honorary
1948	Sigma Xi, National Research Honorary
1976	Chairman, International Committee, American Electronics Association
1982	Chairman, International Committee, American Electronics Association
1990	Export Executive of the Year, Northern California District Office, U.S. Department of Commerce

ABSTRACT

Harry Sello begins the first interview with a review of his childhood which included emigration from Russia and a strong emphasis on education in his household. Sello quickly became interested in chemistry and completed undergraduate work in organic chemistry before applying this knowledge to his Ph.D. research on the rearrangement of single molecules at the University of Missouri. He completed service in the United States Navy and modeled scaling-up procedures of flammable compounds at Shell Development Company. William Shockley recruited him to Shockley Semiconductor Laboratory, from which he departed on suspicion of connection to the founders of Fairchild Semiconductor. At Shockley and then at Fairchild, Sello worked on a variety of chemical aspects of semiconductor manufacturing. At Fairchild Semiconductor, Sello concentrated on the transfer of silicon transistor technology to Societa Generale Semiconduttore in Italy, negotiating cultural and industrial boundaries. Sello remained with Fairchild Semiconductor during its decline, reorganizing research and production. In 1980, he began Harry Sello Associates after Fairchild Semiconductor was sold to Schlumberger Exploration. Sello concludes the interview with reflections on his current role as an expert witness.

INTERVIEWERS

David C. Brock is a senior research fellow with the Center for Contemporary History and Policy of the Chemical Heritage Foundation. As an historian of science and technology, he specializes in oral history, the history of instrumentation, and the history of semiconductor science, technology, and industry. Brock has studied the philosophy, sociology, and history of science at Brown University, the University of Edinburgh, and Princeton University (respectively and chronologically). His most recent publication is *Understanding Moore's Law: Four Decades of Innovation* (Philadelphia: Chemical Heritage Press, 2006), which he edited and to which he contributed.

Christophe Lécuyer is a graduate of the École Normale Supérieure in Paris, and he received a doctorate in history from Stanford University. He was a fellow of the Dibner Institute for the History of Science and Technology and has taught at the Massachusetts Institute of Technology, Stanford University, and the University of Virginia. Prior to becoming a senior research fellow at CHF, Lécuyer was the program manager of the electronic materials department. He has published widely on the history of electronics, engineering education, and medical and scientific instruments and is the author of *Making Silicon Valley: Innovation and the Growth of High Tech, 1930-1970* (2005).

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INTERVIEWEE: **Harry Sello**

INTERVIEWERS: **David C. Brock and Christophe Lécuyer**

LOCATION: **Menlo Park, California**

DATE: **4 November 2004 [Interview 1]**

BROCK: This is an oral history interview with Harry Sello, taking place in Menlo Park on 4 November 2004 with Christophe Lécuyer and David Brock as interviewers.

So we found out, Harry, I think if our research is correct, that you were born on 20 March 1921 in today's Ukraine, in a town, Chernihiv?

SELLO: Chernihiv.

BROCK: Chernihiv. Thanks.

SELLO: Great work. You guys have been digging. Good work.

BROCK: So we wanted to ask you a little bit—if you could tell us a little bit about your family's background both on your paternal and maternal sides.

SELLO: Be glad to. Well, my history starts with that device—a samovar. My history starts with the Russian samovar. The reason I say that, it was practically the only item of belongings that my father and mother were able to take with them, that they could carry. All else was forbidden when they left Russia in 1923. So I was then a year and a half or two and we moved to the United States. Being Jewish, we had to get away, they had to get away, from the Communist changeover, which wasn't a changeover as far as Jews were concerned. It wasn't a changeover for a lot of other Russians, who thought at first it was, too. It just wasn't. It was a bloody time. My mother had told me long stories about this.

My father had to illegally escape. He left in 1922 about a year ahead of the time that I came with my mother, in 1923. The story of his escape is kind of revealing. He was in charge of a military patrol, on the Russia-Latvia border. He carefully arranged that his patrol went to patrol the border but he went in the other direction and he crossed it into Latvia and reached the city of Riga. He described that in great detail when I questioned him years later. So he left just about the time I was born—we were apart at that time—a year to a year and a half or so. He floundered around in the Baltic countries. We were reunited in Hamburg, Germany.

Well, he was a graduate accountant. My mother was a registered nurse. Both served in the Czarist army on the German front, which rapidly disintegrated and became the Communist army, which was even worse. And my mother often said that if she hadn't tried to get away from that, she wouldn't have lasted very long among the soldiers, especially her fellow soldiers. That women in the Russian army were highly touted just wasn't true.

So she started to communicate with my father, who was at that time somewhere in the Baltic countries, later in Hamburg, which is where they left from to get to the U.S.A. Mom had the job of shoving herself and me through the controls at the Russian border. About my dad, she just had to say that he ran away from home, because if she told them he deserted the army, they would have stopped us.

BROCK: Yes. Sure.

SELLO: I had a sister, ten years younger who was born in Chicago, Illinois. She unfortunately, died three years ago. My story to you is easy to tell because I had to repeat it all to her later in our history. Some of this may be detailed and is not necessary. My mother and I went to Hamburg, met my father, and departed from there to New York City, not Ellis Island. That was a lucky break because émigrés were pouring into Ellis Island and it was a mess to try to get through. My folks were afraid they would never be able to get their credentials through. We arrived in New York City on 1 October 1923 on the SS *Orca*.

BROCK: Did your father and mother have any family contacts in New York?

SELLO: No. They had a contact in Chicago. So that was their pass. Their substantiation was my mother's aunt, my great-aunt, who was in Chicago at the time and that was the destination. They spent some time in New York. Anyway, it was difficult for them. They had to scrounge for the money, and my great aunt, although she was very generous, was not quite that generous. [laughter] Things about, "Go find yourself a job first and then come to Chicago." I don't know all of those details.

My dad was a member, in Russia, of a revolutionary group led by a man by the name of Jabotinsky [Ze'ev Vladimir Jabotinsky]. I don't know if that rings a bell with you. Jabotinsky was a Russian Jewish patriot, sort of a Patrick Henry. His *métier*, his group, was organized to form a revolutionary group against the Czarist Whites at that time, and my father had joined that group. However, shortly thereafter, Dad left it because Jabotinsky wanted to go to Israel, to the Holy Land and was raising a Jewish brigade to fight there. And my father had to make a decision. He didn't want to go to Israel. He said, "We have had enough of—." What did he call it? "We had enough of rebuilding the country. We know where there's a country that has been really built and it's already a democracy, and we don't have to build another one." [laughter] He was very adamant about that. Very adamant. So he got away from Jabotinsky. Many years later when Dad visited Israel, he met Jabotinsky again. This was a reunion and there were many tears shed over old history.

But that was his reason for coming to the United States, where there was an established, known democracy. And he hoped to pursue his vocation of accountancy. He did for some time, but it didn't last too long. My mother got a nursing license and so she practiced as a nurse, sort of a practical nurse because she was not familiar with the language. She took on private nursing accounts that her doctor friends referred to her.

We settled in Chicago. I've set a record personally in Chicago in that I think I went to more grammar schools than any other student. [laughter] They moved around in Chicago.

BROCK: Had your father been—was he an accountant for a larger firm or a company or was he working for himself before the war?

SELLO: No, he was an accountant for a small company in Chernihiv. In Chernihiv. His father, my grandfather, then lived there and he was—I would state that his degree must have been equivalent to something just beyond our junior-college level in this country, about a B.S. level—not quite an M.B.A. As a matter of fact, the truth of the matter, he couldn't go any further in Russia because he was Jew. School was—

BROCK: Closed to him. Yes, sure.

SELLO: Closed off at a certain level and he went as far as he could. But he then practiced when he got to this country. He then set up a small consulting practice to Russian friends, many of whom had come over about the same time.

Now, the period of 1923 was a colorful one because it was in 1924 that the U.S. cut off immigration from Eastern Europe. But it closed, immigration closed in 1924, so we just beat the gun by one year. You know, I'm very grateful for this, and when I look at my samovar I'm reminded of how fortunate we were.

BROCK: So had your mother and your father sort of been communicating in secret by letters or something to arrange your departure?

SELLO: Yes. Yes, they communicated. Dad wasn't there, so she would mail—send letters from different places, not always from Chernihiv, from the nearby city of Kiev or from someplace else. She didn't want to establish any patterns. She'd send these picture postcards. She's holding me and she's saying in one line in Russian, "Where are you? I'm ready," you know, this kind of stuff.

BROCK: Right. When you and your mother left, was that a legal emigration?

SELLO: That was legal, yes.

BROCK: Yes, at that time—

SELLO: Yes. Papers. She got papers.

BROCK: Okay.

SELLO: She was afraid of that. It's a very good question. She was very much afraid that she couldn't carry out with the story with my dad, in that they'd wonder, you know, where is he? Why isn't he with his family? But that was a very hectic period. It was also the period of the big flu epidemic in Europe.

BROCK: Oh, right.

SELLO: That was a tremendous one, I guess. We haven't had one like that, knock on wood. She was a nurse, so that helped the situation.

BROCK: Right.

SELLO: But we had the papers. I have copies of what few sheets she saved. I have a copy of an entrance visa, which I thought for many a year was my birth certificate, but it's not. [laughter] It's the exit visa, rather, from Russia, but it states the birth circumstances in it. Well, so theirs was a legal immigration and a legal entry into the United States. That was all cleared up at Hamburg.

BROCK: Great. And when they settled in Chicago with your mother's great-aunt, did they settle into a predominantly Russian community within Chicago?

SELLO: Yes.

BROCK: Can you tell us a little bit about that?

SELLO: Yes. I would say that it was certainly an Eastern European community. There was a Polish neighborhood here, Russian neighborhood here, divided into Russians, Ukrainians, and Jews, and there were a couple of other Eastern European ethnic groups all living in the same area. And as I look back, I am amazed that it worked. These groups were not all very fond of each other.

BROCK: That's interesting. That was exactly what I was going to ask.

SELLO: And there were frictions. If I was on the wrong side of Humboldt Park on a Sunday when it was Constitution Day for the Poles, I had to watch carefully, looking over my shoulder to see if any of the drunken Polish guys were coming along. But nothing ever happened. It was remarkably integrated, and I've always been surprised at that, when I hear of, you know, groups here that can't seem to find their niche.

We lived right around the corner from the Ukrainian church and I played ball on the steps of the Ukrainian church without realizing that it was an Orthodox church. [laughter] My dad let me know about that. He said, "Did you get permission?"

I said, "From whom? God?" [laughter]

No, it was an integrated community.

BROCK: But you were saying that you did move around a lot.

SELLO: Yes. The moving around was economic. We were very poor, extremely poor, and my dad had a pride bigger than he was and he wouldn't ask anybody for a nickel. My mother used to pound on him. She'd say, "You know, what does it hurt? Go ask Aunt for some help."

"Absolutely not. Never. If she can't volunteer it, I'm not going to ask her," and he just never did.

So it was a pretty hard life for him. He had to give up his accountancy and he worked at several menial jobs. He worked for a time as an insurance agent for the Metropolitan Life Company—among the émigré groups. He was also an assembler in a picture frame facility.

On his own he decided to become a milkman because he got a hold of a small truck and he painted it white and then he went around to the nearby local dairy, which was a smaller dairy than the big ones that are there now. I remember it's called the Blue Ribbon Dairies. He offered to become a milkman and they didn't—you know, they couldn't—they didn't want to hire him. He said, "Well, look, I have a truck. You don't have to pay for the truck. I'll use that in the job. All you have to do is let me carry the milk." Well, that worked, so he became a milkman.

BROCK: Was that during the Depression?

SELLO: That was the depression time. It really hit in the early 1930s and they were still, you know, very much uninitiated to the country.

BROCK: Sure.

SELLO: Language was a problem, although they worked like hell and they both quickly became naturalized. I remember very tearfully attending my mother's naturalization. I was very proud of her. She studied like blazes to do that, and she had no patience for anybody who didn't want to do that.

BROCK: Interesting.

SELLO: Yes. She wouldn't be able to tolerate many of the people that are around these days that don't want to go through the trouble of becoming citizens or being naturalized.

BROCK: Could you tell us a little bit more about what your father was like as a person? You've just mentioned his pride.

SELLO: Yes. Pride goeth before us all. He was fairly highly educated, beyond his degree in accounting. He was extremely well read. He kept books in the house at all times. He only had one rule, and I remember to this day, he said, "You can read anything you want in the house. If you have any questions, ask me. If you don't want to ask me any questions, you can read anything you like in the house, but I don't want to see you bringing some trash that I don't know about." He said, "I'd like to read it, too." So that rule held up, because in the back alleys of Chicago at that time you could find trash, all illustrated. [laughter]

But he was extremely well read. Besides the Russian classicists, his favorite author was Jack London. He had read Jack London cover to cover, eight volumes, in Russian. And in later years when I took him up to the Valley of the Moon here, he literally bust. He says, "I am in the home of the saint." [laughter]

But both of them were very well read. He pushed me to some of these heavy novels, but it took me a long time to get around—I mean, how's a kid at the age of fifteen going to read Dostoyevsky? [laughter] But they considered books "religious."

BROCK: Interesting. And so they were very encouraging of you.

SELLO: Very encouraging.

BROCK: Did you take that up? Were you a big reader?

SELLO: Yes, I read voraciously. That helped. Yes, that helped because we had some very nice library restrictions in those days. You could take seven books at home to keep for two weeks, and I always took seven books at home to keep for two weeks and I might have read one or two. And my dad used to watch that. He said, "When are you going to finish those other five?"

I said, "Well, I'll go and renew them."

"Well, why are you taking them out if you're not going to read them?"

"Well, I don't know which one I'm going to read."

Continuous argument. But I was very encouraged to do that. The library system, and to this day, is still very good. Unlimited checkout of books, and I use them liberally. I pick them myself. I remember I read a number of things that Dad hadn't gotten around to, particularly some of the Alexander Dumas stories. Those, of course, I found right away, so I had to read them all. My habit at that time, like many other students, if you find one that's good, read every other one by that author. And I read *Twenty Years After* and 'Thirty Years After' and 'Forty Years After.' [laughter] Yes, reading was encouraged.

There was another interesting thing. But we struggled with language. The struggle was mine. They spoke Russian to each other and when I was small, before I went to school, before five or six, I spoke fluent Russian. To this day I still remember a good bit of it, not to speak, but to read and to understand. But it annoyed me that they spoke Russian to each other at home and for years I accused them of speaking Russian because they were trying to hide something from me. I said, “You’re making a mistake, because I understand what you’re saying, so you might as well speak English.” [laughter] They were just colloquially expressing themselves. I had a bilingual exposure up to the kindergarten days, up to about the age of six. It disappeared fast, although later when they would ask me to, visiting relatives, “Say something in Russian,” I generally didn’t want to do that. I was busy in English—interestingly enough, many years later, I qualified in Russian for my doctorate.

As a child, I was responsible for finding one of my parents’ best friends. I was out on the sidewalk playing with my friends. I was having a hard time explaining what I wanted to do, so I lapsed into Russian. You know, like, “My god, [Russian phrase]. My god, what are you saying?”

And this guy came along and he just heard that. He was a Russian. He heard the Russian, so he wanted to know where did I learn my Russian. So I dragged him home, and he became a lifelong friend.

I always felt proud of my parents in that they conducted business and did everything in English, but there was that little niche of using Russian at home.

BROCK: Think about the household and the emphasis placed on books and reading. Was religion a big part of your home?

SELLO: Interesting question. My dad broke away from Orthodoxy. He broke from his father. His father, my grandfather, was a rabbi, formal rabbi. He taught in Kiev. In Chernihiv, not Kiev. My dad broke away from him. That was part of the breakaway of the Russian Revolution, the so-called Russian Revolution and the civil war. Religious Orthodoxy was out both for the Russians and for anybody else. That was his position and it reflected on me.

BROCK: What about your mom?

SELLO: Same. Same. They grew up in an interesting ethnic background at that time. They were more Russian than they were Jewish. The Jewishness came from the synagogue, from the Orthodox people. In the course of the new Russiana being born, all of this got translated downwards to be more democratized, to be Russian Jewish or Jewish Russian, rather than the Orthodoxy. On this, Dad told me he had arguments with his father.

BROCK: I can imagine.

SELLO: Dad's father later came to the United States in 1936. He didn't stay. I could never imagine that. He came to the United States and there was a great reunion, I remember. I also remember very vividly that when my grandfather went to synagogue on Friday night, he would want me to go with him. So he took me by the hand and I'd go. I had to look to Dad and Dad would say, "You go where he wants you to go. If he wants you to go, you go."

"But why aren't you going?"

"He didn't ask me; he's asking you."

Yes, it was a hell of a bit of logic. Of course, it didn't make much sense to me because if Dad didn't want to do it, why should I do it? Big deal.

Today we call that more or less bordering Conservative/Reform in this country; not quite Reform, sort of Conservative. God was never an issue. To me, God was the Ten Commandments. That's how Dad translated it upwards. He said, "If you want to know what God's thinking about, read the Ten Commandments and understand them, and I'll explain them to you if you don't understand them." That's about the level of where it stood all the way through.

BROCK: Were your parents very politically active? Was that something that was big in their lives?

SELLO: Well, you've been doing this before, I notice. Dad was a violent, vehement, expressive anti-Communist.

BROCK: Oh, really. Well, given his experience.

SELLO: Yes, given his experience. Even given his—while he was in the Red Army.

BROCK: Right.

SELLO: He'd been present as a child in one or two of those big pogroms that were carried on in the 1900s. He definitely wasn't a White [Czarist] either, he said, "They're no better." He said, "The Whites come here and they beat us up and burn the houses, and the Reds come because they're going to protect you from the Whites and they do the same thing." And there were many stories, and I've read a long history of such stories, many recommended by my parents. White generals versus Red Army generals that fought each other. The movie

"Zhivago" that describes that history is a very realistic one. Also, the book by Sholokhov, *The Quiet Don*, is one of several he wrote.

But at any rate, he was anti-Communist. Now, that put him in a very funny position, and I didn't realize that till we came to this country. There were many Jewish folks here that had looked to the Russian Revolution as a new emergence. "Jews will be freed if you go to school." "They were no different than Russians." Here in this country, when he ran into that, he was just—it made him livid. I saw him one time kick a guy off of a podium in Humboldt Park who was exhorting the crowd and saying, "Jews, you want to get a new resurgence on life, go to Russia." Dad realized the U.S.A. was a melting pot; you had Left and Right and all that sort of stuff. However, he knew from personal experience that the speaker was wrong.

Strange thing, just jumping ahead—you're allowing me to do this.

BROCK: Please.

SELLO: In later years in some of my work on the technology transfers, I had a chance to go to Russia, thanks to Bob [Robert N.] Noyce. So I went to Russia and it was a very interesting trip. It was a scientific trip, but another whole story.

When I got back, the first thing I did when I got to Chicago was to say, "Hey, Dad, guess where I was?"

"Where were you?"

"I was in Moscow."

"Moscow? What are you doing there?"

"Not only Moscow, but I was in Leningrad and I was in Latvia."

He says, "What's this all about?"

I said, "Well, we had a chance to see if we could do any semiconductor business there, so I went to see if I could do any business. I saw the Minister of Electronics and I saw his staff and the staff of various research institutes."

I thought I was making a great impression, and he sort of leaned back when I told him that and I said, "Well, have any questions?" [laughter]

He said, "Yeah, I do." He said, "Do you think you can really do business with those bandits?" [laughter] He wiped me out. Simply wiped me out. And this is what, fifty years later. That was the attitude that pervaded through the household.

BROCK: I imagine, in the late 1920s through the 1930s, that's—and especially in Chicago—very politically turbulent times, and so it sounds like your father had maybe some strong political views, but he wasn't a real big political—

SELLO: Well, he wasn't a big political personage as such in Chicago—he was a very 'liberal' person. Russian Jews, of course, at that time largely came from the central plain, the so-called "Pale," which didn't reach as far north as Moscow, but reached up to the Baltic countries in the west. That related to the community we lived in. There were, at that time in Chicago, many Jews who came from towns, i.e. "shtetls," similar to that of my folks, then Chernigov. My folks got to know many people from these shtetls.

And I recall, as refugees began to pour in from Eastern Europe in the late 1930s, there was a regular stream of Russians through our house who would come, you know, to somebody they remembered, as many non-Jewish Russians as Russian Jews. I remember these herring and vodka bouts that went on all night long. You had to drink a little, but then you had to eat something. You know, a hunk of salted herring, which I couldn't abide; it helps you drink. [laughter]

So there was a big mix of that. Our home was a sort of émigré mecca. Very politically oriented, not particularly in any one leaning, not a Socialist or a Communist or any—just an American, just wide-open thinking. And there were church people that came. Dad got into arguments with priests and rabbis—but it was a matter of politics, of what was going on in Russia.

BROCK: I see.

LÉCUYER: So would you say that in the U.S. context he would be a liberal Democrat?

SELLO: Yes, I would say so. I'd be more liberal than he would, I think. As I recall his voting record, yes, he was a liberal Democrat.

LÉCUYER: So, pro-New Deal.

SELLO: Yes, yes especially pro–Franklin Roosevelt. But to him that was the way a country should be. You could go ahead and vote any way you wanted, believe anything you want to; you just couldn't do that in Russia, and that's why we left Russia. So that would be his context. Maybe a little more conservative than I really cared for, but he'd been through—the Depression which took a big toll on him, a very big toll on him. My father was a distinct patriot—pro-American above all. He loudly and vehemently disagreed with the politics then taking shape under Lenin, Trotsky, and Stalin—and wouldn't hesitate to so state—loudly, clearly, and passionately.

In his later years, he gave up being a milkman and he started a small lampshade-making shop, a shop for making lampshade frames. He ran a little shop, which employed 20 to 30 people. They were mostly all black, so it wasn't in a very pleasant area of Chicago; got rebuilt later. But he did a pretty good job of running that place. I think he worked on as many lampshade frames as his help did. But that was the business he was in before he passed away. Thus he was a great advocate of small business like that. He took it very seriously.

BROCK: What about things like, thinking again of your childhood household, things like art or music or science? Were those—

SELLO: Well, it was very much encouraged. My mother, as far back as I can remember used to take me to Saturday morning Pop Concerts at the Chicago Symphony. The symphony was led by Frederick Stock at that time. We'd go into Orchestra Hall, and I think it cost twenty-five cents a seat or something, and we'd be so high up that you couldn't even see the stage. You could hear the music by relay, by reflection. [laughter] You couldn't see the stage. And she dragged me around to all these places. I wasn't always in tune with going to every one of them. She was a great follower of the arts and ballet. The ballet in Chicago was very strong at that time. They had a ballet at the Chicago Art Institute that was also just for kids, and she went there because she liked it herself.

She had to do a lot of work. She had to help support us because Dad's job was not sufficient—so I used to go with her to her nursing jobs.

BROCK: Oh, to her private appointments.

SELLO: Yes. She used to take me along to a lot of them when it didn't interfere with school. I didn't always agree. I didn't like to be around sick people.

BROCK: Sure. It can be frightening.

SELLO: Yes. Very frightening. She was very sensitive. She helped very much. I used to complain that, "Why are you doing all of this? You're a nurse, but you don't have to wash the floors."

“Oh no, that’s part of nursing. If you come here to take care of a person, you wash the floors, if they need to. Get the bucket.” [laughter] I raised the issue so I got the—

I hope these are to point.

BROCK: Very much so.

SELLO: You can always edit them out.

BROCK: Well, no, the point is to get—we’re interested in your experience.

SELLO: Right.

BROCK: And this is part of what—

SELLO: Part of the background.

BROCK: Part of your story.

SELLO: Education was ultimate, was on top of the pile.

BROCK: That was the big emphasis.

SELLO: If I came home and said, “You don’t talk well enough,” the next thing I knew, I’d see some books in the house that they both picked up and they’d ask me, “Should I read this one or should I read that one?” You know, grammar. Well, you don’t ask a youngster, you know, “I’ll go to grammar with you.” I gave them my spelling book. Very, very much oriented to that. And in that regard they were maybe a little bit snobbish relative to some of their everyday Jewish friends and a lot of Russian friends, because those other folks were also working equally hard, and they didn’t quite see that you should take the time to go, on a Saturday morning when everybody’s working, you should go to listen to an orchestra concert and drag your kid along. “You don’t do that.”

“But it’s only a quarter.”

“No, can’t do that. You could be doing something else.”

Especially her aunt, who had vouched for us. It got to the point where my dad would never talk to the aunt anymore because she was so, as my dad says, “ignorant.” She didn’t relish that my mother should work every day of the week, but on Saturday go to a concert. “Why isn’t she working on Saturday?” [laughter]

BROCK: Okay. I see.

SELLO: Of course, you know, as a kid you hear this and it just—it sticks. You don’t know who’s right or who’s wrong, but belittling my mother, I didn’t like that.

BROCK: So your grammar school experience, I know you went to a number of them, but were your parents after you to perform well?

SELLO: Yes. Yes, always. Always. A new school was no excuse. They had to move because of economic reasons. As the rents in a certain sector of Chicago went up, so they had to move someplace else. The dairy moved its bottling headquarters and Dad had to move closer so that he wouldn’t use as much gas to get from one side of Chicago to the other. So we moved a lot, and so I transferred many time from one school to another.

BROCK: What sort of effect do you think that had on you as sort of a young person, moving around so much?

SELLO: It wasn’t very happy. I spent more time at the library because it took me a while to get used to the new school and it was interfering with my baseball. You get to a new school, you’ve been a member of the baseball team for a while, what are you going to do? You have to start all over again. So it wasn’t too comfortable, and I was always conscious of the fact that yet another school.

BROCK: In grade school, or before high school, let’s say, what were your main hobbies? You’re doing a lot of reading. Were sports a big part of—

SELLO: Yes, baseball especially. My dad was an avid Chicago Cubs fan. I mean, he was out there cheering for the Chicago Cubs when he barely understood English. He was in the bleachers sitting in the sun for four hours watching a Chicago Cubs doubleheader. He dragged me along. So baseball played a big part. I was small in those years, so I played softball. In Chicago they played softball, you know, this big—

BROCK: Sure.

SELLO: You've seen that, yes.

BROCK: Sure.

SELLO: Underhanded pitching. We had a team of ten of us that got sports shirts and we had baseball tournaments. Golf was not a part of the action, not to people in our financial circumstances. But the interesting part about it was, I worked as a caddy for a while at a nearby park, and I've often wondered, I said, "Why didn't I continue with that?" I could have been good. [laughter]

BROCK: Was that in high school when you were caddying?

SELLO: That was in high school, right. In high school, right after grammar school. We had a system in Chicago where they had basketball divided into two leagues; lights and heavies. Lights were five-seven and under in height, nothing to do with the weight, and heavies were about five-seven. Well, that helped me a lot, because if I played with the heavies, they'd kill me. No way. I was just about five-six, five-seven, so I could play with the lights.

BROCK: Oh, dominate the lights.

SELLO: Yes, right. At least they were just the same size I was. I wasn't as good as a lot of the guys were, but we played a lot of lights basketball. It interfered with school every once in a while. Didn't like that at all. That was at the high school level, though, because grammar school didn't have that.

BROCK: In thinking about the grammar school experience, was there anything in that that you think prefigures your interest in science or technology?

SELLO: Yes. Yes.

BROCK: Was it math?

SELLO: There was. There was. I read a book in grammar school that I never forgot; it was *Microbe Hunters* by Paul De Kruif, and to this day I'll pick it up every once in a while. That one affected me enormously. I was going to be a bacteriologist. I mean, the stories in there, especially Louis Pasteur and some of the guys that worked on—also that worked on tuberculosis, and all of them, they were my idols. Now, several of them were using statistical math in their experiments. There was another book that went along with it called *One Hundred Million Guinea Pigs*. I don't know if you've ever heard of that one.

BROCK: No.

SELLO: I forget who wrote that [Arthure Kallet]. I'm going to have to look that up. *One Hundred Million Guinea Pigs*. It was a treatise against the cosmetic industry in which the scientists explained that what you got when you bought a jar of cold cream, you got about three cents' worth of chemicals for which you pay three dollars, and the chemicals were so cheesy, you know. So I read that in connection, together with *Microbe Hunters*. I thought, "Boy, I could make a product like that." I ruined a few pots of my mother's trying to make some cold creams which I thought I might be able to sell, but they never worked; they were always liquid. I couldn't get any gel for some reason. She didn't like that one bit.

Now, there was some math involved in that, and it had to do with the number of experiments that you could run to make sure that you had something that was good, basic. I recognize it now as basic elements of statistical math without being the statistics. So math was very important, so I had to read up on that. And of course, that led to other places, which chased me away, was like, the first thing you got into was astronomy, when you got into math, and distances and concentric circles, you know, the orbits, and that was heavy. That was heavy. So I backed off. I backed off from that and I went back to *Microbe Hunters*.

I wrote an experiment, of which I was very proud. In about seventh or eighth grade in grammar school I wrote this. We had projects that we did, so I wrote a project on the "Corn Borer Weevil," the insect that bores into corn. Of course, in the Central United States that's very important.

BROCK: Sure.

SELLO: I set up an exhibit on how to get rid of corn borers out of corn that I'd never seen. [laughter] And I won a prize for that exhibit because it was a series of ears of corn, and I burrowed tunnels into the ear and I showed this little corn borer going through, and got to get rid of those. So I was like *Microbe Hunters*. So the investigative nature, I wanted to do more of that. Of course, there wasn't very much opportunity in grammar school to do that, but it popped up in high school.

In high school I jumped on chemistry. That's where I really got the first exposure in senior high school at the third year, the third out of the fourth year. Mr. Mooney was my mentor.

BROCK: He was?

SELLO: Oh yes. He was the chemistry teacher. And his favorite comment to me is, "What are you doing now, lamebrain?" Very encouraging. [laughter] Very encouraging. I just took to the chemistry. I brought it home, my mother threw me out, threw out the chemicals, she was afraid of it, that it would get into food. I got a chemical set a few times. She bought me one as a gift, as a matter of fact, if I'd use it in the basement next door, but not in the house. So chemistry popped up at that time. I did a lot of experimentation. I built some equipment, very simple stuff.

BROCK: Were you at the public high school?

SELLO: Public high school, yes. Everything was public.

BROCK: Were you at one high school for the four years of high school?

SELLO: Well, the high school was divided into two parts. It had a junior high school portion to it, which was right next to the main four-year building. So you did the first two years of the junior high school part and then you graduated in the senior high school part. I remember doing my project as a senior high school "chemical engineering" project; an electric furnace in order to melt high temperature material by using carbon rods as resistance heaters.

BROCK: Interesting.

SELLO: Oh yes. It was a little bit more than I should have undertaken, but Mr. Mooney was watching like a hawk. He was afraid I'd burn down the building. So that worked out.

BROCK: What was it that really captivated you about chemistry? Was it the connection to this sort of *Microbe Hunters* stuff?

SELLO: It was related. It was related, because the microbe hunters themselves were chemists. Most of them were chemists; even Pasteur himself. I mean, he was very proud of the fact that he was a chemist, and quite a few of them were. They had nice laboratories, and they always showed the pictures of these retorts, you know, the glass equipment with stuff bubbling over, and when I saw the chemistry lab and we had the same things there, that really helped. I think it was connected to that kind of thing, investigative chemistry, we would call it today.

BROCK: What about your other subjects? Were you doing well in your other subjects?

SELLO: Yes, I enjoyed algebra, trigonometry and plane geometry. The only thing I never did very well in was one called economics. Economics was a miserable—I could never understand it. Never could understand the nature of what a massive shift in population would do. What did it mean? I mean, that so many people would behave in a certain way, and it followed a law of supply and demand. And I just couldn't understand that. I just couldn't see—I wasn't really interested in it, so I got mostly As and Bs, but the one C I got was in economics.

In that school we had some very good public service courses, like civics. I thought that was very interesting. Civics and the nature of government. I liked that, because that was built on the French system, and I read up on that all the time. I used to go back and find what the French were doing because Dumas knew a lot about that at that time. Of course, they didn't do what he wanted. But the reading of the birth of the French system and the analogy from that to our own, that worked out very nicely, so I did very well in that.

BROCK: So you were continuing to do chemical experiments and stuff at home?

SELLO: Yes, that's right. And mostly reading, because without a laboratory, there wasn't much you could do. But I jumped on that much more when I got into junior college. I went to Wright City Junior College and then to the University of Illinois.

BROCK: Yes.

SELLO: In junior college I became a private assistant to the organic chemistry professor, and I thought it was delightful that he would choose me, when all he wanted was a slave to wash his jars and bottles at his home. Of course, it was part of a chemical experiment. Dr. Nickolaus Cheronis. The "Mad Greek," we used to call him. [laughter]

When he invited me to come on Saturday to help him conduct an experiment that he was bootlegging at home, I didn't know he was bootlegging it. He was making cosmetics; trying to. I always was very proud of that.

And my mother wanted to know, "Where you going on Saturday? You should be out. The boys are looking for you, your team. You're missing. You're the left fielder and they didn't have one. You're missing. Where were you?"

"I was out with Dr. Cheronis."

She said, "You didn't tell me you were going."

I said, "Well, I didn't know I was going until he called up on Friday and said, 'Come on out.'"

So I got into some of the experimental aspects of—it was organic chemistry; you wouldn't classify as physical chemistry. It was a separate physics course that you took to go along with that.

BROCK: To go back the high school years, before we go into the junior college story, so were your high school years 1934 to 1938, around there?

SELLO: Yes, yes, exactly. Exactly. Still leftovers of depression, yes.

BROCK: Right. And you're getting more and more interested in chemistry and science?

SELLO: Well, the chemistry continued, yes. Of course, I had to take physics and math, that was required, but I majored in physical sciences. Chicago had a system invented by the University of Chicago called the survey system, in which whatever your major was going to be there were certain survey courses that you had to take, whether they were in your major or not. For example, art and architecture were required. I balked all the way, but I had to take that. Then there were the social sciences and then English. Finally, you could specialize in your physical sciences.

The upshot of that was it really worked well for me because I compiled almost twice as many hours, school hours, in the adjoining fields that I never would have gotten into otherwise at all. Architecture and art? Never would have. I had to take an examination where they flashed pictures of the great artists of the Renaissance. Who is that? God, I'm interested in science. What is this? But it was an excellent system. It was one I was very proud of. The system existed, I noticed, out here in California for a short time, i.e. the junior college system, but it slipped away here.

But I was very well-rounded when I got to senior chemistry at the University of Illinois. Because of that, there were a number of courses that I didn't have to take because I had enough hours compiled in Chicago in these related sciences, so I could go right into the chemistry.

BROCK: What was the name of that junior college?

SELLO: Wright Junior College. Beautiful system.

BROCK: Was that in Chicago?

SELLO: In Chicago. And there were three of those schools and they were public; no tuition.

BROCK: Wow.

SELLO: Yes. They, too, disappeared in the course of the years. And California had a junior college system just like that here in San Francisco.

BROCK: That's free.

SELLO: Yes. Yes. They had it here in San Francisco. All up and down the state they had these junior colleges where you could go, because you couldn't get into [University of California] Berkeley, it was too crowded, or into UCLA, and it was a little bit expensive, not much, so the junior college system would take all these halfway mature guys like me and, you know, bring them up into their third year, and it worked out nicely.

BROCK: Did you work during high school?

SELLO: Yes.

BROCK: Caddying, you mentioned.

SELLO: Caddying, restaurant waiter and food checker in the Triangle Restaurants. It was my job to count all the portions of the food that we used on Saturday and Sunday in the restaurants. You sat there with your little keyboard and you counted the number of lamb chops and the number of slices of bread and the number of cups of coffee, and turned in a report. Then I was promoted to being a headwaiter part time, to seat the people and assist the servers.

BROCK: Was that in the evenings or on the weekends?

SELLO: Both weekends and evenings, yes. And I worked as a shoe salesman in Sears, Roebuck.

BROCK: So all this was during high school?

SELLO: Yes, high school.

BROCK: Were your peers doing similar things in high school, your friends?

SELLO: The two or three that I liked very much were doing similar things. A couple of them were working for their families. One of them had a machine shop and another had an automobile repair station, so they used to work there as grease monkeys, you know, working in the shop. Saturdays were pretty busy. That was for pocket money that I didn't have to ask my dad for, not that he wouldn't have given it to me, but I didn't want to do that.

See, I helped him a lot on his milk wagon; I didn't mention that. I used to go out with him. He never woke me for that; my mother woke me. Three o'clock in the morning, she'd pull my blankets off and say, "Your dad needs help."

"Well, if he needs help, he'll ask me."

"He needs help. Go."

So, three o'clock in the morning, an icy Chicago morning, that's not a time to be out on a milk truck, but I used to do that a lot. I knew he could use the help, but I just resisted.

They took a vacation trip. I didn't go along, because that was my summer for working. Stuff like that. You had to do that.

BROCK: So you continued the sort of work with chemistry at home or the chemistry reading and things like that.

SELLO: Yes.

BROCK: Did you get into any other sorts of technical things, tinkering sort of things? Were you in the machine shop or the auto shop?

SELLO: Oh yes. Yes, wood shop. Woodworking shop was a big one, and the sheet metal shop was another one, because you could make containers for the stuff that you needed to use in chemistry, for example. I made a replica of a much better electric arc furnace that I did in high school and then I made a much better one in college. Yes, you could do some of that.

Beyond Wright Junior College, the University of Illinois was famous for its laboratory courses. They had tremendous programs—hours and hours, five hours every day, every afternoon, you spent in the laboratory. Everybody else was out playing bridge or watching football or something like this; you had to be in the laboratory. Very much on-hands. That was a mark of the leadership at the university of the scientists who were there.

BROCK: And as sort of high school came to a close, were you—

SELLO: Oh, I didn't mention a whole stint as a cellist in the high school orchestra.

BROCK: Oh, really?

SELLO: Oh yes. Yes, I got a letter for that. Yes. Imagine what people could say. "Sello, get out your cello." [laughter] I said that before you did.

BROCK: Was that something that your mother also encouraged?

SELLO: Oh yes.

BROCK: The lessons?

SELLO: Oh yes. I showed up at home one time dragging this big cello, and my dad was just coming in, he said, "What are doing with that? Did you find it?"

"No, I'm playing cello now."

"Where? What's going on?" He just was thunderstruck. Thunderstruck. "Why'd you pick that? Couldn't you pick a violin or a viola? Why'd you have to do that?" But he was secretly very proud.

But I wasn't too good. I could play in the orchestra. We did a citywide contest which we won. In that big orchestra, but I was only one of fifty musicians. But it helped my musical knowledge.

BROCK: Did you keep up with it at all?

SELLO: Unfortunately, no. Going away after junior college, going away from home, I stopped that. I could have continued, I suppose, if I wanted to go to the orchestra at the university, but who had the time? I had to maintain a meal job, as well, in the university, full-time.

BROCK: You were working full-time?

SELLO: Full-time at the university, yes. Yes.

BROCK: What were you doing?

SELLO: I was a waiter in a fraternity house. I got promoted to headwaiter of the fraternity house. I was also in charge of mashed potatoes. [laughter] I got a good look at fraternity life.

BROCK: Yes, I can imagine.

SELLO: But it was very helpful, extremely helpful. It paid for my meals.

BROCK: At the end of high school, did you realize that you wanted to be a chemist?

SELLO: Yes, a chemist.

BROCK: Very strongly?

SELLO: Yes. Chemistry was the science. It could have been physics, but I wasn't interested in straight physics. That didn't have the panache that chemistry had. You never knew what would happen when you poured two things together, but in physics you could see it. [laughter] You could hook up a lever arm to a battery, you could see what it was going to do. The laboratory part of the chemistry appealed to me.

BROCK: And so you were at the junior college for two years?

SELLO: Two years.

BROCK: And so—

SELLO: Moved from there to University of Illinois.

BROCK: Were you living at home when you went to junior college?

SELLO: Yes, it's a city college. It's a streetcar school or it's a bus school. That was one of the reasons I had to go; I couldn't afford to go to Illinois or to other places at that time.

BROCK: And then was it your intention, you know, did you have your sights on University of Illinois?

SELLO: That's an interesting question, I've often thought about that. Northwestern University was right nearby, Wisconsin was not that far away, but I guess it was the proximity of the University of Illinois. But there was another factor. There was a very famous chemist who, again a chemist, who was making a name for himself there. It was Carl [Shipp] Marvel.

BROCK: Sure.

SELLO: Carl Shipp Marvel. Big portly guy. Oh, fascinating. When he spoke, it was just hypnotic. As soon as I listened to him in a lecture, I thought, "Hey, this is where I want to go." He talked to the student level. He was in the laboratory looking at his lecture students at the same time.

BROCK: Wow.

SELLO: Yes.

BROCK: And he taught organic?

SELLO: He taught organic. I didn't turn out to be as interested in what he was doing. He was a synthetic organic chemist, DuPont's special consultant. He helped develop nylon. In fact, some give him the credit for most of the thinking. But he was just spellbinding. He didn't look like a chemist. He was round and pudgy and big, but when he fastened his eyes on and telling you what you were supposed to be doing, you just listened. He was an expert in what we used to call 'smellorometric analysis.' I don't know if you know—organic chemistry. He taught a course in the scents of chemistry.

BROCK: Interesting.

SELLO: Yes. Oh yes. Where we identified unknown compounds by their odor. It works. You couldn't get down to one member of the same class, but one class from another class, easy. Oh, it was—and he used to teach that.

BROCK: Fascinating.

SELLO: He used to say, "Now, don't suck on it. Just let it come past your nose and tell me if you've smelled it before." And then he'd put up five test tubes, "unknowns," and you went through and did that, and it was an examination.

BROCK: Oh, it was.

SELLO: Yes. It wasn't just that as a test; that was one of the factors for the identification of chemical compounds. And that's what attracted me to—that went back to Paul De Kruif and

Microbe Hunters, to identify the 605th compound, you know, 606 and all that sort of stuff. Not De Kruif himself, but Pasteur and the others. That sense of detective work to try to figure out what had happened, that stuck with me all my life. It happened to be in organic chemistry.

There was another gentleman who was equally as influential in a different way, W.H. Rodebush, R-o-d-e-b-u-s-h. He's a physical chemist. In fact, he was a schoolmate of the Stanford—the guy who just passed away, the Stanford electronic guy. He got his Ph.D. at Stanford, Rodebush did. Then he went to the University of Illinois. It was because of him, that later I got on to University of Missouri. One of his graduate students from Stanford, became the department head at the University of Missouri. So, as you know, you go by steps, through a series of connections.

So Rodebush and Marvel, they were outstanding. At that time, there was no laboratory course in physical chemistry per se. But Marvel was the key guy. He was later replaced. He became chairman of the department, and a fellow by the name of Charles Price, C.C. Price, he'll ring a bell, too. He was one-handed. Charles Price was missing a hand, but you should see him in a laboratory.

BROCK: And he was a polymer chemist, right?

SELLO: Exactly. He was a polymer chemist. Brilliant guy. So he and Marvel, one following the other, were a very powerful force for organic chemistry and I didn't think would end up anywhere else but organic chemistry. They had a couple of other profs that were excellent. J.C. Bailar, B-a-i-l-a-r, he was an inorganic chemist. It was later his advice that he suggested, "Look, if you like Marvel and you like Rodebush, go the University of Missouri for graduate work." I didn't know I was going to go there. I didn't intend to go beyond B.S. You just roll along.

BROCK: Christophe, do you want to ask some questions about the university?

SELLO: Yes, I have a feeling there's a ton of this stuff. I don't know where it's going to go. It won't show in the talk. [laughter] But if I get a copy of that, I'd love it.

BROCK: Yes.

LÉCUYER: Maybe we could talk a little bit more about the chemistry department at Illinois.

SELLO: Yes.

LÉCUYER: I was wondering about the size of the department and how much research was being done there.

SELLO: It was a big department. The lecture classes were big, very big. They were good. Marvel's class rivaled those at Berkeley, where they had several hundred, a couple of hundred in the class, and this is at the third-year level. So they were big, but the excellence lay in their laboratory work. They took a real close look in all of the major disciplines at the laboratory work. The reason that worked so well was because they broke it up into little sections to work in the laboratory. I worked along with about ten others out of this whole third-year organic chemistry class. Also they brought in a lot of visiting professors in organic synthesis, like from DuPont. That's where I heard the original story of the discovery of nylon. DuPont was very strong at that point and part of the school.

The physical chemistry had an interesting appeal because it was a kind of analytical chemistry. You had to select the right equations, the right description of what you were looking at in order to solve the problem, and so open-book examinations were common. You could just view the book, you have your problem, you have a certain material and you want to determine its specific heat or its heat capacity, and you're told that these are the basic measurements. How are you going to do that? It was something that was duplicatable in the laboratory.

And they were strong on lab. I think the University of Illinois really, as I've seen Cal and even Stanford—Stanford is very strong is graduate-level laboratory. Cal was too big to be that strong.

Did I answer your question?

LÉCUYER: Absolutely, yes. And I was wondering whether you were involved in research, whether Illinois or—

SELLO: We did a senior thesis at Illinois. My senior thesis was under Professor C. C. Price had to do with addition polymerization of organic molecules, which were catalyzed by peroxides. It isn't that you could necessarily make the final product. The question was: why did different catalysts result in different end products? And the quiz would come on at what point did you know you were going to succeed. So it was *Microbe Hunters* all over again.

Does that answer your question?

LÉCUYER: Absolutely, yes. Absolutely.

SELLO: It was organic. It wasn't what I later—

LÉCUYER: Yes, I know. I was thinking of that.

SELLO: Yes.

LÉCUYER: And also I was thinking about your friends there. Did you have a large group of friends there?

SELLO: Yes. That's an interesting question. I was part of a larger group, but we all lived in different places. We did not band together. I knew the guys when I came to the laboratory or in the class, but once we dispersed for the day, I didn't see them. Maybe one, occasionally. I didn't see them. In graduate school it became a little closer. I often wondered, "What are they doing?" I mean, after hours. I knew one of them also had another meal job. It was close enough to Chicago where several of us would disappear once a while every weekend into Chicago. You could go up there, you could get a meal job. You could sell sandwiches on the train and go up in two hours and be in Chicago; you got a free ticket. It's that kind of things. You jumped on a train, you went up.

BROCK: Where were you living at the University of Illinois?

SELLO: On campus in Urbana. In fact, I lived a year each, one year in Urbana and one year in Champaign, the twin cities. It wasn't in the dormitory. The dormitories were for the freshmen and the sophomores. When we were in the third and fourth years already, we had to go out and find our own quarters, so we lived in a place, it was a mixture; there were athletic guys and guys playing ball, and there were a lot of geological and agricultural guys. Illinois is also equally famous, almost as in chemistry, as an agricultural school.

BROCK: I didn't realize that.

SELLO: Yes, it's very, very big. It's a state agricultural institution and it had many experimental farms. They grew more varieties of corn. And of course, I went to look at the corn borers, you know. [laughter] Everything tied together. "I know about those. Let me tell you." [laughter]

Big school, but very big. You were just a microcosm in that school. After junior college it was a little difficult. It was a little difficult.

LÉCUYER: Also moving out away from home and that sort of thing?

SELLO: Yes. But the meal job prevented me from going home to visit. I shipped my laundry home, but I didn't go home.

Let's see. What else came up that was—I took an interest in German as a language at the third year. I knew I would eventually maybe come closer to it, but I didn't know I was going to have to qualify. But I took an interest in it and I did fairly well.

LÉCUYER: So was it because it was required by anything?

SELLO: No, it wasn't. No, it was—well, wait a minute, I take that back. I think at least one year of a language was required. It could be French, German. Those are the two. There was no Spanish, nothing else, like there is around here.

I think there was another course, another non-technical course, non-science course that you could substitute for the language, but I don't know why I took a fancy to German. It lasted me for all my life, to this day. Oh, it was fantastic. I remember Fräulein Zucker was her name, and Zucker means sugar. I remember I asked her one day when I knew her a little bit better, I asked her, "Schmelzen night Sie, Fräulein Zucker?" "Aren't you melting?"

She looked at me, she didn't know what to say, and I was practicing; I worked for a week to get that phrase. She said, "Don't tease me with your kindergarten German. Learn the lesson."

But that was good and it helped me a lot later. It was tremendous. And I qualified for languages.

LÉCUYER: So now we can move on to your career aspirations towards the end of your senior year or what you are thinking of doing.

SELLO: Well, the problem then was, Christophe, a job. I mean, you know, I just couldn't continue to go to school. Also, and I hate to say this, it gives away all the gray hair, but I was picked up by—I was registered for the draft. This is now 1942 and our draft in our neighborhood was very active, and so I registered in the draft. And it's one thing I remember so fondly about these people.

BROCK: Your neighborhood in Chicago?

SELLO: In Chicago, yes. It just, you know, to this day, strikes me. I remember going in there and saying, you know, "I'm reporting for the draft," and I said, "I'd like to join the navy instead of the army, if I have the choice."

And one of the elderly gentlemen said, "Why do you want to leave school?" And it was out of left field. I never expected that. "Why do you want to—?"

"Well, I'm finishing."

He said, "But don't you want to go to graduate school?"

I said, "Well, there's a draft."

He said, "Well, that's our problem; it's not your problem. The question is, do you want to go to school?"

And I thought, you know, that there was that whole world of—these are immigrant people. I mean, ordinary everyday hard-working, not all immigrant people, but descended in that generation, Poles, Russians, Jews, non-Jews.

And I said, "Well, could I go to school?"

"Well, you have to tell us where you would like to go to school and you have to apply."

"Will the draft let me go to school?"

"Well, apply and submit your request."

And I did. And I went home and I told my dad, and he didn't believe it. He checked with the Draft Board later to find out, you know, have I missed something. By that time he was saying, "You've got to be in the service, by golly. Look at all these guys that are in the service and you're going to school? Not my son." [laughter] Oh, no, he was very militaristic at that stage. But the Draft Board helped me decide.

This happened at the end of the summer when I graduated, because I took a summer job at the Melrose Park Steel Works as an analytical chemist in the melt room where they poured the steel. That was a very fascinating experience to work there with all these heavyweight immigrant types, wearing big underwear to keep from sweating and all that sort of stuff, getting overexposed to the heat of the melting steel.

After the Draft Board interview, then I went back to school and went to Dr. Bailar; who was in charge of graduate placement. And I said, "I'd like to go to graduate school."

And so he said, "Are you sure you want to do this? Your grades are good enough, but aren't you going to be drafted?"

I said, "Well, they said bring the application that I've applied for something, and they'll judge the case at that time."

So he helped me make out the application and it worked with the Draft Board. Not only that, they deferred me for two years.

BROCK: Oh, really.

SELLO: The following two years to a master's degree, the same Draft Board, the same guy. He said, "What are you doing?"

"I'm working for a master's degree."

"Well, when are you going to get it?"

"Next year."

"Well, good luck."

There was another reason for that. It wasn't all so generous as it was. Since this was a workmen's neighborhood where they were all people who barely got through high school or had to go and work, they could fill their quotas very easily. So that played a role. And they didn't have that many people asking to go to graduate school. Maybe I could have been the only one in the period that I applied. So I have to admit that it wasn't all due to my—color of my hair or the shape of my nose or anything like that. They were very realistic about it. They said, "Well, the draft can use you when you have a master's degree, too, and we know where you are."

BROCK: Right. Wow.

SELLO: It worked.

LÉCUYER: So this led you to the University of Missouri?

SELLO: That led me to the University of Missouri, because there was a gentleman at the University of Missouri—it goes in steps, I notice. One knows the other. Rodebush had been educated by a Stanford professor who was a close colleague of Henry Eyring. I don't know if you know the name Eyring.

BROCK: Sure.

SELLO: Henry Eyring was at—and Henry Eyring recommended that they send me to the University of Missouri because if I was one of Rodebush's students, one of Rodebush's colleagues, who had also worked with Eyring was the chairman of the department at the University of Missouri, Allen Edwin Stearn. I can't tell you how lucky that was, now that I look back. It changed my whole approach; it shifted, and not so slowly, from organic chemistry to physical chemistry.

When I saw Rodebush in later years, he was so proud of that. He said, "You mean I got one away from Marvel?" [laughter] I didn't even know I was noticed. So that worked.

LÉCUYER: So how did you become a physical chemist then?

SELLO: Well, so then I became a—you know, you become a graduate teaching assistant, so you teach. We had the same system as the University of Illinois and as Berkeley has. When you qualify for an advanced degree, you take a general examination across the board and then later you take a thesis examination. So I had to take a general examination, and one of the courses I had to sign up for was physical chemistry, physical and inorganic chemistry. Henry Bent was the professor, and the organic chemistry course was directed by a lady by the name of Dorothy Nightingale . She looked like Florence Nightingale. You know, she really did, like the famous nurse. She had her hair down like this [gestures] and always kind of a hat around her face. But there wasn't an equation in organic chemistry that she didn't know by name, backwards. You know how the organic chemists are. They give you a list of name reactions—yes, can you do this, write the equation of that name and this name? The Mallinckrodt inversion, you know, or something like that. So I had to take the organic chemistry.

Physical chemistry was taught by this gentleman, Allen Edwin Stearn, and he just charmed me out of my shoes. He was a kineticist. That was the influence, the theory of Absolute Reaction Rates. I'm sure you'll recognize that. And when I saw the mathematics of that, I said, "I can't do this. You know, I'm an organic chemist. How in the hell am I going to get on top of this?"

He said, "You haven't even started." He said, "Just take a whack at it."

I was a student teaching assistant in beginning inorganic chemistry, during my first year at Missouri.

Are we okay on time?

BROCK: Sure.

SELLO: You'll call the shots. Okay for me. How about some more coffee or tea?

BROCK: Sure. Let me just pause this.

[PAUSE IN RECORDING]

SELLO: I changed to physical chemistry under Stearn in graduate school as far as my laboratory interests were concerned—it shows you the effect of personalities and thinkers.

Again, Stearn was a—he gave open-book examinations. "Do you want to copy something? Here's the book." That used to make me a little mad, because I would proctor the examinations and these jerky chemical engineers; it wasn't good enough that it was an open-book examination, they had to copy from each other's papers. [laughter] I said, "You've got that equation right in front of you. All you have to do is substitute the numbers."

"But it isn't the same as his." [laughter]

Well, where are we? How shall we proceed?

BROCK: I'm making sure that it's all recording correctly. So, pardon me.

LÉCUYER: So again, talking about the department, how different was it from the one at University of Illinois?

SELLO: The University of Missouri is smaller. It's a state school. But it's considerably smaller than the University of Illinois or even the University of Chicago. They had a very strong organic chemistry department. The physical chemistry department was really two senior professors, Stearn and Lloyd B. Thomas, both good, both well known in a lot of research. But I expected more that I'd learn into the organic chemistry. I did not expect to do anything in physical chemistry.

Rodebush was a textbook man. There was a physical chemistry lab at Illinois, but it wasn't, you know, it wasn't a graduate lab—so I took the basic courses in organic chemistry, graduate courses and seminars, and of course you had to take a seminar.

Then I got caught quite by chance in a course in statistical mechanics taught by a man by the name of George Vineyard and he was brilliant, a young fellow. I learned not too long ago that he died at a young age. Vineyard taught statistical thermodynamics, and that just fascinated me, to be able to build up what Lewis and Randall had written in their text, but to build it up from Fermi-Dirac statistics and from basic considerations of the molecules, and then come to the Free energies and the Heat of activation and so forth. That took on a whole new appeal. I didn't see anything to—well, I didn't have a major professor at that time. You didn't do that until you'd declared for a research thesis, so out of a period of the first year that was not available to me anyway. I had to conduct the quiz sessions in beginning chemistry, the idiots. [laughter] I didn't have time to—it wasn't allowed to—you took just the courses that you needed to qualify for a master's degree. I couldn't see beyond the master's. That was barely in my sights.

BROCK: So the master's was from 1942 to 1943?

SELLO: Nineteen forty-two to 1944. Two years. Two full years. Then 1944 to 1946 the service comes in again. This time I have to go to the U.S. Navy, and then 1946 I'm back out for a Ph.D.

BROCK: I just was wondering, were you funding graduate school with this sort of teaching work?

SELLO: Yes. Yes, teaching assistant.

BROCK: And at the University of Illinois had it been the job you had at the fraternity house plus scholarships or summer work? Were you paying tuition?

SELLO: There was one—mostly tuition, except—and eating expenses were covered by the job. Then there was a small—we didn't have very many scholarships at the low level, not unless the person coming to the school brought with him the scholarship, that he'd been sent there on a scholarship back at the school. Wright Junior College, City College, was devoid of scholarships. No particular reason; I think it was a question of the way they ran the place financially. So I didn't have that available to me.

But at Missouri, at the University of Missouri, I went there because I could also get the graduate teaching assistantship. I could have gone just as a student, but I needed the teaching assistant and that's where Illinois helped me, where Rodebush and Bailar helped out. I was a teaching assistant in beginning inorganic chemistry. Beginning Chemistry One, let me tell you, if you want to teach—and you've probably done it yourself—that's where to teach. [laughter] You go through a stack of examination papers, and one after another the same mistake and you wonder, "How stupid can this class be? They're all making the same mistake." You forget that these are each individual students, and it's not really a bunch of papers. [laughter]

BROCK: I just had one other question that I wanted to ask, which was, we talked about you having to go to the Draft Board in 1941.

SELLO: Yes, I did each year.

BROCK: What was the feeling, your thoughts about the growing war in Europe and the thoughts of your family and also of your peers at school?

SELLO: Well, of course, the Pearl Harbor disaster occurred while I was at the University of Illinois, one morning while I was at my meal job, and that just—it was a disaster. You know, it just hit between the eyes. That very weekend I went home to tell my folks, "I'm not going to wait for the draft. I'm going to enlist. We can't live with this."

And my dad said, "All right. If that's what you want to do, you know, you can go, but have you talked to the Draft Board? Because they're in charge of what you have to do."

And I did, I went to talk to the Draft Board and same kind of composition and it seems like those guys just did that for the whole four years. They looked the same. I said, "Well, I'm not waiting for the draft. I'd like to enlist."

"What are you doing now at school? I see by the notes that you were going to finish University of Illinois."

I said, "Well, I only have another year to go, less than that, even." This was 7 December, so 1942, February.

And one of the gentlemen, a different one this time, said, "Well, you're quite welcome to enlist, you know. We need you. Pearl Harbor's a disastrous thing, but shouldn't you finish, just finish your degree?"

I said, "Well, I suppose it takes some time to be drafted."

"Well, then, come back when you finish."

LÉCUYER: So then you went then, right?

SELLO: Yes.

LÉCUYER: So that time you came back?

SELLO: I didn't go back because I was reviewed every six months and it wasn't the six-month period. I just said, "Well, I'll come back when I graduate." They said, "Well, when you're ready, you come back." Thus, I had to come back in the six-month period—after completing my master's degree.

[PAUSE IN RECORDING]

BROCK: Maybe we could just go through that again while I have the machine running.

SELLO: Yes, I wanted to mention that my father was never a "pusher"—he'd always ask, he said, "Oh, you got a B in that course. What does it take to get an A?"

"I got an A in the course."

"Very good. Very good. Going to get another one?" You know, with a little bit of a twinkle, but it was there. It was there. And he didn't take kindly to a C in economics, no matter what I explained. But pride showed all over him. When I graduated with honors, I think the fact that I finished university didn't mean too much to him, but the fact that it was with honors, he just puffed up like a—he was a very lean guy and he never demonstrated much, but that one he was very proud of and it made me feel pretty good, too.

BROCK: I'm sure.

SELLO: So then when the time came to say I was going to, you know, check with the Draft Board again, he said, "Well, if they think you should finish school, then you should finish school. I'd like to see you finish school." You know, for the first time out come the words, because he was such a patriot. He wore that "banner." He would say, "How can you stay out of school when all these other guys have to go, have to go to the draft?"

So I said, "No, that's the way they run it. They told me that."

So that played a large role in continuing. I don't know why that came out, but I hadn't said much about it, and it was very important. It was very important. Of course, my mother, she was a mother—I mean, if the draft never comes around, that's all right with her.

BROCK: Prior to that time, did you have family connections in either Russia or Europe?

SELLO: At that time my grandfather, my father's father, was still alive as far as I knew. Those were the only relatives, he and his wife. And it was also just about that time that we lost track of them because the Hitler army overran Ukraine and they sent all the Jews down to the vicinity of Odessa; just moved them. It was mostly the Romanian army that worked with the Germans. So we lost track of my grandfather. He had come to the United States in 1936, he stayed a while and he wanted to go back. He was too proud to stay in the U.S. because he couldn't see how hard my father was working, and after all, my father was an educated man—and it wasn't that way even for him back in Russia, so grandfather actually went back, one of the few who did. I could never understand it. Neither could my dad—I know they argued a lot about it. But we lost track of him at that point.

So we knew about where he was until that move in about 1940 or so, when they moved not just the Jews, but even the Russians, they just moved them right out of the Ukraine down into the vicinity of Odessa. And that's the only family that I could ever remember.

BROCK: I was just wondering from what you were saying earlier about your father's passion about the politics of what was happening back in Russia.

SELLO: By that time he was the biggest "I told you so" in the world, because [Joseph] Stalin's real life came out, and my parents followed closely through the writings of Koestler and through the various observers who had been on the scene and the kangaroo courts and the execution of the Russian editors; Zinoviev, Kamenev. Do you remember that? They followed that very carefully. To them, what else is new? Why would anybody want to stay there? My father never could understand why my grandfather went back. He never understood it. I asked him many times after that, "Couldn't you get him to stay?"

He said, "Nope, you couldn't get your grandfather to do anything that he didn't want to do."

And that was really the tenor of things, too. Of course, immigration then opened up a little bit because it was the advent of Hitlerism and that stuff. Not enough for a lot of people, but it did open up some.

LÉCUYER: And so if we go back to Missouri, you must have done some research for your master's degree, right?

SELLO: Yes. Yes.

LÉCUYER: So I was wondering if you could talk about this.

SELLO: Yes, it's interesting. I had to choose a master's subject after I completed the first year of all these obligations of teaching, teaching assistant. I went out to be a teaching assistant again. And I was so enamored of Dr. Stearn, the Irishman from Stanford and then from Illinois, Rodebush, and I talked to him about, "Well, I don't want to become a synthetic organic chemist," and that's what these other folks were doing. They were all synthesis people because of Florence—Dr. Nightingale and others, and I said, "I don't want to become a pot boiler."

And he said, "Well, what do you want to do?"

I said, "Well, is there something that I can do in physical chemistry?"

He said, "Well, you can do a lot in physical chemistry. What do you want to do?" He turned the question around completely.

Well, by that time I had just gone through this course in absolute reaction rates—Glasstone, Laidler and Eyring [*The Theory of Rate Processes*, 1941]. Christophe, you're one of the few people that even knows about that. How come? *C'est la vie*, huh? [laughter]

Anyway, I said, "Well, how about if I can think of some reaction, the organic reaction, and we can do some rate measurements on it." Because I had just taken the basic course in the kinetics.

And he said, "Fine. Find one."

So I did a little studying and I came up with an organic molecule. There had to be a lot to it. We were very concerned with unimolecular reactions, first-order reactions. That is, if you had one molecule that doesn't have to combine with anything, Molecule A changes to be Molecule B. It doesn't have to react with B to become C; it doesn't have to react with anything. What is the absolute reaction rate for such kind of a reaction? I mean, where does it go? And it was a question that I thought about quite a bit, and I said, "Well, do you even have a reaction?"

Stearn said, "Well, there are a lot of molecules which change their format, they're unimolecular reactions." See, first-order unimolecular reactions, what drives it? If you measured some of the rates or tried to measure the rates you could perhaps figure out what drives the thing.

And I said, “Well, can I ask you about it?” [laughter]

So the challenge was to find a unimolecular reaction, and I found one, which was simply a rearrangement. There’s a class of chemicals called pinacols, which are dihydroxy compounds, and under most solvent conditions they’ll rearrange themselves, they’ll turn around, and they’ll become another new molecule, but the dihydroxy will disappear. It becomes a ketone. Instead of being a dialcohol, it becomes a ketone. And the pinacol is the name of the class of compound. It occurred to me that, hey, you know, if you could measure the disappearance rate of the pinacol, of the dihydroxy compound, or you could measure the arrival of the ketone, or both, then you could get an idea of how come it happens.

I showed that to Stearn. He said, “Hey, can you make something like that?” He said, “I don’t know organic chemistry, but that looks like an awfully unwieldy molecule.”

I said, “No, that’s available. You can buy it.” [laughter]

That appealed to him. He said, “Well, all right, take it on.”

So that was the master’s thesis. It was the first step. I just did a few simple experiments of that. Really in the master’s degree the thesis is evolving the equipment to measure the rate of that reaction.

BROCK: What sort of equipment would you use?

SELLO: Very simple: a constant temperature bath plus pyrex containers. I had to be able to stop the reaction at certain periods of time, take a sample, freeze the sample, and now make the measurement of the result, of the end product. And there happens to be, in such parallel hydroxy compounds, a specific chemical reaction which reacts only with parallel hydroxy compounds. Like ethylene glycol, for example. It will only react with that structure. When you get the two hydroxy groups next to each other, the oxidation compound that you use to measure them will identify them. It won’t identify the product, but it will just identify the disappearance of the dihydroxy. So the thesis was, in the beginning, was just measure the rate of disappearance of the dihydroxy compounds.

Well, the whole matter, for which I got the master’s thesis, was really to figure out how to do the measurement and it turned out to be a very simple one. I just had to use the simplest equipments. There were no electrical equipments, but there were means for taking out a sample, injecting it immediately into a stop bath, stopping that without destroying the nature of the compound, and then analyzing that by organic chemical means, the old techniques for looking for compounds like that, and then doing that periodically at different times under different temperature conditions. So I had to rig a pretty effective constant temperature bath, but it could all be done in simple flasks, simple hardware, nothing fancy, plus a direct measurement of the temperature.

And that's what Stearn insisted on. He said, "Don't come to me with these experiments that are a third-order away from what the product is you're looking for, that you measure two other ones and by difference you get the third one." He said, "That's not going to work. It's too complex for this kind of a thing." He was never convinced that I could isolate the structural compounds, which arose in the reaction.

So I did spend the first whole master's thesis proving to him the number of experiments that I could actually measure, to find measured quantities of the starting materials and calibrate. And then I calibrated at the temperature range that I was going to make the measurement. So really the actual rate of the desired reaction was never measured at the master's level of experimentation. I did that two years later, a few years later when I came back. It came out that it was a wonderful series of experiments, a wonderful thesis, because here's a big starting molecule and you don't do anything except put it in a certain solvent, heat it, and it changes over into another molecule. But where does it get all the driving force to do that? You have to measure the heat of activation, and calculate the free energy of activation. And it turns out that—how shall I put it? It makes the rearrangement at the expense of the entropy, at the expense of the configuration of the molecule. The molecule, when tickled by the solvent or by any injected catalyst, prefers to be in its final configuration.

LÉCUYER: Interesting.

SELLO: It does not like to be where it is, even though that's a natural state for it. So there's a big delta-free energy change to a lower stable state, which is a free energy of activation, and according to the Eyring theory that is a measurable quantity if the reaction isn't too complicated. Then looking at it in a little bit more detail, it's still what kicks it? And it changes not on the basis of the bonds that are broken or the bonds that are made because they stay the same, but the starting molecule changes as a consequence of reaching a more stable configuration, which is an entropy factor, merely the configuration or delta-S change. The rate constant that you measure is at the expense of the entropy and not the heat change but the two together, of course, form the free energy. The entropy change, the configuration, and the solvent molecule are the key determinate factors. In fact, you can change that rate by using different solvents.

BROCK: Right. I see.

SELLO: And I was very impressed with the kinetics of the "activated complex." I went back to Lewis and Randall and all the thermodynamics that I ever had and I took more courses in it. I took statistical mechanics, because I wanted to get back to basics. And it all fit together so beautifully.

I told the statistical mechanics professor about the reaction, and he said, "Oh, you'll never be able to measure that. It's too complex." He said, "You've got one molecule surrounded by millions of others, so how do you know what is effective with these thing singularly?" And so I had to go on, one of the requirements was to—he was on my examining committee—so I had to do the presentation for him, too, because he wasn't convinced.

Stearn was very happy for two reasons. One, he didn't have any graduate students working in Absolute Reaction Rates, and also because as a physical chemist I turned in a higher prelim in organic chemistry than any of the organic chemistry students.

BROCK: Wow.

SELLO: And I was his student, so I beat the "organic-ers" at their own game. Of course, to me organic chemistry was a rote chemistry; you memorized all those reactions. The thesis work was always the preparation of organic chemical reactions. But he was pleased as punch; I was his only student in the period of the four years that I traversed. Stearn was a teaching professor. He didn't really conduct laboratory as such. He said, "You get the numbers and I'll check your numbers, and then you'll have to explain to me just why you think that the activated complex is some sort of a rearranged molecule by itself."

And I realize now that that's exactly what's happening in some of the changes occurring in semiconductor molecules themselves. They move, not at the expense of heat, necessarily, but at the expense of a spatial configuration, and that's an entropy change, and so they get an energy of activation and that's how they can classify certain formations that are in molecules. Later I realized that these formations fit to what the theory of Absolute Reaction Rates predicts. However, the theory of Absolute Reaction Rates to organic chemists was kind of a new thing. You could tell them why there was a large configuration change. "Well, here's a molecule and it's losing one of its pieces. While it's losing its pieces, it's now solvating; it's latching onto all of the solvent molecules that it can. It's charged, because it's broken apart." I'm doing the defense of my thesis. [laughter] And it's charged and these things are broken apart and now it can attach to the solvent molecules, so the rearrangement will be a dragging of a batch of solvent molecules to the end components. Once that's formed, then the solvent falls away, because it has nothing to hang onto. Does that make sense to you?

BROCK: It does.

SELLO: So having absorbed all these solvents causes a large decrease in entropy, a large change in spatial configuration. Then when the solvent lets loose, that change is enormous, it kicks all back, and that's what drives the rate-constant, because the bonds that are made are the same energy as the bonds that were broken. They're still carbon oxygen bonds; they don't change. So if you inject heat to change this, you'll get that heat back when it forms. So where else is the driving force coming from? It's coming entirely from the fact that the reaction complex is solvating and then it loses its solvent.

Now, to prove that—and this is where Stearn really nailed me. He said, "Well, this dyhydric alcohol, this 2OH alcohol, can you think of different solvents that would hydrate differently with that?"

I said, "Well, I don't know. Let me think about it."

So I made a search and I found three solvents, all of whom had a different electrical behavior. They all solvated with—I tested that in separate experiments, they all solvated. They all reacted with this molecule in different ways. So I ran the same series of experiments at various temperatures and in various solvents. So I crisscrossed the solvent effect, and the rate of reaction was strongly dependent on the nature of the solvent that was used and not on the nature of the molecule that was changing. That didn't change. There was no way of saying that it would.

So the first solvent I ran it into was ethylene glycol, which is a very highly hydrated one and then I ran it in dioxane, which is non-hydrated, and I ran it in a third particular kind of structured alcohol solvent, where the oxygen was there, but it was tied up. And everywhere where the reactive molecule couldn't solvate the rate of reaction was—and thus the free energy of reaction—was lower. I could only do three solvents, because it took dozens of experiments, ages to do that, to do it over again and at the same time to teach that physical chemistry class in the same laboratory.

I tell you, it was fascinating. And in semiconductor work, the moment we got into diffusions and the formations of particular structure where there was an energy of activation for diffusion, where is that free energy coming from? You guys, you electrical engineers are talking about energy of activation, I said, "What are you activating? It's already activated. You don't have a free energy of activation. What you have is really a heat of activation."

"No, no, no. If you plug the diffusion rates into the Boltzmann Equation or the Arrhenius equation, you get energy. E out is an activation energy and that's what it is."

I said, "But what's activated?" There was no physical model for it, but the activation is a penetration by the doping atom slipping between silicon atoms in order to fill the vacancies, and that's where it's coming from. You know, it isn't necessary for an electrical engineer to think of that. [laughter] But it bugged me when the engineer was talking about energy of activation—getting a little esoteric—they would assign to it a negative value. Well, that's not possible. If you're going to energize something, you put in the energy. How can the change be negative? "Well, that's just a matter of convention; we'll just change it to positive."

I said, “Nuh-uh.” [laughter]

“That’s nonsense. You want to call it an energy of activation, call it that and give it a proper positive sign and look elsewhere in your equation for why that can’t be positive.”

But you see, then at the master’s level, all I did there really was to define the molecule and then show that there was a change, that A went to B, and B was distinguishable from A by separate chemical means.

LÉCUYER: So you continued the research when you came back?

SELLO: When I came back two years later from the service I really started over again, because I already—I wrote it up as a master’s thesis and I got the M.A. for it. But I got so close to thinking about molecular changes in detail and not just the synthesis of a compound or a new inorganic material. When I went to work at Shell Development and on catalysis, and I began to think, “Hey, what is this catalyst that’s called a semiconductor?” And I didn’t know what a semiconductor was. To me it was something that conducted electricity almost. Very simple. You look it up, that’s what a semiconductor is, but I couldn’t relate that to the molecule used as a catalyst.

LÉCUYER: Interesting.

SELLO: But the seed was born. Well, it helped also in the graduate degree that when Shell Development came along, I got two fellowships from them for graduate study. I was a Shell Development fellow.

BROCK: When you returned after the service?

SELLO: When I returned after the service, I went to work at Shell.

BROCK: Can we talk a little bit about your military service in the navy?

SELLO: Oh, sure.

BROCK: So that was through 1944 to 1946?

SELLO: From 1944 to 1946. That was the tail end of the European war, but the Japanese war was still going on. Well, the Draft Board got me. [laughter] But I told them that I wanted to join the navy, so I was drafted into the navy. I don't know why particularly. I thought maybe they'd be closer to some sort of scientific aspects than just being a G.I. out in the field.

So I went to naval training at Great Lakes, Illinois, Great Lakes Naval Training Station, and I served my brutal initiation, the boot training. As I recall, it was about twelve weeks, something like that. You had to hustle. It turned out to be that there were quite a few master's guys in that squadron.

BROCK: That's interesting.

SELLO: All from different parts of the country, but all about with the same kind of story that I had; you know, the Board had allowed them to go to school and finish. There were physicists, some chemists. So I felt at home.

Right after that boot camp training, I had applied—oh, I specified in the Draft Board that I'd like to get into the navy, I'd like to go to Officers' Training School if I could, and they marked that on the Draft Board; they marked that in the draft papers. But I didn't hear about that until after boot camp. From boot camp I got into what they called the radar tech training. I wanted to go into that because it was something technical and it was very useful, as it turned out to be. I went to radar tech training school, two of them. The second one was in Arkansas, and it was there that I got the word that my commission had come through.

There's an interesting story on the side to that I have to tell. I was interviewed for the commission out of the radio tech school on my way to Midshipman school in New York for officer training. And the guy who interviewed me was a naval officer, he was a commander at the time, and he wanted to know why I wanted to get into the navy, you know, and that sort of stuff.

And I said, "Well, you know, I'd like to see if I could get into the technical stuff, I'd like that, but, of course, wherever the navy wanted to put me."

He said, "Well, let me look at your background here." He said, "Oh yes. Yes, yes." He said, "What was the name of the ship that your parents came from in Europe?" Right out of left field.

"Name that ship? You mean—?"

"Yes, the ship. They came to New York, I understand."

And I said, "Well, it was the SS *Orca*."

He said, "That's right." [laughter]

BROCK: What?

SELLO: They had conducted—I was of Russian background, and they conducted extra special checks on the Russians. They maybe did that on somebody else, too. I don't know. They could have. He was checking to see if my background check fit together with what I knew about it.

He said, "Yes, that's right."

I said, "Well, how did you know?"

He said, "You're asking the navy how we know? We know everything." [laughter] We had a laugh over that. He had the data. Yes, they had gone back, and it was a little bit of a problem, because what I later did is I wanted very much to go to the Russian language school in Monterey [California; Naval Postgraduate School], the one that was just recently, just finished just a month or so ago. Beautiful language school. I think it's one of the best in the world. Maybe there are some better ones in Russia itself or in France or England. I wanted to go to Russian language school because I still retained enough of the Russian that it wouldn't take me long to learn it.

Besides, I didn't tell you, but I had to qualify in two languages at the University of Missouri and I chose Russian as one of the qualification languages, and German. And I had to carry out the qualification tests in graduate school. So I passed the language qual test. French was a third that you could take, but nobody used French. [laughter]

LÉCUYER: Not in chemistry.

SELLO: Yes, right. Not in chemistry. Well, in organic chemistry, yes. Yes, there was a lot of French history in organic chemistry.

Anyway, so I applied for that and I went to Midshipman school and I noted I wanted to apply for that training. I thought, "Well, gee whiz, maybe I could get a head start or something, get into a real special place."

I went to Midshipman school in New York at Columbia University. That was a 120-day wonder. You got out and you got your commission, you got your ensign bars in 120 days, starting with midshipman. And from there I was shipped to Florida to Destroyer Escort training school.

It turned out that was just the time of Iwo Jima, and later Okinawa, but even slightly before Iwo Jima when the suicide planes were hitting us at the rate of one ship a day, and losing officers like mad. So I said I wanted to go to language school. They turned a deaf ear. "You're going to destroyer escort school and you're going right out," which is what I did, so I got in the tail end of it.

But when I got to my ship, it was interesting, the skipper took me aside. He said, "Where the hell you been? You've been twenty-seven days en route. What took you so long? Where are you going out here?"

I said, "Well, I started out in San Francisco and that's where I was told to report, and got aboard a freighter and the freighter took me to Hawaii, and I took another one from Hawaii where the navy put me on, and that got down to Eniwetok and Ulithi and those islands, and then from there was another one that took me to Iwo Jima," just at the time the—before the time the landings took place. Then we were hit with the landings at Okinawa. Then finally after more time we eventually landed up at Osaka at the time of the surrender.

And when I got aboard ship, he said, "Where the hell have you been? Besides, you didn't get into language school and you didn't get my permission to apply."

I said, "I don't know who my skipper was going to be."

He said, "You had no right to do that. If you were going to go to language school, you send me a letter asking for my permission as commanding officer to go to language school, to apply there." I just wrote to the language school myself, to Monterey. I just wrote to Commander Hindmarsh and I said, "Please, I want to join right away."

It was not possible, because the Russian background was a little rough. It was already oriented toward Communist infiltration in the military. They didn't want to take any chances. They'd take me if I wanted to learn Japanese. I could have gone in right away. But to do that, I would have to sign up for six more years, and besides, Japanese, it would have been useful, and as I look back now, maybe it would have been interesting to have. But nope, it didn't work.

So the naval service was Iwo Jima, Okinawa—oh, I mentioned Iwo Jima. Then we hit Okinawa. I remember it. I can speak as a veteran.

BROCK: Were you serving on a destroyer escort when you got to Iwo Jima or were you being—

SELLO: Yes. I was serving. But my billet was supposed to—I was supposed to get there twenty days before it came to Iwo Jima. Iwo Jima was the island where the big bombers took off when they delivered the atom bomb. And the skipper was hopping mad because, you know, we were losing many ships and he hadn't yet gotten his full complement of officers. "Where the hell is this guy? He started out twenty-seven days ago from San Francisco."

Yes, so I got in on the Iwo Jima and Okinawa end. That was not very happy. Our ship was hit by a bomber. It turned out it was not a suicide plane, but knock on wood we're here. So it was a useful experience. I don't know for what. For staying out of another war, I guess.

I was the last officer aboard the ship. I was the last one to come on as new, so then I had to stay the longest. So I stayed two years and a couple of months before they let me go, because I was the last one in command of the ship before all left. We all went back to Hawaii, to Pearl Harbor, then from there to San Diego and there I was discharged and went back to Missouri. Not directly; I went back to Chicago and lay on the beach outside of Chicago until my dad came along and said, "I don't know how long you're planning to stay here, but you're not going to stay here. You're either going to go to work or you're going to go to school, but you're not going to stay here." [laughter] Very understanding. So I went back to Missouri. I was ready. It was fine.

LÉCUYER: So then you were saying that your Ph.D. work was financed by Shell?

SELLO: About two years, yes. Yes, I would say so. There were two fellowships. For the first fellowship was a small one, so for that I actually also taught. I took a half-time teaching assistantship, but only with Dr. Stearn. I didn't want to teach any of the organic chemistry classes, just physical chemistry.

LÉCUYER: So did you benefit from the GI Bill?

SELLO: Yes. Yes, I did. I didn't have to take any meal jobs. I did benefit from the GI Bill. It was a blessing for me. There was no way that I could have done it otherwise. It would have taken a lot longer. I suppose I could have done some teaching in between or whatever. But it gave me a good shot at the teaching of physical chemistry to electrical engineers and to chemical engineers and to pre-med students.

LÉCUYER: That's difficult.

SELLO: There's stuff—I wouldn't wish that on you. [laughter] To teach a chemical engineer something about physical chemistry when they can't even spell it. [laughter]

I guess I'm getting through these questions.

BROCK: So when you came back to Missouri, that was from 1946 to 1948?

SELLO: Right. Right.

BROCK: And got right back into the—

SELLO: Got right back into the—yes.

BROCK: Right back into the work.

SELLO: I had to, of course, I had to take prelim examinations for the Ph.D. They're different than right out here. You took the cold course and prelims in every field that they taught, and then a year later, no, more than a year later, you got a special examination on the thesis work. You had to defend your thesis as a separate exercise.

BROCK: That's interesting.

SELLO: And the prelim examination was tougher than the defense of the thesis. I had to write almost every known organic reaction by name. I couldn't believe it. Sheer brutal memory work, but that's what organic chemists did. The first question they asked me, "Can you write down the reaction for the pinacol rearrangement?"

I said, "Oh yeah, that's easy. That's my thesis." [Sello's Ph.D. is titled "Kinetics and Mechanism of Pinacol Rearrangements."]

"Well, write it down." And then it got tougher from there. [laughter]

BROCK: So as that research towards the dissertation was coming together and you're writing it up, what were your thoughts about your next step? Who were you talking to?

SELLO: Oh, that's a good question. Of course, I had to talk to Shell Development, that was a given, because they were generous, and I knew that they had a wide variety of effort going on, but I also talked to other schools. I talked to Purdue, Wisconsin, for teaching and I talked to Standard Oil in Oklahoma and to an exploration company in Oklahoma; they wanted physical chemists. At that time I was already classified now as a physical chemist coming out in physical chemistry, although at Shell we kept saying physical organic chemistry. [laughter] So I interviewed at quite a few places.

And Shell came back. Dr. William Vaughn was the fellow's name, Bill Vaughn, who was examining the fellowships, and he was the chairman of the physics department at Shell Development. The physics department was concerned with catalysis of organic chemical reactions. That was Shell's business. Shell Development did all the research and all the exploration for getting chemicals from petroleum; chemicals, not oil or gas, necessary chemicals. And he was a very persuasive guy. I went out there to talk to him and talk to a few people around there, and I accepted them. I accepted going to Shell Development. It was not the top offer, but it was as good as anybody else around, a little bit less, but they had a much broader field of effort going on.

They had this physics department that was run by Bill Vaughn; they had the organic synthesis department; and they had an analytical department; and they had even an inorganic one; and they also had a process development department. It occurred to me—and Bill Vaughn seconded it—I said, "Well, could I do something to do with the rate of reactions? Because we can learn some mechanisms from this, I think." Apropos of the kind of thing I did.

He said, "Sure, we've got stuff like that going on."

So I thought I was going to be going to his department in physics, but, no, I was actually drafted into the process development department, and this was the department whose job it was to take known chemical reactions making alcohol from propylene, something like this. Simple. It could be simple; it could be complex. And developing a commercial process to where it would be economical to do this so that you could get the alcohol at a better rate than you could get it from grain or from any other way. But it was a way of using a petroleum product converted to a chemical. That was big, big at that time, and chemicals from petroleum was really something.

I thought maybe DuPont might be a place, but I got talked out of DuPont. They said, "Oh, you're just going to be—you say you don't like to be a pot boiler." He said, "That's all they do at DuPont is boil pots." [laughter] That could have been the competition, you know, talking.

Of course, Shell Development was here in Emeryville in California.

LÉCUYER: Yes, that was the next question.

SELLO: And of course, when I went out to pick up my ship, I left from San Francisco, so I had to spend a good part of those missing twenty-seven days in San Francisco. Of course, Bill Vaughn poured it on. "How can you go anywhere else?" It turned out that shortly after I left

Shell, they moved. They gave up the whole organization and moved it to Texas, which I was very glad for. That's a whole other story.

So I worked there and I joined the process development department. Then I had a chance to actually do some reaction-rate work, gave couple of papers within Shell along the lines that I had done my thesis work. I used activation. I used the Eyring theory for that and it turned out to be useful. I had a nice job lecturing to a bunch of organic chemists as to what that meant and they thought it was a bunch of gobbly-gook. "What's this guy talking about? Did you develop the reaction or you didn't?"

Our job was to take something from the laboratory stage, so it could go into big equipment, "scale-up" was the word, that could be done at some refinery. And there were a couple of those that did graduate to that level.

LÉCUYER: So you must have worked with lots of chemical engineers then.

SELLO: Yes. Yes, I worked with chemical engineers. I was the only physical chemist in the process development department. No, no, I'm sorry. There was Dr. Marion Taylor, a physical chemist, who was the chairman of the department. So he had to approve—he was the one that dragged me into process development when Bill Vaughn came around with all his new candidates, because I interviewed various ones. And it turned out that Marion Taylor was the one I would have liked to gone with anyway, because the work was much more rate-theory oriented, rate of reaction oriented. They were doing a lot of chemical reactions that were dependent on rates—well, they took so long to finish. To make a simple ethylene plus water to make alcohol, just a simple thing like that, they had to build enormous plants, they had big equipment, all that sort of stuff. They didn't know anything about the mechanisms of reactions. "Why don't you just, on the side, just on small scale, build some equipment and study the rate of reaction. Now you've picked a condition."

"Oh, we haven't got time to do that. Stick it right in the equipment that it's going to be sold out of. Scale it up." And that's dangerous. That's downright dangerous, and that led to some work that I did that proved that point. To go into large volume work for flammable material, for example, and, yes, and in large volume, and when you haven't done anything to show where the limits of this reaction is, how are you going to quench it, for example? You quench it by running the hell out of it. [laughter] So, process development suited me.

At the same time I got intrigued with catalysis. Now, a big portion of oil work is this mysterious catalysis, and there is a reaction. Propylene can be converted into propylene glycol or glycerin; either dihydroxy or trihydroxy can be converted by a catalytic reaction with cuprous oxide as a catalyst. Cuprous oxide is a semiconductor, a compound semiconductor. The resemblance between that semiconductor and anything I worked with fifty years later is zero. [laughter] But it is a semiconductor. You know, I didn't pay much attention to that. Well, a semiconductor, that's an interesting property, and I was curious about the fact that it changes color during the course of this particular reaction.

BROCK: The catalyst.

SELLO: The catalyst. It's the catalyst, and the catalyst is not supposed to participate in the reaction. That's against catalyst law. [laughter] But here was this silly cuprous oxide changing from black to red, and there is a state of cuprous copper that's red, and cupric copper is black, and in between it can be a sort of a mix. So there's something going on that starts out a semiconductor as a catalyst and then does its work and comes back as a non-semiconductor.

BROCK: Interesting.

SELLO: I didn't pay too much attention to that, because I didn't know anything about inorganic chemistry and Brillouin zones and band gap theory. Nothing. Nothing. Charles Kittel's work or any of this, all of this was foreign to me; I'd never even heard of it. Well, that you would have gotten in pure inorganic chemistry maybe, not even physical. You might have got it—in Illinois you would have gotten it in electrical engineering; EE. You would have learned about band gap compounds. I was never exposed to that.

But the fact that cuprous oxide was a semiconductor intrigued me because it also lost conductivity when it changed from those other forms. I thought you could put it and do it in a separate little beaker, change its color and put a couple of probes on it and it won't conduct when it's black, but it will conduct when it's red. So it goes from cuprous to cupric and you can measure the conductivity. I thought to myself, jeez, hey, you know, in all of this reaction it must—you can't run this reaction if you just have the black form. You can't make the propylene glycol. It isn't a catalyst for that.

BROCK: You needed it in the—?

SELLO: You needed it in the cuprous form. It ends up either cupric or mixed because not all the catalyst changes; you only need a trace of it. So I thought, gee whiz, if you could, you know, watch it and stop the reaction when the red goes away, you'd save yourself a lot of propylene. [laughter] But that was the extent of my thinking. That's what I was hit with, the word "semiconductor."

BROCK: And so would that particular catalyst need to be recharged or replaced at some time?

SELLO: It's a good question. After a certain amount of passes made with this reaction, you had to take it out and hydrogenate it and clean it up from the CuO stage back to Cu_2O.

BROCK: Interesting.

SELLO: But there wasn't enough of it to make much water or anything like that. It just reduced it back to the cuprous state. And you could tell when it was back there that if you had a couple of probes stuck in it—and I did that as an experiment—you could quit doing that and quit regenerating it when the probes started registering the semiconductivity. I thought, hey, if there was some way I could control this, I could control the reaction. Oh, that was a pipe dream; nothing in that. A catalyst is going to do what it wants to do in the presence of the molecule that it's hydrogenating. It's not going to do it by itself. But that was where I first got into semiconductor.

Then I had a chance to attend a Gordon Research Conference. Those were very, very popular, and I think they're still going on.

BROCK: Yes.

SELLO: Very good. They were excellent, because you didn't publish anything from the Gordon Research Conference, so therefore it could be wide open. You could talk about things. I listened to a presentation on catalytic semiconductors, and the presentation had to do with what is the nature of semiconductivity, and not totally conductive, and it was a question of electrical measurements. But I got a chance to look a bit into the energy bands that went with semiconductivity, and you could see these energy bands, parallel bands, and big dips in them. The catalyst was effective in the band area where there was a dip.

BROCK: Interesting.

SELLO: Yes. Because in that area, it was a different compound. Like the cuprous state might be right there, but the cupric state would be on the other side. And you could draw the activity based on the band gap theory, based on band gap analysis. But it was just a further stimulus, outside stimulus of thinking of new ways of handling catalysts.

BROCK: Was the group working on catalysis? I'm sorry if you already mentioned this. Was that within the process development group or was the catalysis group its own—

SELLO: The reactions we studied always started in the group, process development. If it required a catalyst—and some did—we started the work at that level. Sometimes the catalyst was a soluble acid in the organic material, sort of like the oxidation of dimethylbenzene to make terephthalic acid, you know, a nylon intermediate. That required an acid cobalt, soluble as an oxidation catalyst, and the cobalt slowly changed and the reaction died when the cobalt died. But that was more of a stoichometric change than it was just anything to do with semiconductivity.

But we studied those reactions in small scale. We built the equipment. I built separate equipment to oxidize this material. I didn't realize how dangerous. You know, we were blowing oxygen through a benzene derivative, and that's horrible. Here we are blowing the oxygen. Well, we thought the oxygen is all going to get used up so fast, it will never explode. [laughter]

But the Process Development Group was doing it on the small scale and then it went to the chemical plant's division where they built the big reactors to do that. Now, the chances for blowup are much worse over there than they were in the— [laughter] But I don't know—well, go ahead. It was scale-up. That was the name of the game; scale-up. You did it. on a small scale, whatever reactions were being developed were done in the laboratory on small scale and then we transferred it to the plant's area where they did the same one, but in the enormous scale. And of course, different equipment meant the different behavior. You could talk to the plant's guys all day about energy of activation and they would laugh at you, "What difference does it make? It doesn't mean you're going to have to change the pot." [laughter]

But that was the major work going on. Many products from propylene. Acrolein, that was a —I don't know if you've ever smelled acrolein. It's a very penetrating lachrymator, compared to some of the poison gases they talk about now. But it was a wonderful intermediate to go on to other compounds for Shell Chemical. I didn't realize it at the time, but Shell Chemical's days were numbered. I'm looking back, you know, backwards.

BROCK: Why?

SELLO: Well, the cost of petroleum. The cost of the petroleum starting material began rising, the Middle East activity and all that sort of stuff. And we were trying to make a cheaper form of ethyl alcohol and propylene glycol from petroleum and it would be cheaper than any other way you could get it, and just from the chemicals. Well, now, if the starting oil itself as it comes out of the refinery is already more expensive than what you're going to get out of this—[laughter] And that was a worry. But in that period I never—I've never really quite understood clearly where—how [William] Shockley found me.

BROCK: That was one of the questions I had.

SELLO: That's the question you had. But that is where he found me. Now, I have a couple of theories.

BROCK: What are they?

SELLO: One of them is Shockley knew about Shell Development. It was a place that he knew about.

BROCK: It had a very high reputation.

SELLO: It had a high reputation for chemists and scientists of all sorts. Secondly—and I don't know how deeply this was—Bob Brattain was at Shell Development, the younger brother of Walter Brattain. I tend to put less on that than any other factor.

And thirdly, Shockley was acquainted with most of the department heads at Shell Development, guys like my immediate superior, Marion Taylor or Dave Yabroff. These were physical chemists themselves, and he had known them also from Stanford kinds of relationships, so he knew the people. I got a call from Shockley saying, "I'd like to talk with you." And of course, I knew the name Shockley because he had just won the Nobel Prize just a few months before.

And I said, "Oh, my." I said, "I guess I ought to talk to him. Why did he want to talk to me?"

On the side of Shockley was the other factor, and that is Shockley could see that he needed to have a cross-section of skills. He had a flock of Ph.D.'s, but he had no one who knew anything about organic molecules. Not really. There were some technicians that could handle solvents, and he had some ideas about using organic molecules in some format as masking materials for etching silicon, because it was well known that wax—you can etch silicon or glass or anything like that etched with hydrochloric acid, if you have a waxy material. So we do this all the time, pour out a little blob of wax on and put your initials in it, and then etch your initials into silicon using wax. So he wanted to get a background of physical organic chemistry. I think that is the primary reason.

LÉCUYER: He might also have been interested in somebody who knew a lot about process development, right?

BROCK: Process development and scale-up.

LÉCUYER: He might have been interested in that part of your background as well, right?

SELLO: I was not interested in that personally. I didn't care for the scale-up work. That was—well, that started—you must have read something about that, too. That started something else. From my reaction rate background, I realized that we were very fortunate in carrying out these small-scale experiments at the rate that they were going, because they were small, but when they got large, the same medium was in danger of being within inflammable and detonation limits of reaction. And Shell had just had a couple of very bad explosions up in Martinez [California] in the Shell Chemical Plant, and they were the results of the detonation of molecules. They were trying to oxidize in large scale.

I pointed this out to Marion Taylor, so I got a new project going and I wrote a paper about this, on the limits of flammability of this particular organic molecule. And there was another factor involved in that, in that when you started to oxidize these organic materials, they first go through a region where they would be flammable. They could just burn. But if that burning would continue and you gave the reaction chamber enough length of reaction to take place, the speed of the reaction would convert and suddenly change over into a detonation which was ten times or a hundred times faster than the burning of the material itself.

So I said, well, gee whiz, here we're at the limits of flammability. How do we know when it could convert into a detonation? And I was afraid of that in the big reactor, because it had all the distance that it could use. So I insisted to my then department head, I said, "We have to make some measurements of the detonation limits of ordinary propylene," and we did. We found the detonation range. That killed Shell's propylene oxidation project intended to produce propylene glycol and propylene alcohol.

That was a project which I separated from the laboratory and I went outside to do this. I and a technician built a long pipe with strain gauges along its length. And we ignited the gas at one end and we measured the rate of that ignition by how fast the gauges popped. [laughter] And they would go at reasonable limits for about ten to fifteen feet and then suddenly an acceleration occurred, "wrrrooommmpp," it would blow out the end of the tube. We were in the hills here above the Calaveras Reservoir where we went to do those experiments.

So I wrote a paper on the limits of detonation. It scared the hell out of the process development guys and also the engineers, the process engineers. Later when Shockley was asking me what did I do at Shell, I mentioned—he saw that paper and he wanted to know what that was all about and I explained it to him, while he explained to me Band Gap theory, which went in one ear and out the other. That I learned as a consequence of being exposed to Bob Noyce and to C.T. Sah. Those guys knew it cold.

Shockley was not a very good teacher. He was a very good interferer, but he wasn't a good teacher. He interfered in what you were doing because it wasn't his way. You told him what you were going to do and he'd say, "That's not right," whatever it was. [laughter] But he was brilliant enough to be close to the truth.

BROCK: Before we return to the beginnings of your conversation with William Shockley, there are a couple of questions I wanted to ask, one of which was your life outside of work in this. I guess you're at Shell Development Company for nine years or so.

SELLO: Eight years. Eight years, yes. I left in the middle of the eighth year.

BROCK: Where you lived, what your life was like outside of work.

SELLO: Well, we lived in Berkeley, which was a wonderful place to live, up on Tightwad Hill. I guess you know where that is. That's the hills of Berkeley that overlooks the football stadium, and our street ended right at the football stadium. So us "tightwads" could go watch the game. [laughter] A bit about Berkeley. Beautiful place, I really didn't want to leave it. It had a beautiful view of the entire bay and the Golden Gate Bridge. Mornings there were gorgeous.

Well, in the chemistry part of it, I don't know how it got started, but a group of us, led by me—well, I was sort of the initiator—wanted to do something for the local high schools, the students, into taking chemistry. We needed people, you know, to teach them a little bit about science and chemistry; But not to do it the way Mr. Wizard was doing it on television. He was just pouring things together and saying, "See what happens. Here's A and B and here you get C." So everything was all carefully set up to work without explanation. It had to work, otherwise Mr. Wizard didn't show it, because the show was prepared offstage and then photographed and then shown on TV.

Our idea was to show the actual laboratory, let the work be done on the laboratory desk. So at Shell, a group of us got together and we arranged a show which I called—I named it—*Tempest in a Test Tube*, because it was named after [Michael] Faraday's work. I'm sure you must have read that, Michael Faraday's work, which he called *Tempest in a Test Tube*. In fact, the first four experiments were identically the one that Faraday did. What do they call it? The life of a candle. Faraday lit a candle and he explained physics, all the physics of the world in the nature of that candle. So we did that. I personally gave forty-eight lecture demonstrations of chemistry at the table right in front of the camera, what happened wants to happen, and it did. And the second series was financed by the Ford Foundation, and we made a movie of that and that was shown all over the country on WGBH. I thought that you might have gotten some of your material from that, the WGBH background. But they have the copy of the movie and all of that, before it became this series. See that? We can get a copy from them.

It was twenty-four weekly shows, one hour each, and each one was a series of experiments in chemistry, each series a different one. And naturally there was one involved in the rate of reactions. [laughter] Those took a lot of preparation and it taught me a little bit about committee work, and what I learned about committee work is, don't trust a committee; do it yourself. Those guys were terrible.

BROCK: Who?

SELLO: My presentation committee. We had four guys assigned. I was to be the, so-called star. Each one of them was supposed to prepare a show and get the materials together at Shell Development. Well, that worked for the first four shows and they gave up. We couldn't find them. But every Wednesday night I was scheduled to appear, so I had to go and beat on my friends, the same ones that did it before, to get them involved. They did it, but that wasn't where the rate-determining step was. [laughter] We covered a pretty good gambit of experiments.

BROCK: That was with the local public television station?

SELLO: Yes, KQED.

BROCK: You and your colleagues at Shell, you really wanted to be a program for high schoolers?

SELLO: Yes, we intended it to be at the high school level.

BROCK: And is that what happened or did more—

SELLO: We got more parents than we got students. We got college level and we got a lot of grammar school kids. We had tremendous live audiences in the studio, and some of it got to be a little heavy because, you know, here all this stuff is laid out there, the gases and the flames, and the audience is at arm's length. But it was fascinating. The fact that we got more out of the materials that didn't work than we got out of the experiments that did work—for example, my key piece of equipment was a Bunsen burner, which was the hallmark of the show. The show opens and you see this flame being lit. The burner circulates, you know, in the background. But that was the hallmark.

Well, sure enough, about the third show—I think it was the third—came up, I couldn't light the burner. I couldn't light the goddamn burner. And the camera's on me, the director is tearing his hair out. [laughter]

BROCK: What did you do?

SELLO: So I said, "Well, let's go to the experiments that don't require a burner and we'll come back to this one." Of course, like any good chemist should do, I gave them a line of bullshit about, "Let's check the methane gas tank." It was running off of a methane tank. "Let's check the tank to see if that's working okay." And they're concerned that I'm going to light off the tank. [laughter]

So I went on to the other experiments. There were about six per show, five to six, and I came back to the burner, and what I had done in between, I didn't realize it, is that I had pulled off the rubber tubing to the burner and then reattached it, but in attaching it I didn't see that there was a pinhole, and so the gas was going out the pinhole and not into the burner. But it turned out to be explanatory because then I shut off the burner, I shut off the tank, and I said, "See, this is what happens in an actual laboratory. Here's the tube," and I pulled it out and there was a big hole in it. Of course, I'd made the hole a little bigger so they could see it. We had a lot of stuff like that, and that, I mean, it just happens. But the whole theme was you did experiments live, you picked them very carefully, of course, because you wanted them to work, but Mother Nature often intervened.

In a later experiment I had two tanks of methane from two companies that donated them, because I thought, I'm not going to get caught in this again. If one tank isn't going to work, I'll shut it off and hook the burner to the other tank. I forget the name of it, but one of the tanks was a Shell tank and the other was from a local gas company. I got a long letter from that one that said, "Thank you for the free publicity. You showed that the Shell tank couldn't provide any gas for you, but ours did," because the label was visible right on the front. [laughter] They didn't congratulate me on the experiment at all.

I gave a series of lectures for high schools as a result. High school teachers just loved it. They'd drag their classes—they'd cancel their afternoon hourly class with them and they'd drag them to the KQED studio. Of course, we had to arrange it. But it was very successful by the fact that the experiments could miss; it was real.

And there were some stupid mistakes as well. One time the experiment consisted of taking a piece of pipe and filling it full of water, screwing a cap on both sides, and then immersing that in a bucket of dry ice and letting it freeze. I timed it because at the right time it would split because the water would burst out the side. I left it next to last, and as we came to the next to last experiment, I could see nothing had happened yet, nothing was happening in that bucket. So I shoved it off to the next one. I didn't say anything about what the intended experiment was going to be. I said, "Well, we finished a little early. Well, you know, it's a good chemist that can finish his experiments ahead of time, as long as they go right, and thank you and goodnight and we'll see you next time."

I thought I was off the air, and all of a sudden the pipe burst and I shouted out, I said, "Shit! It's already gone." [laughter] And I forgot to look at the red light which indicates that the mike was still on. [laughter] I didn't hear the end of that for the next two months. I wasn't thinking; just wasn't thinking.

But that was the extracurricular work. I did a lot of lecturing and teaching.

BROCK: Yes, that must have been a lot. It must have been very time-intensive.

SELLO: Yes, it was fun, and Shell got a lot of mileage out it. We did say Shell Development on the air.

BROCK: How did your bosses and colleagues react to you doing this?

SELLO: Oh, they liked it. They liked it. Oh yes, the process development department head, he said, "See, that's what we do in process development all the time." [laughter]

BROCK: Are there copies of the program available?

SELLO: I'm not sure, but I may have a tape. I have a tape that one of my erstwhile friends made. He collected all the pieces of the things that didn't work and he put them into one tape. So somewhere there's a tape of boo-boos. I'd be willing to let you see that. I don't know if I want to reproduce it, but I'll have to check to see if that—I know WGBH has a copy of the *Tempest in a Test Tube* tape, but that's some years back. Maybe they've lost it since then. But I'll make a check from this end. Maybe I could call you when I find out something.

BROCK: Yes, it would be interesting.

SELLO: Maybe you can take a look at it. We tried to resurrect this about six months ago, that whole series, with the local chemistry department, same station, but to use another sponsor—Shell is no longer there. But we ran into a lot of difficulty. For one, the experiments that I did, I wore safety glasses, but not all the time, and we didn't want to take a chance on doing a show showing experiments where the lecturer was not wearing safety glasses. So I said, "Well, we could make a point of that or we could just take out the ones where it showed that I had a lab coat on and lab gloves, but not always safety glasses."

Then there was a question of financing, who's going to pay for these, and the local station couldn't do it. So the program plan fell by the wayside. I would have loved to have done it fifty years later and even show it as a "This is the way you shouldn't do it."

BROCK: Yes.

SELLO: Show experiments done without safety glasses. In a few cases, like the one with the bursting iron pipe, I did wear safety glasses. But the teachers wanted me to—the teachers that reviewed the show, two of them, they wanted me to wear safety glasses every time no matter what the experiments were, which is the way it should have been done correctly in the first place.

BROCK: I can imagine. Christophe, where do you think we should go from here?

SELLO: Is the pace okay for you?

BROCK: Oh yes, very much so.

LÉCUYER: Absolutely. Absolutely.

SELLO: Because there's a lot of ground, you know.

BROCK: There's a lot of ground to cover.

SELLO: But I'm letting you set the pace.

BROCK: I'm thinking that—well, let me just switch this off for a moment while we talk about this.

[PAUSE IN RECORDING]

BROCK: We'll get this back on, then.

SELLO: So I could only guess and not with 100 percent certainty on how Shockley found me. It was Shockley's diligence and Shockley's initiative, but there was a reason from Shockley's side. Shockley was running into the fact that his Ph.D.'s were not acquainted with the area, of physical organic chemistry, that the laboratory work, the work on semiconductors, was seemingly moving into.

For example, a large part of the work, almost all of it, was dependent on the use of organic photopolymers like KPR, Kodak Photoresist, to do the patterns of devices on silicon to make what we call the masks. Now, KPR is an organic molecule and the staff was mostly these metallurgists, physicists, and engineers. Gordon Moore understood, because Gordon Moore was a physical chemist, so he'd been exposed to chemistry. And Bob Noyce, nothing. I mean, that wasn't his *métier*. Jay Last and Sheldon Roberts were metallurgists from MIT [Massachusetts Institute of Technology]. So, you know, Shockley wanted to build up the background on what would come along in the case of organic materials, an understanding of organic materials so we could understand the Kodak resists.

Kodak had two resists at the time, the only things known at that time that would block HF, hydrofluoric acid, from etching glass or silicon. Off of etching silicon they had KPR, Kodak Photoresist, and KMER, Kodak Metal Etch Resist. Two high molecular-weight photopolymers, photosensitive, which the industry was using for etching experiments, but always etching on metal.

Shockley thought that, and rightly, he said, "Look, we're using wax every once in a while. We put wax dots down on silicon and then we etch round circles of silicon out from under the dot. Why can't we make masks out of wax?"

But nobody knew anything about wax, so he went to Shell Development and he found Harry Sello and he asked, "Do you know anything about carnauba wax or any kind of wax that you could use for doing some etching experiments?"

I said, "No. There should be a million of them. If KPR works, we can—and you know that candle wax works. You're telling me you tried it."

He says, "Yeah, but that dissolves off so fast. What are others?"

"There's a whole library of waxes."

He said, "Well, could you do some work and study the evaporation of wax to make masks?"

And to me, that plus other things involved in the organic chemicals, like the washing of silicon, the organic material they had to use to wash off the inorganic material, that you wanted to clean the wafers with, all of that, even the acid attack on silicon. Those guys didn't know much about the existence even of aqua regia until I came to the laboratory and told them, "You can etch silicon with aqua regia and it will go a lot of faster than what you're using now." Trouble is, you can't use it, it's too fast; it dissolves the whole friggin' wafer. [laughter]

Now, the organic influence or the influence of other compounds and what we might—that was typically Shockley, though. He didn't miss a thing. He wanted to cover all the areas of ignorance, and he thought that organic chemistry, or the relationship of silicon processing to organic chemistry was not covered well enough. He was right about that.

BROCK: Now, did he explain this to you in these initial phone calls you had with him or was that once you arrived?

SELLO: No. No. It was mostly done once I arrived.

BROCK: What was the initial contact like?

SELLO: The initial contact was an invitation to come and talk to him, and he told me about who he was and just got acquainted. But what he didn't tell me was what I had to go through before I could be accepted, and that was three days of intense psychological examinations.

BROCK: Oh, as he made other people do.

SELLO: As he had made the others do. And I didn't know that because I hadn't talked to them, and he didn't tell me. And that's a story practically in itself. I mean, I went weekends. I couldn't go from work, so I went on Saturdays and Sundays, three successive weekends, and went through a battery of psychological tests you wouldn't believe.

BROCK: Where was that?

SELLO: The tests were conducted in a hotel in San Francisco, in a suite set aside for the purpose. There was a thematic apperception test. That's a standard test. It's composed of a piece of art, a picture. It shows a man, in one case, a man standing alongside a woman; she's lying on a bed, and in the distance is a wheat field. And he's got a cap on, like he just came in from out of the wheat field, and he's looking down at her. And you have to write a one-page essay on what you think he's thinking of, or what he's doing. You know, I couldn't believe it. "What is he doing? Does he want to jump on her? Surely, Shockley doesn't want me to say that." [laughter] And there were a series of such pictures. That was one. There was about a half a dozen, things like that, thematic apperception.

Then there was some Rorschachs, Rorschachs coming out of the ears. I mean, he must have just learned what the Rorschach was. But I had no idea of the reason for those tests. And the reason for those tests, as we probably already know, was that he did not trust the behavior of the scientists that he had already run across or felt he had run across. It was a reflection of the trouble he was beginning to have with his senior scientists. And he not only wanted me to take those tests, but he forced the other scientists to take it, too.

LÉCUYER: Again?

SELLO: Yes. I don't know if all of them took them. I know Noyce did and I know Moore did, but I never did quite find out if the other guys did. The recent arrivals certainly took the test. And he took them himself. He said, "Don't worry about it." He said, "I've taken this test."

And I asked him, "How'd you do?" [laughter]

I thought they were just ordinary, you know, mental tests, mental agility or whatever, and I was just sort of halfway kidding. He said, "Well, I'll tell you when you've taken the test."

And there was another reason for that, too, because it would have been very tough to convince the guy if I did any better on the test than he did. There was that danger. I'll leave that on the record, because that's something he exhibited later in a couple of other ways.

BROCK: Just this competitive nature?

SELLO: The competitiveness to do better than the person, no matter who he was, at the job than he did. It showed up in a swimming contest when I went with him for a swim on a very hot afternoon, at Stanford, one day at the pool.

I said I wanted to go swimming and he said, "Hey, I'll go along. Do you mind?"

"No, not at all, Dr. Shockley. Fine."

He said, "Call me Bill."

"Okay, Bill, let's go."

And I can conclude with this if you wish. You know, he's older than I was at that time. So I jumped into the pool and I barreled like hell and I did about—I didn't look up until I had done two laps as fast as I could go. The third one I slowed up a little bit, and then I completed a fourth and a fifth, and then sort of barely got into a sixth. At the end of the sixth, I was bushed. I looked around to see where Shockley was and I didn't see him, so I got out of the pool. I thought, you know, this is strange. I don't know why I had to show off. He went to thirty lengths on the pool. Thirty times back and forth. And that was the reason he wanted to go with me. He says, "You call yourself a swimmer. How'd you like that?" [laughter] You know, with a smile, kind of joking, but yet underneath, a needle.

I showed him my work on the limits of detonation of organic compounds, and what you do when you finish the experiments. You plot a ternary diagram. And a ternary diagram is like a regular binary graph, but it's oxygen, nitrogen, and hydrogen, or the flammable molecule. You can plot those compositions on an equilateral triangle; you can plot those all on one plot. So the diagram is drawn where all the flammable limits are, and the flammable limit was an envelope within the diagram. And I showed him that and I explained what it showed, and he asked me—I thought he was going to ask me something about it, and he did. He said, "Can you derive for me why it is that all points add up to 100 percent on the ternary diagram?" That was the question. It had nothing to do with the work. And I had to sit there and puzzle it out for him.

Why is it that all points in an equilateral triangle add up to 100 percent? Well, of course, geometrically it's symmetrical, and if you draw perpendicular through each apex, each segment is going to add up to 100 percent by congruent triangle geometry, you know. So he started—you know, he was very polite. He started—I looked at them and I said, "Well, that's just the way it works."

He said, "Well, yeah, that's the way it works, but, you know, this is plane geometry, two-dimensional. Derive it. This is the paper you wrote."

I said, "Well, I didn't have to derive it in the paper."

He said, "But derive for me."

And so he started me out. He drew the perpendiculars, the clue, bisecting each side. "Now," he said, "you use that." So I started to stammer, and I finally got through the proof using congruent triangles. I proved two sides congruent and then the third was congruent to one of the other two, and you had to do that three times. It had nothing to do with the actual chemical work, but it was a test.

BROCK: He had just gotten the Nobel Prize, so you knew him from that, from the announcement.

SELLO: Oh yes, I knew that. Yes, I knew he'd gotten the Nobel Prize, I knew that Bob Brattain, also at Shell, was Walter Brattain's brother. In fact, before I left Shell, I went around and talked with Brattain. I said, "Hey, I got an offer to come and work with Shockley. What do you think of him?"

He says, "Oh, you want to leave Shell and go to work for Shockley?" So he didn't answer right away. He hemmed and he hawed.

I came back later and said, "You never answered my question, Bob. What do you think? Should I take the job?"

He said, "Well, I can direct you to a couple of people that maybe you want to talk to them. I don't think they'll advise you to take the job."

And already the word of the incompatibility of Shockley with his staff had gotten out. It had gotten back to Bell Telephone Laboratories, but Bob Brattain had heard about it from Walter Brattain. Bob was something less than enthusiastic about my leaving Shell to go to work with Shockley.

BROCK: With your exposure to the radar work during your stint in the navy, you must have gotten an acquaintance with electronics, at least in that, pretty intensively.

SELLO: Yes.

BROCK: Were you familiar with the transistor before Shockley's prize got announced?

SELLO: I wasn't familiar with them. I knew that the—for example, my specialty was sonar tracking of submarines and I was familiar with the transistors that were in sonar equipment. These were germanium transistors because they had a finite life. And I knew that they were the little amplifiers, I knew that, but I couldn't recognize that it was a semiconductor. I didn't even know that it was a piece of germanium, but I knew that there was an electrical amplifier that was made of solid-state material. I thought it was more like a ferrite crystal. I had made little crystal sets in earlier days and I had tickled the crystals, the garnets, with—well, everybody seemed to have done it, and I sent out some interesting messages, dot-and-dash messages. So I thought it was like a piece of garnet, but I'd never been acquainted with semi—other than the cuprous oxide; that was the only exposure.

But my big attraction; I was just so flattered by the fact that a Nobel Prize winner wanted me to join his team.

BROCK: Oh, sure.

LÉCUYER: If we could go back to the photoresist thing.

SELLO: Yes.

LÉCUYER: It was really around this time that the Diamond Fuse Lab was doing some work on photoresist and photolithography, right, on germanium?

SELLO: It was a little earlier than that, because the Diamond Fuse Lab never worked with germanium; they worked with silicon. And the earlier work on photoresist was also done with germanium because the photoresist was available. It was a commercial compound. But you're right, Christophe, Diamond knew more about the production use of resists and we learned a lot from them. We went there when we were trying to supply them with product, and they did some work in using resists.

LÉCUYER: You mean at Shell?

SELLO: No, no, no, no. I'm sorry. They were a colleague of Shockley. So we went to visit Diamond to see what they were doing with their semiconductor and we knew that they were using photoresist. But so was Bell Telephone, and Bell Telephone was the know-all and be-all at that time.

LÉCUYER: So that was just after you arrived and before the eight left to go to Fairchild, right?

SELLO: Yes. Not long before. They were there—they spent two years there, so did I. We overlapped for about six months. And when I got there and first walked into the laboratories, Noyce and Moore took me aside and told me about the problems of Shockley.

LÉCUYER: From the very first?

SELLO: Right, very first thing. They didn't want to hide it. They said, "You might have some problems here. We have had some problems." They didn't know at that time that they were going to leave. That didn't come—because a few events had to happen first.

I said, “But he’s a Nobel Prize winner.”

They said, “Yeah, we know. We know.” This is Moore and Noyce. “Talk to the other guys in the laboratory.”

The trouble was there, the incompatibility especially with the—well, I’ll say it flat out, especially with Jay Last, also with Sheldon Roberts a little bit, but more with Jay Last, the metallurgist, was already beginning to be visible. Noyce too was getting irritated by Shockley.

I didn’t know what to say, and I said, “Well, I’ve already accepted the job, so I’m glad you told me, so maybe you can help me get integrated,” and they did. They were extremely helpful. They didn’t mess around and we worked on Shockley’s projects, all did. They didn’t want to. They thought Shockley was too one-sided, and I agreed, as I began to learn about him.

BROCK: Too one-sided with the—

SELLO: The one device. He wanted to make one device, the four-layer diode, and that was the diode that was going to replace all the hundreds of thousands of mechanical switches in all of the Bell Telephone telephone systems. All it needed was one diode per switch, and he couldn’t make even ten that behaved the right way. That diode was going to be his entry into the world of semiconductor.

Also we never worked on germanium. Germanium never entered Shockley Laboratories. It was silicon.

BROCK: Well, maybe we should pause here, and then next time we’ll return back to the opening of the Shockley story and continue.

SELLO: Okay. Good. That’s a good spot.

BROCK: Let me just switch this off.

[END OF INTERVIEW]

INTERVIEWEE: **Harry Sello**

INTERVIEWERS: **David C. Brock and Christophe Lécuyer**

LOCATION: **Menlo Park, California**

DATE: **7 January 2005 [Interview 2]**

BROCK: This is a continuation of an oral history interview with Harry Sello, conducted by Christophe Lécuyer and David Brock, taking place on 7 January 2005, in Menlo Park.

To go into the story of your transition from Shell Development to Shockley Semiconductor in 1957, could you tell us a bit about what acquaintance you might have had with William [B.] Shockley, and with the transistor, at the time leading up to the point where you actually got a phone call from William Shockley?

SELLO: Well, while I was at Shell I knew nothing of the work of William Shockley. He'd gotten the Nobel Prize, and one keeps knowledge of this kind of thing, but it was in a field that was interesting to me, and I'd read about it, but it was all publicity kind of reading. Then I got the call and that sort of energized things all over, and we talked a couple of times on the telephone and he asked me about the work I was doing and if I was doing anything with semiconductors.

Of course, I had to scratch my head to try to figure out where I was connected to semiconductors. But there was an interesting connection, I believe I mentioned it before. The catalysis of the formation of chemical compounds, simple chemical compounds into bigger molecules, was carried on using cuprous oxide, and cuprous oxide was classified as a semiconductor. It's not a conductor, it's not an insulator; it's a semiconductor, and it changes color during the course of reaction. And if you watch through a glass tube reactor while you're pumping in propylene and air at one end, at the other end the effluent is propyl alcohol, and you can see the catalytic reaction zone, you can see this blue-red fluctuation within the catalyst. We knew that when it was blue, it was dead. The reaction would stop. Convenient kind of control; you could then examine the catalyst.

Prior to the phone call, I was curious about semiconductivity. So I read a little bit of the work of some of the guys in physics who were concerned with semiconductors, but nothing to do with silicon or germanium. These were compounds which had conductivity themselves. I had the crazy idea that if I could stick a couple of electrodes into the tube and hook them right up to the catalyst—we knew when it was blue the catalyst was dead. Well, if it was a conductor while it was working, would it be a non-conductor when it wasn't working? And a few sporadic attempts at that never got anywhere; couldn't figure out a way to hook onto the contact. And the currents, if they were there, were so tiny that there was no way of, not to me, no way of amplifying them to look at anything. It was a research project that was not exactly supported by Shell. [laughter] It was kind of a semi-bootlegged project, a very short one.

But that definitely raised the term “semiconductor.” Again, I saw that coming up with respect to a Gordon Research Conference that was held about that same period of time, and there was a whole session on conductivity of various organic materials, solids; not the kind of conductivity they talk about today of organic molecules, but the inorganic solids, cuprous oxide, sulfides, others. So the word “semiconductor” was bouncing around in my head at that time, coincidentally. I believe it was entirely coincidentally.

BROCK: Could you describe those initial phone conversations that you had with him?

SELLO: I told him about the same story. Shockley, if it wasn’t on his point he wasn’t interested. [laughter] I mean, he was curious about what I had been exposed to, but a quick couple of questions, he and I realized that I didn’t know anything about bandgap theory or any of the solid-state transitions that were occurring at that time in the field of true semiconductors. Nobody could write a bandgap description of cuprous oxide. You’d see nothing but intermixing curves and a mess that you couldn’t define; had nothing to do with that.

So he said, “Well, why don’t you come on down and let’s talk down here and tell me a little bit more about your work.”

BROCK: How did he describe what his firm was getting up to?

SELLO: Well, he was no shrinking violet, so he immediately started out with, you know, the Nobel Prize had just been awarded a year earlier, and he was accumulating a group of people to work in the field of silicon; did I know silicon? I said, “Only as a component in sand,” this kind of thing. I was trying to impress him a little bit, you know, that I wasn’t totally cold. That never worked with Shockley. He just shucked that off unless it was right to point.

So we talked about my work at Shell, and he mentioned a little bit about what they were doing. He gathered his group of people. He said, “Got fourteen or so free-running Ph.D.’s and they’re all doing what they want, and sometimes I know what they’re doing and sometimes I don’t know what they’re doing. They don’t bother to answer all the times, but I don’t expect them to.” So it was a—what did he call them? Fourteen free-running Ph.D.’s, that kind of thing.

“Then, of course, before we go much further, I’d like you to take a few tests.” That was the next conversation. Once I had decided I’d like to go further, he was very particular about that. He didn’t want to go another single step any further than he had to. He very jealously guarding proprietary details of what he was up to. Later it came out many, many times that there was a big focus on him from BTL, from Bell Laboratories, from other places where his colleagues had gone, and everybody in his field were very interested to see what he was up to and where his research was going, because this was a research organization that he organized; research and development organization, let's say.

He was very proprietary oriented, in fact, to a point of being paranoid about it. "Be very careful to whom you talk, because the moment you tell them something about what you're doing and it's with Shockley, then they're all going to go do the same thing." That was the attitude. So, very, very proprietary. It was something I was not that used to with Shell Development, which was also proprietary because we had to get patents and things, but he had a different aura about his work. And I did mention about the examinations.

BROCK: Right. So was that before you even went down to visit the operation?

SELLO: Before I went down to visit the operation, yes.

BROCK: And so he wanted you to have this battery of psychological tests?

SELLO: Tests. There was a weekend of tests in San Francisco, not down at the Shockley Semiconductor Laboratories; at a hotel. It was conducted in darkened chambers at a hotel. We're talking about the Ukrainians. [laughter]

BROCK: Was it just you and consultants or something?

SELLO: Yes, there were a couple of consultants. He was there and he would step in to see what the progress was.

LÉCUYER: So you would be there for the whole weekend, as well?

SELLO: Whole weekend, yes. Yes, Saturday and Sunday.

BROCK: Did you have any social contact with him, or was he just checking in on—

SELLO: No, no, he was just checking. I didn't know quite how to take that, you know. It could be very loose and, "Well, anything you want we can do." Why all this—not secrecy. Secrecy was something else. But why all this effort to judge my mental stability? So I took the liberty of asking him a couple of times had he taken these tests. [laughter]

BROCK: What did he say?

SELLO: He said, “Well, yes, I have. I’ll tell you the results after I see yours.”

BROCK: Did he ever do that?

SELLO: He did, but not in detail. He didn’t answer the psychological parts. If it was a question of physical data, you know, when was this invented, or when was that invented, something like that. But I asked him what was his interpretation of that thematic perception test in which the man is standing by the side of a woman at her bed, and it’s dark, and to the question was “Please describe in one paragraph what he’s thinking.” And asked him, “What did you think was going on?” [laughter]

I can’t really say that he was a little bit miffed, but he didn’t expect the question, and he said, “Well, take them all and we’ll talk about it afterwards.”

At the end of those tests, end of the two days, he said, “You did fairly well, considering they were new to you. Mine were a little better.” I didn’t know what he meant by better, because, well, what do you—he said, “But I passed and you passed.”

BROCK: Then was it soon thereafter that you went to visit the operation?

SELLO: Then I went down to visit, yes. I met with, at the time, all the guys that were there, but mostly [Robert N.] Noyce and [Gordon] Moore. They were sort of the kingpins of the operation and I was introduced to them. It was a one-day visit, and then after that I had to make the commitment. I didn’t pay too much attention to it at the time, I see now why, but I was warned by Noyce and Moore that there were problems. They had taken the psychological test. I said, “Does everybody take the test?” As far as they knew, yes, including Shockley. And they said, “Well, the old man’s kind of paranoid about some of these things, and there are problems here and I’m sure he’s trying to solve some of them.”

Then at a later visit, he was trying to avoid—he kept putting it in the category of, well, you know, is this man fit scientifically for this organization coming from Shell Development. But he did have some questions about some of my work at Shell, and later it turns out that he had a perfectly good reason for wanting somebody to come aboard with that experience, and that was the organic chemical part of it, that I was familiar with organic chemistry, and they were having a raft of problems at Shockley on resist problems. Shockley was trying to do some work or get some work done in that area to where they could ease those problems.

So I started on—shall we just continue this way?

BROCK: Yes, if you could talk about the resist problems.

SELLO: Right. I started to look into—he was thinking of—he said, “Could you do anything with wax evaporation?” And I asked him: what did he mean by wax evaporation. You know, there are tons of waxes and they all have boiling points. [laughter] But they were using—it was the practice in the semiconductor industry to use black wax as a resist in the etching of silicon, that tarry, gummy-looking stuff that comes in black sticks. It’s a derivative of a petroleum product. We call that a “bottoms product” of a distillation. What’s left over is black wax.

So I knew what black wax was, but that covers a multitude of things. They were using it as an etch resist. You’d glue down a wafer to a surface, to a glassy surface or to a metal surface with this black wax and overlap the edge of the wafer with the wax, and you could then dunk the wafer in various etchants, hydrofluoric acid, mostly, and nitric acid, and the wax would resist. But the black wax was a staple of the organization of semiconductor work even for the next five years. We were using black wax at Fairchild when Fairchild came up with integrated circuits. Black wax still had its place.

BROCK: Sorry to interrupt, but was it commonly available? Where did you get the black wax?

SELLO: Oh yes, you could get it. It was a laboratory chemical.

BROCK: Just from a laboratory supply catalog?

SELLO: That’s right. Yes, that was a very easy thing to do.

LÉCUYER: I was wondering, would Shockley be using KPR?

SELLO: Yes. Yes, this was one specific purpose. You couldn’t do a very good job of pattern masking with wax. You could glue it, glue down things. You could etch a whole wafer or a part of one, etch the top and not hit the bottom, with black wax. You had to smear it around by hand or with little probe, hot probes and stuff like that. But you could not do fine pattern etching with black wax—for that you needed a photoresist like KPR. However, KPR would “life” and the pattern would be lost.

He wanted to know—it was a reasonable question. He wanted to know if the resistive material in the wax, could that be evaporated, and if it could be evaporated through a mask, sort of like a regular screening material that you have on these boards, regular screening, rectangular screen, you could then put on a wafer. You could then etch the wafer into little squares or into little circles or something that the wax could do—if you could get it to evaporate.

So I spent a lot of time working on a whole derivative series of the carnauba wax family. I had bottles and jars full of wax from every part of the world, and we'd stick it in a crucible, in a vacuum chamber, and whatever wanted to evaporate we'd evaporate through a screen, and it would hit a silicon target and you could do squares nicely. If you did that on silicon, then you could etch the silicon very nicely. The wax really resists. It was like being able to evaporate black wax. But black wax, you try to heat that, you don't know what's coming off that. It's "crud." [laughter] So my first problem was to see what I could generate in the way of black wax.

Now, at the same time I was asked to work together with Noyce and with Jay [T.] Last and a couple of the other guys, to see what we could do with KPR. Now, the problem with evaporating wax is that you can evaporate squares and you can evaporate round complete things, but envision a donut, you cannot evaporate a donut pattern. Not without anchoring a little dot in the center to the periphery on the outside, and if you did that, then where it was connected it wouldn't etch. So you didn't get the same structure that you thought you were getting. There are a variety of shapes, interlocking shapes that you can't get with a single evaporation. But he was after the little squares, and the main reason was that he was so anxious to get the PNPN switches out, and they were etched only once; namely, etched out of the wafer after the diffusions were done. So if the wax worked, you could get a whole wafer full of little PNPN squares after diffusion, after total processing.

It was an interesting idea, but it had its limitations based on geometry, and it never would have worked in later structures at all in any kind of intricate structures. You'd have to have a little connecting pattern between images. You couldn't etch two circles, if they were connected as with a dumbbell. You just can't form that in the pattern of the wax.

But, as I say, Shockley was interested in the PNPN switches, and the KPR was giving the guys a terrible amount of problem, because the PNPN switches are integral squares. They're all one, and the KPR would lift. If you wanted to etch through the silicon and separate the little squares, you'd have to etch through forty microns at least or more of silicon, and if you got a good one where the KPR stuck all the way, that was a good one, but quite frequently a third to a half of them would wash away at the corners and the KPR would lift. You call that the "lifting" problem. And it got a name that lasted throughout the industry; "the witch is back." The lifting witch is back, you know. We were getting the lifting problems with resist all the time. But KPR was the only way to get intricate images. Shockley didn't need them for the PNPN switches and he didn't want to work on anything but PNPN.

LÉCUYER: Interesting. So the KPR was something that was pushed by Noyce and Last rather than by Shockley?

SELLO: Well, they used it. Of course, they wanted to make transistors, and you could separate the transistors with KPR, but you couldn't etch the mesa or you couldn't do fine etching, because the KPR just wouldn't stick, was not totally resistant enough. At that time Kodak came back; we tried some new ones. We were constantly in touch with them. I was in touch with them for their new stuff. They had one they called KMER, Kodak metal etch resist, and it was a much thicker, heavier kind of thing, and if it stuck, it would last longer than a thin film of KPR. But it was too heavy; you couldn't do fine structures.

Then we jumped on to KTFR, which was Kodak thin film resist. It was a new one developed in between the two of KPR and KMER. KMER types were known commercial resists. They're used by metal engravers quite frequently, etching, for example, printing plates for money, the money plates, they use that kind of stuff.

BROCK: Who was your opposite number at Kodak when you were dealing with them? Was it a salesperson?

SELLO: A salesperson, yes, a local distributor, and from time to time he'd bring somebody in from Kodak, but Kodak wasn't too interested because our quantities didn't account for anything. I mean, they could sell two bottles of KPR a month or something. [laughter] And Shockley didn't want to put up with those kind of problems; that was not his *métier*. He was, "Well, get them to give you some more. Get them to develop something new."

"But there's no incentive, Dr. Shockley."

"Well, tell them they're get rich if it works." And those guys are looking at one order per week; one bottle per week. [laughter] There was a lot fun about that topic.

They were generally cooperative wherever we could get their interest, because they could see that there were possibilities, but it would mean they would have to launch a research effort themselves or a development effort. As a matter of fact, KTFR didn't come out until about four or five years after KPR was used. Then the problem was HF. The problem was entirely HF. HF crept under the corners and the interface between the KPR and the silicon, and any geometry that you had you tried to preserve what we lost. You'd get these diffraction-looking rings. We were playing with wafers that were only forty microns thick, very, very thin, and lots of them were slightly bowed from the lapping that we did on them. And they were slightly bowed, so if you tried to press them onto the KPR and you pressed too hard, you'd shatter the whole damn wafer and all the diffusion results would disappear.

He had a good idea, but he went out to find a chemist who knew about wax. That was the starting point; then there were others. Then he wanted somebody to get into the photoresist problem and the chemicals. So that was one of the motivations for hiring me.

BROCK: What was his concern there about the chemicals?

SELLO: How to use them, HF and nitric acid. I mean, HF is a beast. You have to be so careful in how to use it, and people would get it under their fingernails, that was the common practice, common evil, get it on under their fingernails and then wouldn't be able to work because it itched; it irritated. Your fingernail looked like a wafer. [laughter] But it was a horrible kind of thing to use, and the mixture of HF and nitric acid is one which will dissolve silicon. HF will not dissolve silicon, it will dissolve only the silicon oxide. If you wanted to etch the oxide layer off and leave the silicon, you would use HF, but then you didn't care about the structure that was in there.

So it turned out then, and it's still today a pain in the ass, as far as etching problems are concerned, it's more controlled because they've developed it to a high degree. It's more adherent than it was in the early days to silicon. It was not developed for use on silicon; it was developed for use on metal engraving, thin sheets of metal, and for that it was excellent. But the problems that I worked on spread across the whole spectrum.

BROCK: It sounds like you were dealing with some of the basic chemistry fundamental to the technology.

SELLO: Yes, the only guy that knew any chemistry in the place, besides me, was Gordon Moore, and he came from infrared spectroscopy, so he wasn't an honest-to-goodness organic chemist or organic oriented, that's it, and he was the first one to say so. But he's the one who trained me in how to use KPR without wrecking my hands, how to use HF without wrecking my hands, the wax bottles and all that.

BROCK: And he, at that time, was more on the diffusion side?

SELLO: Yes, his job was pretty well concentrated on the diffusion for the PNPN devices. Shockley tried to get the whole effort going only on PNPN. That was a driving force for him. That was clearly his ambition, and he didn't like anybody else taking time off and doing something else if it wasn't related to the four-layer diode.

BROCK: And his big ambition for that was for the telephone company?

SELLO: Telephone switching systems, yes. Electrically it would be a marvel. It would take the place of one or more relays in any one of the switching matrices, and there's no mechanical wear. But it was an animal to try to make the—it had to be made very thin, because to achieve the switching action you had to diffuse two junctions on top of the one that was already there and do that all in one diffusion from top side and bottom side, and you had to stand the wafers up.

BROCK: On end?

SELLO: Yes. And then they had to come to within a set distance; each junction had to come to within a set distance. If the wafer was not perfectly parallel, lapped perfectly parallel, it would not make parallel junctions, if you can envision that.

BROCK: I can.

SELLO: Yes. And that was one problem. The second problem was if the junctions didn't come to within the set distance, you wouldn't get a constant switchover current in a PNPN. As you raise the voltage, you know that it pops over into a high-conducting state, and the turnover voltage is critically dependent on the distance between the two junctions. And any given wafer, though small, would produce a scattering of switching voltages, because if they weren't perfectly parallel to within micron range, then you'd get a different result from the part that was parallel closer to the part that wasn't, and that was controlled by how well you lapped it parallel. So we'd sit around many times—I guess I'd mentioned to you, I just to sit around with a wafer under every finger and a lapping plate, and try to lap ten at a time on one side, and then turn them over and lap it on the other side.

BROCK: Could you describe the lapping plate for me? I don't know what that is.

SELLO: It's a metal plate, a flat metal plate very much like is used in optics, like on an optical bench, but it's not quite that; doesn't have to be quite that rigorous. It's usually a very hard metal of a tantalum base or some sort of a titanium base, a very hard metal, perfectly flat itself, and you would scatter carbide dust on the lapping plate, very, very fine grit carbide dust, and then the thing would rotate. And of course, you could do one by holding the wafer down.

BROCK: The plates rotating?

SELLO: Yes, in parallel. Right. And one side at a time. So quickly the technicians who were good quickly learned how to use all ten fingers. [laughter] You can get a batch of wafers out. Now, even there you can see where within a micron or two of the distance between junctions, one that you lapped holding here or holding here would be a different voltage. And so how do you guarantee that the pressure of your fingers is exactly the right pressure you want?

Later we learned how to use two parallel lapping plates and engineer them to within careful distance. But look at what you're working with. You've got forty microns total space to work with, and these enormous plates themselves have to be parallel to within forty microns. [laughter] You could do this. It took some engineering and occasionally we'd get them, but if you wanted to get them out in a hurry, you werein difficulty.

BROCK: And those lapping plates you could get from an optical shop or something?

SELLO: You could buy those, yes. Yes, optical benches, places where they made optical benches. There was a lot of metalwork going on in the industry, in this industry, as well as others. Machining and lapping kind of things, they were pretty good at making surfaces. They were very good at it.

But, you see, the point I wanted to make was, if you weren't working on a PNPN device, Shockley wasn't interested. You know, "What are you doing and why isn't it related to something to do with the PNPN?"

You know, it's awfully hard to tell him where Mother Nature is going. "Well, we were working with a new kind of a resist."

"Well, how do you know it's going to work?"

"We don't know, but we thought we'd try it."

"Well, what kind of guarantees could you get that it might work?" Or, "What do they know about it?"

But it was all related to these four-layer diodes, and fully half the staff was working on those diodes. It's not exclusively what they wanted to work on. They wanted to work on transistors, which would have been much smarter, much smarter, because you don't have these restrictions on diffusions. You can do successive diffusions from one side.

BROCK: Do you think it was a case of—it almost sounds like William Shockley wanted to sort of skip a technological generation.

SELLO: Yes, he wanted to. Yes, clearly a case of ego. He wanted to sell millions of these things to BTL, to Western Electric, where he came from, to show them that "Here's something that will replace all those millions of mechanical switches that you have." He would send them there; they would evaluate them. His influence was great enough to get that done, but it was a lost cause because they varied so much. One batch, one diffusion batch would give you 40-volt devices and another diffusion batch would give you 60-volt devices, and some would give you both, because just one or two microns of displacement in the space between junctions was enough to change the voltage at switchover. Mechanical switches switch it only one setting; these things, all over the lot.

BROCK: So it was really that reliability.

SELLO: Yes, reliability. Right. And there was another problem, and that is to assemble those into a package. We had to devise our own little packages. See, all transistors and integrated circuits today are assembled from one side. The bottom part is the base of the package and the top has leads attached to another part of a package. Well, you can't do this on the PNPN switch, because you won't get the same voltage unless it's all contacted, symmetrically top and bottom. There will be no reproducibility. So we had to devise a package, and one of the technicians was very good at that; he devised a package which looked like two kinked gold wires which, when pinched together, would hold the little tiny 40-micron-thick piece between the two kinks of the wire. The wires were then attached to a header. On top of that you'd put a can and seal it up.

Well, of course, the inevitable always happened. Every once in a while, more frequently than not, the little chip would fall out from the pinch between the wires. It would fall out from between the contacts. "It was working yesterday. What happened to it today?" There were a lot of assembly reliability problems.

BROCK: We talked about where you were getting the black wax and the resists. Were you getting your chemical supplies, like the acids, from laboratory supply catalogs?

SELLO: We were getting them from Monsanto, Baker, and other chemical houses, always looking for the CP category, the chemically pure category. And a lot of them hadn't developed many of their chemicals to our level of purity. They didn't need to. If they were going to etch metals, why would you need to make sure that trace metals like copper weren't in there? Copper is death to a junction. And you know, you had to get super CP chemicals, and you can't talk Monsanto into that unless you can buy a million bottles. [laughter]

BROCK: So how did you get around that problem?

SELLO: Well, we just learned to use it as is. We got CP each time, and hoped the structure that we were working on was not that sensitive. We could get it copper-free, for example. If we could ask for something specific, they would come back with copper-free, until we defined that copper was a deathnium, then, you know, what would you ask for? "You want it even more pure than it is? It's already five nines purity. What do you want?"

"Well, can you make it seven nines?"

"Well, we don't know what to do to make it seven nines. What are you trying to avoid?" You know, you'd get into these discussions. You couldn't tell them. Well, when we learned about copper, we could tell them that. If it had a little gold in it, we didn't care too much, because we learned it didn't make too much difference. What else? [laughter] But chemical purity and the use of the chemicals was a very important part of my function, too, and that was also part of what Shockley intended for me to wander around the lab and try to do.

The other part of my work, the largest part, was based on masks; mask making. There I worked largely with Bob Noyce. Bob was the kind of a guy that the chemical problem didn't stop him. He said, "What do you mean you can't get it pure enough? Well, let's use what they have, and we'll see what happens." So he could have ten failures and see one good one, and then he'd chase after that. Diligence was his big number; very tenacious and very good.

Of course, we had to work on optical problems as well, in order to expose the resists. Now, imagine that in the case of the PNPN device, which half the laboratory was working for all the time that I was there, you didn't need that. You only needed circles. Okay, you could do a circle. You could take a perforated plate, stick it under a lens, shine ultraviolet through it, and you get a whole series of images of the perforated plate. Now, you'd also get whatever imperfections there are, the spurs and the unevenness of the plate, but that was something you could handle, or use a metal screen for rectangular dots of silicon. Shockley didn't like rectangular PNPN switches; he wanted them all to be the same, little round dots. I never could learn quite why. I'd say, "Well, look. We'll be able to do it with screens."

He'd say, "Yeah, but can't you take the corners off the screens and round them off?" [laughter] You know, nothing stood in his way.

Bob Noyce also didn't allow for limitations, but he was different; he would turn around and roll up his sleeves and go do it. Never Shockley. Shockley would walk away, and he'd come back maybe a week later, "You still got the corners problem?"

"Yes."

"Well, what have you done to try to get rid of it?" [laughter]

"Well, we worked all week at it."

“Yeah, but what have you done? Show me the experiments.”

A very precise kind of a guy, but he never did it himself. He was a supervisor.

LÉCUYER: Actually, this might be a good place to talk about Shockley as a manager.

SELLO: Yes. Well, of course, a lot of this was an old story with him. He was very impatient, although he exhibited moments of patience that sometimes I wondered, you know, “Where did they come from? He should have more of those.” But he was very impatient, in particular with guys like Jay Last, [C.] Sheldon Roberts. I never could quite understand why. Before that, he didn’t exhibit that kind of impatience with Bob Noyce or even Gordon Moore, at least not where I noticed it. But I sat next to Jay Last. My bench, my desk was next to his. Jay was frequently just totally perturbed by it. I could swear one time he was reduced to tears by Shockley’s attitude. He just kept picking at him, you know, “Why aren’t you doing this? Why aren’t you doing that? What kind of a materials scientist are you?” And he was a good physicist, a Ph.D. metallurgist with a specialty in optics.

BROCK: Roberts?

SELLO: And Last.

LÉCUYER: He had a Ph.D. in physics.

SELLO: Physics, yes, but he had a lot of work in metallurgy. Roberts was a Ph.D. in metallurgy. Jay was a physicist. But Jay worked a lot with Noyce and also with me on the resist problems. They were intimately tied with the lens. We had to have spectroscope-type lenses that could expose the dots that we wanted to make. Now, that got to be a little bit tacky, because working in the same laboratory was Bill Happ. You might have come across—I think Bill Happ was also from MIT [Massachusetts Institute of Technology], but I’m not sure. I really don’t remember. Bill Happ was working on a field-effect transistor, which seemed to be his little private corner. Shockley didn’t go into that corner very often. I often wondered what was Bill up to all those times. He was hard to talk to. Nice guy outside of the place, but very serious. He was sort of left out there in the cold, at least during the period that I overlapped with him.

He was working on a field-effect device, which is one of those that Shockley also favored, had invented originally, but no one had yet made it in manufacturing. It’s a three-electrode device, so it required a third electrode to be on top of the chip, and so one of the junctions could be biased at various voltages. It would be like a PNPN switch, on which you could adjust the voltage. That would be the nature of the circuit function.

BROCK: And he was essentially doing that?

SELLO: He was sort of was sitting in a corner for the time that I noticed he was there. When I saw him, he was sitting in a corner doing his work on field-effect devices. He very seldom reported on it to the group. In fact, there was very little reporting done in public or to Shockley on anything but PNPN devices.

BROCK: Did you have regular meetings of the staff or anything like that?

SELLO: No. The only meetings that turned out to be of the staff were when the problems arose. When personnel problems arose, then there were several meetings held of the total staff with Shockley, but that was not in the direction of the research activity. In fact, that struck me personally, early in the game that, you know, oh, he's the director of the laboratory. Well, where is all this manna from heaven going to come from, of all the good things that he knows about that could be worked on?

Later in my stay there, and it was just about the time that the other guys left, he wanted me to work on a power version of a PNPN. Instead of working on ones with these little leads, put the chip on a heavy stud and then worked on a power version of it. It was a very good idea. So I started. Shockley woke me up one night about eleven o'clock at night and asked me to come down to the laboratory; he had some work he wanted to discuss with me. So I went down there, and he described what he wanted to do with a power device. I began assembling the various parts, the packages, which you could get at that time, transistor-oriented packages, and only we'd have to put a PNPN chip inside tailored to that purpose. And that meant it had to have a certain geometry, a certain diameter. It couldn't be made like the little low-voltage kinds of switches; it had to be bigger and thicker to conduct the power. I started on that and I conducted—oh, I did about two, three months' worth of work on it, and then Shockley killed the project. It just ran out of his purview. But by that time we were already in personnel problems.

BROCK: Maybe before we talk about when the personnel problems got really bad, I just wanted to ask maybe two questions. One was, were you growing your own silicon crystals at Shockley and slicing them?

SELLO: Good question. Yes, we had Dean Knapic with us, and Dean had worked out, in the time I was there, he had worked out a sort of a contracting arrangement with Shockley, where we didn't have room for the crystal pullers in that little space, so Dean went off and got a lease on some space where he put up at least two crystal growers, where he was growing these half-inch crystals, the little ones. And he would supply us with silicon according to the resistivity they wanted, basically for PNPN diodes. That was our silicon source.

BROCK: Would he deliver them as wafers or just as the crystal?

SELLO: He would deliver them as the whole crystal. We had to cut them. We had a very good shopmeister—Gene—oh, it suddenly slipped me—who had set up a rig. I'll think of his name in a minute. He had set up a rig to slice. You could get saws and you could slice hard material. You had your usual losses. Of course, these wafers had to be very thin, but if you sawed them too thick, you had a hell of a lot more lapping. [laughter] But Gene supplied us with little rigs, little experimental apparatuses. For me he built a small base to measure the heat dissipation of these power devices I was supposed to try to make, and he worked very closely with me on that. He was a very bright guy, a very good machinist, and he would have been a credit to any laboratory.

LÉCUYER: Could he be Gene Troyer?

SELLO: Yes, Troyer. Good work. Troyer. Must be because it's French-oriented, huh? [laughter] I have to be careful. And Knapic only has one *p*. Don't let anybody spell it with double *p*.

BROCK: My other question was, one of the reasons, I guess, that Knapic went and got his own space, was the laboratory was pretty small. I was wondering if you could describe a little bit more the physical plant in those days.

SELLO: Very simple. It was just a square open space. It was a former warehouse. But it was a rectangular open space about 30' wide and 50' long. The only enclosures were one for Shockley and one for his accountant and the secretary at one end of it. Then Shockley also had, in addition to his own little office space, he had built an enclosure across the width of it. There were a couple of swinging doors that you went through, that isolated the noise and the dust of the laboratory area from those office areas. All the other people sat within the workspace. We sat like little dummies in a row. There was Roberts, Last, me, and Sam Fok, all four of us in one row. Then there was a row of several other guys, Gordon Moore and a couple other guys in another row, but inside the workspace. It wasn't very big.

Then it had one whole corner occupied by Gene Troyer's shop, and another smaller corridor was entitled the conference room, in which they built a box, partitioned off a box, and you went in there to have meetings. It was used a lot during the troubled times. I could always tell when these guys were up to something, because they'd always use the conference room. They didn't want to do it offsite, because they'd never be around. I wasn't excluded from them, but I didn't attend every one.

LÉCUYER: Maybe we can now talk about your co-workers, people like Gordon Moore and Robert Noyce.

SELLO: Well, we started with Noyce. Noyce was the leader, no question about it. He knew semiconductor work because he had been at Philco, although it was germanium. So he was the semiconductor guru. Whenever somebody wanted to know something about semiconductors or transistors, you'd consult Noyce. He had been experienced in making Philco's high-frequency transistors out of germanium. He was the only one with germanium experience. None of us had that. And he was the leader. He didn't have the title of a leader; he was still a member of senior staff. We were all members of senior staff. But he was sort of the titular leader. If you had a problem on a device or a problem on where to go for an effort in this transistor business, you went to Noyce.

Gordon was the diffusion expert. Gordon built a diffusion furnace. He was a very good glassblower, not commonly appreciated. He didn't like that to be known, because everybody wanted him to make something. [laughter]

Sheldon Roberts and Last worked on—Roberts worked on metallurgical problems having to do with dislocations and internal structural problems in silicon, and working in all the analytical tools, metallurgical analytical tools, lapping problems and so forth. Roberts later on was the one who solved the famous episode of the pin, which you probably heard about.

BROCK: The push pin?

SELLO: Yes, the push pin that Shockley felt somebody had placed to try to get him, and Roberts simply took the pin out of the door, stuck it under the microscope and saw it was a thumbtack which had broken off. Shockley never believed that; he just didn't believe it.

Jay Last worked on optical problems with Noyce as well, although Noyce was a very talented device man.

Vic [Victor H.] Grinich was the electrical engineer; brilliant electrical engineer. He was the device measurement specialist, and a wizard with a "curve tracer" oscilloscope.

Bill Happ, we mentioned. I don't know where Bill came from, but he had a PhD. degree; I mean he was well trained. He was working on a field-effect device on his own.

Sam Fok. Sam was a Ph.D. chemical engineer, and Sam had come there shortly before I had. He was working on crystal-pulling problems over and above what Knapic was doing to supply. In fact, Sam was trying to build—in fact, got most of it done—a crystal puller on the premises. A very good chemical engineer.

BROCK: Were you having problems with the material from Knapic or was it just—

SELLO: No, no, I think it was just a standard problem. There were problems that had a start with Knapic. Maybe it was the starting material that he bought, something like that, but he was pretty good at it. There were very limited sources for single crystals in silicon at that time. If he could cadge, Shockley would cadge some material from BTL.

Let's see. Going down the cadre, Gene Troyer we mentioned as the machine shop head. You know the trial structures builder. What do you call them? If you want jigs, special little jigs, special little mechanical jigs for accomplishing certain jobs, fast turnaround.

Julie Blank was a mechanical engineer, a non-Ph.D., but he was in charge of acquisition of equipment.

Gene Kleiner, also a non-Ph.D., worked on facilities and space, and those two were always seen together, with all the planning that they had to do.

Who have I left out?

LÉCUYER: It was at that time that James Gibbons joined the group, right?

SELLO: Gibbons' arrival is a bit later than all of this. I think he might have come once or twice when the group was still there, but mostly he arrived as a consultant afterwards. I met him there, but it was after the group had left.

BROCK: Was Tom [C.T.] Sah there at the time?

SELLO: Oh, yes. Oh, don't forget him. C.T. Sah, brilliant, brilliant physicist. I guess his degree was in electrical engineering from Stanford. Very modest guy, but, god, he was sharp.

BROCK: What sorts of things was he working on at the time?

SELLO: C.T. Sah, he was working on recombination rates of carriers in silicon, and, in fact, he and Noyce and Shockley published a landmark paper called "Recombination in Silicon PN Junctions." Brilliant paper. He went on later at the same time I did to Fairchild. At Fairchild he worked on surface control devices, MOS. Nothing better or more fundamental has been written about MOS structures than what C.T. Sah has written. He was brilliant. He was just a kid then. He was only a year out of Stanford [University]. He used to stay up late at night. I said, "Tom, don't you sleep?"

One time he said, "Here I'll show you where," and he took me out in the back lot where he had his car parked. He slept in his car overnight. He'd work very late and he'd go out to sleep in his car and come back in the morning. He was really a dynamo. Not too communicative as a person, but excellent as a silicon structural theoretician. I can't say that he was the equal of Shockley, but I find very little difference relative to their experience allowing for that. Of course, he stayed; he did not leave with the group.

He was working on MOS structures even then, but he was mostly working on the detailed diffusion of phosphorus in silicon. He and I put two papers together on the masking effects of oxide on the diffusion of phosphorus and silicon. That was the main diffusion that was done. That guy was just sharp, and he was a bit shy. He didn't like the whole idea of this whole thing that was going on. He just stayed aloof from it. I very seldom saw him communicating to Shockley. I know Shockley had a lot of respect for him, especially when they did that paper. That paper was written just before I arrived, the recombination rates in silicon PN junctions; Sah, Noyce and Shockley. Terrific paper. He carried on with his device junction work and MOS structures, even all through the time I was there and then through the time we both got employed by Fairchild. Very shy kind of a guy, reticent, but still quite forthright. He didn't mess around. He didn't mince words.

BROCK: On technical matters?

SELLO: Technical matters, right. He did not get involved in any of the discussions and I didn't talk to him too much about it, because he didn't like it. He didn't want to be part of that. He and I had a conversation. Probably if he hadn't been there working, maybe I would have left with that group. I don't know. I've often thought about it. Maybe. It's hard to say, but I stayed. I have to say I stayed because I was only recently arrived with Shockley and I couldn't believe—I couldn't put 100 percent credibility on all of the reasons that Shockley wasn't a good research director. He was still a Nobel Prize winner; he was still as brilliant as ever.

I mentioned Gordon Moore.

BROCK: Yes.

SELLO: Who are we missing?

LÉCUYER: We're missing Jean [A.] Hoerni.

SELLO: Hoerni. Lovely guy. Jean Hoerni was a prince of a guy. He lived in a motel and he did nothing but work out diffusion curves; diffusion curves in silicon from solid sources or diffusion curves from moving fronts. He wrote the whole text for diffusion structures that we used at Shockley and that were used later at Fairchild. He worked by himself. Gordon put it a nice way one time when he was asked what Jean Hoerni does, and he said, "Well, Jean Hoerni, he sits around and thinks." [laughter] A very lovable guy, very disturbed by Shockley, by the Shockley attitudes. He was completely a part of the group that had been offended by Shockley. He's, of course, the inventor of the oxide-covered junction, as such, the precursor to Noyce's integrated circuit. So he was a very vital part of it. The only time you saw him was at lunch at Ricky's or something like that, when the group met for lunch, occasionally we'd run over and call up Jean, get him out of his motel, "Let's go have lunch," to tell him what we were doing, you know. He was definitely a part of the team.

Did we talk about Grinich? Vic Grinich?

BROCK: Yes.

SELLO: Electrical instrumentation, excellent. He was later the first head of Fairchild's fledgling Instrumentation Division that they later formed.

BROCK: Should we talk a little bit about how the personnel problems started to develop from your perspective and how some of your co-workers seemed to react?

SELLO: That's a good question. I knew there were problems right from the beginning, because Noyce and Moore, at Moore's house, took me aside and said, "We don't want to influence whether you should come here or not, but we do want to tell you that we feel there are problems, and Shockley is just impossible to get along with and he has offended so many of us so much of the time." Now, I didn't feel that, when they were describing the situation, that they were personally offended. That didn't come through, but the group was offended, and Shockley was impossible to talk to. He very seldom came to consult with them. They just worked on what they thought was important, and they were very much concerned about the fact that they should not be making the PNPN diode, they should be making field-effect transistors or diffused base transistors, because those had been demonstrated to work, and a lot of work had to be done on them. I wouldn't say bootlegged it, but they did it in connection with their other work, and Shockley didn't like that. He didn't like that at all.

As I sat there and looked at them from day to day, I did not see much interaction between them and Shockley. Occasionally, one or more of the other guys would go into Shockley's office, for a one-on-one conference, ostensibly about the work he was doing, and it was the result of one of those meetings that Jay Last was practically in tears. He had gotten such a dressing-down from Shockley, words to the effect that I overheard, "I don't know how you qualified for a Ph.D.," and that just broke him up. It really was not a very happy situation, and affected everybody. I was affected by it as well, but at the same time I didn't know what Jay did, what was it that he was doing or what he did. Had Shockley asked him to do something and then he just didn't perform or something like this? I didn't know that, and I didn't know that, as well, about Gordon or Bob Noyce. In my observations, he did not carry out the same kind of critique of those two guys. Now, he might have privately, but I didn't hear it.

BROCK: In talking to Gordon Moore, he said that he had a perfectly—

SELLO: Good relationship?

BROCK: Yes, between the two of them and he attributed it a bit to the fact of his area of expertise was—

SELLO: Different than Shockley's.

BROCK: Yes, he wasn't a physicist.

SELLO: Exactly. Exactly what I attributed mine to. Exactly; the very same. And that was also part of the reason why, in my relatively recent arrival, I decided to stay on because Shockley was decent to me. He was critical in the series of experiments I had elected to do on the power PNPN, but the experiments were the ones that he was criticizing. I had done a cross-matrix of experiments and he pointed out very clearly, he said, "Look, after your first two sets of experiments, if you had done this one and that one and that one, you'd know the answer. But you're shooting blind." But it was the critique of the matrix of experiments and it was perfectly reasonable. It was not of me as a person. Early on, we had talks about the differences between us.

BROCK: Between you and Shockley.

SELLO: Yes, that I was a chemist, physical chemist, and Shockley wasn't, and I had to explain to him what a ternary diagram was. I've mentioned some of this. That was the time he asked me only one question, he said, "Can you derive the sum of the various altitudes are 100 percent?" which I did, with my struggling. [laughter] But we had a good relationship. I wasn't there long enough to be party to the outside conferences that were being carried on at the time. Last, Roberts, they want to leave, and the other guys are trying to talk them into not leaving, all that sort of thing. I never got into those kind of conferences, although I was once asked, "We're going to be doing this and you're welcome to join us if you like." But only asked, asked only once. Ask me twice more and I'll tell you. [laughter]

BROCK: Who asked you, do you remember?

SELLO: Gordon.

BROCK: That must have been right before they were—

SELLO: Just before they left, yes. Right. They said, "We can use you. We can use you at Fairchild, at another place, so you're invited to come with us." It was nice to feel that, but it was that feeling, versus breaking away from Shockley.

BROCK: Right, someone your own age who's—

SELLO: Right. I didn't have the guts to foresee that something wasn't right. I could not point to a bad experience with Shockley, nor could I point to a bad experience with anyone who had a bad experience with Shockley, other than by hearsay. So I wasn't present when Jay was chewed out, or I wasn't present when other guys were unfairly criticized and felt they had to leave. These guys, all of us were, to a lesser or greater degree, I lesser, were kind of prima donnas in a way, and that—"prima donna" isn't the word—very sensitive to their technical status. And you had to appreciate that. I had been beaten about in Shell for six years, so I was already part of a cadre that took that kind of stuff.

BROCK: Right. You weren't alone in really deciding to keep with Shockley, because I guess Arnold [O.] Beckman made a decision, a management decision, to keep Shockley as the manager of the organization.

SELLO: Yes. That was while I was there.

BROCK: Could you describe that?

SELLO: Beckman came down one time and he gave a little talk in which he recognized that there had been difficulties. What was the term he used? "Chafing" or words like that; difficulties of the crew with Shockley. He had already talked with the group, as a group, a couple of times, and he said he felt that there was a sufficient reason to constructively criticize Shockley's attitude, but he said, "After all, he is the head of the laboratory and he's a Nobel Prize winner." He said he wants to suggest and he would carry on that they would appoint another manager, that the manager would run the laboratory and that Shockley would be responsible only for the experimentation, only for the science, but the manager would be in charge of the personnel. He later did that. It was Maurice Hanafin. Beckman's talk was taken, and rightly taken, as a decision point. He had already talked to the group by themselves without Shockley. I was not present at those discussions; those were earlier. He had already talked to them more than once, and they had described their various problems and what they wanted to do that Shockley didn't want to do, and it was mostly in connection with the experimental programs.

BROCK: This transistor versus the diode?

SELLO: Yes, right. Why only the diode? Why can't we work on the other stuff that we can get out fast and make product and sell it? They were very, very economically conscious, which Shockley wasn't, not at all, and it was very tough to take. Shockley was only conscious of wanting to impress BTL, and flood them with chips that could take the place of all their switches and to show what he could relative to silicon that they couldn't do at BTL any other way. Very important to him to do that.

So this talk with Beckman was the culmination of decision making: that Hanafin would show up, which he did, and Shockley stays on.

BROCK: Was Shockley there for that?

SELLO: No. No, he was not there for that.

BROCK: It was just Arnold Beckman and everybody else?

SELLO: Arnold Beckman and everybody else, right. And he, himself, was a very high-status individual. Arnold Beckman was well known and he was respected, they all did, we all did. That talk was the decision point.

BROCK: And then it was not long thereafter that—

SELLO: Right thereafter, yes. When they saw Hanafin arrive, they saw it was a done deal, then they had checked out.

BROCK: Do you have a question?

SELLO: Well, there's some—could be a gray area here I'm a little fuzzy on.

BROCK: It's very clear, the way that you've described it. I guess I was wondering, how did you find out that eight of your co-workers quit? How did you find out?

SELLO: Oh, they told us. I mean, they told us they were checking out. They turned in their resignations, yes, shortly thereafter, after Hanafin arrived. And they told us. They had told several of us that this is what they were doing, and that was when they also reiterated that, "Look, if you still feel you want to take off for some other reason, please talk to us." I have to say that they did not exclude me from the participation. They paid great respect to the fact that I had just recently arrived with Shockley. I don't know whether they talked with Sah. I know if they did, Sah was pretty adamant about this; he was in his own little world.

BROCK: Well, it seems like that's a pretty large fraction of the core technical group.

SELLO: Yes, I'll say it is, yes.

BROCK: It's a big group, a big loss. How did that affect you, how did it affect Shockley, and how did it affect the work?

SELLO: Well, of course, the work was affected greatly. Everybody who was left had to turn to work on making PNPN diodes. There was no room to make anything else. Those who wanted to were gone. [laughter] Hanafin had just arrived, but it took him a while to take hold and come in, I'm assuming. In my first conversations with him, he said, "Look, we're moving the PNPN diode over to the Beckman Building in Palo Alto, and there's where all the assembly is going to take place."

Oh, we didn't mention the other guys that stayed. What's his name? Did I get Sam Fok in there?

BROCK: Yes.

SELLO: The guy from Oregon. Didn't contact him, but there was Elmer Brown, who was the chief technician, and Dr. Smoot Horsley. I'm sorry. Smoot Horsley was a nonentity to me. He was Shockley's assistant and lackey.

BROCK: He was a scientist?

SELLO: He was a scientist, he was a Ph.D., and he was the one who was put in charge of the PNPN operation from the beginning and in charge of trying to get the cooperation of other people to do that. But I don't know to what he felt about the satisfaction. I got along with him fairly well, but I didn't think he was very effective.

BROCK: As a scientist?

SELLO: As a scientist, right. He was, word for word, Shockley's man. Everything that Shockley wanted to do, he did.

BROCK: So they moved the PNPN work to the Spinco [Specialized Instruments Company]—

SELLO: To Spinco Division, yes, of Beckman in Palo Alto. And then Hanafin started to come around to try to be a manager.

BROCK: Was he between the two locations then?

SELLO: Yes. Mostly he was at Spinco, because he as concerned, very much, about the economic viability of the PNPN. That was a number one concern in his book, because that had been a big problem the first time around.

BROCK: Could you talk a little bit more about Maurice Hanafin as a person and the problems that he was facing and what he did?

SELLO: Well, as far as I'm concerned, what he did was quite correct; he came around to each of us to meet and wanted to know what was I doing and how could I change it, how did I feel, did I have any specific comments—he was a good manager. He was doing exactly what Shockley should have done from the beginning, just in terms of relations with people. I explained to him what I had been doing, including that last set of experiments on a power PNPN, which Shockley then decided he didn't want to go after anyway, which was smart, because you can't make one just to turn on and off. How are you going to make one that's going to dissipate a measured amount of power? [laughter] And did I have any particular gripes, and I said, "No, I'd just like to continue to be part, more part, I should say, of what are the goals and objectives of what we have to do and where we have to go."

Now, he was not technical, so I did mention at that time that it seemed to me that there were positive aspects to what the previous guys wanted to do and that we might do well to take on some of that ourselves as competition and not necessarily only on PNPN. I said, "But in my view, that's got to be Shockley's decision more than anything else." Because I mentioned at that time that I thought manufacturing PNPNs was a marketing decision. Could you sell them? Did Bell guarantee to take delivery of these? We had no assurance.

Then he explained he was going to help Shockley to go around and get other people to replace. I said we had to replace the skills we had lost, and he said, yes, Shockley was going to do that. And in fact, some people began coming in to replace the ones who had left previously.

I didn't get to interact with Shockley anymore after Hanafin was there than I did before that. He would transmit what Shockley wants to do, but I told Hanafin that I didn't think that was very satisfactory, because I couldn't ask Hanafin technical questions that I could ask of Shockley. He said, "Well, I'll talk to the old man about that."

Then other guys came in. There was a fellow by the name of Rudy Biesele. I don't know if you've heard that name. Rudy Biesele, B-i-e-s-e-l-e. Rudy Biesele was a—I never quite figured out. I thought he might even have been a chemist, but I'm not sure. But he was a managerial type; he was sort of an assistant to Hanafin. It's like Biesele had some sort of charge of the research area, while Hanafin was going to concentrate more on the production area and to get to the marketplace. That was my feeling. But it was never quite made out at all clearly.

I talked to Biesele; easy to talk to him. I don't even know what his technical background was; more technical than Hanafin's, but nothing like Shockley's or any of the other guys that were there. But I never got to really talk to him about what Shockley wanted to do. He came around a couple of times to say could we do a little bit more to increase yield on PNPN devices? Well, you could see that, you know, he was just carrying a message, which is in his position maybe the question I would have asked. But I said, “Well, there’s not too much more that we can do that we’re not already doing. We need more hands to do it.”

See, the optical operation was wiped out because Noyce and Jay Last were gone. Some technicians were coming aboard, and then I made a suggestion to Tom Sah, which I thought was excellent, and that was to immediately start an orientation program for new technicians, because we had lost several of the technicians along with the main scientists that left. Then we started an orientation course, which Tom Sah led. It was held twice a week in the conference room on the nature of a PN junction and what’s meant by transference currents and voltages and switchover and all this sort of stuff, stuff that I wanted to hear about anyway, that I had never heard of in the first place, never taken the time to really hear in detail, and Tom was the leader of that. He was the class leader. I later added one or two pieces on some of the chemistry, some of the problems that we had.

But then a strange thing happened. One morning Shockley showed up and he walked into the conference room. We purposely used to meet at eight o’clock. I think it was twice a week, eight o’clock. He wanted to know, “What are all you guys doing here and why aren’t you working?”

And Tom was aghast. I was aghast. Shockley just showed up. And Tom said, “Well, we’re doing an orientation for technicians.”

Shockley said, “If any orientations are to be done around here, I’ll do it, and I want this cancelled immediately.” So we couldn’t do that. That’s one of the few times that I saw Shockley.

I mentioned that to Biesele, and Biesele said, “No, I didn’t hear anything about it. What all happened?” And I thought if you’re in charge on this side, what do you mean you didn’t hear anything about it? Shockley came around to do that. He said, “Well, maybe you’d better leave it alone.” So we ceased and desisted our excellent orientation course. To listen to Sah on the physics of PN junction behavior would have been delightful. It would have been actually the first chance that I had, that later came at Fairchild, the first chance that I would have had to understand the nature of a PN junction from a bandgap theory point of view, but Shockley refused to do it and so we didn’t do it.

Then other people began showing up. Gibbons began coming around, and I was never clear on what Jim had to do. I know he was conducting some kind of consultation with Shockley or maybe with Biesele, maybe even with Hanafin, because he checked us on what we were doing; checked me. He carried on some projects, but he was also occupied at Stanford. It was a consulting kind of a thing. Later I heard this, I mean even after I left, that he took on more of a role, but the first time he arrived, he was not a full-time employee on the premises of Shockley, that I understood.

Now, Tom and I and Doug Tremere—that’s the missing name up on top here. There was a technician by the name of Doug Tremere.

BROCK: Was that someone who worked closely with you?

SELLO: Yes, he worked with me. He was practically Tom's first assistant as a technician, and then in that connection he worked with me, too. But he was present prior to the group leaving. Doug Tremere, a very good technician. He was Sah's technician and mine when I needed him, and we worked pretty heavily on the oxide masking of diffusants in oxide, largely phosphorus, and we got two papers going out of that. It was kind of laughable; it was Sah, Sello, and Tremere. [laughter] Doug and I did the diffusion work.

Now, I also worked on—and I didn't mention it earlier; I should have—one of the projects that I got started was the design and building of a diffusion furnace. This was back while the original gang was still there, before they left. Shockley asked me to take that on. I have to show you something in Shockley's book. In his book – I'll show you the flyleaf – he describes, "To a man with a red-hot idea." [laughter]

I had been nagging Shockley on and off, and others, that we just couldn't live with the single diffusion furnace that Gordon Moore was using all the time, which was a platinum-resistance wound furnace which did everything. Gordon even had some funny experiences with it. The windings were platinum for resistance heating, and Gordon evaporated a bunch of platinum. [laughter] He'll probably tell you that story. And that was the only furnace we had to conduct experiments in. Even the ones that were not on course relative to other devices, such as transistor devices. We made a few transistors; they worked pretty good.

So I told Shockley, I said, "Look, we ought to have our own diffusion furnace. We can't buy them; they don't make them. Let's just design and build one." I'd had some experiences with furnaces at Shell and I thought, god, what a wonderful chance to do something that I know about, that I could add to. So I took on the design and the building of a monster diffusion furnace.

BROCK: Just size-wise?

SELLO: Size. It was a big one. There was a basic—it turned out it was a good furnace, but it was built without regard to overall size, except to just achieve this high-temperature profile that we needed while still having cool sides. You needed a profile of about 1100-degree centigrade flat through the whole length of the diffusion tube, and it was my idea that, look, if you're going to do that, you have to put enough insulation around the furnace so that you don't lose the heat and it doesn't go out the tails, so you could have a flat profile. So I designed it with plenty of thickness in order to get it built. It was a furnace that was built of Kanthal resistance windings. This was a brand-new winding that was just recently invented. They're far better than platinum.

Kanthal is an alloy of cobalt and nickel, and would last indefinitely, especially in the cooler end zones of the furnace tube.

So the furnace turned out to be maybe as wide as this table and probably as long. My design intent was that you could put your hand on the outside of the box without getting scorched because the tube temperature was at 1200 degrees inside and you wanted to put your hand on it, and the only way you could achieve that is with enough insulation. So it did come out a big one. They called it Sello's monstrosity.

Later it turned out that Gordon and company built their own furnaces at Fairchild based on that same design, except that they decided that let them operate hot. I was insisting that you should be able to put your hand on the outside, and they said, "We don't want anybody putting their hands on it. Let's just build it half the thickness and if anybody touches it, they'll get burnt and then get the hell away." [laughter] So the difference in the design was, let it dissipate the heat. It wasn't very efficient for that reason, but when I did it, it was designed in order to conserve a little bit of energy as well, so it came out to be maybe half that width. And that furnace still remained even after I left; it remained at Shockley's. But that was another project that I carried on while I was working on these other things at Shockley's as well.

So Tom and I and Doug Tremere had these projects on the diffusion of silicon, and we were planning to use that furnace for that purpose. On the oxide masking of dopants in silicon, particularly phosphorus, how much oxide does it take to really mask any given dopant like phosphorus or antimony? How much oxide do you need on the device, on the silicon, in order to block the flow of any of the other diffusants? We published two papers in that area. They were somewhat laudatory about it, our former colleagues. They wanted to know who was Sah, Sello, and Tremere, you know. Who's the tail on that one? [laughter]

BROCK: We talked just a minute ago about the economics of the diode and would Bell or Western Electric guarantee to buy so many of these things. So there's the telephone company as a customer, but I guess there was also this relationship with Bell Labs. Your laboratory was a licensee, I guess.

SELLO: Yes.

BROCK: Could you say a little bit about what connections in that tech transfer sort of area were between Shockley Semiconductor and Bell Telephone?

SELLO: Well, first of all, Bell was very good at silicon diffusion. All of the senior staff guys, myself included, made a visit to Bell, and it was really largely due to Shockley's influence because BTL did not want to license anything. But the Securities and Exchange Commission forced them to. Since they were already a monopoly on telephones, they ruled that one monopoly per company is enough, and I always thought that was a smart move. So BTL would allow licensees, but all the guys, the senior staff and myself and Tom Sah and others, had made a couple of visits to Bell to talk about what they were doing and we talked about what we were doing. So we were in exchange and we knew about diffusion problems, we knew about masking problems. They were very much interested in the results that Tom Sah and I were getting on the oxide masking, so before completion we sent them an early paper.

We maintained very good relations with BTL, but they didn't have a very high respect for the reliability of the PNPN diodes. They just couldn't stand up. They ran them under stress tests, and as many as a half of them at one test would lose. The pinch finger contacts would lose their chips. You could hear them rattling inside. It wasn't a very reliable product per se, and Shockley continued to push that end of it trying to make it reliable, but you'd have to have another kind of a packaging mode to do that. And you didn't want to change that, because that also would change the turnover voltage, which depended on the goodness of the degree of contact with both sides of the chip. If it were a soldered contact large area, it would be one kind of a turnover voltage. If it were a light "pinch" type contact, like it was doing, it's another turnover voltage. And Bell never really picked up on those products; they were just not reliable. I think that hurt Shockley a lot in his own feelings. He was in the need of something to convince—he felt he was in the need of something to convince BTL that he really knew what to do with the silicon technology.

BROCK: Who were some of the people on the Bell Lab side that stand out as people that you worked with?

SELLO: Oh gosh. Morry Tanenbaum. Then we talked with the photoresist guys. And Frosch and Derrick. Frosch and Derrick, who had done the original work on diffusion and oxide masking, they were around. Let's see. We did have one conversation with John Atalla's group, and he had a number of people in his group, but Atalla was kind of a strange guy himself, very ego-ridden. He's the type of guy that Shockley never would have invited to join him in his group.

There were a number of people that Shockley wanted to have to come from Bell to the Shockley Laboratories, but they didn't want to come. Some came and left earlier. There was one guy by the name of Vic Jones who was in the group, original group, but he was somehow offended—I don't know the details—by Shockley, and he left.

But we maintained easy access to Bell Laboratory.

BROCK: In your opinion, how would you assess the importance of Bell Labs to the development of the silicon technology in the fifties? How would you characterize that?

SELLO: That's a good one. Well, there's one thing that stands out in my mind. Bell did come up with a diffuse-based transistor, but Bell was never able to make an integrated circuit. To me, that's the big separating point. How come? Just why not? The technical strength was there. I mean, the brain power was there, but why couldn't they make an integrated circuit? That meant that they really couldn't put their strengths to work even in such things like they should have, like oxide masking, the Frosch and Derrick basic work, never really came to device fruition. They were very good at material progress, metallurgical progress, scientific understanding. They knew little to nothing, in my experience, of photoresist problems. We didn't get much from Bell in that area. They also were strong in crystal pulling. Christophe, you know who that is, that's the crystal puller.

LÉCUYER: Teal?

SELLO: Yes, Teal. Yes, he's one of the early ones. Anyway, they were strong crystal pulling, and Sam Fok did make use of some of their information when Sam was building a crystal puller at Shockley. So he went back there to BTL to look at how they were pulling their crystals, and he did learn a good bit about that.

Furnace making, nothing. [laughter] They were excellent in the theoretical aspects of silicon, but why they couldn't come up with even the kind of integrated circuit that TI [Texas Instruments] came up with in a later period, which was sort of a kluge kind of thing, that was not in their makeup. We never could quite understand that.

LÉCUYER: Vic Grinich told me once that it wasn't something they were able to push very much, but that's interesting. He told me once that there was a group at the Bell Labs developing that process – a diffused based structure – more or less at the same time as Fairchild, but the management at Bell didn't believe in it, because the yield was atrocious and they felt that they would never be able to improve the yield, and they abandoned the process.

SELLO: Yes, right in the middle of it.

LÉCUYER: Right in the middle, and that's basically, I mean, if you don't know the kind of process, you have a hard time doing—

SELLO: You have a hard time going further. That's right. That's right. Yes, that's true. They never got to the true value of oxide covering. They also never really, in my sense, understood the details of the limitations of aluminum bonding, which we spent a lot of time on later at Fairchild, not Shockley's time. They were doing almost exclusively gold bonding. At Bell that would be the limit of the most non-corrosive, the most non-reactive, the most inert metal, so they went to beam leads in order to make product, make integrated circuits. And gold doesn't stick to an oxide. Therefore, they missed that one, just as TI missed it. But they had been working on beam-leaded devices. Marty—what was his name? Damn, I knew these like I knew my own name at the time. Put down "Marty, question mark," was working on beam-leaded devices, because the gold, of course, is inert, you could bond it to silicon, but you couldn't bond it to oxide. Lepselter, Martin Lepselter. Convinced to the very end that beams were the way to go. But how do you get a gold beam to cross an oxide? You can't do that.

LÉCUYER: So there were all these technologies that they didn't quite master, right?

SELLO: That's right. That's right, and they were all in the direction of economic reduction to practice of devices. It seems that out of BTL, that didn't go. If it got to Western Electric, some of that got through. But they considered most of their projects finished when they answered the question of, could you make an integrated circuit with a beam leaded device, and you did and it was reliable. Their problem was, they were stuck with the telephone-line mentality of reliability. If it wasn't going to last for a thousand years under the sea, forget it. Just forget it. And their management just didn't want to look at anything but that.

The other thing that they didn't—so let's see. I mentioned the leads. They never really truly understood—and this didn't come out necessarily as a result of the Shockley experience, but in later years, BTL never really understood the nature of the cleanliness of an oxide surface and the intermediate surface states, the sort of thing that Bruce Deal and [Andrew S.] Grove and the guys who did it all at Fairchild. They didn't believe that—later years, we went there and we explained to them how we were able to make stable devices with an oxide surface, and our trick was that we made them—not only were they planar, but we called them planar "two", because we did extra cleanliness work on the top of the oxide as well as oxide isolation for the collector junction. They said, "You're pulling our leg. You always come here and tell us everything has got to be clean, and here you come back again and tell us it's got to be clean and you've even got an oxide over it." So it was sort of a bone of contention. They never caught that. For those reasons they never really got into integrated circuits, not at that time; later, of course. They came to get what technology Fairchild had. That, to me, I thought that was the biggest medal you could get. *They're* coming to *us* for technology.

BROCK: That was pretty amazing.

SELLO: Amazing. It made us feel very good.

LÉCUYER: Actually Bell hired engineers from Stanford, right?

SELLO: Yes.

LÉCUYER: So I mean, the technology would be coming from Gibbons, from Fairchild through Gibbons to Stanford.

SELLO: Oh, sure.

LÉCUYER: And then the technology would go from Stanford to BTL, for instance, right?

SELLO: Yes. Sure it is, and, Christophe, the channel is there, but the mental block was also there. It's not that they—they just didn't believe in reducing that to practice or cost. This would make it cost, because if you used gold leads, the cost of an ordinary transistor is immediately out of sight. You just can't sell it. So, right off the bat. And that's what defeated—I'll tell you later about that. That's what defeated the patent litigation of TI on the integrated circuit, which I like to feel I had a little bit to do with, was that gold on the oxide. They pulled stunts with the gold that you wouldn't believe. They used to come by—I guess we can save it for a later session.

BROCK: Or we can go with it right now while it's on your mind.

SELLO: Okay. Well, of course, when Fairchild was formed, then there were three immediate developments; two inventions and a development. The development was the mesa transistor. This others had done. Motorola had made a mesa transistor, so we did, too, both an NPN and PNP. Then immediately, brilliant Jean Hoerni comes up with the oxide cover junction—that's one patent—and Noyce grabs that and sees that, oh, this is easy, we'll go from this area over the oxide to another area and we'll have a differential amplifier, and if we go to several other areas we'll have a flip-flop and a register. And we made five devices in rapid succession by merely going over the oxide with aluminum. Now, aluminum is the only metal that will work in that regard. It's crazy, you know. Mother Nature sort of gave us a clue and we followed it, but Bell didn't. Bell didn't believe in it. They wanted gold leads. Everything had to be gold, because that's reliability forever. The fact that you can't make it stick to the oxide was neglected.

Well, later when the patents were granted in rapid succession, the one by Noyce before the one by Kilby, although the work was going on simultaneously, Noyce and company were just more diligent in going out there after the patent and getting it in there and pushing it, and Kilby was still mucking around with gold wires in going from one area on silicon to another area on silicon.

So when he saw the development, they leveled a suit and said, "Oh no, you connected two integrated circuits with a gold lead across the oxide. We connected the two circuits with a gold wire going from one to the other." And we quickly pointed out that that's not the same thing. The gold wires are flying wires; you can't get reliability that way. How can you attach a hundred or more devices with these flying wires?

BROCK: Flying wires mean that they have to—literally off the surface?

SELLO: They have to go right off the surface of the silicon and then they'd have to end up coils bunched somehow.

BROCK: Right, like a net on it.

SELLO: Yes, like a net, sure, although the circuit was an integrated circuit. There were two differential amplifiers. So the patent litigation quickly boiled down to the method of attaching the aluminum to the oxide. TI said, "Oh no, that's nothing. We put gold on oxide all the time." "Oh, yeah? Show us one." So that's when I got into the discussion—I was at Fairchild at the time—to defend our point of view.

BROCK: In court?

SELLO: In testimony. Yes, in deposition. The court judge was there, but not in open court. In deposition I pointed out that just what was the fact, that you can't attach your gold wires to an oxide. "Yes, we can." Well, the arbiter said, "Well, show us the evidence." So they went back three times and they tried their damnedest to get gold to stick on silicon.

BROCK: On the outside?

SELLO: Yes, on the outside. We could tell what they were doing merely by examination in the electron microscope; they were blasting the outside of the oxide with ion beams.

BROCK: Wow. Interesting.

SELLO: Yes, they were shooting an ion beam out of an electron microscope or out of an accelerator with doping the oxide with enough ions so that the gold would have a similar metal to stick to. And it came down to that. We took those, and when they handed them in, I said, "Look, let's just analyze them and see what they have." We etched them and took them apart and you could see the oxide was loaded with foreign ions that they had implanted. I launched into a lecture of Van der Waals forces versus chemical forces, and the arbiter said, "Well, Dr. Sello, can't you say that more simply? What's a Van der Waals force?"

I said, "That's a force that TI is using by which gold does not stick to the oxide." [laughter] And we could demonstrate that, because you could etch that; you know, you could see it. Three times they did that and came back, each time to prove that gold will stick to an oxide, even though it isn't lying on top.

We won the litigation. Of course, both patents were granted because they did have an integrated circuit piece of theirs, a differential amplifier, but we had the practical one. And they paid us a license. They paid us a patent fee. It wasn't very much, but they paid. As matter of fact, we were all rejoicing in the fact that Bell Telephone came and bought the license from us. [laughter] I'll tell you, there were moments like these that were just gold, pure gold. I'm sorry for the metaphor. They were just invaluable. And that's really what made it.

Now, you didn't know at the start that you couldn't stick gold down on an oxide. I guess you could blast the oxide or you could do something with it in order to have the gold stick if you insisted on using gold, but it would be a very complex device, and what reliability would it have?

BROCK: And with that treatment of the oxide, if you blasted it, would that affect it's insulating characteristics?

SELLO: Oh, sure. Sure, it would. Sure. And the fact that you could make it stick was reducing the problem to a ridiculous minority. You know, that's not the real problem. All right, you make it stick. Now, how about a device? How about connecting two things which are ten microns apart, with a gold connector, and not use beam leads? [laughter]

Well, that made a very interesting story, and I was asked to testify on behalf of Noyce. Roger [S.] Borovoy at the time was the attorney, and he has some nice stories to tell about that. They just wouldn't give up. And of course, BTL never picked up on that either, until later, until later when they started to make integrated circuits, or Western Electric started to make them, on our license, if you will, please.

You'd better cut out some of the ex—I'm sorry we jumped ahead like that.

BROCK: Oh no, not at all. It's better to follow these lines sometimes, I think, but I just want to loop you back a little bit to your life—well, your life outside Shockley Semiconductor Labs, during the time that you were there, both in terms of socializing with your colleagues and then just your life outside of work.

SELLO: Well, we didn't have much of a life outside of work relative to the colleagues. Everybody just went home. They all went home late. I was called in, as I mentioned, at one time late at night to come and work on this problem because Shockley thought it was necessary and, of course, if the old man asked, you just did it. So we socialized a little bit. I was over to Noyce's and Moore's house many times. Noyce was a music fiend, as well as other things. He was a very pleasant guy, and his wife, they were good company. They lived in Los Altos at the time. So we enjoyed socializing. Gordon and his wife [Betty Moore] came to my house.

But during the Shockley years, there was remarkably little socializing outside. As far as I can see, the only socializing that I was aware of among the group was what they were doing when they were holding these internal discussions as to where should they go and what should they do. They came within an ace of not finding the money to start. So they spent a good bit of time to try to figure out a way to go. As a matter of fact, guys like Last and Roberts, to my knowledge, were ready to leave far earlier than the group left, and so was Grinich. Far earlier. So Noyce and Moore, in an attempt to hold the integrity of the group, they spent a little longer, a little longer. So we did socialize somewhat, but not a hell of a lot, because Shockley's demands were very excessive. Very excessive. Late at night.

BROCK: Then I wanted to ask you, you decided to leave Shockley Semiconductor in 1959, is that right?

SELLO: Right. Early 1959, almost two years from the date I arrived. And that was an unhappy moment, because I didn't voluntarily leave of my own initiative. Neither did Sah.

BROCK: Can you tell us more about that?

SELLO: What happened was Hanafin, who had been coming around talking to us, came around less and less. He was busily occupied with Shockley and the assembly of the four-layer diodes. Of course, Smoot Horsley never came around. We didn't like him. Smoot wasn't a likeable guy and he was a Shockley lackey. I would say that to Smoot if he were here. He obeyed Shockley, and so did Elmer Brown, the technician, on the PNPN stuff.

But then one day Rudy Biesele came up to me and he said, "Have you considered that maybe there's a different line of work you could be doing?" And I asked him what he meant by that. I thought he meant—he used to ask, "Are you happy here? Do you think you're doing a good—can you consider this work or another line of work?" And I thought he meant working on other projects with the silicon thing, but I pressed him on the issue after he had asked that question and mysteriously disappeared, and he gave me a cryptic answer, and I never have liked it to this day, and I never got it really clarified. He said, "Have you considered that maybe you could work in this field but elsewhere?"

So I said, "Rudy, are you asking me to leave?"

"No, no, no, not at all. I'm just trying to give you a feel for the ambient around here. Shockley is having great difficulty trusting people."

I said, "Well, what's that got to do with me?"

And he said, "Well, you have connections. You still have connections with the other group, and he worries about that."

I said, "Rudy, are you worried about that or is he worried about that?"

He said, "It doesn't make any difference." He was clearly bringing down Shockley's word.

So I said, "I can't think of what you're worried about in connections. I haven't talked to that group for months, and when I do, it's quite by chance at some technical conference, or even they might call me from time to time."

And I guess he did the same thing to Sah and to Sam Fok. So the thought was already in the wind that Shockley had lost confidence in anybody who had previously contacted the other group.

BROCK: Anybody who had been around had—

SELLO: I got that impression right away, and I said, "Well, if that's the case," I said, "I clearly can't stay here. I have to make my own moves."

And he said, "Well, maybe it will be a smart thing. I'm sure you won't have any trouble finding anything."

And so I was so shook up, I came home that night and practically cried to my wife, saying, "How can he say that to me? I stayed because I wanted to work with Shockley, and he leaves me the impression that Shockley can't tolerate that anymore."

It turned out that Shockley was deathly afraid of spying, and he had reason to think that some of the technicians maybe who had left and gone with the original eight were still maintaining a liaison of what was going on, that there might have been some kind of interaction, but Shockley was paranoid in this area, very paranoid in the area of spying, and that was one reason that occurred to me at the time.

The second reason was, he wanted to hire more people from Germany and Switzerland, because he did have this crazy idea that he could lead a bunch of Germans a lot easier than he could lead a bunch of recalcitrant Americans. He obviously didn't know Germans. [laughter] I think you would agree with me on that one.

LÉCUYER: So they were Germans by the time you were—

SELLO: Goetzberger, for example.

LÉCUYER: Queisser?

SELLO: Yes, right, they came. They came while I was still there and they worked, Goetzberger and Queisser worked on the other side of the wall, the PNPN thing, and I got less to do when they got more to do. So I think that what Shockley was looking for was to increase the staff in that direction without adding more heads.

So that Biesele conversation about "Maybe you'd be happier somewhere else," was the key. I never got an explanation on it. I never even got to talking with Hanafin about it. I did mention to Hanafin that I was leaving. He said, "Oh, well, maybe it's for the better," just like that, you know, and so we went. And Sah and Sam Fok got the same treatment.

BROCK: Did the three of you speak to one another after that?

SELLO: Well, to Tom I did, and Tom said he's also leaving. So Tom and I agreed that we were going to go do the rounds of other companies. By that time word had gotten out that Shockley, even before then, was releasing the people he had before. So we got lots of phone calls.

BROCK: So where were some of the places?

SELLO: On the East Coast, to the old-line guys, to Raytheon, to Transitron [Inc.] and Sprague. So Tom and I agreed that we would attend a conference together and see the Eastern companies. Tom turned out to be a very good friend, and he asked me for a lot of advice in this area, and I said, "Well, let's go there and set up some interviews and the same guys you talk to I can talk to, or the other way around. I have nothing to hide." So we did that; we went there.

BROCK: So did you do those interviews together then?

SELLO: No. Individually. During the time that we were on the East Coast I talked to the director of research at Raytheon, the guy who wrote the big book on germanium devices. I thought I'd never forget that. He explained what they were doing, they were thinking of getting out of germanium. [laughter] But the key was, Jay Last caught me, he caught me at Durbin's Beef House in Boston, where we attended the conference. And he said, "What the fuck are you doing mucking around out here making interviews? You don't have to. We're waiting for you." And he says, "Drop all this crap." He said, "Let's go. We got a job opening for you, even."

I said, "What kind of a job?"

He said, "Don't worry about it. It'll be a high-level job. It will be like as if you had never really left." He was very insistent on it. He really did a number on me, and the way I felt at that time, it was just the right thing. He said, "Don't even bother to change your house." [laughter] He was a very outspoken guy. So he convinced me.

So I did the rounds anyway. We talked to the old major houses, and they didn't have anything going really, and planar silicon was something to them was still way out, even for them. They were still in germanium. General Electric was another one, and in General Electric you had to work on power devices, great big shots of silicon that big [demonstrates], that they used for rectifiers for locomotives. And these things had to carry current densities of 1000 amperes per square inch, this kind of thing. And I looked at that and I said, "Wow." I said, "We could make a lot of good integrated circuits out of that." [laughter] It wasn't very good to say to [W. Crawford] Dunlap. That was his name. It wasn't a very good thing to say. He said, "Well, why don't you come here and teach us how." Very receptive. When the word got out, we got requests to come for interviews before we even got there. That was a bit encouraging, because I remember I went home that night and I said I'd never been let go from anyplace before and I wasn't going to start now. But that really helped the whole situation. But it was a Shockley impulse transmitted through Biesele and Hanafin that it was a smart thing to do.

We jumped ahead there a little bit.

BROCK: Well, before we get to your move over to Fairchild, did you see Shockley again before you left?

SELLO: No. No, never. He didn't say goodbye. He didn't say anything. Never saw him. But I would have been surprised if I had.

BROCK: It would have been uncharacteristic for him?

SELLO: Oh yes. Yes. He never even congratulated us on those papers that we had published, Tom and I. No, because he felt that was part of the old regime, that we might have stayed there, but he was so prejudiced.

BROCK: Because he associated you with the other people?

SELLO: Right, and he was sure that there were terrible leakages of technology from Shockley. I don't know quite where. Where the hell was the technology coming from? How to pinch a diode together? [laughter] And the diffusion work that Sah and I and Tremere were doing, we published right away, so there was no secret there. The old man really was paranoid. I never talked to him, and I missed that. I thought a handshake and something nice to say about good luck would have been the thing.

BROCK: It would have been.

SELLO: Shockley could have owned the world at that time.

BROCK: I think maybe before we get into the Fairchild story, a couple questions just about your impressions, looking forward in time, how that four-layer diode story played out after 1959.

SELLO: It was dropped like a hot potato. They never used it; too unreliable. Too unreliable and also too diverse. They could never produce a constant batch of breakover currents. The worst thing you could go in to Bell with and the beam-lead-oriented guys is with a pinch contact like that. I mean, I'd be ashamed to show that to them. I'd figure out some way of soldering the leads on the thing without doing that, but not that pinch contact.

He was so pushing to get that thing going and to show BTL that—and they bought a lot from him. They took a lot from him. They put it on life test and all. I never saw the results. We could have told them what the results would be. You cycle that through a normal temperature cycle, and after about four or five seconds the chip falls out. You know, this is just metal contracting against silicon and expanding. You didn't have to be a seer to predict that.

BROCK: And if you could similarly reflect on maybe how the whole Shockley Semiconductor Laboratory, as an organization, how that played out after you left.

SELLO: Well, of course, I did not see anywhere where they ever made any other devices other than PNPN, even when they moved the laboratory under the direction—moved it out from Beckman over into Clevite. They never produced any marketable devices.

BROCK: And then Clevite, they sold it to ITT?

SELLO: Yes, and then Shockley was dismissed. He was asked to resign. Actually, what happened was, from the stories I hear, was that the chairman of the department at Stanford asked him to take a chair before they asked him to resign, on purpose, so he would never suffer of the inglorious fate of being fired. He had one hell of a reputation. It's amazing.

No, they never produced anything, and as far as I know they didn't publish much. I never saw a device paper come out on any particular topic. Jim Gibbons, of course, you've talked—have you talked to him already?

BROCK: No.

SELLO: You probably will. Gibbons went ahead to do some work at Stanford, which related to silicon semiconductors. He did a lot of ion implantation, equipment work, and he was good. He also came and he was a consultant to my department at Fairchild after I had left and gone from Shockley. Great guy.

BROCK: Another question is, by the time Jay Last is asking you, "Come on, just come with us," would you have identified as being a physical chemist or would you say you had become a silicon scientist by that time?

SELLO: I was a silicon semiconductor chemist.

BROCK: Semiconductor chemist.

SELLO: Yes.

BROCK: And can you talk about the differences of being a—

SELLO: Now, you want me to give my paper now? [laughter]

BROCK: Well, I'll wait, then.

SELLO: No, no, you can. That's all right.

BROCK: Or just prefigure it. I mean, I think it's an important—

SELLO: No problem. No problem.

BROCK: It was kind of a new discipline.

SELLO: Yes. I was no different in many respects in that regard to Gordon Moore himself. He was a physical chemist, but he was an infrared spectroscopist. He did his work in that area. I had done my work in the orientation of organic compounds in physical chemistry. I still felt I was weak in device design technology, production technology. I didn't feel it was a—not at all an insurmountable problem by any means, but that part which I thought we would get at Shockley we never got, and that part is exactly what the Fairchilders went out to do, is to get devices that they could market. Very single-minded and that's what made them succeed, one of the factors that really made them succeed, very single-minded on getting reliable silicon to market. But at that time it wasn't planar or anything like that; it was just reliable silicon devices to market.

But I had already gotten some knowledge about bandgap semiconductors. That had come out of the work at Shockley, so I feel I learned that from Shockley, the bandgap theory of solids of semiconductors, and especially of surface states. Now, I didn't learn much of that directly from Shockley on surface states because that wasn't even his *métier* when he won the Nobel Prize. It was [John] Bardeen who came up with the surface-state picture that answered those questions, but there was nothing Shockley didn't know about it. I mean, he sure could handle it.

So I had learned silicon technology. I'd learned production technology, the need for production technology, although we didn't have much with that, and some little about design of production equipment, and then kind of a broad category which I'll just call Materials and Processes in silicon.

BROCK: So, for example, knowledge of things like—well, I guess maybe not at that point, but which materials will bond together, issues like that.

SELLO: Yes. Yes, that's right. The chemical aspects of metallurgy. The phase diagrams I learned, but metallurgists know those things. But the material's interaction with silicon and the production thereof. And in fact, the job I was thrown into, even before I filled out an application, was just that, was the pilot line production—we called it preproduction at the time—at Fairchild of silicon transistors. Right smack in the middle of where we should have been two years earlier. Thank goodness that Noyce and Moore had already had that banner that they were going after, so I could fall right in with it, and I took on the job of head of the preproduction group. So in level of status, I didn't lose anything for the two years I'd spent at Shockley. What I lost was the chance to get 30,000 shares of stock out of Fairchild Camera and Instrument [Corporation], which those guys got. But as far as the technology was concerned—

BROCK: Did they have products at that time?

SELLO: Yes. Yes. In 1957 they announced a mesa diffused-base transistor, the NPN, and rapidly following that at the end of 1957, early 1958, they came up with the PNP. It was a bitch. The PNP was tough for reasons that now we understand, but then we couldn't fathom. We'll probably get into some of that. They came up with the two, not planar but mesa devices, and the mesa was needed in order to isolate the emitter junction. And it came up pretty fast. It was the 2N657, I can't remember all those numbers, and it became a real winner on the marketplace. They also came up with silicon PN diodes, made in diode cases where two studs contacted the chip—this is the way Elmer and those Shockley PNPN guys should have made the damn thing. You take a glass envelope and you take a diode and put two copper studs, gold-plated copper studs, up against the diode and it fits into this little glass envelope, which is only about an eighth of an inch in diameter, and it's locked solid in there with a glass-to-metal seal. You can shake that thing all day long and it nothing would happen. That's the way they should have made the PNPN diode.

BROCK: Who came up with that?

SELLO: Oh, diodes were being made all over the place. Hughes, I think is the one who takes the credit for it; Hughes Semiconductor.

BROCK: For that sort of packaging?

SELLO: Yes, for the typical diode package. There were a lot of those kinds of packages around. By that time that was already in the trade. They were even being used on germanium.

BROCK: So was it a new line that you were thrown into the—

SELLO: It was a new production line, yes. It was an entirely new production line that I had not experienced before, that I could quickly see, had no trouble understanding, and quickly see where the difficulties were, but we had those two lines going in preproduction to a level where we could then feel confident transferring them to production itself. So we were a pilot group.

BROCK: Were you developing new processes or what?

SELLO: Yes. Yes, you had to also. That's the process development part of it. One of the process development parts of it was we had to develop the mask making that went for it, and that's a whole other area that we haven't touched upon. But that's a separate and independent technology where the simplest version of it was where you had to have matched lenses with matched focal lengths so you could imprint into resist a series of successive patterns, each one of which would lead to a diffusion step. The first such camera Noyce built at Fairchild. He went all over the marketplace and found aerial mapping cameras, lenses with matched focal lengths. We used those for the first mask-making kind of material. At Fairchild they started on that right after they built their furnaces. There were a number of businesses which were started as a consequence of what they did at Fairchild.

BROCK: Of equipment?

SELLO: Yes, the equipment.

BROCK: Well, so—

SELLO: So I get invited and I come as the head of preproduction, and I then fill out an application for employment. [laughter]

LÉCUYER: That's a good way of doing it.

SELLO: Yes. And Sah was right behind me.

LÉCUYER: And then Fok followed that?

SELLO: Fok followed, right, about the same time, only he didn't make the tour. He didn't go to the East Coast. He also ended up at Fairchild, doing crystal pulling, by the way, and mask making.

BROCK: So you were in the research and development laboratory?

SELLO: Yes, I reported to Gordon Moore, who was the head of R&D [research and development]. Let's see. How did it go? Bob Noyce was the first head of R&D, and a fellow by the name of Baldwin was the plant manager. Baldwin escaped to form Rheem, taking with him the specifications for making transistors, the specifications that were supposed to be mine, because later we found those in his office at Rheem. We found them, and he had to turn them back and pay a fee for stealing them. I got his office and we got those specifications back a little later. So I became the preproduction head. Since Baldwin left, Noyce moved up a notch, he became plant manager or operations manager. Moore became R&D director. He kept the preproduction under him as R&D director, so I continued to report to Moore, which was a breeze and a blessing.

BROCK: Could you talk about that in the early years while you were at Fairchild?

SELLO: Oh yes. Absolutely. Oh yes, about the—

BROCK: About that relationship.

SELLO: We talked a lot about that, and he mentioned a number of times to me, he said, "I wish we could have worked harder on you to get you to originally come with us." He said, "But each one of us felt we had our own problems to solve relative to that, but we're sure glad we got a hold of you now. And now you've got Baldwin's former job and you haven't lost any ground." We talked about that. We didn't talk a lot about Shockley, because by then a lot of the word was out, not too much.

BROCK: How was he as a director of the laboratory?

SELLO: I think Gordon was quite efficient in his style of asking you what you're doing and trying to get some answers to the questions that you're raising that you feel you can't solve. He never came in with a project to do something unless it was already defined by the product committee or something like that. Now, like we're going to make PNP mesas; that was defined by the marketplace. But he was a prince to work with. Never met a guy so easy to work with as Gordon.

One thing I'll always remember about him, it came out in later years, when he said, just out of left field he said, "You know, Sello, you ever realize what it is I like about you?" Holy cow, I thought, where is this coming from?

I said, "No. What is it?" I thought he was going to tell me a joke.

He says, "It's because you never flap." And just one statement like that, and I walked away quite flattered from that. I thought, "Wow. My wife thinks I'm volatile as all hell, but I never flap." I thought that was quite a compliment. So he always threw the tough jobs at me.

Another thing Gordon was not too desirous of doing is letting people go when they didn't perform a function. If it was in my area, I got the duty. In later years that became a very difficult job, because I had to release Sam Fok from the work he was doing. We didn't need that work anymore. We had started under Noyce to build a step-and-repeat camera. There weren't any to buy, so we started to build one. We got the lenses and all that, and Sam led that work. It was a very large thing. It was built on highly polished granite blocks so that it was like a measuring table so that it wouldn't vibrate. It was accurate to one micron in focus across a width of about two feet, and we started to build and use that. It worked in a step and repeat mode. You couldn't buy one. Later it turned out, a couple of years later, that David Mann came up with a camera that you could buy. Well, Sam had the job of developing that granite monster. David Mann still built, and still does build, step-and-repeat cameras.

Now, I jumped ahead of the story a little bit because this occurred—when I first came, Sam was reporting to Bob Noyce, because step-and-repeat are in that area. When Bob vacated the R&D job and Gordon took it over so Sam continued to report to Gordon, and so it continued.

Not too long thereafter, on September 1, 1962, I was shipped off to Italy as Operations Manager for SGS [Societa Generale Semiconduttore]. That's a whole other phase. Gordon called me back from Italy after two years there to come and help him build the R&D division as a separate division. It was only a little group inside of the operating group in early Fairchild. The R was the part guys like Tom Sah, working on MOS transistors and device physics, for example. He did some brilliant work in that area, which he later felt he had to follow and went on to the Universities of Illinois and Florida to do that.

So Gordon headed the R&D group with Preproduction in it, and that's where I remained for, going on two years, enjoying it tremendously, to build up the production group from this group; that is, these processes that we have developed in the Preproduction group were supposed to be like a pilot plant. The process in Preproduction were supposed to then be transferred to production, and so we did that during those two years.

BROCK: Were you in one facility altogether?

SELLO: No, for a while we were in one facility on Charleston Road in Palo Alto. Then after, I guess it was a year and a half or so, we moved to Whisman Road in Mountain View, where we built the first manufacturing facility.

Now, we did one thing more. At that time it wasn't economic to have one group develop a product to a certain yield level and then transfer that to a production group. Why not combine preproduction *with* production, and thus avoid the physical transfer? But the R&D was now coming apart from that. So preproduction was then shifted to come under manufacturing. That's when Charlie [Charles E.] Sporck came aboard. He became Operations Manager, and the preproduction group was transferred under him. At the same time I had already been asked to go to Italy where I became Operations Manager of SGS.

LÉCUYER: So everything happened at the same time.

SELLO: At the same time, all at the same time. So it was an ideal time for us to institute preproduction of silicon devices at Italy and then manufacturing using the same guy (me) that had introduced the preproduction at Fairchild. That was one of the strengths of both Moore and Noyce. They were quick to move organizations to meet the talents of the people, not the other way around. I thought that was—to me, that was the only way to fly. Just the opposite with Shockley; couldn't seem to accomplish. He would squeeze the talents of somebody into something he didn't want to do or didn't fit.

So the first two years preproduction; preproduction then goes into production. Sello goes to Italy. But during those two years that I was doing the preproduction, I had some very interesting interactions with then the manufacturing group under Charlie Sporck. We had transferred to them, and Charlie wasn't happy with the way our group was performing, so he said, "Your teachers are not doing a good job."

And I said, "Charlie, lay off. I'll show you that they can," and we moved their foreman and their technicians into our preproduction group and we taught them how to do the job, and then we moved them back into manufacturing.

BROCK: Could you talk a little bit about him?

SELLO: About Charlie?

BROCK: Yes.

SELLO: Charlie came onboard at Fairchild about the same time I did. There was a guy there before Charlie named Frank Grady, who didn't stay very long. Charlie came from General Electric and he was a well-known builder of capacitors. He knew capacitors and he knew mass production and he knew low-cost production. He was good. I used to walk with him personally down the line of girls working on the devices and he'd ask, "Are they doing it right? What should they be doing that's different? How did you do it? We're having trouble here. How did you do it in Preproduction, and why aren't they doing it here?" There were a lot of places like that. He was totally interactive and very easy to work for. He was tough, extremely tough. He did not tolerate lateness; he did not tolerate sloppiness; anything to do with maintaining getting the devices out. He would walk down the production line and you could hear the whisper, "Here comes Charlie," and everybody would speed up. [laughter] Foremen especially, "Here comes Charlie." So I got the habit of calling them and saying, "Charlie's going to be looking at the group tomorrow. You'd better be there." [laughter] It was all in fun.

He was very much into pure manufacturing techniques. It was under his regime that manufacturing introduced the "Work Factor." Work factor is a precursor to time and work analysis. You analyze the motion, and if a girl's bonding, she puts the transistor in, she puts the wire there, she then steps on the peddle and she then lowers the boom, and it's bonded and she goes to the next one. Work factor comes along and says, we will look at all the motions she's doing to do that. They bring the Industrial Engineer on the line, who will take the motions apart and say, "Well, you know, she should really be doing with her right hand what she's now doing with her left hand, because she's crossing over all the time. It's just not an efficient way to do that." Charlie was skilled at that work factor, and the girls hated it. [laughter] But production techniques had to be low cost. If it cost a penny less, Charlie went for it.

But I got along with him very well because it was a physical chemist reacting with a manufacturing engineer. I said, "Charlie, let up. It (the bond) isn't going to stick if you do that. Do it a little slower or clean the wire more."

He said, "Well, you should have done that in preproduction."

I said, "We should have done a lot of things in preproduction, but we're doing it here."

He was easy to work with, but he was tough. He was a tough man with the cost of production. He lowered the cost to where our transistors were probably lower cost than most all you could buy on the marketplace.

LÉCUYER: He also reorganized production on the product lines?

SELLO: Yes. The product lines, yes, in the mesa transistors. Good question, Christophe. In the mesa transistor we had two types, we had an NPN and PNP, so we organized a NPN line and PNP line. The real difference between the two, we came to recognize later, was not in bonding the wires, but in the step of isolating or etching the mesa. You had to take a silicon wafer with all these transistors on it and stick it down with black wax, in production, to a surface, and then you would dip-etch it so that all the devices which were not covered with the black wax would etch a little mesa in the top of the transistor. Then it would get rinsed, washed, etc., etc., and go down the line. What we didn't realize at the time was that—and we had one line for PNPs and one for NPNs. NPNs required a different test program also. As you tested, you would be testing more often an NP diode instead of a PN diode. But what we also didn't realize is that even making the mesa, etching the mesa, turned out later to be a striking difference between the two. We never really realized why we had to do both, why we had to do them on different lines, but we knew it worked best that way. So there was one line for NPNs and one line for PNPs.

By the way, diodes were also manufactured by Fairchild, but by that time they had been moved to San Rafael to a diode plant, because they were totally different, and different guys were bought in to run it. They were totally mechanized diodes. Hughes had been making for years, and they were these pinpoint diodes where you attach two hairlike leads to a diode in a little glass sleeve. So that was a whole different operation. It came under production, but it was done in a different plant located at San Rafael.

LÉCUYER: We didn't talk much about preproduction of integrated circuits—you must have worked on that—and moving the stuff—

SELLO: Okay. The way it worked was, when I moved to Italy about a month later, I started on the preproduction—it was now in the manufacturing. But I started on the preproduction of integrated circuits, the simple integrated circuits, the flip-flop, five pieces, on the same production line as PNPs and NPNs, because the etching was really only the difference, the black wax hold-down and the etching of the devices. The testing later, of course, was different because these were five transistor devices, flip-flops, but it resembled very much a PNP in its behavior. Now, that went on while I was in Italy, during 1961, 62, and part of 63.

When I came back from Italy, the mesa version of the transistor had practically disappeared because they had brought in the planar version and wiped out all of that crazy black wax etching, just wiped it out completely. And there are some stories to tell that we shouldn't miss talking about.

In the course of developing the PNP and NPN, which took place in Fairchild Palo Alto, and then moved to Mountain View, during that time we ran into peculiar problems because the PNP and NPN transistors, while they were both mesa transistors, just didn't behave the same. The yields were much lower on PNPs than they were on NPNs. We didn't know the reason then; just didn't know. Didn't find out until much later, until the time when we all even gave up doing the mesa etching. And it had to do with the fact that the surface states on a PNP device, PN junction looking at the outside world have different surface states than NP junction looking at the outside world. So we had to do a different kind of preparation.

But during that period, and we made a lot of money on mesa transistors, and we got into some of the Air Force computers, we then brought in the planar. Now, why did we bring in the planar? It was like we must have saved the company. Jean Hoerni has got to get credit for saving the company in that regard. The mesa transistor was already in the line, both of them, PNP and NPN, but they had a basic problem that was baffling the hell out of us. When you assembled the mesa transistor, you put the chip on top of the can and you sealed the can down, we had what we called a "tapping" problem. You probably heard about that.

BROCK: The tapping test?

SELLO: The tapping test. And you should have seen the force of ten Ph.D.'s, each with a pencil in his hand, tapping transistors to throw out all of the ones that would fail on tapping, and it was causing a terrible loss of yield and the loss of yield more on PNPs than on NPNs, although it was so bad that we didn't recognize the difference. Noyce came to me and he said, "God, make like a chemist. Why can't you protect it somehow? What can you do to protect it?"

I said, "Bob, the only thing we can do is this short circuiting due to tapping is coming from flying objects inside of the can, possibly during the welding process, possibly before that." I said, "The only thing we should do is we should protect the chip, so let's coat it with a thin layer of silicone."

He said, "Well, does that work?"

I said, "Well, GE uses it on their power transistors. They can't make a good power transistor unless they have it coated with silicone."

He said, "You mean we have these reliable transistors, once they're made well, and then we have to fill a can with a gunk like silicone?"

I said, "You want to stop the tapping or you don't want to stop the tapping?" [laughter]

So we coated a few as tests, thin film of silicone. It worked. It worked, but it meant we had to take the lots off the line, we had to paint them in a special station, cover them all with a little thin film of silicone, only those, split runs so we got the right ones, and then we had to reassemble them. It was a pain in the buttski.

And while we're doing this, Hoerni walks in one day, he and Noyce walked in, and he handed a chip to Gordon and says, "Gordon, what do you think of this?"

He says, "Oh, that's the new planar."

"Oh yeah? How good is it?"

"Well, spit on it."

And Jean Hoerni spit on the transistor and it didn't bother it one bit. And I'll always remember that particular event—there was the answer to the whole damn mesa problem. You just don't try to fix the mesa and coat it with silicone or anything. Get rid of it and bring in the planar transistor. That was during the next two years, 1957 to 1959, that we put the planar on the production line.

LÉCUYER: And then you were involved in transitioning that from Hoerni's lab in the production line?

SELLO: Yes. Hoerni did it on a few devices. So by that time the preproduction line was already a part of production. So we took the PNP part. We took part of the PNP mesa device. That was the most sensitive to this crazy-flying-objects problem inside the can, and we converted that to planars.

LÉCUYER: So the first transistor to be planarized was the—

SELLO: NPN. 2N 1613. It was identical to the way Jean made it, only nobody spit on it. [laughter]

[END OF AUDIO, FILE 1]

SELLO: Sure. Where the NPN is being introduced. That, of course, immediately tested its worth. So we had to consider making an NPN planar as well as a PNP planar. You have to have complementary parts. It wasn't as easy as falling off a log, because we still didn't understand a lot about surface states, about what was going on underneath the oxide. If you wanted a really high-voltage transistor which should have no leakage pass of any kind, zero, which you'd never achieve with a mesa, even with a planar it could do better, but it was also difficult. The planar was also slower. You see, the size of the mesa itself, tiny pinpoint compared to a flat structure of planar, it's longer path for a conductor, so it was slower. But there was no question that it was the answer to a maiden's prayer. My gosh, we couldn't convert fast enough. Of course, we, for a while, had to run all three devices. So we had to run all three, PNP, NPN, and planar, but they were developed two years apart, actually.

Yes. We found two things, that even in the mesa transistor that you could increase the lifetime and reduce the leakage by nickel impregnation to kill the centers which caused leakage inside the bulk of the transistor. So we learned to nickel-plate the back of the wafers and then heat-treat them. Then later by the structure itself, a gold dope transistor works on different trapping sites in the bulk and so it will increase the speed, and that worked nicely on the NPN, on the planar, which needed all the speed they could get.

But I must admit that the entry of the planar transistor was a revolution in the business. People came from all over, just all over, including Bell, which they never used. They could have because they were licensed, *our* licensees in the process.

But it was very hectic during that period of trying to make both PNP and NPN mesa with the tapping problem. I thought we'd never get out of that.

BROCK: How large was the preproduction group in this 1959 to 1961—

SELLO: I would say fifteen engineers and maybe ten technicians, and thirty to forty assemblers was the preproduction group. But the life of it was not too long. It was clearly an extra transfer, to transfer research to preproduction and the preproduction into production. So we did it the easy way; we just took the technicians and the guys off the line of preproduction, kept the line in the same place, and moved the foremen in from manufacturing. So they just ran it as the manufacturing line.

It wasn't as easy as I make it, because we had fits and starts. There was something that we hadn't really learned yet, even on an oxide transistor that we had to learn in later production, and that was that the surface states under the oxide are not the same on the PNP as they are on an NPN. That's always what made the PNP both slower and lower production. We didn't quite fully appreciate that until the planar came along. It brought the PNP and NPN a little closer together, but still we had to concern ourselves with the cleanliness to avoid the surface states underneath.

BROCK: When did you first hear, in this period, about the integrated circuit work anywhere, at Texas Instruments, what Noyce was doing?

SELLO: The first time we came up with the structure for integrated circuit that anybody knew about was when Noyce and Hoerni made a flip-flop using the planar technique.

LÉCUYER: So you mean both of them, right?

SELLO: Yes.

LÉCUYER: Wow. Interesting.

SELLO: Yes. Jean had held the patent, holds the patent on the aluminum on the oxide covering the junction, which is the big step. Noyce patented the connection of two oxided surfaces with aluminum, one to the other, and they came within—I think even in the patent they came within months of fruition. And it was then that we learned that, as Noyce had pointed out, he said, "We'll work with the NPN transistors because we have to plug them into the same sockets where the PNPs were, and we want people to keep buying our devices. But they're not going to last long." He said, "These are going to take the place of everything." That is, the multiple chip integrated circuits will take the place. Now, it actually didn't do that, which—

LÉCUYER: It took a while, right?

SELLO: Yes. You could do a differential amplifier where the two junctions are really close together, and if you hook the leads up as a differential amplifier, it could be an integrated circuit. But if you just didn't hook up through the leads and stuck them into the can so that they went out to the outside, that would be a transistor. But it was not practical. It was not economically feasible.

So I went to Italy and I introduced there the mesa devices. In 1959 to 1961, that was my job to do that. 1959 to 1961, that was the—and the guy responsible for that is good old Robert Noyce.

BROCK: Yes, can you tell us how that whole Italy story came—

SELLO: I wish I could figure out just exactly how it started. It was a brainstorm of Noyce's. The basic reason was, we were not big enough to market in Europe any of our products on our own. Everybody by this time was making mesa transistors, so we would have to compete with Motorola and Texas Instruments, and we just couldn't do that.

So the driving force was the penetration of the marketplace. Now, to do that with a minimum of money, we needed a partner and we chose a double-headed partner, unfortunately. Not too many people realize this. The other two heads were Olivetti and Telettra. They were telecom instrumentation and computer houses. Telettra was telecom instrumentation. You probably know better than I do, even, Christophe. And the other one, Olivetti, was computer. They wanted to get into the planar business, so we said, "Okay. That's coming along, but right now let's get into the marketplace because we're having requests, tons of requests for our mesa transistors still coming from Europe that we can't fill. So you'll start with that and then we'll switch you over to the planar and then the integrated circuit."

For me, as Operations Manager, that was to get them going in PNP and NPN transistors. That was the driving force. They provided the money and facilities. We worked out a deal where Fairchild became a 40 percent owner, whereas the other two then split, were 30 percent each. I was elected to go and do the job by myself; one person. [laughter]

BROCK: To transfer all of that.

SELLO: To transfer the process.

LÉCUYER: So you were the only Fairchilder?

SELLO: The only Fairchilder.

LÉCUYER: Wow. That's something.

SELLO: Yes, really. Noyce was quite a salesman. He showed up one day at my house. Maybe I told you that story. He showed up in there and he said, "What are you doing?" I was busy grinding the floor to put in new squares of linoleum, and the place was filled with dust. My wife hollers out, "There's somebody here to see you." It was Noyce. He said, "We all want you to go to Italy." Right now, bang.

"Wait. Wait." I said, "I can't go."

He said, "Why not?"

"Because I got to finish my floors."

He said, "Forget 'em. I'll take care of that. Go to Italy." [laughter]

Then I talked with him afterwards. He had just completed the agreement with Telettra and Olivetti. Actually, it was a third, a third, a third, and then during my tenure we worked it up to 40 percent, because we had the silicon technology and product. We negotiated a little better number.

So I was alone. Of course, I made use of one or two engineers temporarily from the PNP, NPN lines to come out there to help do the teaching on the line. There was one more American assigned later, Don Rogers, a sales guy who was to take care of the marketing end. I was the Operations Manager, and I couldn't go out and call on customers and be the operations manager at the same time.

BROCK: Why do you think Bob Noyce asked you to go? Did he tell you why?

SELLO: That's a damn good question. He says, "As I look around at the guys I have, you're the only one I think can understand foreigners."

I said, "Well, why do you think that?"

He said, "Well, I see you bring them around. They come and they visit, you talk with them all the time, and you know the processes. Why shouldn't you be able to educate a few Italians?" Little did I know. [laughter]

I think it was a combination of background and the state of the development at Fairchild at the time. We could do it with a lot less people, using me, than they could using a couple of guys for PNP, a couple of guys for NPN, and another one for diodes and all that. So it was a really fly-by-night choice, in a manner of speaking.

I was introduced to Roberto Olivetti. He came over to interview me, and so did Virgilio Floriani, the head of Telettra, and they were convinced I could do it. They had turned down several guys, one of them Dietrich Jenny from RCA [Radio Corporation of America] of Somerville, N.J. He was big in germanium transistors for television and later in 3-5 compounds. He had been the plant manager before me at SGS, and he failed miserably. They wanted him out. But now they also wanted the new silicon products. See, they wanted the planar right away and we didn't want to do that. We started with the mesa silicon devices.

LÉCUYER: So does it mean that they had already a fab?

SELLO: They had an existing, going alloy germanium transistor fab. I had never seen a piece of germanium, and to this day I wish I never did. That stuff is miserable, the way the devices have to be processed. Ge has all exposed surface states because there's no stable oxide. Germanium oxide is soluble in water, so you've got to process the devices through a series of cascade washers which looked like baby Niagara Falls baths. You've got to wash and rewash the devices and etch and re-etch, then dry them. It's the way it was done in the process SGS originally got from General Electric. The devices don't have much reliability, because you haven't washed or dried them properly. The Italians were smart; they wanted to get in on the planar silicon devices.

BROCK: And where was this?

SELLO: It's just outside of Milan. Agrate. I have to admit that I had fun for the two years. It was a pain in the buttski in many ways, and that's a whole other story. I was *really* glad to come back. Christophe, you'll know what I mean when I say I never succeeded in teaching the Italians teamwork on a production line. I saw the problem, I could recognize it. If I set a small 4-man team working on a problem, the real problem is not solving that technical problem, it's who gets the credit. So if you appoint someone to lead the team, it's understood he immediately will get the credit. That means the other three guys that are working on it are not going to get anything, and they know that, so they don't want to work on the team. I'm not exaggerating too much. But it's a difficult problem to get that teamwork going, and since I was the expert, I had to look at the problem, and they'd say, "Well, let's call Dr. Sello. He knows all of these answers. Why are we struggling? We're wasting time."

So I'd come in there, and they didn't know the answer to the PNP problem there anymore than they would have known on the line at Fairchild, and I said, "Well, a team of three people will attack the problem."

"Well, what do we have to do?"

I said, "Well, look. Whenever you attack a problem, you have to go analyze it first. Where is the problem and what is it doing? Now, you haven't done that." I said, "I know the answer, but I'm not going to tell you. You go find it, and then when you find it, I'll tell you." And it took longer, but it was the only way we could get the job done.

LÉCUYER: So an Italian team would be different from an American one.

SELLO: Yes, totally different. The ones that could do the work that didn't have too much difficulty in silicon were the test engineers. They used the same kind of testers, the same boards, the same kind of things, so we had no problem with that. But the process engineers who had been working with these successive cascade washers, you know, the baths which looked like a series of Niagara Falls, had difficulty. Transistors start out at this line, they're etched, they're washed, they're etched again, and the third step, washed again. There are three washings and three etchings in germanium device processing, because germanium accumulates surface states as you go. I had never seen a piece of germanium. I didn't know how the detailed process worked—I said, "Show me some germanium."

"Oh, we'll be glad to. We'll tell you all about it."

"No, don't tell me all about it. Just show me." [laughter]

BROCK: Well, maybe we better pause here.

[END OF AUDIO, FILE 2]

BROCK: Okay. We're back on.

SELLO: Only later a marketing man came out, Don Rogers, to handle the customers. Rogers was in many respects a pain in the buttski, because he was a typical American who if he learned how to say "a cup of coffee," that was enough for him, and he was there for two years. That's what he learned how to say. He just sold. "I just came here to sell devices, not to speak this funny language," he says. You've seen guys like that, I'm sure.

His wife, on the other hand, and their two kids were of Italian extraction and all three, especially the kids, learned Italian within a month, and they were so fluent, it was unbelievable. They were a big help. They were particularly useful when we went on trips and Don Rogers was the driver. So if we went down to Florence, for example, on the Auto Strada and he's the driver, why, he knew how to get there. Of course, Don thought he knew everything about Italy within a month after he was there, even though he didn't care for the language or the "funny money" that they had. They had those "big bedsheets" at that time, 10,000 lira notes.

He ran a stoplight on the freeway and a policeman came up alongside, and he stopped the car and he said in Italian to Don, "You ran the light."

Don says, "Don't understand. *Non capisco*. *Non capisco*." And his little daughter was sitting in the backseat and she said, "Hey, Dad, he said you ran the stoplight." [laughter] It was so funny that the policeman laughed. And he said, "Is that what he said? Well, I won't do it again. Tell him I won't do it again." And the policeman laughed. That's the way the kids were, very outspoken.

BROCK: Was the SGS effort really the first effort to break into a foreign market, like the European market, by establishing some sort of organization in Europe?

SELLO: In that way, yes. It was not the first effort to sell product in Europe, because we received many orders for product that came from Europe as a result of European contacts at trade fairs and things like that, but this was our first organized presence in Europe.

BROCK: Did you have any salespeople who were responsible for Europe before that?

SELLO: Not directly. We had guys on the East Coast who would jump over into France and Italy and England from New York, and they would sell product there. We also had a lot of inquiries from Europe that came as a consequence of trade shows, and they were handled through the office on the East Coast. So there was enough activity to warrant an investment to satisfy company needs, customer needs directly. Unfortunately, there wasn't a lot of money to set this up, and also that if we set it up we should have had some kind of marketing organization to work with. And SGS, Societa Generale Semiconduttore, had two other partners, each of whom had good marketing organizations in Europe. So they took on the marketing in Europe immediately of our product. A presence in Europe greatly aided the marketing effort of Fairchild silicon products.

So whenever orders came to the United States to be filled, it generally was siphoned through the European organization. So it would end up down where we were, down in Agrate, and we would ship from there, provided it was not the very latest product that we hadn't transferred to manufacture. We had a sales organization which kept an inventory of all the Fairchild products that they shipped, but it wasn't all manufactured in SGS.

Now, the big problem of SGS was that they were a germanium house. They started out as a germanium device company and they had something like two or three years' worth of unsold inventory in germanium. And germanium, as you know, is not a stable product; it doesn't have an oxide. It goes to pot. So we had tons of testing and retesting to do in order to make sure we were not sending out defective goods. We also had the Italians to fight, because they did not like the idea of writing off the inventory. That was an American practice. I fought like hell, and succeeded in part only, to write off the inventory. "Let's just get it out there, scrap it, throw it away, give it away, whatever we want to do, but let's start marketing the products that we're making there." And we were making Fairchild product within less than three months from the time we started, because you could do the assembly at SGS. See, you could send the chips over there and assemble them, test them, and then send them out to the customer. We did not set up a diffusion wafer fabrication organization until much later.

So it was a challenge to get rid of that germanium inventory. There was one product in germanium that sold like hotcakes, and that was point contact diodes. These were rectifier diodes for radios, very cheap; they were maybe a penny or two apiece. Germanium was the only thing that could satisfy those specifications. So that one was saleable, but none of the others. They were just passé. Bob Noyce's orders to me were to "Get this crap out of here as fast as you can." Well, you know, you have two other partners, and if you're going to scrap it, how do you write it off? In Italy you have to choose the right set of books in order to write it off, because their accounting leaves a little bit to be desired. It's not according to all the rules of the United States. Here we wouldn't hesitate to write it off, take a big loss, and then extend the loss over a period of years. Couldn't do that in Italy, not unless you kept more than one set of books. [laughter]

So, technologically it was a good venture. The Italians learned quickly; they had good technical guys. I sent several of them back to the United States to learn, to work in Charlie Sporck's manufacturing line, so as to get the techniques and the specifications, then come back and install them in Italy. But that was the easy part of the job. The hard part was just to convince them that germanium just wasn't going to be around, and anytime we took with it we'd be wasting. They were so proud of their big germanium lines, that you can't tell somebody that's useless when he's spent his professional life putting it together, the way you do it in the States.

LÉCUYER: Actually, I do have a question. Did SGS precede the agreement with Fairchild or was it—so SGS was a company that existed prior to—

SELLO: Oh yes, SGS, Societa Generale Semiconduttore, was a combine of two companies, so it was already a Societa group. Fairchild added a third, but it still stayed as a Societa.

LÉCUYER: So Fairchild took essentially a position in that company.

SELLO: We took a "partnership" position in that company and it amounted to 40 percent of the ownership. Therefore, when the profits were split such as they were, they were split 40-30-30. There were a lot of particular problems besides the technological ones. The salespeople of SGS did not want to give up the old alloy germanium product. They just simply had commitments that they had made, and how could you go to a customer and say, "You can't have what you ordered"? "I can give you something newer, not quite the same." But it's a hard sell for a novice in sales. In the United States they just would have said, "Hey, there's no longer around. Here's the catalog. Pick what you want," this kind of thing. [laughter] It just quite didn't work out.

Then there were a lot of customer problems because the germanium product was not a reliable product. It died on the shelf. It died in customers' computers. We had a steady diet of trying to pull back rejects, returns, and retesting them to see if they were any good. That was just one horrible mess. So out of the two years that I spent there, I probably spent a year just trying to get rid of the germanium lines. We finally accomplished that, but it was a battle all the way.

LÉCUYER: It was phasing out the technology and putting in a new one.

SELLO: Yes. Not only that, the number of people involved in the silicon assembly at that stage were about half of those involved in the germanium production. So we had to release—now, that you don't do in Italy either, because they're all members of unions. I mean governmental unions. It's not just political; it's a governmental organization. If you have to ask a Director to resign, a guy who was, say, in charge of the particular production or equipment line, the only job you can offer him is as a Director someplace else. It's a national designation. I think France had similar things in that period, conceivably. When you get above the "Direttore's" level, they call it, director's level, a guy who is let go, not his own fault, can only be given a job elsewhere at the same director's level. So how do you do that? Here it's easy; you say, "There's the door."

So we had those kinds of problems, and that was something that we never really had a good fix on when we first started. We probably would have shored up the contract a little bit more heavily. Technologically they were good; the guys were good. They learned what a silicon planar transistor was and what the integrated circuits were. They were very pleased to be able to assemble and market those, and we never could get enough of those because it was a small production line. And there were a number of other problems that popped up when you least expect them.

I got a letter from Noyce while I was there, saying, "Can you stop for a moment and go to Sweden and talk to a certain high level customer there?" They were in the automobile industry. It was Saab, an automobile customer. "He's been sending us complaints here at Fairchild that the silicon products," not the ones that we at SGS were making, "were not good," and Noyce didn't have anybody to send, so he sent me because I was from Fairchild United States.

So I said, "Hey, Bob, you know, we can't service the guy from here."

He said, "Oh, just keep him happy. We'll send him what he needs from here. But see what you can do to mollify the case and find out more about it."

So Noyce sent him a letter saying that I was coming. "Harry Sello of Fairchild is coming to talk with you. We'd be pleased if you would explain to him the problem so he can help you out."

We got a fast letter back in Agrate, back in Italy, saying, "Would you please ask Mr. Sello"—not doctor; Mr. Sello—"that it won't be necessary for him to come from Italy. There's not much business we do with southern Italians. Therefore, we would prefer to be serviced directly out of the United States." And he sent a copy of that to Noyce.

Noyce called me up and said, "What have you done to deserve that?"

I said, “Nothing.” I had never even heard of the guy. That’s their way; they don’t do business with southern Italians, and Sello—

LÉCUYER: Sello sounds like southern Italian; it could be southern Italian.

SELLO: You see, it could be southern Italian. That “suditalians” kind of thing.

Noyce had to answer the letter, saying, “No, this is an American from the United States, well acquainted with the process. He is on temporary duty there and he will make it his business to get up to your country.”

Well, it took that to get me up there. What it was, it was a reliability problem on planar transistors which hadn’t met the specification, and they wanted another shipment. But I had to go in under the masquerade of not being a southern Italian. “We don’t deal with southern Italians.” I remember that. I saved that letter here somewhere. It was a surprise then, but not a surprise later. There were a few such incidents.

But we did convert the fab. We got rid of all the germanium finally, and we did build a small inventory of planar and mesa transistors, not the integrated circuits, because those were not yet finished from Fairchild. We turned loose the particular salesman that we had and got another one from the States in his place.

During the time I was there, Gordon Moore came out to visit the factory. So did Charlie Sporck and Dick [Richard] Hodgson, who was then president of Fairchild. I, of course, took them around and showed them everything there. Dick Hodgson said, “Hey, you must be having an easy time here. You know all the tricks in Italian.”

I said, “Well, it wasn’t easy to come by.” I studied the language, by the way. It warranted that. I had no problem with it. I didn’t have to, but I did. I took tutorial courses from the Italians and it helped me enormously, just enormously.

Then Hodgson came out and he said, “Well, how do you think they’re going to do? Will they be able to handle these new integrated circuits that we are now making?”

I said, “Well, I don’t see any reason why not, as long as they have the proper test equipment and the other equipment, which we’ll have to supply them.”

Well, so I made up an estimate of how much that would cost to stock the factory with the test equipment required for the integrated circuits, and it was a sizeable few million dollars of things. Well, at that time, unbeknownst to me, SGS, without telling their American partner (they told their Italian partner), had gone ahead and contracted to build two more factories, one in England and one in Germany, on the hope that the silicon devices would fill up those factories and make it all one set of products. However, the cost was enormous, and there was no way we were going to do that at the present level of the operation of SGS. We would do it if the partners would ante up more money, because it was a sizable investment, but then the structure would have to be realigned, and they squawked and weaseled for about six months and then didn't want to do it.

So we elected to pull our investment, which means we started from scratch again after being there. We had to start a whole complete semiconductor operation somewhere else, which was quite costly. SGS didn't want to put up with it. They thought they could go ahead and make the planar devices themselves, and I pointed out you can't do that without a license. "Well, we won't tell you. We'll just do it."

I said, "You won't tell us, but we'll find out about it and it will cost you."

So it remained a stalemate, and in 1968 we sold our share in SGS and we, in turn, opened a small test and manufacturing operations unit in England.

BROCK: In 1968?

SELLO: In 1968. And this was—let's see. We'd been in Italy four years. This was another three years after we started. But this one was not dependent on any other owner; it was a solo Fairchild operation to penetrate the market. So we didn't lose any money on the SGS operation, but we did lose time. It was not too effective from that point of view.

BROCK: Did you have a fabrication plant then for the transistors? Did that happen after 1968, similar to Europe?

SELLO: Yes. They themselves went ahead and they linked up with a company called Ates, A-t-e-s, which now makes it a third party to take the place of Fairchild, only this Ates was a branch of RCA Semiconductor. And they made silicon products. So SGS linked up with this Ates firm, who was in Sicily, and came to join, bringing their licenses for silicon products from RCA. So they ended up working in that field, but at a much, much reduced pace, and we maintained a small operation in England for some years to come.

BROCK: And they were manufacturing devices there?

SELLO: Yes. They were not diffusing them; they were just doing the assemblies.

LÉCUYER: So then SGS can be doing only assembly until the seventies or so.

SELLO: That's right. That's right. When they got Ates, they then got licenses from RCA to try to build diffusion operation.

LÉCUYER: So SGS-Ates Electronics was based on RCA technology?

SELLO: Yes. Yes, it was. The basic products, but RCA did not have the integrated circuit line that Fairchild had. We would have continued, but SGS got into this nasty habit of building buildings before they had sales, and we were getting these awful bills for two and three million dollars every six months for a new plant going up someplace else in Europe, without the definition of the product to be made there, so all hell broke loose.

LÉCUYER: So it means that there was not a great deal of transfer of technology from here to there, right?

SELLO: No. No, not very much.

LÉCUYER: Transferring assembly technologies.

SELLO: Assembly was transferred, and in the small English operation a small pilot line of diffusion was transferred, a very small one, just to be able to say that we met the European requirements on duty and on foreign manufacture. That was the big requirement. By that time the Europeans had learned that we put a stop to the Agrate assembly operation. They wanted the duty that goes with making the whole product.

BROCK: Oh, so then by having a line there—

SELLO: Yes, having the line there, we could ship more from the States, but at least we had a line there. And the English operation lasted for some few years. It was never very big, never very effective.

BROCK: Was it then closed down in favor of another European operation?

SELLO: Yes. It was closed down in favor of a larger, more broad operation out of Germany, a factory purchased in Wiesbaden, Germany, to be a total silicon plant. But now you have to realize this was already something like ten years after the introduction of the first silicon devices assembled in Italy. So we lost quite a bit of ground in penetrating the European market, unfortunately. By that time, Motorola and TI had grown considerably. They were making their versions of planar products under our license, in England.

BROCK: Where they had plants or where they had subsidiaries?

SELLO: They had plants already, yes.

SGS moved its operation to France, to the south of France, and joined up with—Christophe, you would know if I mentioned it.

LÉCUYER: Thomson?

SELLO: Yes. They became SGST, Thomson. Right, thank you. SGS-Thomson. SGS by that time had developed their own silicon, different than the United States. They were making commercial linear products, linear integrated circuits. And they joined in Thomson with that and became a combine of SGS-Thomson.

LÉCUYER: And now it's one of the largest companies in the world.

SELLO: Yes, it's one of the largest companies in the world, yes. They brought in this ex-Motorola guy, a little short guy; I forget his name at the moment. He came in to run the SGS-Thomson unit, and, of course, Thomson transferred its silicon unit into that one. So for them it was kind of a coalescing of companies.

BROCK: So back in, I guess it must have been 1960 or so, when the initial SGS deal was getting set up, did SGS come and approach Fairchild about the possibility?

SELLO: Sure, they did. They did, but, of course, by that time we had learned that the only way they would get it is if they licensed the process. As a matter of fact, at a later time we did license SGS-Thomson to make a line of telecommunications integrated circuits at Agrate, at the place in Italy. We wrote a ten-year license for a million-dollars-a-year-worth of technology fees, which wasn't bad. But shortly, not too long thereafter, within the first four years or five years, they wanted to abrogate the license. They felt they didn't want to pay a fee anymore, so they would just go ahead and make it on their own, and we brought suit against them out of Fairchild.

I was the chief technology expert witness—I gave the lead testimony on that project, because I was the one who knew the process and I had actually done some work, together with SGST, in order to start that work. So I was asked to go to the International Arbitration Court in London to pursue that litigation, because SGST did not want to pay the fee. They claimed that they had the knowledge before and all they got was some sort of temporary relief which didn't amount to very much money, and they wanted to pull out of the contract. It was not successful. My testimony was more than they could stand. [laughter]

LÉCUYER: So this would have been the late seventies?

SELLO: Late seventies, yes. So the entry of Fairchild into Europe was later than it could have been if based out of the original successful entry with the Italians.

Well, also at the same time Gordon Moore came out and he sent me a wire. He said, "You've had enough of Italy. We need you back here. We want you back in the States." So that was in about 1966 or so, not too long before the Fairchild crew left for Intel. They left in 1968. So he sent a wire saying he wanted me back because he had been starting a formal R&D organization for Fairchild in Palo Alto, and he wanted me to become a department head in that organization. He said, "You've had enough fun in Europe. Why don't you come back now."

Well, you know how the semiconductor industry works. When you want to come back, you never can, but when somebody invites you to come back, you'd better jump at it. I joined him, and I started the Materials and Processes Division at Fairchild R&D in Palo Alto. Gordon was the R&D director in that same building.

BROCK: So you were in Italy from starting in 1961 until—

SELLO: July 1964. Physical presence in Italy. Then after that I was based back in R&D, but I went back and forth in administrating the Fairchild licenses with Italy and judging the viability of the other operations that they were trying to set up.

BROCK: So that was from 1964 to 1967?

SELLO: July 1964 to 1967, right.

BROCK: Were you about half your time in California and half—

SELLO: Yes, that would be about it. A lot of traveling. It was kind of fun, but it was heavy. And this was before the Fairchild guys left to form Intel.

BROCK: I guess Last and Hoerni had left in 1961, isn't that right?

SELLO: Yes, they left early. They went to Amelco [Semiconductor].

BROCK: How did that impact you or the place?

SELLO: Not too much. Not too much. They had sort of—I wouldn't say outlived their usefulness, but they had kind of withdrawn from the operation. Jean had already got the planar patent. Jay Last had been working on the step-and-repeat camerawork which had been all developed already by Bob Noyce and on integrated circuits. He was getting itchy foot, too. So they left to form a duplicate operation of Fairchild at Amelco. They didn't last there very long. In fact, Jean Hoerni, unfortunately, took a bath in that venture where he got into a lousy financial deal. It cost him a lot of money. He just was so anxious to leave Fairchild, he didn't care to specify conditions; he just wanted to get out. That's a pity, because he could have done very well, conceivably done it very well, but it didn't work out.

BROCK: So when Gordon Moore got in touch with you about really wanting you to come back full-time to R&D, was it specifically to set up this Materials and Processes?

SELLO: Yes, specifically. I took over a group that at that time was called Materials and Process, but it was run by kind of a "lamebrain" from Shell Development, by the way. He was having trouble with it. Gordon said, "I want you to take on these other engineers and form a full-fledged Materials and Processes Group." And one of the missions, one of the main missions that I had a hell of a time trying to implement, and it's one of the weaknesses of our own approach in this country, is to convince Ph.D. engineers that the problems on a manufacturing line were serious enough to warrant the high-level technical effort that you could find. The R&D types had difficulty working with the line engineers. The R&D attitudes concerning line engineers were just, "Oh, hell, let them—they created the soup, let them get themselves out of it."

Of course, some of us knew processing already. Besides me, I also had two, three guys that were really good at processing that we had hired in R&D, in Materials and Processes, and I used to argue with those guys, "Get over there and get the line out of the soup. It will show up in your merit raise for sure, and you'll have the satisfaction of salvaging the line," but it's an awfully hard job to do. Just didn't want to do it. And that's a basic fault of most chemists. What does a chemist type know of the deficiency of device performance? He doesn't have to be an electrical test engineer, but he has to understand the results, and you have to be able to go in and say to somebody, "This is correct, and just do it that way, because we have a lot of data that shows that's correct."

It's a hard thing to turn an R&D guy back into a production man. He doesn't see the immediate value. First, he can't publish. Okay, that's one thing. But secondly, what does he have to do to claim that he has created an improvement? It usually has to be some sort of specific problem solved or a yield increase in a device. Those are not easy to come by. You have to have patience in order to do that.

LÉCUYER: Is it also a matter of social status?

SELLO: It is also social status. "They got into this mess, let them get themselves out of it."

"Yeah, but they're a part of your company."

"No, they're not. They're in manufacturing. I can't tolerate—." A guy, I remember who came back one time, he said, "You know what he's doing in linear integrated circuits? He's marking wafers so he'll know which are the good ones and which are the bad ones would start in the line."

I said, "Well, what's wrong with that? It's a good idea."

He says, "Yeah, but he's painting the pure silicon wafers green and red, and he got the paint from Sears, Roebuck." And he went into the line and half the runs that he wanted to test he painted red and the other half he painted green. You know, it's idiotic. The guy's off his rocker. I mean, you don't go in and poison wafers by contaminating them with paint—by this time these were big ones, you know. These were costly wafers. But that was the caliber of some of the engineering work, and you had to have guys who could recognize that and say, "Do it the right way. Just take the wafer, cut it in half, don't mark it, run one half on one day and the other half on the other day."

It's awfully hard to get the process—Ph.D.'s, I'll get to that. I mean, I'll confine it to that—Ph.D. process engineers to act as chemists or physicists or whatever they want to act at, to go into the production line and solve the problem that they have. It's a tremendous source of personal satisfaction and cost-saving when it happened, but they just don't—and to this day you have that kind of a problem in production. As a production engineer, you have to accept the interference into what you're doing, by somebody with higher technical knowledge. It doesn't help if that somebody goes in and says, "Tell me what you're doing and I'll tell you how to do it better." That's not a way to start a conversation. [laughter]

BROCK: So what was the general mandate or function of that division? Obviously to solve these problems of the type you're just discussing, but was it to—

SELLO: Of the R&D Division?

BROCK: Well, of specifically the Materials and Processes.

SELLO: To develop new materials and new processes for production. It's just that simple. Not devices. We weren't asked to design devices. That was the function of the Design Engineering Group in another area. That called for electrical engineering design work. We were asked to go in and solve the reject problem, improve the process, and increase the yield. Why are they turning out rejects?

BROCK: So what sorts of activity did you initiate then?

SELLO: Well, first I went over there myself with the full weight of my heavy position. [laughter] And together with the engineers involved or Ph.D.'s involved, and find out what the problem was, to define the problem. What's happening at this stage and this stage and that stage? That took a lot of guts, you know, just to get the guys to admit something was wrong.

Then we would assign a particular Ph.D. as a troubleshooting engineer, and he would do what we called split runs. It's a self-explanatory term. When they started a batch of twenty wafers to the product line, he would take ten of them, cut them in half, and ten he'd put the existing process and the other ten he'd put through on what he thought to be the improved—they improved it, and look at the difference when it comes out of the line. "Split runs" was the magic word.

Of course, you could do some instrumental examination along the way. You could test the test devices on the wafer and so forth, but "split runs" was the magic key towards solving that, and on top of that was the patience of working with somebody who didn't know what it is you wanted to find out. So you had to explain if somebody got a zero yield on a split run that it was necessarily bad, all it was was a zero compared to the other one that you're using on the same line, and you might have made it bad on purpose in order to see if the effect was real. A very hard job for a process engineer to do.

LÉCUYER: So what were the kind of processes you were working on in the—

SELLO: This would be diffusion process, oxidization, and metallization, mostly the masking problems. The mask sets had defects and the defects were getting through to devices being made. And, of course, a steady diet of testing problems. You could find out step by step in which step the various devices were disappearing, were rejects. Then, of course, at the same time when you found that, you took them for analysis through the electron microscope analysis, you took them through the electro-chemistry analysis, all of this kind of tools that you had to scan them with.

Of course, there were some interesting events that occurred during all of this kind of work.

[END OF AUDIO, FILE 3]

SELLO: Let's see. Where were we?

BROCK: We were just talking about the types of problems that you had on the production line.

LÉCUYER: And we talked also about the processes that were developed in the lab, in Materials and—

SELLO: Materials and Processes.

LÉCUYER: Yes.

SELLO: Yes. The toughest process was to convince the R&D engineer who was sent over from Materials and Process that production had a problem, because his whole reaction was, "Whatever you're doing is wrong." And these guys just couldn't seem to give credit to the lowly process engineer, who was a bachelor's-level guy, usually, in manufacturing, a master's at best, that he had a credible problem. It was then that they discovered in one place that there was one guy who was marking pure silicon wafers with green paint and then other ones with red paint for purposes of identification. And that was so horrible, and story circulated for a long time, much longer than the time it took to clear up the problem.

But that was the toughest problem, because they the R&D engineer, the Ph.D. mostly, considered the process problem to be below his level of competence. Now, that's a continuous problem today. It just is. Why that is, is a philosophical thing that we can argue about for weeks, I suppose. But that's a tough one. And those were the kind of guys that Gordon wanted to see could be sent out and help solve the immediate low yield problems. All of sudden, “Hey, get out to the NPN planar line. We have to shut it down. It’s spewing out rejects.” They’re doing everything the same, everything is the same as they ever did it, but now it’s spewing out rejects, and how do you define that?

I don’t know if it’s a Ph.D. versus non-Ph.D. behavior. There’s certainly a chemist versus non-chemist. There’s certainly a practical user of the knowledge versus a new user of the knowledge. There are barricades all the way along the line, and that’s something that Gordon always felt that maybe I could do a better job than others could because of the experience. I was familiar with process development problems. It takes one to know one. [laughter] But to solve that means that you lose production, lose yield, lose money, and the manufacturing managers are on your neck all the time to say, “How can you let us get away with that? Now fix it.”

There was another aspect that Gordon’s view of research and development had to pursue, and there’s another little anecdote that goes with this that I want to introduce you guys to. In addition to the R&D work, we were developing new processes like epitaxial deposition. There are all kinds of ways of doing epitaxial deposition. New equipment has to be developed, bigger equipment, larger wafers. Larger wafers were a pain in the buttski. Every time we developed something on a smaller wafer, the next ones we had to put in were larger, in order to increase the yield. The tubes were never big enough. So it’s a continuous press of scale-up. Now, that’s production problems.

On top of that, we had to do troubleshooting out in the field among the customers. You know, especially if it was a problem that came back via manufacturing, “Hey, manufacturing just got a big return of rejects and we don’t know what they are. Go find out,” and I was sent to find out. One time it sounded big enough that I had guys tied up doing this so I went out first, not that I could recognize the problem, but I could at least get a feel for the scope of what commitment of people would be involved. It turned out that there were a bunch of rejects that had come back from IBM. They had failed in the field. The Fairchild planar integrated circuits had failed in the field in the computers after a reasonably long length of time. And what are we going to do about it? So, “Sello, get yourself a ticket right away and go out to IBM and take a look.”

BROCK: Who sent you?

SELLO: Gordon. “Go out and take a look.”

“Hey, why me? Send the product engineer who deals with those customers, the product development engineers.”

“No, no, you go.”

I said, “Well, look, I’ll go, but what kind of a problem is it?”

"Well, they have something to do with the fact that there are shorts developing on the devices in the computers in the field."

So I said, "Well, I'll take Ilan Blech with me." Dr. Elan Blesh is a metallurgist and one of the brightest guys I had, and he was very good at troubleshooting and recognizing what we thought would be metallization problems. When we received the sample back, I asked Ilan to take a look at them and it looked like they were all shorted out, like somebody had put a blast of current through the computer and it caught those devices.

So we went to IBM, who was a big customer at that time, and discussed the problem there. They gave us the rejects. They told us what they had done to examine them, and we took all that with us back to our laboratory and began to examine them. And a wonderful thing happened, now that I look back; we discovered what is now known as electromigration failures, something that was not known in ICs at the time. It's a chapter in this book written by Blech and Sello on electromigration. It was a peculiar problem and had not been seen before in ICs. If you look at the strip of metal on the device, under high magnification, it looks like it has burned out. There's a strip of metal, and somewhere along in the middle there's a bare spot, and it looks like that's been zapped. It looks like a shorted wire in a light bulb. It looks very much like the same thing.

BROCK: Yes, I'm just writing down the title.

SELLO: Sure. We didn't know what we were looking at, at the time, but we started to analyze the thing and we found a very interesting phenomenon, that what happens is when you have a high current which is running down a thin strip, which means high-current density, the "electron wind" carries the atoms from one end to the other end.

Well, you look at that and you normally say, "Well, that's easy. It was zapped," except the current or electron is opposite to the Al ion flow. And this is what Ilan and I looked at when we first saw it. We thought about it and then we said, "We'd better go look at this closely, because it isn't kosher."

So we looked at it under the electron microscope, and we went so far as to create the migration of Al atoms under the field of the electron microscope and watch those little buggers move, actually move. We made an electron microscope movie of it. I'd be glad to show it to you. You can actually see the Al whiskers forming. You can see that the metal is leaving the cathode, and aluminum is a positive ion, it has no business leaving the cathode. And it's because of the electron current wind pushing the Al ion.

There's a problem like that in light bulbs. This was the find that we stumbled on, looking at the literature. Filaments in light bulbs will frequently go out and leave a lump and an open, but they'll leave it where the direction of the current is just the opposite of the charge on the atom.

So we played with this and we looked at it, and then we showed it to Gordon and Gordon refused to believe it. Ilan was good; he's a fine metallurgist. He knew electron microscope work, so he put a test strip of aluminum metal under the electron microscope, and we focused a movie camera on it and we watched the silly atoms move from one end in about a half-an-hour type of film. Increase the current, they move faster. They all moved from the cathode to the anode, and we reported the first time at a reliability conference on the lifetime of aluminum strips.

Nobody wanted to believe this, because aluminum was considered "sacred." It stuck where you deposited it. Now, sometimes it could be if you overheated the Al you could alloy aluminum through fine holes in the oxide and it would puncture through because it'd gotten molten. That you could find from time to time, and it was a problem that we saw lots of time in our work. But these devices had failed at room temperature in the computers in the field.

LÉCUYER: So how do you solve such a problem?

SELLO: With great pain. The thing that emerged from that is one very simple thing. There is a new "design rule" being used for the past two decades now already, that specifies the maximum current density that you can use on an aluminum-bearing device, and not risk electromigration failure. So that means it's a limitation. Here's the picture of it. Here's a strip right down here. This is the cathode. This is the anode. Notice the voids that are formed when aluminum particles within the strip move out all in that direction and then they pile up. Sometimes they pile up so high that it can grow a whisker. If you saw the film, it would scare the hell out of you.

The normal failure on devices that you see in aluminums are devices like this. Here's an emitter and here's a collector, and when you run high currents you'll get the aluminum/Si melting and punching through holes in the oxide into silicon underneath. That's a "normal" failure, but nobody had ever seen this before, the electromigration. And that on the books today is a design parameter on designing circuits using aluminum interconnects.

I'm very proud of the finding. People weren't convinced. We gave that paper. They wanted to know, "How the hell do you know that? You dreamed it up. You're talking about something that isn't true. We've never seen this kind of thing." Ilan did a beautiful job of preparing test strips of different sizes, and he showed them all in a movie on the electron microscope, and you can see the little hills form bigger hills and the bigger hills form voids and so forth.

BROCK: And what finally convinced people? The film or—

SELLO: The data. This publication.

BROCK: The publication.

SELLO: The publication, on top of their experience. They had seen this, but could not credit the fact that there was aluminum bumps in the vicinity of the anode and its opposite polarity. It doesn't have the right to be there. Aluminum doesn't change polarity once moved through a strip.

So we immediately issued a design parameter and went out into the literature into all the design handbooks of all the companies that thought about this, that beyond 1.8 times 10^5 amperes per centimeter squared, and that means the size of the strip taken into consideration, don't design circuits with any current above that.

Now, a lot of work went on to try to stop it. For example, one of the earliest works was to investigate all the metals that you could. Are they all going to do this? And they do, with very little exception. Gold especially. Gold doesn't have any place to stick on aluminum. That was another piece of evidence that gold doesn't stick. I could have used it earlier.

We did find that if you included a little copper in the aluminum, it slowed down the electromigration rates. So we published a whole table in the literature on electromigration rates under various conditions in aluminum. And it was all due to the IBM failures. Oh, they thought we were magicians when we came back with that.

BROCK: How long did it take you?

SELLO: About six months. Six months, and most of that time was preparing EM samples, electron microscope samples, where you could tell a guy that, but he wouldn't believe the before and after, but when he watched it form—not only that, if you reverse the current, you'll reverse the procedure, you fill up the holes. Oh, that was, you know—when they talked about it, Ilan said to the skeptic, "All right, you've got a point. Watch this." All he did was switch electrodes on a little DC generator, and immediately those whiskers began to pull back in and the little holes began to disappear and those voided areas began to fill up. So he could restore the movement enough to where the strip worked.

Now, of course, on a device it wouldn't be that easy to fix it, because by that time the device would be destroyed anyway due to current pockets and so forth. This was the kind of case for which the Materials and Processes Department gained a little bit of notoriety in its field. The IBM guys thought we were magicians that we found it on field devices, and they were all ready to sue Fairchild for bad aluminum on devices. “What do you mean sending us crap like this that fails after the first few dozen hours?” Always you’re accused first and then you have to disprove it—you’re guilty before you’re proven innocent.

So the electromigration was one of my finest satisfactions about the work I did at Fairchild, was to find that, together with Ilan Blech.

It turns out that every metal exhibits electromigration to varying degrees. The only ones that are very slow are copper, but they can’t use copper on devices because it poisons junctions. So RCA started to play around with copper-aluminum alloys, and if it wasn’t alloyed properly, then the copper would separate from the aluminum, the aluminum would migrate. Mother Nature has a way of finding those things.

LÉCUYER: Talking about the Processes, I was wondering whether your group was working on ion implantation.

SELLO: Yes. Yes, it was worked on in two places. It was worked on in the device groups themselves, the device designers, because we had to use the same equipment. In R&D we had only two ion implanters. So we had to work together with the device design groups on ion implantation and its effects. On the ion implanters, the device designers did more of that kind of work than we did, but we followed very closely with them. And that, in turn, was a lot of the work that Jim at Stanford got famous for. He was doing a lot of ion implantation development.

LÉCUYER: So you’d be using the tables that he had produced?

SELLO: Yes, that he had produced, right. We produced smaller tables in the beginning. Then when he really went at it, he produced these long tables, in depth.

LÉCUYER: So Fairchild was one of the early firms to use ion implantation.

SELLO: Yes. Not as strongly as others did. Others spent more time with it on total production time, but we were one of the early ones interested in the development, right. Of course, the difficulty with doing projects like that in research and development is production always has a two or three times larger unit to produce devices in volume, and you’re trying to solve a problem that may or may not be volume-related. So you have a small ion implantation unit and you have to make sure that you can correlate the two sets of results. Thus we would do it jointly. We would have them do the ion implantations, we would look at the results. It was a function of the equipment in that case.

BROCK: Was your group doing anything with chemical purity issues in this time period, or was the chemistry group?

SELLO: Yes, we did. We had an active chemist in the group, and his full-time job was in looking at the purity of chemicals from a standpoint of using what we called test vehicle MOS devices in order to decide whether or not there was a problem. However, we had a very practical test that we used, and that was the formation of an MOS diode, a test diode. We would impose a bias on the diode and then make an MOS measurement, a CV curve (capacitance voltage curve) on that capacitor. And if it was contaminated, then the capacitor voltage would just drift all over the place. If it was clean, the capacitor would have a pure CV curve. So you knew what was bad, but you didn't know why. Most of the time it was due to sodium contamination, which we at Fairchild found to be the worst ion or best contaminator of devices.

So we did MOS evaluations using test structures, but the analytical work that went with that we did outside on a contract basis because of the equipment. But that really wasn't definitive enough, because you could have a liquid which apparently was okay, but until you tested it on an MOS device, would it drift or would it not drift? You do a CV curve, capacitance versus voltage, you get a certain curve. If you impose a steady voltage and there's something moving in the oxide, that voltage will drift. It's not supposed to if it's clean. The voltage could either have a kink at the top, a step in the top, or a step in the bottom, and you know you've got a contaminated diode, but it's an electrical measurement, not a chemical test.

BROCK: So that's how you were almost screening chemicals—

SELLO: Exactly. You know who's an expert at this is Dr. Bruce Deal. That's his *métier*. He is great at MOS tests, CV tests, on chemicals.

But the CV measurement is a much more sensitive one than chemical measurements, because you can be in parts per billion contaminations on a chemistry basis, but that contamination may be enough to cause a drift in the CV measurement.

BROCK: And if you found a sample that you were having a problem with or that was affecting the MOS, CV curve in that fashion, then you would send it out to an analytical lab?

SELLO: We would do that, and usually we found that the analytical lab could find nothing, because the CV measurement is much more sensitive.

BROCK: So you just ruled it out?

SELLO: So we just ruled it out anyway. We just issued information to circulate to the people using that reagent; "Don't use Monsanto acids for this kind of thing." It was an arbitrary choice, not very pleasant, but it had to be done. But that work of figuring out what went wrong in production is one of the most vital jobs you can ask an engineer or a Ph.D. to do, and it's the hardest job to get that guy to do it. Some termed such analysis "chicken-shit stuff", which did not appeal to him. However, a correlation between electrical, MOS, analysis and chemical analysis was always sought.

BROCK: But Gordon Moore and you both agreed very strongly on it?

SELLO: Right. Right.

BROCK: The opposite view.

SELLO: The opposite view, absolutely. And that's why guys like Ilan Blech were so valuable. Ilan came up with the fact that electrical wires in light bulbs suffer failure because of electromigration. I, as a chemist, didn't know that, but he did. He was a metallurgist and he knew about the failure of these wires in light bulbs. And the only reason that he knew was that the movement of Al occurred in the opposite flow of current. The stupid aluminum atoms were smarter than we thought they were. [laughter]

BROCK: I was interested how the Fairchild R&D lab had changed by this period of 1967, just in all aspects. It's eight years after you joined the company then.

SELLO: Let's see. How did it change? First of all, the big change was Moore and Noyce left in 1968. So that had a tremendous effect. They were leaders. Certainly Gordon was the leader in this kind of R&D effort. I continued to work with the Materials and Processes Group. The group got smaller because we needed the R&D guys to assist to operate production lines satisfactorily. We just assigned them there and transferred them out of R&D because we just needed those guys and that kind of experience. So the R&D effort in that regard diminished; the total effort diminished. In addition, several of the key members of my department followed Moore to Intel.

Of course, a lot of other problems were solved so they weren't problems any longer. But it took a toll on the R&D Materials and Processes effort to work with that. We had an equally capable team on epitaxial problems, having to do with the preparation of epitaxial materials in silicon. We had a brilliant guy there that later would move, with Gordon, move to Intel, I gave the eulogy at his death. Ross Tucker. Dr. Ross Tucker, also from the University of Illinois, same as I was. He was an expert in epitaxial silicon deposition. He could look at the surface of a wafer, an epitaxially made wafer, etched with the proper etching to reveal the bumps and the uneven surfaces. He was a genius with high resolution microscopic analyses. He specialized in detecting faults in silicon wafers prepared in the epitaxial reactors and so forth. Guys like that were also valuable on the teams for making devices, as well. So you had a choice of how you put these guys to work. Does that answer your question?

BROCK: Yes.

SELLO: In every area where there were problems, there were scientists like Drs. Ilan Blech, Ross Tucker, and Bill Shephard, that could also work in production and solve the problems there.

LÉCUYER: Could we talk about the movie that you were involved in on integrated circuits?

SELLO: Oh, you're changing the—

LÉCUYER: Subject.

SELLO: An entirely different topic. There was a movie that we wanted to—it was made earlier than some of the work I've just been talking about. It was not an R&D movie. Well, wait a minute. I guess it did occur during the R&D period.

Fairchild was still in the mode of trying to really get onboard of the market and push planar devices, planar integrated circuits. So the movie guys—if I can find these things when you're gone, I'll show them to you. The marketing guys said, "Let's make a movie and show it sometime to the entire United States and tell them what is a planar integrated circuit, and that Fairchild makes them and you should be buying those."

So we took the time to make such a movie, and in order to give it a kind of educational credibility we chose one of the professors from Stanford, Jim Angell. Jim Angell, a very pleasant guy, and I went down to Hollywood, into the incestual bowels of Hollywood, and we shot this movie to be shown for one half hour, in the morning at eight o'clock on, oh, several, four or five different days, on what is an integrated circuit. And we did this and it was a resounding success. Business rolled in. They also wanted to buy the movie. [laughter]

But it was a movie describing the processes of making an integrated circuit. I don't know exactly how I was selected to be part of it. That came about for another reason, and that was the stuff that I had done earlier all the way back with Bob Noyce at the Shockley Laboratories. I may have mentioned this to you, but before I became a semiconductor scientist, I was employed as a physical organic chemist. We made a movie called *Tempest in a Test Tube*. Did I tell you about that?

BROCK: Yes, the television show.

SELLO: That whole movie was a great success and led to yet another one which I made together with Bob Noyce when Bob was at Fairchild, the beginning of Fairchild. And because of the experience of those two, the publicity guy said, “Hey, we’ve got just the guy to do that.” So we went on to make this third movie, which was paid for by Fairchild, and which was the manufacture of a planar integrated circuit.

BROCK: What was the film that you did, the second one that you did?

SELLO: It was called *This Week in Science*. It was a general film, an educational film in which every week we picked a different event of science we had somebody onboard who could assist in the explanation. That was the time, for example, remember the Lockheed fiasco?

BROCK: No, I don’t know.

SELLO: The Lockheed Electra was a commercial jet prop, and three of them crashed at different times because a jet engine just pulled out of the wings. Lockheed found out in the air testing tunnels here at Moffitt Field that the engine had picked up a vibration a low-frequency vibration, as a consequence of its design and rotation. It would vibrate enough after something like 500 hours, and it would pull right out of the wing causing the Electra to crash. There were three such Electra crashes. So that was one topic of *This Week in Science*. Another one was integrated circuits.

BROCK: In what period did you do that?

SELLO: That was in the period in transition from Shockley to Fairchild. So it sort of came with the territory. “We’ve got this ham here. Let’s put him to work,” type of thing, you know. Of course, he didn’t have to twist my arm; I loved to do that. But the third one was the making of an integrated circuit, and that was strictly a sales tool. It worked out very well. We got a lot of response to that.

BROCK: And did *This Week in Science* program, that was on public television?

SELLO: Public television, yes.

BROCK: And was that paid for by ads on television.

SELLO: It was paid for by the Ford Foundation, the same ones who had paid for the earlier film, *Tempest in a Test Tube*. So we got them to fork up some money. But the last one was paid for by Fairchild.

BROCK: And you hosted *This Week in Science*?

SELLO: Yes. And each week a different scientist appeared in his field on the program.

BROCK: And that lasted about a year?

SELLO: No, half a year.

BROCK: Do you have copies of all of these programs?

SELLO: I'm going to try to get them. I haven't done much to try to find them. I'd particularly like to have you see the Fairchild integrated circuits one, as well as the electromigration under the electron microscope. We didn't make that film in time to show it at this particular reliability conference, but it would have been very impressive. I'll try to get those for you.

ROCK: That would be great. Before we get into just the situation that led up to Noyce and Moore leaving, I guess Grove, too, we talked about when Last and Hoerni left. Right around this same time period, 1967, isn't that when Charles Sporck left, as well?

SELLO: Yes, yes, he was among the first.

BROCK: What effect did that have on Fairchild?

SELLO: A very strong effect.

BROCK: Can you describe that?

SELLO: Yes. Well, he was the first manufacturing manager, a very prominent one and a very effective one, and he left a large void in manufacturing that we had to fill somehow. We could get somebody in there temporarily, but never to the degree that Charlie Sporck was. I took a little bit of time to do some of that myself, but I was too busy at that time in these other problems. We had to have a manufacturing manager anyway, so we went right out to try to get him. I'm trying to remember who the heck actually took Charlie Sporck's place. There were a couple of guys. It was even split up, I believe, at one time, in a PNP and NPN kind of division, but it was never as effective as it was when Charlie had it. But if you happen to talk to Charlie, he'll certainly tell you about that.

BROCK: Did you have any inkling that Gordon Moore was going to leave and that Robert Noyce was going to leave?

SELLO: No, no inkling. I knew ahead of their leaving that they were going to leave because Noyce told me. In 1968, I had just returned, together with Nelson, our corporate finance guy, from Europe where I had participated in the negotiations for the sale of the Fairchild share back to the partners in SGS. I don't know how much we got for it, but it was far less than what it was worth, I'm sure, because we wanted to get out of it. SGS had gone hog-wild in setting up factories all over Europe without Fairchild's agreement. It just wasn't viable, but we were only 40 percent partners, so we had no say in yes or no, so we sold our share.

On the way out, Noyce called me and said, "While you're leaving SGS, I'd like you to take an exploratory trip to see what you could find out in Russia about the possibility of technological arrangements of the kind that you had in Italy. See if you can work out something with the Russians."

I thought, hey, that's interesting, okay. Noyce, that was the way he did things, he shows up in Mountain View and I end up going to Italy. Now he's showing up, calling me from a hotel in Europe, "Hey, why don't you go to Russia."

So I and my SGS colleague, Piero Alderisio , whose picture I showed you here, took a prospective trip going to the various institutions in Russia.

BROCK: To companies in Russia?

SELLO: To companies in Russia, actually to companies under the guidance of the Ministry of Electronics Industry.

I said, "Bob, we can't sell a thing to them. There are embargos here."

He said, "Well, can't you figure out something that can come in under the embargo window or isn't so terribly important to us but might be to the Russians?"

So I said, "All I can do is find out what they're interested in," and I did that. I went to four Institutes in three cities, including one in the satellite country, Riga, Latvia, and also in northern Russia, Leningrad. I went to these four Institutes, two in Moscow, and talked to the Russian scientists of the Russian Ministry of Electronic Industry, then under the guidance of Minister Kolesnikov, the guy who was then in charge.

My pitch consisted of, "We know that our technology is embargoed in Russia, but there seem to be points, places at which maybe some are not as important as others. Like computer-building technology is tough. But we have a feeling that if you are interested, we can sell you the technology to make consumer linear integrated circuits, ordinary amplifiers and consumer digital products. So that's available, and we'd like to talk to you about that."

Of course, the Russians were completely interested. They were interested in anything that anybody could come and tell them about; they were so starved for such information.

Well, I convinced Alderisio of SGS, as one of his last acts with Fairchild, to go with me. Well, there were some interesting little highlights. One of the interesting highlights is when we got to Russia, we were treated like kings. We were met at the airlines with special buses and taken into special places that we probably couldn't have gotten in otherwise if we hadn't previously been invited. The invitation had been issued to Noyce, and Noyce passed it along to me. He said, "Do you still remember some of your Russian?"

I said, "A little bit."

He said, "Good. Now you can go." [laughter] No difference between Russian and Ukrainian in that case.

We got there, and there are some anecdotes involved, at least one I have to tell you about. When we got there, we were met by a group of two very highly placed technological people in semiconductor in the Ministry of Electronics Industry, two Russians, and the third was the interpreter (a lady) who, when we sat down, was an interpreter for Italian into Russian or Russian back into Italian. So I said, "Oh, well, it happens. I can handle it." And Alderisio was with me, he's an Italian, so he can handle whatever we have to handle. So we started going in that discussion

We started going round and round and talking about the technology. I don't know quite what happened, but I said, "We have planar technology," and let something out which was English. One of the Russians answered in English. And I said to the Russian, "Oh, you speak English?" Till that time I had been speaking in Italian. So at that point I got into English, and he said, "Oh yes, I've been to New York. I haven't been to California. If you'll invite me, I'll come." You know, this usual kind of bullshit.

So we started talking back and forth in English. The interpreter got irate. She couldn't handle English. Italian, Russian; Russian, Italian. She was assigned because she would be working with two visitors from Italy on that trip, and when we weren't working in Italian, she was deathly afraid that she would lose her job because she couldn't translate the stuff. She explained in Russian. Of course the Russian guy understood this. She explained in Russian that she has to—how can she be assigned to somebody for Italian when they're speaking English? She said, "That's an insult. Can't do that."

So I picked up on that in the Russian and I told it to Alderisio in English—Alderisio spoke English fluently, too—and we decided what we would do so she shouldn't lose her job is we didn't want to slow the Russian down, so he spoke from Russian to English to me. He spoke from Russian to me. I translated the Russian to Italian for the benefit of the interpreter. [laughter] "Oh yes, well," she says, "Non capisco." Don't understand. You know, she just kept going on and then she would repeat in Italian what the Russian had just told me. [laughter] We sat there for about an hour going around in a reverse circle, but in the end she did her job; she translated from Russian into Italian and Italian into Russian.

It was such a bureaucratic kind of a snafu. You know, they looked at the visiting delegation and they saw Sello, Alderisio; Italians again coming from SGS. [laughter] "Well, yeah, we'll take an Italian interpreter." And she was good. I mean, there's nothing she couldn't handle from the Italian to the Russian. But there it was. She did not accompany us on that trip to the various institutes. We then got an English interpreter, which the Russians preferred because they knew English before they knew Italian.

So we went to the various Research Institutes which they had opened up, very surprisingly. A couple of them, we later learned, really had never seen an American semiconductor scientist before, and they were anxious to listen to us and what we had to say. One of them, as a matter of fact—anecdote number two—one of them listened to my presentation on—it wasn't a presentation actually, it was a series of questions based on his work on silicon oxidation, and I asked him some questions about it in Russian. I said, "I don't understand quite—could you explain it to me again?" And suddenly this guy, after a couple of questions, bursts out and he says, "My dear Dr. Sello, I am familiar with your work with C. T. Sah and D. Tremere on the penetration of phosphorus into silicon dioxide, and I'm very familiar with that work. In fact, you just did it a few years ago. And I am familiar with that, and I wish to tell you that there are some errors in it." He was taking my questions as a critique of his oxygen analysis work.

So I said, "I'd be very pleased to hear what you have to say." And he spent about fifteen minutes pointing out to me, in a copy of my paper translated in Russian from the English, where the errors that he felt were there. I took a copy, I said, "Would you please sign it for me? I'd like to have that and I'll see that my copies get fixed up." Oh, he was beaming all over the place. They were so paranoid about this kind of thing, about knowing what had to be done in all of this work, but they didn't have the equipment. They just didn't have the diffusion furnaces, they didn't have the deposition furnaces, ion implantation wasn't there yet. They just were starving for information.

Minister Kolesnikov, by the way, after that came to the United States to meet Noyce, I arranged for him to do that and we got him a permit and he came in and visited Fairchild as a return visit.

The Russians really understood the problems and they wanted to be sure that this was the equipment that they knew how to use and would we support it and back it up. And of course, Bob Noyce said yes to everything. I mean, what else can you say? "No, we're not going to do it"? This is crazy, why are you here?

"In fact, Dr. Sello will come and help train you in that."

So I said, "Hey, thanks a lot, Bob. Glad you're telling me."

Of course, they knew of our earlier visit to Russian, so their visit was more or less a dressing, you know, coverage on the project, but it never worked. They had a problem. The big problem was not just lack of information. The Ministry of Electronics Industry in Russia at that time was in charge of making the products that the Russians couldn't buy from the outside world. Now, if the Russians couldn't buy ordinary audio amplifiers made in silicon, they'd have to have the technology to make it and we would sell it to them, and that would have been easy. A piece of cake.

But the same ministry that had to lay out the few million dollars' worth to buy the equipment to run that also had to defend to their boss why is it that you can't make these and you want to buy the equipment from the Americans. [laughter] So they had another cross to bear. We didn't know that at the time. I learned about it afterwards.

BROCK: So there was an issue with the supplies that they would need to buy to make it work.

SELLO: Right, and they had to admit they didn't know the technology to make those products. That's why they had to have the Americans, and to admit at the high levels and in the presence of the Americans they didn't know what the Americans were doing, or they didn't have any knowledge of that, how can they do that? Somebody's head would roll. They rolled for less than that.

So it was a very interesting little go-around that we had during that period. Noyce was very advanced in that regard. He went balls-out, I should say, to get the Russians invited to Silicon Valley. We had to clear them through a myriad of different agencies down here on the Peninsula.

BROCK: I mean, it sounds—of course, I don't know, but it's very hard to imagine that that took place. Were there any other semiconductor companies trying to do business there?

SELLO: No, not in the same way. That's where our program that still needs to be described a little bit more, our program of technology transfer to penetrate the marketplace had the edge. If we could sell them how to make consumer audio amplifiers and then get 10 percent of their market, which they would gladly do because they want to sell them, too, we'd have a good thing going. Ours looked like a good venture, but from their side, you know, the Ministry asks the Russian group, "Why are buying this? Can't you make it? After all you've got all this beautiful set of fifteen semiconductor factories and you can't make a silly audio amplifier? How do you justify that, Mr. Kolesnikov?" And Mr. Kolesnikov has to justify it.

It was premature, and at that time there was still enough byzantine thinking in our Congress that if you sold them a diffusion furnace that was going to make audio amplifiers, somebody in Congress, some of the hard right, would come up and say, "Hey, they're going to make atom bombs with that thing. How do you know they're going to make audio amplifiers?"

"Well, we know because we're going to be there and we're going to be checking on them, and you can't make atom bombs in the equipment that you make audio amplifiers."

"Oh, no, it's all hype, the same high-tech."

There was a lot of that that we had to put up with during those years, wrongly or rightly; I'm not taking any position. Now, if that were to be something that were for super fast digital integrated circuits, we wouldn't have tried that, because for that they need certain computers, they need certain equipments that they could turn to use on something else. So we didn't want to get into that. But this was just an exploratory trip by invitation to find that, but it made an interesting search.

BROCK: Maybe there's time for one more question, and it's just a simple one. Was it in the context of that working with Bob Noyce about the Soviet possibilities that you intimated that you heard that he was going to be leaving?

SELLO: It wasn't in that context, but it was 1968, early 1968, when I returned from Russia to give my report to Noyce. He, at the time, was in a hotel in New York, so I had to find out from his secretary where he was. So I said, "Well, I've got to go through New York, so I'll just stop and give him a personal report." It was fun talking to Noyce anyway.

So we had dinner, and it was over dinner that he told me that there was a likely possibility that he and Gordon would be leaving Fairchild, but that it was extremely secret, that I was not to say anything about it. The word had gotten around anyway, which he thought, to me, was extremely secret. I hadn't heard about it, but that didn't mean that it didn't get around, that they would be leaving to form another company. Not to go to another firm, but to form another company.

So I kidded him about it and I said, "Oh gee, that's nice. Include me in."

"Okay."

Noyce was open. He had very few secrets. And of course, too open to many people.

Then when I got back to the "shop," it rapidly developed that Moore and Noyce were going. Not Grove, but Moore and Noyce. Grove was a later addition to that group. Grove was a later addition because if he hadn't gone to Intel with those guys, Charlie Sporck would have stolen him or tried to steal him for National. He was a fairly pricey character, yes. He was telling you about that?

BROCK: Yes.

SELLO: They wanted him at National to direct their research, which he could have done. I mean, a good man is a good man.

BROCK: Well, maybe just because of the time, we just need to stop here.

[END OF AUDIO, FILE 4]

[END OF INTERVIEW]

INTERVIEWEE: **Harry Sello**

INTERVIEWERS: **David C. Brock and Christophe Lécuyer**

LOCATION: **Menlo Park, California**

DATE: **16 March 2005 [Interview 3]**

BROCK: This is an oral history interview with Harry Sello, taking place in Menlo Park, California, on 16 March 2005, with Christophe Lécuyer and David Brock as the interviewers.

LÉCUYER: I'd like to go back to SGS, and I realize that we didn't talk about one of your colleagues, named Don Rogers.

SELLO: Don Rogers, yes.

LÉCUYER: I was wondering whether you could tell us a little bit about him and his work at SGS, and your interactions with him.

SELLO: I can. Do you want me to do that relative to a specific chronology, or at any time?

LÉCUYER: Any time.

SELLO: Any time. Certainly. That would be part of my SGS story, and he plays a very important role. We can start right there, if you like, as far as that goes.

LÉCUYER: Yes.

SELLO: My first contact with Don Rogers was at Fairchild. He was a very prominent and active member in the Fairchild Semiconductor Marketing organization. He reported to Tom Bay, who was then Director of Marketing for Fairchild in the period of Noyce and Moore.

When I was asked to go to SGS, to transfer, to start Fairchild Technology in Europe, I was appointed by Noyce at the beginning, to effect the transfer of silicon planar and mesa technology to SGS in Europe. I was assigned by myself, alone, as the Director of Operations, in order to get a better feel for what SGS was all about. SGS at that time was a germanium device organization—manufacturing alloy germanium devices on technology gotten from General Electric.

I might as well proceed as if I would be talking chronologically about this, so then I won't have to repeat that. SGS, Societa Generale Semiconduttore, was made up of three organizations, Fairchild, naturally the most important; Telettra, an electronic and telecom instrument organization; and the famous Olivetti, then a computer and business machine organization. It was a triumvirate. Fairchild succeeded, under Noyce's influence, and at that time under Dick Hodgson's influence, in acquiring 40 percent of this triumvirate, so the remaining 60 percent, 30-30, was split between Telettra and Olivetti.

I was interviewed by both parties, by Roberto Olivetti, the son of the founder of Olivetti, and also interviewed by a wonderful gentleman, *ingegnere* Virgilio Floriani. He was the founder and the sole owner of Telettra, this electronic instrument organization, related to the kind of products HP [Hewlett-Packard] was doing. In fact, Virgilio had to be the one to give the imprimatur to me. He came over here to my home in Palo Alto with his wife, where we met. They had had several candidates, a couple of people from RCA at the time, who were under consideration but they didn't make it. They were thrown out by the Italians. [laughter] So I was lucky enough to be accepted.

Being alone at SGS was a little bit of a desert. I mean, I was a pinpoint in an Italian sea, a small stone in an Italian sea. One of the things I had to do, that I felt I had to do very quickly, was to learn the language. I took that as my first problem, and Virgilio helped me. He got me to be taken on by one of the professors of the Polytechnic Institute of Milan, who came to my apartment regularly, once a week, and we studied Italian together. I didn't have to do this; this was entirely on my own. I insisted. Of course, the Italians were very happy that I sought to do this.

Without this knowledge, I never could have succeeded in doing what I felt Fairchild should be doing in Italy. However, being the Director of Operations, I was spread pretty thinly, because the problem was to convert—as Noyce put it very directly and succinctly, "Throw that garbage germanium out, and put good silicon in its place." [laughter] You could put the good silicon in its place, but throwing the germanium out was not very easy, because SGS had been selling germanium devices, diodes and transistors, for years to the European marketplace, and things like point-contact diodes in germanium were an essential ingredient of radio sets. And if you pull that out, whoever has the radio set can't operate it. And here was SGS with an enormous inventory stacked up in their hallowed halls, of germanium diodes and alloyed transistors, not even diffused transistors, but alloyed transistors, the kind that Noyce had been familiar with at Philco, for example. But they were germanium.

The problem of replacing the germanium was really in two parts. One was, how do you move out what's already there, before you have something you can replace it with? Because it counts for business, and they had customers. I had a terrible battle, right from the initial stretch, with the marketing people in SGS, in the Societa Generale Semiconduttore, who wouldn't listen. They said, "Sello, if you can't give me something that I can deliver and say, 'Use this instead of that,' we're not going to do it." So I communicated this problem very quickly back to Noyce and Bay, and the result was the arrival of this dynamic individual, who was close to only five-feet high, maybe five-three or five-four, but a veritable dynamo, Don Rogers. And he took charge of the marketing to be required of silicon that we were bringing in to get that replacement of germanium effected.

The interesting part about Don Rogers was he had a pair of myopic glasses on that couldn't see the word *germanium*; they could see *silicon*. And he had to go to customers and tell them, "You're using, in effect, a sort of a junky product, when you have here a permanent product that can take its place. So why don't you convert to silicon." Well, the customers immediately reported back to SGS that they were not going to be assigned delivery of their products they have ordered. "What's going on, and where is this silicon that you've been talking about?" It's not there yet. This was just within the first few months, and Rogers was out there selling silicon that he didn't have, out of SGS.

LÉCUYER: He was competing with the existing marketing people.

SELLO: He was competing with the marketing people in SGS. I immediately felt, and communicated that back to Dick Hodgson of Fairchild, through Noyce to Dick Hodgson, that we were in trouble here, and how could we convince SGS that they had to go through a valley of lack of use of germanium, even to scrapping their inventory, and promising customers to be replaced by silicon transistors and diodes.

Now, this was before planar silicon, in the beginning, so the devices consisted mostly of silicon simple diodes, the kind that Don Rogers was entirely familiar with, and of mesa transistors, both PNP and NPN. Rogers just raised hell with the marketing people. When I communicated this back, what Hodgson and Noyce succeeded in doing was to order SGS, because of this 40 percent edge, more or less, to have the SGS marketing people report to Don Rogers, and not be independent. That was even a bigger disaster, because Don Rogers had a very simple approach. He said, "You tell the customers the stuff they've been getting, they're going to get what's left in the inventory that they can use, but that's all going to go, so they'd better get ready to have it replaced by silicon transistors and diodes."

"All right. Fine, Mr. Rogers. Where are the silicon diodes and where are the other devices, the transistors?" Well, we had a problem because we could only ship those directly from Fairchild to Italian customers initially, and these were in terrible shortage already, in this new organization.

Rogers, the dynamo, used to jump up and down, practically pounding on tables, saying, "You want me to sell something? Give me something to sell." He'd come to me and he'd say, "Come on, you're the Director of Operations. Shut this stuff off and let the customer starve. When he's hungry enough, he'll wait and get the silicon." That was Rogers, very dynamic.

LÉCUYER: It's a very full transition, right, from one technology to the other, right?

SELLO: Right, totally and completely. SGS had to go through a transition. The single biggest obstacle to that transition was this enormous inventory of germanium devices that SGS had, and the difficulty with that is, it's still germanium, and germanium is unstable. It requires 100% testing—not sample test. So we had to test and re-test literally hundreds of thousands of germanium devices in the inventory, even before we could ship out the finished conventional Ge devices to the customers. They had been in inventory so long they weren't stable, and you can't ship something that the customer's immediately going to test, and may not work. So the process of shipping germanium devices, which I came to learn for the first time, consisted of, you don't ship anything without, immediately testing before shipping.

Now, silicon is the opposite. You don't have to continuously test. You store it, if it's stable coming off the line, and you store it, especially the planar style. It's stable. Of course, we didn't have much inventory at Fairchild of silicon anyway, because it went out practically as fast as it was made. So Rogers started going back and forth to Fairchild from SGS, trying to convince—to get allocations from Tom Bay. "All right. You promised you would send these devices, finished devices. And don't forget, you promised you have to send Sello the chips, because he then can assemble them into product," which is what SGS is really supposed to be doing.

Well, he wanted product from both ends of the Fairchild silicon spectrum, from the starting material, which were planar and non-planar dice, to the finished packaged devices. Now, Fairchild needed them in both places as well, so his battle was entirely twofold. One was, go out and placate the germanium customers, and the second part of the battle was, convince your buddies, through Tom Bay at Fairchild marketing, that they won't be able to get marketing figures, shipments, until they give the Operations guys at SGS the dice.

Well, Doc didn't mind. He worked well in talking with Tom Bay and some of his marketing buddies. That was Don's field. But he didn't do very well in talking to the SGS germanium customers. I remember very well, he said, "It's very simple."

I said, "Don, how are you going to do this?"

He said, "Very simple. I go up to the guy and I'll tell him, 'If you don't stop using germanium, we'll cut you off.'"

LÉCUYER: That's American tactics, right?

SELLO: More correctly, that was Roger's tactics. There was no hanky-panky to this. He said, "We'll cut you off. We've got much better stuff." And he never gave the answer to "when" except a promise. It was hard even to imagine a date. "Well, it won't be any later than six months from now."

"Six months? How am I going to make my little radio transmitter, if you cut me off now?"

He said, "That's not my problem. Talk to the germanium people at SGS." [laughter] And I went with him on a couple of these missions, and I started to point out, when he let me—he led the conversation, because he represented marketing. He cautioned me, "Don't say anything that I don't tell you you should say, because I've talked to these customers, and if you start making a promise, another promise on top of the ones I made—." He was very sensible. He said, "We'll just get nowhere, so don't say a word."

But I'm sitting there and he's berating the customer. I can't even ask him to go out to the washroom and say, "Don, slow down. These guys, we'll lose them. They'll never come back." At that time there were very few competitors of silicon in Europe. There were two prominent ones, Motorola and TI [Texas Instruments], and they were delivering more silicon in total than Fairchild was, worldwide. So it was conceivable that if a customer like Nixdorf, for example, who is one of the big ones in Europe—at that time he was not that big—would go to Motorola.

The wonderful thing that they would love to hear is, "We can't get anything from Fairchild. How about you selling us some product?" My God, it'd be a disaster. And Motorola had a plant in southern France, and TI had a plant, on silicon, in England, and here we were coming in with these new devices of silicon through SGS, just starting up with us. So Rogers had quite a problem on his hands, but he was certainly up to the problem. I've never seen a guy who could berate a customer and then go out and have a drink with him afterwards, and make a friend of him.

I said, "Don, you know, the least you could do is learn a little bit of Italian."

He says, "What do I have to know Italian for? You do the Italian." He said, "All I have to know is how many fingers on one hand, and *tassa di café*," a very favorite comment of his. [laughter] I couldn't get mad at him, and laugh at him at the same time.

So we spent some months working our way through the problems of obsoleting that enormous sick inventory of germanium, actually throwing it out, even—stop testing. You start testing, that means you're going to have to keep shipping. We stopped testing. "Tell the customer we've got an enormous problem, and no transistors." He said, "I can do that if I have a few silicon parts," said Rogers. "If I can't give them something to keep a promise, they won't even let me in the door."

So his promises were marketing types to pacify the customer and placate the worry. The SGS customer already had known of the wonderful Fairchild devices that were coming, but there was no way of getting them other than pirating shipments directly from the United States. But that would not work, because then there would have to come another transition when such customers looked to Fairchild California for the delivery of parts. Rogers and I had to convince them, that now they could shut that source off and get the same thing from Italy. That's another shutoff. We're shutting them off from new germanium in the first place, and now we're talking about the fact that we have to shut them off from the stuff we promised that we're replacing. It was a terrible, terrible marketing problem—and resulted only in losing SGS customers.

I don't remember exactly how long Rogers stayed in Italy. He was called back because he was needed very badly in the US. Another fellow took his place, a fellow by the name of Chris Coburn, who was an excellent English salesman, operating out of the SGS London Office. He knew the European mentality, but he was still a Brit, and the Brit mentality did not go together with the Italian mentality at that time, as far as marketing is concerned. Fortunately, we were lucky to have Corrado Innocenti, our Italian SGS counterpart to take over the SGS marketing effort.

On the manufacturing side, the problems that I had, there was not that same clash. Technical people speak more of the same language, and since I could address their problems in Italian, I could keep the customers' purchasing agents happy, as well as their engineers as to how their SGS parts were working out. Both Rogers and Chris Coburn had a separate problem. Neither of them handled the language, and I think, in retrospect, that was a very serious problem. We should have solved that one. We should have forced these guys to learn the language, pay for their lessons, do whatever we had to do, just force-feed them this sort of thing, and not let Don Rogers get away with, "Oh, what do I need to know?"

Don also liked to tease the Italian users, "I can handle the funny money, these bedsheets." They were these big 10,000 lira notes at that time. He said, "These bedsheets," and he'd uncoil a whole roll of it, say, "That's all I need to know, and I can handle that. They're big enough to see." [laughter] A very dynamic, but a very shrewd marketing person, with a big sense of humor, who knew very well not to make promises. He had learned that in the bitter USA marketplace.

For a technical man like myself, when I couldn't quite see the production problem, I could always say, (in Italian) "Well, we have this problem, and this problem will be solved when we get the new testers in, and then from there on it'll be your priority." So I can hide behind this kind of thing, plus language.

Does that give you some background?

LÉCUYER: Yes, absolutely. Thank you.

SELLO: Does it fit to your—

LÉCUYER: Yes, absolutely.

SELLO: —Rogers' mentality?

I wanted to say one more thing about Rogers. He is about five-three. Tom Bay, his boss, was about six-four, and to see the two of them walking along in the halls of SGS was a sight. You know, the Italians marveled at it. Who are these, in our terms, Mutt-and-Jeff-type characters? "Which one is the boss?"

"Well, the little one."

"That's not possible. It's got to be the big one." [laughter] All of this creates a different aura.

BROCK: Just a couple of questions occurred to me from what you were just telling us, Harry. One was, I was wondering about your competition in silicon in Europe in this period, Motorola and Texas Instruments. Did they set up subsidiary companies, or did they do joint ventures like yours?

SELLO: No. They set up subsidiaries, unfortunately, for competition to us. Motorola had a big subsidiary in France, the south of France near Cannes, of all places, and TI had long been established in germanium in the south of England. Then when silicon came along, it was their type of silicon, which at the beginning was a grown junction, silicon, these little silicon bars that they cut from crystals—they actually had an independent organization in England. They even had an R&D organization in those days, in silicon, in England.

Now, they were no tougher a competition in Italy than TI was in the US, against the mesa, later the planar devices. But in Europe they had an established foothold, and they could point to the fact that, "We make silicon at our plant." And that was a very tough competition, when we had to force ourselves to tell them that we are shipping in dice, prepared dice, which you can do with silicon mesa and planar devices, and just assemble them in Italy.

BROCK: Then going back to your comment about having to re-test all that germanium stock that was around, what was the cause of the instability of those germanium devices? Was it the reactivity of the material?

SELLO: Yes, I should have mentioned that. Yes. Germanium doesn't have an oxide. It can't be passivated like silicon can, and the germanium junctions are sensitive at lower temperatures than silicon, and also the junctions are exposed and they're susceptible to attack by moisture.

BROCK: All right. That's what I was wondering.

SELLO: They will just become unstable, even after being tested. Also sitting around for a long period of time, impurities permeate into the germanium devices. So if we had germanium stock that was tested less than one month ago, we could ship that, but otherwise we had to re-test. And also the customer had to re-test on incoming. So the instability of germanium was very evident in the germanium point-contact diodes and alloyed germanium transistors.

LÉCUYER: And then the devices would fail in the product, right?

SELLO: Then they'd fail in the product, yes. And, of course, if the device failed in the product, it's not the fault of the product. It's the fault of the shipper of the device, when actually the product itself may have contributed to that. For example, a point-contact diode is very susceptible to vibration. All it is is that little tiny point, and if that point gets rattled or breaks loose, the diode will fail. Well, that could be due to vibration at the point of use, so a transistor radio, for example, not the tiny ones, but at that time modestly large, if it were shook up and bounced around, diodes would fail very frequently.

Of course, that problem on such diodes came right back to SGS when they were sold by SGS. "What's wrong with these things? Why can't we keep our radios going?" So they'd have to get another shipment, and these would be re-tested again, and then the reject rate was enormous. It didn't even pay to re-test them, because you knew that if you couldn't ship them out immediately after re-testing, you're going to have to test them again in another month or two. So it was a very considerable problem that confronted Rogers, and when he looked at the inventory and said, "Forget it. Throw it all away. We can't bother with that," he was right. He was right, but we would have had to shut down SGS' marketing organization. They just refused to do that. After all, the two together, Olivetti and Floriani (for Telettra), were 60 percent; Fairchild was 40. That led to other problems, of course, within the triumvirate. But I just wanted to mention Don Rogers.

Interestingly enough, and a very colorful point, I have to tell you at least one incident. His family, his background—his wife is Italian, and his two kids, young girls at that time, took about one month to learn Italian. So his family spoke Italian fluently, so whenever I saw them, I would use my Italian with them. We had a good time.

But Rogers never learned it, and that can be illustrated by a little incident. At one time the Rogers family and the Sello family went down to Florence, to Firenze from Milano, on the freeway, on the *autostrada*. And Rogers drove. Now, there was nothing he couldn't do. He might have been small in stature, but he was big in intellect. He took over the driving, and he drove on the *autostrada* from Milan to Florence. Along the way, he ran through a detour where there was a traffic light, and to Don an Italian traffic light meant nothing. He could see the color, but so what? You know, "It's just an impediment to my driving." He used to say, "The Italians don't pay any attention to them. Why should we pay attention to them?" So he drove through the traffic light.

But shortly, right after that, two motorcycle cops came along and stopped the car and pulled him over to the side, and Don said, "Oh no. What do they want?"

And his little daughter told him, and said, "Dad, you ran through a traffic light."

"What traffic light?"

"Right back there." And I was sitting in the back, and I didn't say anything. Don was the driver.

So I said, "Don, we're Americans. We don't know what's going on, so I'll let you handle it." [laughter]

So the motorcycle cops came up and we lowered the window. We used our general, our usual ignorant approach. "Well, we're stupid Americans. We want to go to Florence. What have we done that's wrong?" And that was Don's motif, and it seemed to work a lot of time. "What can we do, officer? Is there someplace—we have to get down to Florence by so-and-so. Do you think we can make it?"

Of course the Italians would launch into a lecture about the fact that "You can, but at the same time, you ran that traffic light."

"Oh, what traffic light?"

And this little kid sitting in the back says in Italian, to Don and to me, she says, [Italian phrase], "I told you about him. You passed it," in Italian. [laughter] And the Italian guys, the officers are there.

Don says, "I don't understand the language. I can't tell where the traffic lights are."

And the Italian cops, bless their hearts, they bust out laughing. They said, "You ought to listen to your daughter." [laughter]

"Oh, she always talks in this language, and I don't understand it, and she doesn't really understand it." He talked his way out of it, but the cops just understood completely.

His family loved it there, but he couldn't stay. He had to leave, to get back to marketing in the States.

LÉCUYER: He was sent to Hong Kong at some point, right?

SELLO: Yes. Then he was sent to Hong Kong. He did a lot of international travel for the marketing people.

BROCK: Was Tom Bay in charge of marketing and sales?

SELLO: Tom Bay was in charge of worldwide sales and marketing for Fairchild.

BROCK: Throughout this period.

SELLO: Yes, throughout this whole period.

BROCK: Could you tell us a little bit about him and what you think his main contributions were?

SELLO: I would say that a combination of his technical understanding of silicon devices, plus the ability to discuss the application with customers, their application of those devices, was at an extremely high level. So he understood the problems, and quite frequently there were other marketing guys that he had that could do similar things.

He was without peer in being able to go in and do two things, explain the application and what might be going wrong that the customer was unhappy about, or to explain the application in terms of the silicon devices, as to what they could do relative to the ones they were presently using, faster, more power, etc. He understood that application, plus his understanding of the customer. He was just as smart as Don Rogers in understanding the application to the customer, but he was not as pointed as Rogers in telling them about that. But his talent in the ability to explain our new devices to the application that he was consulting, for example, even to IBM, why they should, say, for example, use small-volume, small silicon transistors for their application, instead of what they were already using, that is, the Fairchild specifications, was really without peer.

He was pretty good at running his organization, but he had a kind of a *laissez-faire* attitude to his marketing people that they loved. They could go out and, you know, guys like

Jerry Sanders, who at that time was a marketing man reporting to Tom Bay, just loved that kind of interaction. They were a team. "I know your problems, and then you've got to listen to me," type thing.

BROCK: I was just going to ask about Sanders and your impressions of him and his contributions.

SELLO: Sanders was a much more flowery, colorful person than either Tom Bay or Don Rogers. Sanders got his start in marketing in southern California, and he fit the ambience very nicely; Cadillac convertible to drive around in and so forth. So he made use of all his tools, but he was a technical genius relative to the applications of silicon devices. That emerged even more pronounced later, when he started up his organization, when they broke away from Fairchild. He was a cracker-jack salesman who thoroughly understood the new applications for mesa and planar devices.

He was very good with the applications and customers, and he understood the devices. He was technically very, very sound, much more so than Rogers, for example, a different area, much more complicated, and probably even more so than Tom Bay, although they were neck and neck with this kind of application. But his flamboyancy and his outward attitude of keeping the customer happy, he could talk you out of your shoes type of thing. He was not quite a Phineas Barnum, but almost.

He also had the ability to command loyalty from his people. They were extremely loyal to him, and that showed up later when he formed his own company. The same guys that reported to him when he was in charge at Fairchild he had no trouble taking with him. Now, when you did this, and they're mostly marketing strength, you could immediately get to the customers, even before you could make your own devices. So you could start generating promises, and you understood their problems, and suggest alternate ways until your devices came along, so in the early days of AMD he was able to command that kind of loyalty from people that went with him to AMD.

It was similar in the case of Charlie Sporck. The nucleus of men that went with those people, with Jerry Sanders to AMD, were mostly strong marketing guys. There were some technical guys, of course, that they interacted with. The nucleus of guys that went with Charlie Sporck were production people, the same strength that he exhibited at Fairchild. Sporck also captured the technical designers who invented the linear integrated circuits at Fairchild—like Bob Widlar and Dave Talbert.

The team that Gordon Moore and Bob Noyce, also with Andy Grove a short time later, they assembled, were really the technical guys, really technically skilled in the manufacture and in the development of new structures. They had to acquire good marketing people. They got a few from Fairchild, not very many, not as many as, say, Jerry Sanders was able to take with him. But they immediately went out after more technical people, because they were trying to introduce new devices at the same time, new devices other than the Fairchild devices, with which they were in parallel.

That's a long-winded answer to Tom Bay, but—

BROCK: That brings us to this period of 1967 and 1968, where you have these departures of key people and key groups from Fairchild.

SELLO: I'm looking at the end of the previous—we ended with the Shockley organization.

BROCK: There's another transcript that we're still waiting on for our second interview session, where we went up to about 1968.

SELLO: Oh, so we covered Shockley.

BROCK: We did cover Shockley, yes.

SELLO: Excellent. Excellent. I was prepared to—I was reading from this.

BROCK: Right. Our other discussion is still being transcribed.

SELLO: All right. So you have the period of my leaving Shockley—

BROCK: Yes, and joining Fairchild, and your early work at Fairchild. That's the stuff that's getting transcribed now.

SELLO: With which the SGS was involved—did we talk about SGS?

BROCK: We did.

LÉCUYER: A little bit, yes, and that's the reason why I came back to it.

SELLO: Oh, and Don Rogers, all right. Now I'm on the same wavelength with you. No problem.

BROCK: So it was in 1967, right, that Charlie Sporck left to go to National.

SELLO: He left for National with a small group of production people, the ones that he had led while he was at Fairchild.

BROCK: Were you back in California at that time?

SELLO: I returned. Maybe I mentioned it. Gordon came out to SGS in late 1964 and told me he wants me to come back to Fairchild.

BROCK: Oh, I don't recall that part. Could you tell us that?

[END OF AUDIO, FILE 1]

BROCK: We were talking about the effect on Fairchild when Charlie Sporck and his group left to reinvigorate or reinvent National.

SELLO: Yes. National became a new company. Previously, Charlie had originally been at Sprague Electric, before he even came to Fairchild, so he knew the eastern wing of the production. I guess Sprague people actually were partly financing National. I don't remember exactly. You'll have to correct that. There was some financing from Sprague, who invited National and Charlie to become silicon people. I'll have to check that out. I believe it was Sprague, but I'll catch that in the edits, I'm sure.

LÉCUYER: I think it was financed by one of the Sprague people, but it was not the people who controlled Sprague Electric. It was a different branch of the family.

SELLO: A different branch, yes. But it was a different branch that never did get into silicon, but seized the chance with Charlie as the manager. Now, Charlie had worked for Sprague in a production job, sometime before he came to Fairchild, so he was known to Sprague, per se, although not to the top management, the same people that helped him start National. So he took with him some of the key Fairchild manufacturing people. Some of these people were ones that had been members that I had worked with during my Preproduction Engineering and Process Engineering days at Fairchild, before I went to SGS. When Charlie left, he was General Manager and headed a strong Fairchild manufacturing organization.

So they immediately started to manufacture transistors and ICs in competition with Fairchild, and even more than that, more importantly than that, Charlie took with him some key guys who had designed and invented linear integrated circuits, that is Bob Widlar and Dave Talbert. They joined Charlie; Pierre Lamond, former Device Development in R&D, John SEntous, Ed Pausa, and Greg Harrison of Fairchild Manufacturing.

BROCK: Oh, yes, I've heard his—

SELLO: And he started independently manufacturing, in competition with Fairchild, these linear integrated circuits. Operational amplifiers was the big one, and they did a very good job of it, and got some immediate, good position in the marketplace, and, of course, competed with Fairchild. Naturally, Fairchild lost market share as a consequence of that.

Charlie tried to convince, as the story goes, Andy Grove to join his organization, but Charlie had no place for R&D. That was not his *métier*; production was. He never had an R&D organization as such, and he prided himself on the fact that he didn't need it. He said, "If I need to, I'll find the right guy, and hire him," a fact of the industry that you couldn't deny.

He did hire several guys from Fairchild R&D who'd been under Noyce and Moore, not particularly from my materials and processes guys, it turned out, fortunately, but from the applications engineering group, fellows like Tom Klein, who some say, and I tend to agree, was really one of the originators of the silicon gate process at Fairchild. And they really got going, and quickly, into the linear integrated circuit end, supplanting Fairchild in that area.

BROCK: Was it very disruptive to the Fairchild organization?

SELLO: Very much so. We in R&D in the Materials and Processes group, for example, were struck by it. Noyce and Moore certainly had made a special effort in the area a few years earlier, to get a guy like Charlie Sporck onboard to handle manufacturing problems, volume manufacturing, which was a skill that was not present at Fairchild before Charlie got there. So it was very disruptive and it created a gap. There were guys working in manufacturing, but in 1967 it was only a year before Noyce and Moore themselves left, so we were hurt by that.

BROCK: Did Sanders leave before Noyce and Moore, or right after?

SELLO: Right after.

BROCK: Could you talk about, then, when you learned about Noyce and Moore and Grove, and I guess several other people, when you learned about their departure to form a new company, and the effect of that on the Fairchild organization?

SELLO: Well, the effect on the Materials and Processes in the R&D organization was very profound. Moore was the head of R&D, and a lot of the people, like myself, who had come to work there, were there because Moore was there; Noyce as well, in my own case, because I had worked with Noyce before that. So the Digital engineering research group in Fairchild R&D, Dr. Bob Seeds, now deceased, stayed with Fairchild—it also was a strong effect on guys like him, but the design of Digital integrated circuits remained very strong.

In processes, I was very saddened by it. I saw Bob Noyce personally in 1967. I was then returning from SGS on a mission—maybe I told you about that—and Noyce said, "I want you to make this trip to the Soviet Union." And at that time he told me he was leaving.

And I said, "Hey, Bob, when's all that going to be?"

He said, "I can't tell you the details." It turned out to be a year later, but it was quite disruptive. I was really shook up by it, and if I hadn't been on several projects that I had to work on very quickly, I might have gone with the group.

There was really no one left at Fairchild that could come up and say, "I want you to stay," you know, that kind of thing. I made it known that we would be affected, and so would some of the people in the group. As members of my group, none of them at that point went with Noyce and Moore. Dr. Andy Grove was in the Physics group, so there was no question about him. Actually, Gordon Moore was alert enough to support Andy Grove as Assistant R&D Director in 1967 in order to keep him from being pirated by Charlie Sporck.

There were some interesting immediate effects. There was a fellow—the fellow who took the place of Moore in directing R&D was Jack Kabell. Have you met him?

BROCK: No, I haven't.

SELLO: Jack was at that time interested more—his main interest was in some aspect of linear circuitry, as I recall, and he had been in the device side, and he was immediately appointed to take charge of the research organization. Was I offered the job of taking over the research organization? I guess it came close. But I earlier had said that I did not feel that I was the right one to do that, because I was a process developer and not a device designer. Jack Kabell was a device designer.

I immediately kept charge and held together as much as I could the Materials and the Processes Department. They were all very shook up, and I had a hard time to convince people that they should make their own decision. Materials and Process would continue. Guys like Bruce Deal might have been seriously affected—have you talked with Bruce yet?

BROCK: No.

SELLO: Do you plan to?

BROCK: I would like to, yes.

SELLO: He'd be a worthwhile guy to talk with. I wouldn't say that he's as garrulous as I am. [laughter] Bruce, you might have to pull it out of him. Jack Kabell and I got together and we agreed that we'd lost Grove from the Physics group, and Bruce then was in the Physics group. I had talked with other members of the Physics group, to find out where they stood. Bob Seeds and his Digital Engineering Group agreed they'd stay with Fairchild and see what came up. This was a new thing and they wanted to stay with that.

Kabell asked me to step out of being Department Manager for Materials and Process, and take on the job of technical planning for R&D; as Manager of Technical Planning, that was the title. One of the reasons, which was one I agreed with, was that he made Bruce Deal immediately my replacement as the head of materials and processes, in order to keep Bruce from being picked up by Moore and Noyce. And it worked. Bruce took over the group, and all of the guys that reported to me now reported to Bruce.

I was a one-man department at that time. Let's see. Not quite, because I retained an element of control on the mask-making efforts that had started and Fairchild, and so Sam Fok, Dr. Sam Fok reported to me in that capacity of the mask-making aspect. So since I had recently returned from SGS, the technical planning was a needed function. There was another part to it. We saved Bruce Deal, but we also—or the thing that I had worked out under Moore's direction, and he felt that way, was to supply sustaining engineering to the manufacturing group.

That was a part of process development and process engineering that was desperately needed by the manufacturing group, the ability to pick a guy out of that group and send him over to manufacturing when manufacturing had a problem, and have him reside there, not just troubleshoot, and have him reside there, was something I had developed under Noyce's encouragement, partly as a result of some of the SGS work as well, in order to save their skin when they ran into problems. They had a myriad of problems in manufacturing, particularly with guys who were leaving out of manufacturing.

So Bruce was not too keen on that kind of work. He was more of the individual scientist. He was working on the problems of sodium, of trying to identify why were UPN junctions, MOS PN junctions leaky, which he did very well.

BROCK: Was part of the dispirited atmosphere in the R&D laboratory—a couple of people have mentioned that for people working in the R&D organization that Gordon was really respected—

SELLO: Yes.

BROCK: —that his insight, I guess as a general technical leader for this group that, as you said, had technology coming out of its ears, he was really—there was a lot of respect that people had. So was it just uncertainty that his departure—

SELLO: Great uncertainty, great uncertainty. Gordon was the organizer of the entire R&D organization, in the first place, and within that organization of Materials and Processes, which he asked me to come back from Italy to lead, and to do two things under my leadership. That was to get the new processes going that we needed to have; that is, sustaining engineering, i.e. assistance to the production group.

Now, with Gordon present, I had much less trouble trying to convince the Ph.D.'s who reported to me in Materials and Processes to go over to manufacturing and help them solve problems. We had good men like Dr. Ross Tucker of Illinois, now deceased; and we had Dr. Bill Shepherd, on the epitaxial deposition of silicon; we had Bruce Deal, specialist in oxidation of silicon; Bernie Yurash, who was the analytical chemist. We had two very brilliant materials scientists, Ilan Blech and Gene Meieran, both Ph.D.s, in the group. Meieran was a materials scientist from MIT. Ilan Blech was a metallurgist/materials scientist who came from Haifa University at Tel Aviv, but went to school at MIT. We had Art Lewis, a Ph.D. in physics, who also left shortly after Gordon Moore left.

All of these guys, all but Bruce were very easily influenced to go and do sustaining engineering process support, and, of course, I was convinced. Gordon didn't have to convince me about that, because that was where my métier fell, too. We had desperately needed that. This group began to feel very queasy about, "what's going to happen to us". I said, "Well, the work is needed. The problem, the technical stuff is on the shelf."

"Yeah, but Moore isn't there."

"You're not telling me anything I don't know." So to talk with these guys and keep them pacified as to problems, the only thing we could do, the only thing I felt we could do was concentrate on the problems at hand that we really had to work on. There were a couple of those problems that turned out to be very significant ones, and the ones for which we got to be known in the industry. One was the electromigration of aluminum. I guess I mentioned that before.

BROCK: Yes, we did.

SELLO: The other one was the cracking of metal, cracking of aluminum rows over sharp oxide steps. Did I mention Ross Tucker? I think I did.

BROCK: You did.

SELLO: Ross knew more about the surface effect of ions and atoms on silicon than anybody around in all of Fairchild, including Gordon Moore. Several of these guys later also left and went to join Moore—Tucker, Meieran. Blech returned to Israel, his home. He came back to the U.S. and started a small metallurgical testing company.

LÉCUYER: In the late sixties, right?

SELLO: Yes, right in the early sixties. I don't know, I never have asked them, whether they were approached by Moore, but I had the distinct feeling that they went looking for the job. Now, I came to my own dilemma. Why did I not go with Moore to Fairchild? Well, I've asked myself that question a hundred times, at least. Two reasons: one was, I really wasn't directly asked. And the reason I wasn't directly asked, Noyce told me, was he was afraid of a lawsuit. He said that would have crystallized it.

There were discussions about a lawsuit during that period, but they never were really pushed by Fairchild against Intel, and Noyce felt that this was one of the—not the, I can't say I'm that important—the contributing incident. But it would have been—I and Bob Seeds, for example, who was the head of Device Engineering, in parallel to what I was doing in processing, who also did not go to Intel, were in the same boat. I talked with Seeds about that. We felt the same. We could not be asked because of the lawsuit. Did I go around and say, "Hey, I want a job"? I didn't do that.

There was a second reason, and that was because I was snapped up very quickly into the International effort, into technical planning for the Corporation, by President Les Hogan, who then appeared, and I moved into that very quickly. I became the Manager of Technology Transfer and Planning for all of Fairchild, not just the R&D group—reporting directly to Les Hogan.

BROCK: I definitely want to ask you about the arrival of Hogan and Corrigan, and the other Hogan's heroes, but this job that you took up in 1968, of technical planning for Fairchild R&D, what was involved in that?

SELLO: The major effort of that was that, besides the use of the R&D-type people in production, the establishment of Fairchild Technology elsewhere, to look at the whole Corporation and its Technology to see, how could we use that technology, as we did at SGS. That's why it fell to my lot. As we did at SGS, could any of our technology be used to transplant into other markets, into other places where Fairchild would have to operate? That was the seed. They asked me about that. I remember Noyce did. "Do you think you can do what we did at SGS someplace else?"

And I said, "Sure. It's going to take an effort, because we have to use the people." When they left and I was asked to do the Technical Planning for the corporation, I could see that this type of structure is going to be difficult to operate, because it needs supporting technical people. I can't, of myself, go over and say, "We'll establish a process, or work a process in your shop, in your plant someplace else," like in Japan, "and see to the implementation of that process." We need a team for this.

I established a small team, in effect, for Technical Planning, to go into the foreign marketplace somewhere, to define a customer, also including elsewhere in the U.S., establish a transfer team reporting to me, and then establish that technology elsewhere.

I felt that the manufacturing organization, be it the MOS, bipolar ICs, CCD, or discretes, should appoint technical people, maybe one or two, the same way I did at SGS, to be assigned to me on a temporary basis; to define the project, set up its rules and organizations technically, and then use those guys from that organization to actually transfer the technology from the manufacturing organization.

We actually did this. We told the bipolar integrated circuit people, "We want to establish the Tungsram operation in Hungary." I found the customer and negotiated a contract with them. I and our lawyer Roger Borovoy talked to them, gave them all the things they needed to know, the design, and we formed the team that went over and did the transfer, and stayed with it until the process was transferred. So that's why I felt I was needed at the central point of this. I mean, we only did one step in the transfer, from the manufacturing organization directly to the user—no intermediate transfer was needed.

Now, interestingly enough, I did that for SGS, my old buddies, who by that time themselves had changed a little bit. They had added a new member, a group in linear integrated circuit that had been under RCA, in Italy. They still had a triumvirate going called SGS-ATES. Fairchild was replaced by this group. Well, I went back to SGS and we transferred two processes, one on bipolar digital integrated circuits, and one on MOS microprocessors; the Fairchild F-8 family. My team transferred that from us to SGS-ATES, and I administrated the contract, now under Hogan. In both instances, I personally carried out the work with the US Department of Commerce, to acquire the necessary export licenses.

Now, that took up my time quite freely, and I continued with that work, and it was so much fun that I wouldn't have thought to go over to Gordon Moore at Intel and say, "Gordon, I need a job." Once in a while, I thought of getting Gordon to suggest, "Hey, let's establish this same group, only do it in your organization." They were too busy. They were too busy getting their new MOS devices into production; the memories and microprocessors. Interestingly enough, they did carry out one technology transfer on 1103 memories with a Canadian company in order to establish an alternate source for that product.

I shifted a couple of people into my group to become a four-man group, who came out of Materials and Processes work in Fairchild. They weren't Ph.D.'s. They were experienced process engineers, and they were just right for the mentality for process transfer.

BROCK: Was it soon after Hogan and that crew arrived that they—

SELLO: Yes, that we established this? Yes. Of course, I met Hogan as soon as he came. He came right after Moore and Noyce left. I met him very early on, and he was an impressive guy. Shortly thereafter—this was, again, in 1968—I met Wilf Corrigan, who was at Motorola with Hogan. He had been in charge of bipolar discrete devices. I met Wilf, and he also convinced me that I should stay there and do this process transfer planning, which I had tried on Hogan first.

He said, "Well, what do you want to do at Fairchild? You're no longer in R&D. What do you want to do?"

I said, "Well, this is what I feel we need to do. We have tons of processes and device knowledge on the shelves that we can't use, won't use, that we should cash in. We should use those, for example CCD—"use that plus any of our existing processes, simply to get into the new foreign marketplaces, not just to sell the technology."

And this was a continuing battle that I had inside of Fairchild. There were people who felt, "Why are you selling technology? We need it." We got money for the technology. That was not the driving force. The driving force was to get a share of the market plus money for the technology.

LÉCUYER: So then you would negotiate an agreement whereby—

SELLO: I'd negotiate an agreement whereby we would supply devices and technology, and we would get a percentage of their sales. It worked. It worked in Hungary and at SGS. They didn't like it, but it worked.

LÉCUYER: So essentially, in the agreement you would say that, "We are going to give you the technology on the condition that 50 percent of your sales are going to come from our shop."

SELLO: Or some percentage will come—yes, in addition to cash. Of what you sell as a consequence of this technology, will be given as first choice to Fairchild to sell. We couldn't command the sales. That'd be unfair practice. But, for example, in the F8 microprocessor, we transferred that to SGS and we said, "For your sales of the devices made from this process, the F8 microprocessor, we would like something like a 10 to 20 percent return of second sourcing for your sales, that you would give us the first call on the devices that you sold resulting from that technology, and if we wanted to supply it, we could, or if you did we would like a share of that."

LÉCUYER: So then you would sell these devices as Fairchild devices, rather than SGS devices.

SELLO: Both ways. We would be second source, alternate supplier. What happened when a company introduced a new device, the first question that was always asked, "Well, who's an alternate supplier? Because if you're new, you can't supply it. We'll be out on the limb if you go out of business." The device—well, the alternate supplier would be Fairchild. So we had a double-pronged technique.

LÉCUYER: So you finally said, well, basically, in that agreement they would be second sources, disclosing that the special devices would be sold as special devices, as second-source devices within SGS devices, right?

SELLO: Right.

LÉCUYER: On the other hand, you are also disclosing that some of the devices sold as SGS devices would be also manufacturing in California. Am I right?

SELLO: No. That part we didn't do. We didn't manufacture that, no. But in a couple of cases, particularly at the Hungarian client that we established, we insisted that we are transferring bipolar integrated circuitry, simple bipolar integrated circuitry. Now, 20 percent—it started at 25, and dropped to 20 percent—of whatever devices you sell in your marketplace—

and it's in Hungary and Eastern Europe; at that time that was all they were handling—20 percent of whatever you sell from this process you will buy from us, you will pay us for, in the original contract for the transfer of the technology.

Now, we pushed that one a little harder than that, to say—and this just worked out during the negotiation—not just of the devices that you make from our technology, but additional bipolar devices that we did not transfer, but we know you need an alternate source for. We want to be the alternate source. So it spread out a little bit, depending on the person. Now, alternate sourcing was a very frequent tool used by any new device manufacturer to allay the fears of the user. So that was the motif. That Les Hogan jumped on with great glee.

BROCK: And the strategic thing there was that you're building markets for both devices made using Fairchild technology and direct sales.

SELLO: Yes.

BROCK: So not only are you increasing direct sales, but was it that you were building connections to those customers?

SELLO: Exactly. Exactly. And, of course, part of the agreement was that, any of these agreements was that if the receiving body felt that it was no objection to them, they could recommend us for devices that they themselves weren't making that Fairchild was making in other areas, but that would be purely a recommendation. That could not be a matter of contract. That would be restraint of trade. We were very careful about that.

There were two ends to be careful. Restraint of trade was one of them, and the second was not to sell technology purely for money. That, today, is still an amazing battle that goes on. "Why are we selling technology to China? You're selling your birthright, and just for money." Well, you can get a lot of money if you want to do that. There's nothing wrong with the money. But in those days, the market growing the way it was, we insisted on share of market, provided we met the specs and all regulations.

LÉCUYER: So we talked about agreements in Europe. Did you have agreements of that kind in Japan?

SELLO: Yes, we had agreements in Japan with several companies. We had agreements in Taiwan. We even had an agreement in South Africa with Philips, who was making diodes in South Africa, and they were not able to supply the kind of diodes that South Africa wanted, but we had the technology for, so I sent one of my engineers down there. It was a simple enough project for which I helped negotiate the contract, but he went down to transfer the diode technology from the Fairchild manufacturing organization.

We had a string of various companies who purchased our technology. Some of the technology had not yet seen the success that it did later. For example, CCD technology was developed at Fairchild R&D with Dr. Gil Amelio, and Fairchild built a CCD operation as part of their manufacturing unit. It worked, but it wasn't too successful as a Fairchild product. But even when that was just getting started, I wrote a contract with a Japanese firm to transfer some of the first results of the production technology to them, under the same terms. Some of these turned out to be technology exchanges for some technology that we wanted to get.

A big one of that type was one that was worked out with Philips Europe, based in Eindhoven. Our first contract soon after Dr. Hogan's arrival was a great big exchange contract with Eindhoven, where we would sell them our MOS circuit design technology in return for their manufacturing technology. Now, we had trouble with manufacturing in MOS, mostly because of the loss of the Fairchild people, and also because it was new. What we suffered as a consequence of losing Moore and Noyce was the MOS silicon-gate manufacturing technology. We had it in R&D. It was in my Materials and Processes group under Dr. Bruce Deal. It was in Dr. Bob Seeds' group in digital device engineering. But we never got it into manufacturing,

LÉCUYER: So if we go back to your program of transferring technology as a way of opening markets, was this coupled with licensing of the patents?

SELLO: Yes. Where there were patents involved, we had to work on an agreement. Usually the way that worked was, especially with people like IBM who loved to write patents, "Look. We have this whole string of patents. We can make that available to you, and what have you got?" You match patent libraries. But that was not the motif, not to get access to other patents. If there were patents involved within the transfer technology itself, those needed disclaimers on both sides, and with Philips we had to work out an agreement to this with the lawyers stepped in to help, guys like Roger Borovoy, and Alan McPherson, guys who have become famous in their own rights as patent attorneys.

They're still patent attorneys, and both are wonderful, great friend. Roger was the one—this is a side issue—under whose direction I helped defend the Noyce patent on the integrated circuit, the metal over oxide structure of the integrated circuit. Alan did a fantastic job on winning a patent suit for Fairchild's Isoplanar structure.

LÉCUYER: So maybe the next type of discussion would be to talk about financial returns from this effort, right?

SELLO: Right.

LÉCUYER: Licensing, and at the same time transfer. In the past I talked to Fairchild's general counsel about this. They told me that the licensing fees made the company profitable.

SELLO: Yes. Yes. There was one I was especially very proud of. The example was bipolar integrated circuits, simple bipolar integrated circuits. We wrote a contract in Hungary, of all places, with the Tungsram Company. Tungsram was the GE of Eastern Europe, still behind the Iron Curtain at that time. Two things happened. One was, that's the only permission ever given by the Defense Department and the Department of Commerce for an export license of technology to an Eastern European country; and the other was—we succeeded in writing the contract.

BROCK: Across the board?

SELLO: Across the board, I mean, for that bipolar device process. I ran to Washington so many times I wore a path. It was my personal responsibility.

BROCK: What do you think accounted for that?

SELLO: My magnetic personality. Not solely that, certainly, but also my strong technical background. I had to persuade, talk about the worth of it to the US, also the personal intervention of others, especially of President Les Hogan, to come to Washington and to say this was the sort of thing that we had to do, we should be doing, to put his weight behind it. But that never worked anywhere else, to my knowledge. But it was really the constant harangue of just exactly what we're doing, how we're going to do it, as well as the control and visibility we would have. In fact, I had to write the conditions of the granting of the license that Commerce had to turn around, that should have written in the first place, to tell me about.

BROCK: Oh, to the regulators.

SELLO: To the Department of Commerce and to the Department of Defense.

LÉCUYER: Which is great. It's great to be in that position, right, to write the rules—

SELLO: That's right, write the rules. And, of course, well, you wrote you're going to send so many dice at so many periods of time. "How do we know you're going to do this, and you won't send something else?"

I said, "Well, you don't know. I'll be violating the law if I do that, and if any of that comes to light, it's like murder. It's always—it's retroactive. And here is the system we're going to use to show that we can demonstrate at any time, to your satisfaction, that this is exactly what we're doing." I wrote the report saying we were doing this, and we were, like measuring the dice, counting them, I mean on the wafers. And no one ever came from the Department of Defense, in ten years. No one ever came to look at it. I sent reports and that was it. Paper was all they wanted. [laughter] It was idiotic. Now I laugh at it, but it was a ton of work we had to do. We had to count dice in order to say, "Well, we sent these. This number was used. This number was rejected."

We took back the rejects also, so we could identify the devices that were made from the good dice, plus those that were rejected, and we sent that in a report. But no one ever checked it. No one ever asked me about it. It worked for ten years, we got a million dollars for that. Each month Tungsram would make a payment on this million dollars, and the deal that I worked out inside with the Bipolar people, with George Wells and Ed Browder, Bipolar IC Manager, was, "Look. We're going to be getting that million dollars. I have no Department that needs it. But that payment will go directly to you," after all you provided the technology.

And on two occasions we kept the Divison from a loss condition in the Bipolar department. Nelson Stone, our accountant, was so proud of that, that there would have otherwise been loss. They'd probably shut down as a result, because it was a tremendous amount of cash, and would come in four payments, so they got a quarter of a million dollars, you know. All of a sudden it was a bluebird for them, and it came from the bipolar contract that Sello had written with Tungsram Company.

LÉCUYER: I have another question. The Japanese agreements were very profitable? So a lot of money was coming from these—

SELLO: Yes, they were profitable. They weren't as striking as the bipolar agreement with Tungsram. But they also introduced us to the Japanese companies as outside customers for other devices. The CCD was part of that. The Japanese also began to look at other things that they could buy from us that they weren't buying, that they were making themselves, perhaps, but they would rather buy it from us. So it made additional sales for the other groups. Now that was something that we could track if we wanted to, but we didn't spend a hell of a lot of time doing that. There was an initial sum paid for the transfer of the technology, of the knowledge, an initial sum, but then after that it was royalties. They would pay royalties on whatever they sold of those devices. So the contracts were individualized.

In the case of Philips, which was the single biggest one, we agreed that we would sell them our CMOS integrated circuit design technology—even the silicon-gate technology—in return from them for their manufacturing capability, of which we could share when we needed devices. They were excellent at manufacturing. The fact of the matter turned out that their manufacturing broke down and we didn't get the devices. [laughter] We had difficulty sustaining the contract in that area.

Philips also, at the time, had their own version of oxide-covered junctions. They had the—Christophe, you'd remember. They had developed LOCOS, l-o-c-o-s, which was a special process for oxide isolation of junctions. We had developed what we called Isoplanar. That was after Gordon and Bob left.

We had developed what we named—which I actually named—isoplanar, meaning alternate planar, but which Doug Peltzer invented. The difference from that and ordinary planar junctions in integrated circuits was that this isoplanar was oxide-isolated collectors, as well as emitters. When you ran an oxide island to isolate the active structure, it also isolated the collector junction, as well as the emitter junction. The original planar, the original oxide isolation, was just for the junctions on the surface of a chip. They were not for the deep junction, which was the collector junction. That wasn't buried in the silicon. But the emitters, everything on the surface was oxide isolated in silicon gate.

There were a couple of companies who worked on oxide isolation of the collector junction. One was Philips, which they called LOCOS, meaning local oxidation, and another one was SGS, called PLANOX, who tried to copy the Fairchild way. SGS tried to negotiate PLANOX with us as an exchange. They never really proved it, so I never bought off on the exchange, and, of course, I had to be the one to go back there and negotiate that point with them. But the collector junction isolation called isoplanar—I named it after a chemical name, you know, isotope, or an isopolymer. That is something that we had exchanged with Philips in return for their "LOCOS," but their "LOCOS" hadn't yet been developed. In fact, Alan MacPherson, our patent attorney won a 12-year fight with Philips on their LOCOS, pointing out that they infringed on our process.

BROCK: If you look at this effort, which in a way is a market-building effort—

SELLO: It is market-building effort, entirely that. Yes.

BROCK: It's not like one; it is one.

SELLO: That's where the chemist now departs. That's not a problem, but that's the trend now. He goes to process engineering. He goes to the process engineering of the manufacturing organization and solves their problem for producing devices for the marketplace. Now, here is another way in which that skill of the transfer of the process and the knowledge of how it works can be used also to market devices. So now I have to face the problem of, how can Fairchild market these things? Do they have the right applications, or do I have to get the technology out of Fairchild somewhere, in order to help them with their marketing problems?

In the case of Philips that wasn't necessary, because Philips thought they were the worldwide best marketeer of devices anyway. But, of course they couldn't market their LOCOS. That didn't work it, so—and they had another technology. I digress a moment. They had the Injection Logic, internal injection logic, IIL, which they used to make CCDs. This was different than the process that Dr. Gil Amelio had worked out at Fairchild. Phililps was trying to peddle that technology, and so they tried to work out arrangements where we get that technology in return for one of ours. The internal injection logic was worked out by the Philips research organization in Eindhoven, not by the Manufacturing Organization. That's one reason why I felt at the time, and convinced Wilf Corrigan, now CEO after Hogan that, "It's a nice technology, but they're not in manufacturing with it. How can we get that and manufacture with it, if they don't?"

"Well, but they'll pay us enough," you know.

"No way. If you want to do it, you're President of the company, I'll follow the orders. But I don't agree with it."

BROCK: If you take sort of a before-and-after picture of before you started doing all this foreign work in the seventies, if you look maybe at the end of the sixties and then you look at the end of the seventies, how successful do you think the efforts were in building market share outside North America for Fairchild?

SELLO: It's a good question. Up to the period of I would say in the middle seventies, early eighties, it was extremely successful, because Fairchild R&D, dating all the way back to Gordon Moore and Bob Noyce, had compiled such a volume of new technology, on the shelves, that we never used ourselves, that it was surprisingly successful.

One of those was the MOS silicon gate technology. Moore and Noyce and Grove made it work at Intel. We finally got it into our production later than they did, but it never was a real solid winner in the MOS field for us, in the marketplace. We continued to be one of the world's ranking bipolar integrated circuit manufacturing firms, so that technology was quite marketable. We just fell down at the switch in silicon-gate technology, commercially. Why did that happen? We didn't have the right management. It's the best single answer I can give. We didn't have the right management. Corrigan and Hogan were good. The rest of the outfit was lemons.

BROCK: Oh, really.

LÉCUYER: You mean the guys who came in through Motorola?

SELLO: The so-called Hogan's heroes. They just didn't have the suds that Fairchild had, even, to carry on that manufacturing. And we were still losing people. That's my recipe. We were still losing people, because the silicon-gate technology was put to work at Intel. The guys in my former group, that I had just left to become Technical Planning/Technical Transfer manager, were all knowledgeable on silicon gate and the extensions of it—Dr. Bruce Deal, himself. They didn't want to stay with it. The guys even that had worked on problems like electromigration and metal problems, who had begun to solve all of these, saw the similar problem arising in production that they had already solved, e.g., metal cracking over sharp steps with the oxides. A very serious problem for us began to come back in the manufacturing organization. Why? There were old metallization problems long since solved that, when I heard about them I said, "Why are we even working on it?"

LÉCUYER: That was loss of competence, right?

SELLO: Loss of competence. We lost manufacturing competence in IC manufacturing, with the exception of the power transistor group that Wilf Corrigan led. We lost leadership and technical competence in manufacturing, not in development. We had all the reports on the shelves, but to put it to work—

LÉCUYER: It seems strong to me that this organization that knew how to solve these problems had forgotten about the solutions, huh?

SELLO: Yes. Exactly.

LÉCUYER: That's amazing.

SELLO: Yes. That's the shortness of the memory of silicon integrated circuitry manufacturing.

BROCK: The churn of people?

SELLO: The people who knew the solutions were gone. I noted such problems that were brought to my attention in manufacturing that I was shocked to see. One of the particular problems when the Hogan's heroes were there was in, of all places, linear integrated circuit manufacture. Now, we had been pretty strong in that. We still had guys left, even after Charlie Sporck and Bob Weidler left. We had guys in linear circuits who were good. And I remember one of the problems that came to my attention was, "They've lost the process in linear integrated circuitry. You can't make any. They can't make any good ones. What's happening?"

I said, "What are they telling you?"

He said, "Well, we put in wafers and they're down to something like under 10 percent yield." It was a disaster. We went down there and looked, and I found, to my chagrin, that there was an engineer in the production line who was identifying wafers by painting them. He went to Sears, Roebuck, bought red and green paint, and he painted silicon wafers. "Runs number one through so-and-so are red. Runs number—are green." [laughter] I mean, what the hell are you doing?

LÉCUYER: That's amazing that the management of the line didn't pick that up.

SELLO: They picked it up finally, but how many wafers had been run through in these hundred-wafer batches? They lost the process. Not only that, the paint crapped up the entire line.

LÉCUYER: And it went into the different furnaces.

SELLO: Yes. [laughter] I heard there's a problem with painted wafers. I was aghast. I went back and I told—

LÉCUYER: So it ruined everything for months, right?

SELLO: Oh, sure, weeks into months, just killed it, killed that whole product line. And he was only trying—he said, "I have to identify the runs. We're required to have run identification. If the devices are bad, we want to know what did the wafers look like that started for that run." And you can't mark them, and you can't scratch the things, or he didn't think he could, but he sure could paint them. He had a wonderful idea. [laughter]

But we lacked sustaining engineering, process-support engineering, the art in which Gordon Moore had encouraged training in, and which he was now carrying out at Intel. We knew that problem. We'd solved it. You round the step. They ran into it in Intel, quickly solved it because the solution was known. Moore had a microscope at his desk. That was his way he operated. Back at Fairchild, as far back as I know him, Moore had his own little microscope. You told him about a problem. He'd say, "Let me see it. Bring me a wafer." Brought the wafer; he looked at it.

BROCK: Didn't he have his own little work area near his office?

SELLO: He had his own little work area at his office. It was largely a microscope and a probe, and he loved looking at them. "These things, there's something wrong with the metal. They're cracking. Why are they cracking? What are you doing? We solved that problem." [laughter] But that kind of loss, the interaction back to research and development was lost with the experts that left the Materials and Processing group and the Engineering groups.

The reason we were successful in bipolar integrated circuits was twofold. One was that Dr. Bob Seeds stayed with us. He later got killed in an automobile accident. He developed the technique of inspecting a wafer and understanding the distribution of the process defects that you saw—all of the little bubbles or dirt specks or missing metal, those so-called process defects. He looked at it and developed the idea of—it wasn't a randomness. It was a kind of a clumped—you'd get a batch of bad ones in a wafer here, and another batch here, and everything in between was good. Well, Bob looked at that and began to realize that defects in both bipolar and integrated circuits were related to how they were clumping, and the yield was related to the clumping—when he was gone, that knowledge sort of got dissipated and disseminated.

There were other such kinds of things. We were very strong in bipolar integrated circuitry. We also retained—at the time Moore and Noyce left, shortly thereafter—a computer-aided-design engineering group, also headed by Bob Seeds, and brought in fellows like Rob Walker. Rob was very good at design engineering and libraries of cells, cell-library effort, and we had a pretty good group of that in Fairchild. That kind stayed on. Rob stayed at Fairchild until Wilf disappeared to form LSI Logic and until Fairchild disappeared, so that stayed in good work. There was even CCD work that Tom Longo also pushed to continue, and also high-voltage bipolar work in integrated circuits. There were some very strong areas, but by and large those production areas managed by the Motorola people, with the exception, as I say, of Wilf Corrigan, just weren't competent.

Then other guys were brought in from other companies, from TI, from RCA, to help with the MOS problem. We just couldn't produce MOS integrated circuits properly, even silicon-gate circuits, and we got some guys in that were downright nice people but downright incompetents as management for the MOS group. The MOS group in production at Fairchild never really got off the ground. They did make a number of microprocessor circuits, the so-called F8, single-chip microprocessors. That did come to fruition, but most of the MOS products didn't.

LÉCUYER: I want to reflect now about what we talked about, in terms of process knowledge, right, manufacturing knowledge. I mean, if we compared Fairchild in the seventies with Intel, National, these other places, the secret of Intel, National, if I understand you well, was really to sustain this practical knowledge on how to manufacture things right.

SELLO: That was certainly a secret of both AMD and National. In Intel there was an added research and developmental capability that was also present, together with the processing. How well they handled silicon gate in production is because they had the guys that knew the background of it, that had worked with it in production at Fairchild, and in development at Fairchild. So they added more than just a manufacturing capability. They added a sustaining process engineering, or process support capability to solve the problems.

Now they ran into their own set when they began to get more and more complex in their processors, especially. They also had some very bad—and I guess Moore can tell you better about this than I can. I don't want to throw rocks, but they never really were successful in making DRAMs. Why didn't Fairchild get out of the DRAM business? They never really produced it successfully for the marketplace, as a result of their manufacturing. They had to get out of it. They couldn't compete with the Japanese. They couldn't meet the price.

BROCK: You mean when Intel got out of the memories, the DRAM.

SELLO: Yes. Right, when Intel got out.

LÉCUYER: So that was also out of fine tuning the process, right?

SELLO: Fine tuning the—yes. Not being able to support the process in manufacturing. The fellow who had a lot to do at Intel with the early running of the DRAM process was Ron Whittier. Ron Whittier was an engineer, a Ph.D.—he was a chemist, actually; I hate to say that—was a chemist who left with Andy Grove, one of Andy's cohorts, and Ron, somewhere along the line, was put in charge of DRAM production. The DRAM production was up at Washington. The plant was up there, and Ron was the manager of that plant. The plant failed at the job. They couldn't compete on price with the Japanese. He was in charge, therefore. He was removed from the job and that plant was shut down.

LÉCUYER: So the issue was really not to be able to improve the yield on it.

SELLO: That's right. They lost—they couldn't get sufficient yield, sufficient good dice per wafer to keep up with the Japanese prices.

BROCK: Yes, this quality issue.

SELLO: The quality issue.

LÉCUYER: And also the yield issue, which is critical for the economics of it, right?

SELLO: Yes.

LÉCUYER: If you can't improve the yield, then—

SELLO: That's right. Yield is the name of the game.

LÉCUYER: Then you eventually lose.

SELLO: That's right. What you do is, the yield drops. You begin to stuff more wafers into the tubes, in order to make up for the loss in yield, and because you process larger quantities of wafers at quantities which you haven't really developed or tested, the yield goes down even further. So you begin to drive—it's like skiing downhill. I mean, you try to solve the ski job by going faster downhill. It doesn't work. [laughter] And they ended up not being able to compete with the Japanese in cost and in quality. It was a wise move to get out of it. And Ron Whittier was at the apex of a lot of this. So Intel had their set of problems. They did work their way out of a number of problems, but that was one particular one that they didn't.

LÉCUYER: This happened in the 1970s, right, and even the 1980s.

SELLO: Sure, because as the wafer increases, you don't decrease the problems, because you have the problems of the large wafer itself, of the new process itself. Now the process—what you did previously, nicely, on a two-inch diameter wafer you had under control. But now you went up to four inches in order to get more dice. Now, fortunately, increasing the size of the wafer, for Intel, was a very good strategic way of getting more production, provided you kept the yield. You'd get four times the number of dice, but you couldn't take a beating on the yield to do that.

LÉCUYER: You have to really redo the process.

SELLO: Exactly. You have to, we say, tweak the process, but you have to redo it. New equipment has to come in, and here is where Fairchild took a violent beating and finally lost out of the ballgame.

LÉCUYER: So that was, what, mid-seventies?

SELLO: About the mid-seventies, yes. Well, it ends in 1980 by itself, 1979, 1980. But Fairchild never had a successful MOS integrated circuit process in production, but they had all the knowledge that they needed.

LÉCUYER: Because I mean, if we look at the history of Fairchild in the seventies, right, they were in a bit of crisis, I mean major crisis, at least 1971, 1970, 1972, right, when the company almost went bankrupt. There was a crisis again in 1974, 1975. I mean these were crises in the industry, but they were especially strong at Fairchild. And then I think there was again one in the late seventies, right?

SELLO: Late seventies, yes. Now, at the end of the seventies, Fairchild, as I remember, was a six hundred million dollar a year organization, one of the biggest. Earlier on it was bigger than the others.

LÉCUYER: We know it's some interesting phenomenon, that Fairchild was wounded very badly at each of these crises, right?

SELLO: Yes.

LÉCUYER: But nonetheless, the sales over that period grew very substantially.

SELLO: Yes. It's remarkable, isn't it? And they grow substantially from the successful devices, the bipolar integrated circuits, and our discrete line, and I would also say our linear line, because we weren't hit as hard in linears, although we were competed for by National, and a related device, and the discrete devices, transistors. So we still, in spite of that—it's like the serpent is still plenty strong to choke a few people when it's fully dying. [laughter]

And the attempts at remediating were ineffective. Wilf Corrigan became director, became president and chief executive officer after Hogan, a short time after Hogan. After Wilf Corrigan, no one who was brought in—in my view—to become a successful equivalent of

Moore and Andy Grove. They brought in a bunch of people, I mean, people who came from TI, people who came from RCA. I always liken the fact that in the semiconductor industry, if you don't work for several companies, they wonder what's wrong with you. And here I am, my entire career, not counting Shockley, with one company. There must be something wrong with me.

My point is, I didn't have to move. The managements I worked for within the company kept changing. With the exception of Wilf Corrigan, and he shifted around within the company, the others brought with Wilf were not competent enough.

LÉCUYER: And a very unusual dynamic there, right?

SELLO: Yes.

LÉCUYER: I mean, the company is integrated somehow, right?

SELLO: Yes, yes. It's hard to go back and say, how could it be resurrected, or how could that have been solved. Well, it's really the choice of the second-level, of not the top manager, but the choice of the second-level management. There were guys brought in from RCA who were so-called experts in MOS technology, and I could see the particular guy that was brought in was totally incompetent, and I didn't know that much about MOS high-volume production at that stage. I could tell. But Roy Pollack was from RCA. RCA was supposedly an MOS expert. "Roy, why don't you get a team together and troubleshoot the line? Forget about now what's not working. Concentrate," you know, in my little simple-minded way, "two or three good process engineers. Let them run the line." Run—what we used to call it, special runs. "Let the line run by itself. Siphon off wafers at various stages, under the guise of two or three very good, topnotch process developers, and let them run special runs, targeted runs to solve the problems. But let it run."

No, he wasn't going to do that. He said, "Well, I'm going to lose production if I do that."

"Well, you're losing it anyway."

"Well, Wilf is going to get mad if he sees the number of wafers coming out decreasing."

"Well, let him. Let him get mad. Tell him about it."

"No."

You know, it's easy for me to say to another manager, "Go tell the chief that there's something wrong." But that's what I would have done.

LÉCUYER: So would this be different from Fairchild in the 1960s, when—couldn't a mid-level manager have told Noyce and Moore that—

SELLO: They could. And they did. That's what Noyce—that's what Moore and Andy Grove encouraged in their development of the lines is, you had to have guts, but you had to talk back. You had to point out the problems. Andy would kill you if you couldn't stick to your guns on a problem. He'd get mad, and scream and holler, and you might get terrified and run out of the room, but if you stuck to your problem he had more respect for you.

LÉCUYER: So basically, so Intel was really nurturing these strong, vocal, mid-level managers, right?

SELLO: That's right.

LÉCUYER: And Fairchild was—I mean the people like Corrigan or Hogan were not doing that.

SELLO: Were not doing that. In Corrigan's case, where he got some visibility they were. We had Tom Longo brought in, who was an expert in bipolar digital technology, and Tom finally took technical charge of technical planning on a development basis for all of Fairchild. But Tom was not universally recognized as very easily able to work with. He thought that he was peaches and cream whenever you talked with him, but he wasn't. We even had Dr. Jim [James M.] Early who came from Bell to be the head of R&D, who, in effect, took Gordon Moore's place.

Jim Early was a brilliant technical guy, but Jim couldn't do the sustaining part that Gordon Moore had developed through Sello, in supporting production. It never worked. Jim was a pure R&D guy. You gave him an R&D problem, he'd solve the R&D. He took as much time as he needed to solve it, but he'd eventually solve it. By that time the production line's down the tubes. He had a tremendous reputation at Bell Telephone Laboratories, but for production problems, I don't remember where Jim Early got in on any production problems that I knew about. Of course, I didn't know them all. [laughter]

LÉCUYER: If we want to encapsulate our discussion so far, the way I would summarize it would be that the secret in the semiconductor manufacturing business is to have very strong mid-level managers who can support the sustaining of the process, I mean, keep the knowledge going on, and also develop new processes. These managers can also integrate the various functions in the field, right?

SELLO: Yes.

LÉCUYER: Manufacturing, marketing, that sort of thing. So in some ways, the secret of Fairchild was its ability to do that.

SELLO: That's right, its inability to do that.

LÉCUYER: The first Fairchild. The second Fairchild, the one in 1960s, I mean the downfall of the Fairchild in the late 1960s was its incapability of doing that, right?

SELLO: Yes, and especially at MOS; not in bipolar ICs.

LÉCUYER: And that the success of a place such as National or of Intel was to do what they had been able to do at Fairchild in the early days, right?

SELLO: Yes, but to build it larger. Now you have to go in that direction, to augment the production, to scale it up. We never got into that stage, but it's right there at the middle-management technical level. Now, it's hard to say that's what's at fault, because after all, they have managers themselves, who should be able to direct them into this case. Well, the ability of the line managers themselves to get down into the line and observe the problem like Andy Grove did, even though he was a CEO, although hard rock, although obnoxious in many cases, and screaming and jumping, still his ability to do that, and to hold the guy to the solution, was what made it work. Superior talent, in the first place.

LÉCUYER: In the right places.

SELLO: In the right places.

LÉCUYER: And making sure that the people are going to talk to each other and communicate.

SELLO: That's right.

LÉCUYER: It has been argued that what set Intel, in the 1980s, was not Moore, was not Grove and people like this; it was the mid-level managers. The mid-level managers had lots of power at Intel, and they were the ones who controlled the production capacity, and they understood quite rapidly that the memory business was a losing business for them, and then they allocated production capacity to microprocessors. So basically, these guys decided much before Noyce and Grove that the company was not a memory-business company anymore. It was a microprocessor company. And it's only several years later, that Moore and Grove really understood that.

SELLO: That's not the way I see it, entirely, Christophe. What happens is, as it happened even beginning with Noyce—what happens is that the personality—it's important that the technical personality of those who are the managers, the top managers, get permeated in their thinking to the mid-level managers. There's an actual flow of personality, so that when the top manager, like Andy Grove, no longer can run around and look at all the detailed problems, he has by then imbued his objectivity, or his mode of looking at the problem, to those who report to him. When that happens, then it succeeds. For example, consider the strong developmental capability of the Noyce, Moore, Grove team. When the competition caught up, Intel moved new devices into production staying a jump ahead of the competition.

What kept Noyce the leader that he was, and Moore, was that they, right from the beginning, let their personalities permeate in even their everyday work, down into the working level, by the way they arranged the offices, by the way they talked to people coming in late, by the expectations of, "Don't ask me how to solve it. How would you solve it? Then why don't you pursue that. Don't come to me to ask me for all of the answers. Come to me with the solutions. If I can think of a better one I'll tell you. Otherwise, you have to pursue what you're doing. Don't wait to come to me, even." That personality, that's what comes down.

They knew DRAMs would work. Andy Grove knew very well that DRAMs would work. But to get it down to where it go into cutthroat competition and cost with the Japanese, that was the transition that was difficult. Now, they knew how to make them, but there was still a discipline that the Japanese exercised in their lines that we did not exercise in the American lines.

LÉCUYER: So the mid-level managers followed Moore and Noyce?

SELLO: Yes, you're close.

LÉCUYER: The mid-level managers were little Noyces and little Moores, right?

SELLO: And little Groves. Right.

LÉCUYER: And that these people had both the strengths and weaknesses of the real Moores and real Noyces, and that there was maybe something missing. I mean, they didn't push the engineering as far as the Japanese pushed.

SELLO: That's right. They didn't push production engineering down to the—yes. Yes.

LÉCUYER: So that's basically it. So that's something that came from Moore and Noyce that was really reproduced, I mean redeployed by the mid-level managers, right?

SELLO: Right. Right.

LÉCUYER: But at the same time, the mid-level managers didn't have autonomy somehow to redirect the company in different direction.

SELLO: Exactly. There was a lack of the total autonomy that was necessary. Now the Japanese supplanted that with their own thinking. They would obey to the letter, wherever it came from. It didn't make any difference if it was Manager A or Manager B or Manager C. That same management permeated all the way down. It was identical.

In the case of Grove, nobody's identical. But there were guys who were reporting to Grove who were quite good in their own right, the talent was there. What they lacked when they needed to, was the ability to talk back to Grove, the courage of their own convictions.

BROCK: In this period when they were having problems with the memory.

SELLO: In this period, right. What exactly went wrong at the Memory factory, which I lay at the feet of Ron Whittier, is very difficult for me to understand, because Ron Whittier was a personal friend of Andy Grove's. But there was something in the relationship that didn't gel, and maybe Andy just ran right over Ron, because Andy, it didn't make any difference if it was a friend or not a friend.

And that was an economic, a really real economic factor. The Japanese were able to marshal economic resources that Fairchild didn't have, and some will even say it was due to something called dumping, and that certainly played a role. There is a minimum cost below which you cannot produce profitably in that given marketplace. Well, the Japanese had saturated their marketplace, so we couldn't sell to them, but on the other hand, their prices were low enough, even below cost, that they could outsell us. So there was dumping. So you have 99 percent of it, Christophe.

I remember the search for talent that I associated with Intel. Whoever they got, they always tried to get the best. We at Fairchild didn't settle for that, in manufacturing. Somehow the search for talent was incomplete, though I have to go back to the middle managers to say what each one's problem was. You can't say, well, no, that all goes back to Wilf Corrigan, because he's made a success of LSI Logic. But it has to do with that ability to search for talent and put the best to work in that period, to be flexible enough to do that.

BROCK: Do you think that part of that was a recruitment problem, that maybe Fairchild had—you know, certainly in the early 1960s it was the place to go. Maybe in this period, in the mid-1970s or something, other places seemed like the place to go.

SELLO: Certainly that was a factor, definitely a factor, the ability to recruit talented people, to make an effort to recruit talented people. I don't remember. I can only think of one recruiter whom I had respect for at Fairchild. He was not a technical guy, but he was certainly a skilled recruiter. But the ability of Ph.D.'s to go out—I haven't talked about that whole thing. That was part of the job that I had, other Ph.D.'s had, to go out to universities and to recruit technical people at that level. I considered that to be a very important function that I performed from time to time, although when allowed; ability to recruit highly trained people, and convince them that research and development wasn't very good by itself until it was reduced to commercial practice, at a profit.

BROCK: Did you want to talk for a few minutes maybe about the sequence in 1979 and 1980, and how that affected your thinking about staying with Fairchild?

SELLO: Oh, after what I call the fall of Fairchild?

BROCK: Yes, sure.

SELLO: Oh, it fell, no question about it. The meteor hit its height in 1979. It was still, I think, the third or fourth largest company, six-hundred-million-dollar, and then began to decline.

BROCK: What happened?

SELLO: It was sold. That part, you mean?

BROCK: Yes. I'd like to hear about your experience of that.

SELLO: I will, if I—

[END OF AUDIO, FILE 2]

BROCK: So we were beginning to talk about the fall of Fairchild in 1979.

SELLO: One of the big successes of Fairchild from all the way back, and now and then followed by the major semiconductor companies, were options. We cannot overestimate—and then thus underestimate the fall—the net effect on the talented technical guy, not necessarily the secretary or the janitor, of the effect of being rewarded directly, in terms of options. You watch the stock price. You know what your option price is and you want more of those options. I looked forward to be rewarded by options. It was great incentive.

It's going to kill the semiconductor business in this country if the present stupid option restrictions really go into force, to my mind. It might take a little while. You know, things don't change overnight. But when guys like Craig Barrett of Intel, who have suggested, "We will give up the total option picture for the top five people in the company, if we can keep it for the talented guys below," I mean, that's a big deal. He gets a good enough salary that he's a rich man anyway, and so they all are, but there's a real, real path there for the talented middle level engineers and scientists.

So in 1980, when Fairchild was sold to Schlumberger—the period is really 1979 to 1980. Fairchild was sold to Schlumberger after being approached by a number of different companies, that option feature at that level was profound. It affected me directly.

BROCK: How so?

SELLO: I went to Tom Roberts, to whom my reporting channel was transferred (before that I reported to Wulf Corrigan or to one of the VPs), to talk to him about what I was supposed to be doing, and to explain to him what I was doing. He did two bad things, which were not his choice necessarily, but he was the CEO. Number one was, "No more transfer of technology, zero, of any kind. Technology is a private tool, private product which belongs to Schlumberger. We do not sell or transfer technology."

Now, that was their operating motif in Schlumberger Exploration, you know, the way they looked at oil wells, for which they contracted their services. They had wonderful techniques for doing this, but they never, never, ever wrote any contracts with anybody for selling Schlumberger technology. So he said, "You've been selling technology, or marketing technology. Can't do that anymore."

Second thing is, "We've pulled in all the options." I said, "What have I done?" [laughter] Schlumberger only gives options to the very top senior managers, like the managers of the divisions. He, Tom Roberts, would have an option, but then below him no one would get options, zero. And he said, "You report to me, therefore you won't get an option."

"Well, Tom, what have I done to deserve that?"

He says, "Well, Schlumberger works on loyalty. They will pay you sufficient salary so that you stay loyal." He said, "That can get to be pretty large if you let it, but no options."

So I said, "Well, Tom, you know we don't do this in the semiconductor industry, and when you impose this across the board you'll lose all the good managers. Right? They'll go across the street where they can get an option. They don't have to stay."

He said, "I know. The ones that we'll lose, we deserve to lose. We don't need them." That was the philosophy. Now, that had a very profound effect. Nobody wanted to come to work for Schlumberger with their policy of leaving managers not even a possibility of an option. On the technical transfer, my work, it was even more serious than that. The most serious case I can think of was to go to the Chinese in Beijing—I guess I told you this story.

BROCK: You told us a bit of it.

SELLO: Well, the net of it is to go back to the Chinese and call off the technology contract that we hadn't even started. I said, "Why? This is business."

"No. We can't tell them how we make stuff. When they learn how, or they need to buy stuff from us, they can, but we do not sell technology." Well, the upshot of it was that I refused to do that.

I said, "I'll do this, but you'll have to come with me and tell the Chinese that, because I can't." The same thing happened in Austria, where I had already succeeded in getting an agreement on a contract. To accomplish that, I went to see and got permission from Chancellor Kreisky himself, at the time, the Prime Minister.

And Tom said, “Go back and tell him that there’s no longer going to be any kind of relationship in Austria in this business.”

And I said, “Well, you can’t do that. He’s going to object. He might even sue us.”

Tom said, “Let him. We at Schlumberger are supplying the parts for all of the Aircraft engines he needs from France, and we’ll cut off his aircraft, if he doesn’t accept this.”

I said, “Tom, I don’t want World War III. We don’t need that. That’s not what I’m doing.” But that was the personal thing that affected me, and that, of course, I said, “Well, I have a decision to make.”

He said, “Well, let me help you. I’d like to keep you reporting to me. We have lots of things that you can do,” etc., etc. But when word about options got around, man, the exodus was tremendous. There was no place to go within Schlumberger. Oh, there’s a job. Yes, you can keep a job and maybe get a good pay, not the best in the world, but you can get a fair pay. But the incentive that had already been established for so many years, that’s the bad part. If it hadn’t been established in the first place, if Schlumberger started from scratch that way, they could do that. But we could go anywhere and get an option, because an option didn’t give you anything. It was only paper. You had to be successful in order to do that.

So that would have been—I don’t know whether Gould, who was the white knight, who also bid on Fairchild, would have done that. It’s unlikely, because none of those kind of large companies themselves in the United States maintained options for their top managers. So it was not an industry-wide kind of practice. It was specific to the semiconductor industry. It wasn’t even specific in the eastern branch of the then-existing semiconductor companies, such as Sprague, GE, or Transitron.

LÉCUYER: When I listen to you, I think about the thing behind the Roberts way of doing things is both the French mentality and the East Coast mentality.

SELLO: There’s a real difference. And he really called it the French—“We French,” and he was Roberts from Texas, “We French don’t believe in artificial incentives like that.” I never heard of an option referred to as an artificial incentive. “No, we pay salaries for loyalty. When you are loyal you get paid.”

“And how do you determine loyalty?”

“By how many years you’ve worked, and what you’ve done.”

“But that might take a long time in semiconductors. For five years?”

He said, “Well, you’ll get paid for what you do, and if it’s good you’ll be well paid.”

LÉCUYER: But at the same time the push toward new technology is also the French approach, right?

SELLO: Yes.

LÉCUYER: Technology is really a tool for survival—

SELLO: Exactly.

LÉCUYER: —and so you don't give it to other people.

SELLO: You don't give it to other people, even for money. Yes. Their oil well logging technology was developed for just specifically that purpose. It did find them oil. They have these portable computers, these CDCs and others run in portable equipment that they run all over the world, in Indonesia, in China, everywhere, where they log oil resources, and they were successful, more than most, at finding the resource. To be able to read the graphs that come pouring out of an oil well logging outfit through a computer, these are impossible. It's like a hundred million brainwaves all at once.

But that was a Schlumberger practice, and Tom was very candid about it. "We have options for the top-level managers, of which I happen to be one," he says. That was their major disincentive in our industry—the inability to compete. Wilf Corrigan, one year later established LSI Logic, and the first thing he used were options. [laughter] He got the guys from Fairchild who he wanted to take with him, like Rob Walker, Bill O'Meara, all the good crackerjack guys that we had, that were doing a great job.

I guess you could call it those two factors, but it's really not fitting into the ambient. By the way, Shell Oil failed at trying to buy ZiLOG—

BROCK: Around the same time?

SELLO: —at the same time, for the same reason. It was another example of that at that time. Federico Faggin was at ZiLOG, where he had gone after working at Intel, and he's full of stories about this kind of thing.

BROCK: Why were these petroleum concerns buying semiconductor companies?

SELLO: Well, they wanted to diversify into other businesses.

BROCK: Just to become a conglomerate?

SELLO: That's right, to become a conglomerate. Well, Schlumberger had another rather fallacious reason, that once we understood we could quickly talk about. They thought they could employ integrated circuits to improve their down-the-well logging circuits. The one thing that they didn't realize is that an integrated circuit won't operate over 150 degrees Centigrade. If you run a log down into a deep well it gets quite a bit hotter than 150 degrees Centigrade, and the IC isn't going to function. So you couldn't put integrated circuits into the probes, and that's what they really thought they might be able to do. On the other hand, they did use a lot of software and hardware in reading their printouts from their various computers, which required semiconductor. But they thought that they had a product market that they could cash in on, other than just another company.

Now turn it around and even Tom admitted this candidly at dinner a couple of times. If Schlumberger had said to Corrigan, at the six hundred million dollar level of Fairchild sales, "We'll finance you. We're your venture capital outfit," it might have worked.

Wilf made that proposal. "If you finance me, we'll be glad to be a division of Schlumberger, but we have to run the Semiconductor Company in competition with other semiconductor companies." So if Fairchild had an infusion of capital, even less than the final purchase price they actually refused at that period, but gave Wilf Corrigan the authority to operate it as an independent profit-making organization within the semiconductor business, the sales curve would have continued, I'm sure. It was on the way up, even though Fairchild was losing people in competition, we were still at the six hundred million dollar level. I mean, we still could do things.

Or if someone had come along with a healthy infusion of independent cash—now that's the reason these old petroleum guys and the old chemical guys couldn't manage that. They could not allow themselves. They did not have the ability to give an independent company, at that size, complete autonomy. It had to come from the board of directors.

LÉCUYER: Which means that there was no money flowing in, which means that Schlumberger was not invested enough.

SELLO: No. They did invest almost 50 percent more money than what they paid Fairchild for, paid to get Fairchild, but it went right down the tubes. Same problems.

LÉCUYER: Interesting, because I mean if you look at the story from the French point of view, right, that was a time when the French government made a decision that ICs or semiconductors were essential technology, and that the French needed to control it.

SELLO: Right. Yes.

LÉCUYER: My view is that it's really what's behind the purchase by Schlumberger of Fairchild. They wanted to—

SELLO: Get control.

LÉCUYER: —control it, and get access to technology which was not mastered in France.

SELLO: Yes, exactly. That's it. Very good, that's right on, that the right way to have operated it would be even to not necessarily buy it, but, "We will be a silent partner. We will invest to the extent of half of your R&D, half of your developmental and manufacturing costs, but now we are on your board." A typical venture capital-type of approach. And that's also what Standard Oil refused to do in the case of ZiLOG.

LÉCUYER: Interesting, because I mean also it was the time also when Signetics was sold to Philips, right?

SELLO: Yes.

LÉCUYER: I think Philips made a different decision. They may have given quite a bit of autonomy to Signetics.

SELLO: Yes. They made them a division of Philips, to be run by the same people who were running Philips. Remember that Philips, however, was already a high tech electronics company. They understood the business.

LÉCUYER: Oh, I see. All right.

SELLO: Signetics came in under the broad umbrella of Philips, but they let them be independent enough to where they could maintain options, for example. However, they cut back on the options quite a bit, and lost guys as a consequence of that. Options isn't the end all and be all, but it's certainly a profound factor.

LÉCUYER: Absolutely.

SELLO: And they did that under the strength of the fact that they had a superior technology, or as good a technology to Signetics, in their own shop, that Signetics could recognize, that Signetics could even borrow on. They did a lot of works at Signetics. In fact, Dr. Kooi himself came from Philips Research in Eindhoven, and he came to work at Signetics. And there were a couple of guys from Philips who they were smart enough to bring over and pay on an American basis, and let them work in the division of Signetics just as they would have worked back in Eindhoven or in the production units. That sounds like a very simple kind of factor, but it was a tremendous factor.

Well, then Schlumberger did one more thing, which Charlie Sporck told me about in England. A couple of years later Charlie bought Fairchild for a song. He bought it for far less than what Schlumberger had paid. I met him at the airport on a trip and I said, "Hey, Charlie, how do you like backing Fairchild again?"

He said, "Well, of course, you know Fairchild." He said, "You know, I gave a price to Schlumberger for Fairchild. They never even negotiated. In fact, they never even sent anybody around to talk to me. I never saw a negotiating team from Schlumberger. I offered a price and they sent me a note saying, 'Write us a check.'"

LÉCUYER: So they were that eager to get rid of it.

SELLO: That eager to get rid of it. [laughter] And Charlie sort of chuckled about all of that. It was an interesting result, the same French thinking applied. "We no longer need it. Out. Finished." He got it for a song. He got Bruce Deal and he got a bunch of good people, and of course—and Kirk Pond, of course. Then you know later Charlie sold the name to the so-called new Fairchild, which is now doing fairly well.

BROCK: Well, I think maybe the last thing that we have time to ask you about today is just your decision to set up your consultancy, and what your major tactic was there.

SELLO: Oh yes. That was an outcome directly of my conversation with Tom Roberts. I said to Tom, "I can't stay." I said, "What we've been doing has been such a good thing, I want to do it, but I'll do it on my own. I'll set up Harry Sello and Associates. You're welcome to participate if you like, from any point of view, if you think you can use the talents of Harry Sello and Associates, but I want to do the same thing. But this time I'm going to have a free hand to go around and talk to companies who are interested in setting up technological operations elsewhere, or marketing technology for share of market."

And that's, in effect, what I went into business to do. I had one daily partner, now talking on the phone, who set up the office and ran it—but I had a team of associates. I used key experienced guys who were also then available, because of this silly option thing—that I used to call on for collaboration in doing transfers, if we had to do the transfer itself, or setting up the team for it.

I had already established that, only three or four people were necessary to go out and find the projects, that any more than that would have to be from a company which wanted to do the transfer anyway, just as we had arranged under Les Hogan and Wilf Corrigan. So in 1980 or 1981, I started Harry Sello and Associates and was amazingly successful; surprisingly, I should say, not amazingly.

There was another thing that got added to my skills that worked out very nicely, and that was my ability to work with the U.S. Government to transfer technology. I immediately could go to the same people that I had become acquainted with over those Fairchild years, like the Defense Department and Commerce Department, on the problem of getting the necessary export licenses for the export of semiconductor technology.

SELLO: The US Defense Department and Commerce Department at that time would listen, and if a case was made, just as I made it in the case of Tungsram of Hungary. And it worked. It wasn't anything as big as the work at Tungsram. There were more design and development kinds of things that we could do, test equipment, expand it into test equipment and the other parts of things. So my ability to get export licenses, not only just in semiconductor but in related applications of that, that plus the experience, plus the calling on of associates who had this kind of work, worked out very nicely.

One thing I did learn from my exposure is that I got acquainted with Robert Redford. [laughter] Sheila hates this. I met him when he was doing that book, the movie on the book together, you know, the Nixon exposure.

BROCK: Oh, *All the President's Men*?

SELLO: On *the President's Men*, yes. I was in Washington pursuing a license, and I was staying at the Madison Hotel where Robert Redford was staying. We went up and down on the elevator a couple of times and got acquainted. Sheila thinks that's the nicest thing that ever happened to Harry Sello and Associates. The only thing is, I haven't been able to capitalize on that.

So that was a continuing effort on the part of mostly middle-size and small companies, to establish their technology in places where they didn't have the skillsto go out and market directly. And now it's coming back to life in an interesting way, that there are so many people now interested in trying to pursue China, that I'm getting approaches from various companies to see what can be done in China on that same basis. It's a whole different problem, but it dates back from Harry Sello and Associates, so I hope I can continue to work at that.

Now in the case of doing that work after 1980, I could compile a list if it were useful, but I don't think I'd get into that in the talk anyway. But there must be—I had something like a total of, by last year, over a hundred clients in that period of time, who were interested in some fraction of this work, the sale and establishment of technology, plus export control.

BROCK: I guess I just have one last question, which would be, did you ever do any technology transfers in the reverse direction, for a non-North American company wanting to transfer technology to a U.S. company?

SELLO: Yes. The contact work, but not the transfer of technology for a couple of Japanese companies that had been earlier recipients of Fairchild transfers, one of them for CCDs and another one for magnetically oriented devices. They came around and talked to me, and although I wasn't part of the transfer team; I actually just found them, arranged for them to meet the companies who would be interested in working out a license or a sub license.

Now along in with this came a very important part of Harry Sello and Associates, say a third part, and that was the support of litigations in the semiconductor business. I had done so much in the way of working with licenses and patents that there came to be many litigations in this area. In fact, SGS wanted to cancel the license that I worked out some years earlier for Fairchild. So they sued Fairchild at the time, and that case went all the way up to the International Arbitration Court in London. I testified as a key witness. That time it was on behalf of Fairchild, because it was SGS who was violating the patent rights and the technology rights of what we had transferred. So I was on the other side of the fence in that case. That was probably the largest one.

BROCK: And then the expert witnessing became a part—

SELLO: Expert witnessing became a part of—in addition to those hundred companies, I must have been an expert witness at over maybe seventy or so litigations, as expert witness on the technology for the transfer. That became the business of Harry Sello and Associates.

Now, that, too, was, you might say, a kind of direct outcome, more or less, of the work of chemists and physical chemists and materials scientists in semiconductors, used to support a process, because this type of litigation was usually based on; the transfer wasn't performed properly, or we received artificial information, or we didn't get the information that we paid for. Came the litigation based on that, then that was right down the alley of what we had done so much of at Fairchild, too, i.e., litigations in support of technology transfer, and patent violations.

Patents violations themselves were concerned with using me as an expert witness. The actual litigation was lawyer work, but the lawyers had to call on expert witnesses who had been in actual transfers that could testify—. Now, I relate that directly to my experience, originally, as a process chemist in semiconductor work, but I can't think of any other process chemist that made a business out of it. But it was, to me, a reduction to practice of what you needed to do to stay in the business as a scientist in semiconductor work; to recognize the market value and attempt to capitalize on it, because many times I had to judge on, "Well, you really don't have a process."

There was one case where I was litigating in favor of the American firm who had been accused of not transferring the right technology, and it turned out the case was that the Koreans, against whom we were litigating, did not supply the trained people that they had to supply in order to receive the technology.

BROCK: To receive the transfer.

SELLO: To receive the transfer. I actually had to get with the Koreans and query them as to what they knew or what they didn't know, or what they thought they had received or not received. It was evident that they did receive the transfer, but they put inadequate people in charge of the reception, who didn't know what to do with it. So they piled up specifications that were never used. [laughter] I mean, process specifications, you know, the flow sheets. And they were attempting to say that they were deficient and inadequate, when it was obvious that the receivers were not properly trained to do that. So that litigation went the way of the American. That's usually the way it came out, because the Americans are the ones that are most litigious. We are mostly—we're more litigious than the French, for example. But then they don't have to be. They don't write the same kind of contracts. [laughter]

BROCK: Well, I think we've come to the end of our allotted time.

[END OF AUDIO, FILE 3]

[END OF INTERVIEW]

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