CHEMICAL HERITAGE FOUNDATION

JAMES M. GOLDEY

Transcript of Interviews
Conducted by

David C. Brock and Christophe Lécuyer

at

Allentown, Pennsylvania

on

18 February 2005 and 8 April 2005

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Oral History Program
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JAMES M. GOLDEY

1926	Born in Wilmington, Delaware on 3 July

Education

1950	B.S., Physics, University of Delaware
1955	Ph.D., Physics, Massachusetts Institute of Technology

Professional Experience

1954-1989	Bell Labs Director, Linear and High Voltage Integrated Circuit Laboratory

Honors

1969	Fellow IEEE

ABSTRACT

James M. Goldey begins the interview with a discussion about growing up in Wilmington, Delaware. He discusses his education, his involvement with World War II, the influence of the Great Depression, and his early interest in electronics. He also details his education at the University of Delaware and at the Massachusetts Institute of Technology (MIT). Next, he chronicles his involvement with the electronics industry and his career at Bell Telephone Laboratories, Inc. Then he describes his interaction with William B. Shockley, Julius Molnar, Jack Moll, and Ian M. Ross. Goldey continues the interview by describing his work assignments at Bell Labs, along with his involvement with the Nike-X missile, silicon transistors, integrated circuit development, and hybrid circuits. Finally, he recounts his accomplishments at Bell Labs and his involvement with historical works about the transistor and integrated circuits. Goldey concludes the interview by reflecting on his career and the industry.

INTERVIEWERS

David C. Brock is senior research fellow at the Chemical Heritage Foundation. In 1995 he received his M.A. in the history of science from Princeton University, and in 1992 he earned an M.Sc. in the sociology of scientific knowledge from the University of Edinburgh.

Christophe Lécuyer is research historian at the Chemical Heritage Foundation. He holds a Ph.D. in history from Stanford University. He has published extensively on manufacturing districts, university-industry relations, and the history of electronics and scientific instrumentation. He was a fellow of the Dibner Institute for the History of Science and Technology and taught at MIT, Stanford University, and the University of Virginia.

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INTERVIEWER: **David C. Brock**

LOCATION: **Allentown, Pennsylvania**

DATE: **18 February 2005**

BROCK: This is an oral history interview with James Goldey, conducted by David Brock, on 18 February 2005, in Allentown, Pennsylvania. I learned that you were born in Wilmington, Delaware, in 1926. I was wondering if you could tell us a little bit about your family background.

GOLDEY: All right. My father came to Wilmington to stay with his aunt when her husband died. He was in Philadelphia. He was a student at the time. My mother was from a Wilmington family. My grandfather, my father's father, worked for the Pennsylvania Railroad. My mother's father had a butcher shop in Wilmington, and also was in local politics and, in fact, was a city councilman in Wilmington at one time. My father's family all left Philadelphia. Most of my mother's family, except for one, stayed in Wilmington.

BROCK: What did your father do for a living?

GOLDEY: He got his degree at the University of Delaware in Agriculture, and that was an avocation of his for a long time. His primary work was as accountant for Hercules Powder Company. He did, for example, all the landscaping at what they called the Experiment Station, which, today, most people would call Research Lab for Hercules [Powder Company].

BROCK: Do you have any siblings?

GOLDEY: I had an older brother who died in 2005, Bob [Robert H. Goldey], who was a chemical engineer. He also went to [the University of] Delaware and got his master's degree at MIT [Massachusetts Institute of Technology]. He spent his working years with [E. I.] DuPont [de Nemours and Company, Inc.], in South America, a couple of tours down there, and in Wilmington. He lived at Hilton Head [South Carolina. I have a sister, Mary Ellen [Goldey], a year and a half younger than I am. Her husband is a minister, and they live right outside of New Haven [Connecticut].

BROCK: Thinking about the area what was it like growing up? Did you live in Wilmington itself?

GOLDEY: My early childhood was spent in Holly Oak, Delaware, a Wilmington suburb. It was not in the high density part. We were in an area with single family homes and a lot of vacant lots around where we could play ball and things of that nature. It was a great place to grow up. Walking distance to the Y [YMCA, Young Men's Christian Association]—it was a mile and a half, but we still walked to the Y downtown. I enjoyed Wilmington. It's a different town now than it was then, like so many are.

BROCK: Was the chemical industry in that area something you were aware of as a youth?

GOLDEY: You couldn't not be aware of it, with DuPont,, Hercules, and Atlas.

BROCK: Do you think that made any impression on you? Thinking about your future career in science, do you thinking growing up in that context had any connection?

GOLDEY: Nothing major. There were a few people in the research part of DuPont that I knew through church. Some of the people at Hercules were very helpful at getting me some scholarship aid at MIT. But as far as the ideas are concerned, I don't really think so.

BROCK: Was there anything in your early youth that you think foreshadowed your interest in science and technology? Do you remember how you first became interested?

GOLDEY: I actually didn't really think about this until I was in college, but I had always had an interest in math. I liked puzzles, and math was a series of puzzles to me. Also, my father used to get some science service, printed with yellow banners, it was very well-known. They would send little experiments out, once a month. So there was always that coming in.

BROCK: Did your father have an interest in science?

GOLDEY: A layman's interest. He understood the importance of it and he encouraged both my brother and me to get into at least technical areas—my brother is a chemical engineer.

BROCK: What sort of puzzles intrigued you as a youth? All sorts, or were there any in particular?

GOLDEY: I like mathematical puzzles and some of these crazy things where you try and take things apart and put them together, forerunners of a Rubik's Cube. Those kinds of things. More, I think, the mathematical, the logical ones.

BROCK: We talked about the puzzles, we talked about playing ball; do you have any other strong memories about your activities as a young boy, let's say up until the high school age?

GOLDEY: I spent a lot of time at the Y after school. We swam there or played billiards, ping-pong. I was probably there three or four days a week. I was also a boy scout. I think those were the major activities.

BROCK: In thinking about your household, was education something that your parents stressed for you and your brother and sister?

GOLDEY: Very much so. My mother did not have a college degree, but my father did. There wasn't any question from the time I can remember that I was going to go to college. The same for my brother and sister.

BROCK: We talked a little bit about your father's interest in science. What other things played a large role in the household? Was religion a big part of your home?

GOLDEY: My father was a very avid stamp collector; he spent a lot of time on that. My brother and I each had some collections, too. My father always said, "If you know the story of the stamps, you know the history of the country." There is a lot of truth to that.

My parents were active members at Grace Methodist Church. My father was a trustee and my mother was a Sunday school teacher. My great aunt, with whom I've lived with, was also very active in the church.

BROCK: Is that something that's been a part of your life since that time?

GOLDEY: It had been. I'm not as active in a participatory sense as my parents were, but I'm a regular church-going member. I was just telling somebody the other day that there is this continuing conflict between science and religion. It's something that I don't have a problem with because maybe fifteen, twenty years ago, I first asked myself a simple question. That was, "Who wrote the laws of physics?" That answered the question for me. [laughter]

BROCK: All right, that was going to be my follow up question, connecting that part of life to the scientific stuff. You mentioned, I think, that someone from your mother's family was a city councilman of Wilmington?

GOLDEY: Yes.

BROCK: Were politics a part of your home?

GOLDEY: There were always lots of discussions. My mother's brother, who lived near us, was very active in the Republican politics in Wilmington. My parents, my brother and I, we never were active in office. I do remember jumping ahead a bit when I was in college and I was the managing editor of the *University of DelawareReview*. That was the year that both the Republican and Democratic conventions were held at Convention Hall, in Philadelphia [Pennsylvania]. I was working in Philadelphia that summer. I happened to call up Alf [Alfred M.] Landon, who had run for president in 1936 against [Franklin D.] Roosevelt; and I got him! We had a nice conversation. He wanted to know all about me and so forth.

BROCK: As you were growing up, of course, there was the [Great] Depression. Could you tell me a little bit about the impact of the Depression on your family and maybe on your community in Wilmington more generally?

GOLDEY: I'm not sure I can tell much about the impact on the community because I was less than ten years old during part of that period. My father lost his job and was laid off. Later he got back. He opened up a little store down on King Street selling seeds. Somehow or other we made it. As a young kid, I heard all the talk and everything, but I always had clothes and food to eat. It didn't have an impact on me that I was conscious of. I was really too young to assess any impact on the community, other than that I was aware that there were many people out of work.

BROCK: Do you think that that was an important experience for your life thereafter?

GOLDEY: I don't really think so. My father used it to emphasize the importance of getting an education, and to work hard. There was a favorite saying in our house: "There's always room at the top."

BROCK: Did you go to the public elementary and grammar school?

GOLDEY: I went all the way through public school in Wilmington. At one point I was offered a scholarship at Tower Hill School, one of the private schools. But, at that time, the public schools were pretty doggone good. My parents thought that if I went to private school I would get a distorted view of the world. They did not accept the scholarship.

BROCK: Was that a scholarship for high school?

GOLDEY: Yes.

BROCK: Were you chosen for that scholarship because you have an aptitude for academic study?

GOLDEY: I was pretty much an "A" student. I was always involved in some sort of extracurricular activity. I was never particularly good at sports so I would try out occasionally. I was an usher, I was on the school paper, and various activities of that type.

BROCK: Did that continue, then, into your high school experience? Did you continue to excel at your studies?

GOLDEY: Yes.

BROCK: Was that across the board or were particularly good in math and—

GOLDEY: Particularly good in math and science. It was pretty much across the board. I'd get an occasional "B" in history or English, maybe, but I also got "A's" in those courses.

BROCK: What academic experiences and interests really stand out in your recollections of your high school years?

GOLDEY: I took four years of French, all the way through. By the time we were in the fourth year, I think the class was down to four or five of us, three women and myself, "girls," as we'd say. I found it all pretty interesting. But I'm not sure that there's any memory that really stands out.

BROCK: Do any teachers or mentors stand out in you mind as having been important to you?

GOLDEY: Well, they were all supportive, but Miss Heindle, the French teacher, she was certainly. I don't think they were particularly influential in what happened in my later life.

BROCK: What about activities outside of school? You mentioned some of your extracurricular activities, but was work a part of your experience in the summer?

GOLDEY: In high school, my main summer job was delivering groceries once I was old enough to drive a truck. I used to do that occasionally during the week as well, but then all day Saturdays.

BROCK: Was that for the butcher shop or—

GOLDEY: No. My grandfather was retired by that time, and my uncle had his own grocery store. No, it was for Hearn's Market, a local market.

BROCK: While you continued to do well in your math classes and your science classes, was that interest deepening or was it just something you were good at?

GOLDEY: I think at that point it was something that I enjoyed and was good at. There's a reason for that, by the way. It's that I didn't realize until I got into college that there was a way that I could earn a living that way.

BROCK: Was that something you were beginning to really think about in high school?

GOLDEY: Well, I was beginning to think, "What will I do?" Not seriously, it was not a big issue in my life at that point. I thought about it.

BROCK: But it wasn't clear to you that there could be a career in—

GOLDEY: In something like math.

BROCK: During your high school years, of course, the Second World War was going on. Could you talk about the general impact of the War [World War II] on the school, on your family?

GOLDEY: While we were in school the biggest impact was probably gasoline rationing—from a kid's point of view. Couldn't have the car, or if I did it was very limited. Then there was the news every day and every night. A little later on, my brother went into the service and I did, but that was after high school, obviously. It was on the news every night, on the newsreels at the movies. It was the major event, there's no question about that.

BROCK: As high school is coming to a close, going to college, for your parents and for yourself, was a foregone conclusion. What were your thoughts about where you wanted to go, what you were going to do?

GOLDEY: Of course, I knew military service was ahead of me. In fact, my cousin and I, crazy as kids are, tried to join the marines, though they wouldn't have us. My eyesight, and he had the same problem. It was half a joke, but one of my friends and I said, "After this is all over, let's go to South America and build bridges." That had an appeal. I also thought at that time that I wanted to be an [United States] Army officer, maybe as a career. That was about it.

BROCK: Was that from the context of the War going on, or was there a tradition in military service in your family?

GOLDEY: Well, no. Only wartime. My father had been in World War I. It was obviously the impact of the War.

BROCK: Could you tell me when your military service began?

GOLDEY: I graduated from high school in June of 1944. I was drafted in September and went to Fort Knox with Kentucky, basic training, armored division. There was a rather funny little incident that occurred then. Several of us had applied for OCS [Officer Candidate School]. One morning they called three of us out, told us to go over to some building. We went over and we never found the right place. The next week, they called the other two fellas out. They went over and they were told that they were too young for OCS. I was younger than they were. My mother's brother had been active in politics. What had happened was that I had received a second alternate appointment to West Point [Military Academy]. That must have scared them off of saying I was too young. So I got into OCS. I actually got my commission when I was still seventeen years old.

BROCK: Oh my goodness!

GOLDEY: Somebody told me later that they saw in *Yank Magazine* that I was the youngest officer in the Army. I've never found it, I don't know whether it was true. But I got through OCS at Fort Sill, field artillery, Oklahoma. OCS at that time had gone up from thirteen weeks to seventeen weeks, but they wanted to give us another thirteen weeks of training. So they gave us ten days to go home and then come back. Well, we were back in about ten days, and they said, "Oops, sorry! We want you in Fort Ord, California, in two weeks. You can go home again." So we did—they were getting ready for the Japanese invasion.

I was at Fort Ord, which was a staging area, part of it, POE [port of embarkation], for two and a half weeks. That's when I fell in love with Carmel California. After a couple of weeks we ended up getting on the bus, going up to San Francisco to get on a ship. As we got off the bus to get on the ship, there was a young kid. In those days, you know, without instant television and so forth, whenever a major story happened the newspapers would put out an extra. This kid was shouting, "Extra! Extra! Read all about the atomic bomb!" Well we didn't know what it was, but it sounded awful good.

We got on the ship and sailed. Then they put the news on the loudspeakers system, and so we hear all about the bomb at Hiroshima.

BROCK: Oh my gosh! On the day that you were setting sail.

GOLDEY: We continued to go, and seven days later we pulled into, of all places, Pearl Harbor [Hawaii]. After five days, the second bomb had gone off. While we were in Pearl Harbor—where it had all began—the War ended. They wouldn't let us off the ship, but there was sure a lot of fireworks we could see.

It's an interesting story because, essentially, the same thing happened to my father. He had gone from a camp on Long Island, gotten on the ship to go to Europe on Armistice Day, only they turned around and came back.

We went on to the Philippines and then Okinawa and then Japan. I was in the Army of occupation there with the First Cavalry division—which is still fighting these days in Iraq—for about six months, and then I went to Korea for six months.

BROCK: What sorts of jobs were you doing while you were in Japan and Korea?

GOLDEY: Particularly in Japan, General MacArthur's idea was to get the Japanese economy going as rapidly as possible. I was in what they called the division artillery as an artilleryman, and the whole First Cavalry and probably other divisions would just go through, street by street with an interpreter, and anything that looked like a commercial establishment; go knock on the door, talk to the proprietor, write a report, and, if there was nothing, particularly, of military value, then they'd get them open in a hurry. I learned to drink a lot of green tea and eat some boiled yams, because they always offered that.

One of the most interesting things that happened to me was, one day we had moved out to fifty, sixty miles out and set up a little base. Near the end of the day, my interpreter and I saw an insurance company and went in. It looked all right. Then there was this other building, and we said, "What's in that building?" They said, "Oh, essentially nothing." The way they said it aroused suspicions, and we said, "We'd like to go in that building." "Oh, no. That wouldn't be possible." Well, you can see how things might have escalated.

BROCK: Yes.

GOLDEY: Finally, I pulled out my .45 and said, "If you don't unlock that, I'm going to shoot the lock off." At which point he went scurrying, and soon the mayor of the town came out and they let us in. In there were files and files and files, and those files were the records of the Japanese Diet all during the War.

BROCK: Oh, my goodness.

GOLDEY: So we quickly got a guard up there and got on the radio and phoned back. The next day the CID [counter intelligence division] intelligence people were out there in a hurry and took over. That was fun.

It was that kind of activity. That was unusual. It was stopping and interviewing people. [In] Korea, we didn't do much of that. It was more just ordinary training.

BROCK: All right. Then after your service in Korea—

GOLDEY: This was well before the Korean War. Then I came home. I was a little late for college—I think I got home in October—but my father had arranged that I could get into the University of Delaware. So, that was a no-brainer to go to Delaware.

BROCK: Did you live on campus or did you commute from home?

GOLDEY: Actually, first year off campus in an apartment in a private home, and then I joined a fraternity on campus.

BROCK: Was the school bursting at the seams with people returning?

GOLDEY: I think that's probably fair to say, though in those days I think enrollment was something like twenty-five hundred and today it's ten times that. It seemed like it.

BROCK: You were there from October of—

GOLDEY: Nineteen forty-six to June of 1950.

BROCK: Could you describe your general impressions of the University during those years?

GOLDEY: That's how I got into physics because I had a physics course which was a lot of fun. By that time, the bombs had been dropped, and so physics was very much in the news. I talked to one of the professors and I said, "This looks like a place where you might be able to have a career." He said, "Absolutely." Because of my strong interest in math, I had originally signed up for pre-law, but one thing that I have never learned to do is read fast. I had all these history courses with textbooks. Both the skill of the professor who was teaching the physics course and that of another professor who was kind of guiding me in the physics department helped me make a quick switch into physics.

The University at that time had about five or six professors on staff, probably only two of whom had Ph.D.s. They have a pretty good school there now. They were really trying to build up their physics program. Delaware had, for many years going back before the War, been good in chemical engineering, primarily because of the local interest and the fact that Professor [Allan P.] Colburn and later, [Robert L.] Pigford, were brought in. It has been a top ten chemical engineering school for fifty-sixty-seventy years.

We were a relatively small bunch. I think in our senior physics classes, just like my senior French classes in high school, there were about four of us. But it was still good. They had facilities there and the whole University had a good football team and that, of course, was a big deal. It was abuzz.

BROCK: Sure.

GOLDEY: Interestingly, since many of us were veterans. You still hear some horror stories—there was one just a week or so ago about fraternity initiations. They didn't mess with us too much. We had silly things we had to do, but nothing really tough. But college was a great experience, I enjoyed that very much. Both socially and academically.

BROCK: Was your work with the student paper your main extracurricular activity?

GOLDEY: Well, that was certainly one. I was on the golf team. Particularly, as I got into my junior and senior years I got into a number of honorary societies. They took a little bit of time, but not too much. Then I ran through the offices in the fraternity and that took some time.

BROCK: Did you work during your undergraduate years?

GOLDEY: Not during the school year, but I did summer jobs, yes. The first year, I went back into the Army for thirteen weeks, repeated OCS without all the malarkey. I was a reserve officer. I just did the coursework; it was a good summer job. Parked cars at the Delaware Park Race Track one year. Worked on the Pennsylvania Railroad one year.

BROCK: What were you doing?

GOLDEY: Just odd labor jobs. Worked for the Speakman Company, a plumbing facility, which still is a manufacturer of showers. Typical summer jobs, nothing very intellectually challenging.

BROCK: It sounds like the idea of being the career Army officer was still in your mind during your undergraduate years.

GOLDEY: Not really. I stayed in the reserves because OCS was a chore and I figured, "I'd like to go back in as an officer if I have to go back in." I figured that was the smart thing to do. Nobody foresaw Korea, and that changed things. As a matter of fact, that happened the summer of 1950, after I graduated from Delaware, before I went to MIT. I went down to the reserve office in downtown Wilmington and talked to the sergeant major there. I said, "Sergeant, I'm going to be going up to MIT. Should I be transferring up to Massachusetts or stay in Delaware?" He said, "Well lieutenant, I know I'm getting a call for nine infantry officers next week, and all I've got in the whole state of Delaware is five. I'll probably be going into other branches, like field artillery." He says, "Up there in Massachusetts they probably will appreciate you a lot more than we will, they knew what MIT's all about." I said, "Sergeant, can you cut those orders for me today?" He said, "Yes, sir!" He cut them. I didn't have to go back because I was not called. I was in the reserves for another five years or so.

BROCK: I'm thinking of your physics coursework as an undergraduate. I suppose you kept up with your mathematics coursework as well. Did you have any opportunities to do experimentation or have any research experiences?

GOLDEY: We had the usual labs and so forth. I signed up for a program there called Degree with Distinction. I had a senior thesis, and so I designed and built a Wilson cloud chamber, and got it working.

BROCK: Were electronics at all—

GOLDEY: Yes, because I had electronics and it was all vacuum tube, of course. I actually built the control circuits. I had a fire or two because electronic circuitry was never my skill, but I got over it and I didn't burn the place down. [laughter]

BROCK: As you get into your physics studies, and you're approaching the end of your time at Delaware, how had your career thinking evolved and how did MIT appear on your radar screen?

GOLDEY: Well, clearly my interest was physics but not at that point narrowed down into a specific field of physics. MIT was on the radar screen; it seemed like it had always been there. Well, my brother had gone for one thing and several of the people a Hercules, I think I mentioned, had been MIT people and were influential. It was one of the obvious choices.

I'm probably telling you more things than you need to know, but there was another interesting anecdote. I applied to about six or seven graduate schools, as people did. In addition to MIT, I think it was Princeton [University] and Columbia [University], [University of] Michigan, [University of California at] Berkeley, Caltech [California Institute of Technology], maybe [University of] Wisconsin. And the Colorado School of Mines because I had an interest in geophysics. I got admitted to all of them except to Colorado School of Mines. [laughter] They said, "Well, if you want to come here and take a year or two of undergraduate geology courses than we'll consider it." But when the MIT thing came through, it was a no-brainer.

BROCK: It was in the fall of 1950 that you went up to Cambridge [Massachusetts]. What was MIT like? I guess you were there from 1950 to 1954.

GOLDEY: I graduated in 1955, but I finished all my work in September-October of 1954. I was impressed in many ways. First of all, you walked in and you see names you recognize because these are the guys that you studied, and so forth. I also got up there three or four days early, as I guess they had encouraged us to do, and I went around to various labs and professors offices to talk with. I thought I might have an interest in acoustics. You may have heard of the firm Bolt, Beranek and Newman—they actually later did the Avery Fisher Hall. I talked to both Dick [Richard H.] Bolt and Leo [Leo L.] Beranek. I talked to Wayne [B.] Nottingham, and I talked to Sandy Brown. What surprised me, in a way, was they were always very glad to see you, gave you time. Here were these people that were practically gods, treating this poor little incoming graduate student with great dignity, and that was impressive. I knew I was in the big leagues because of that. Yes, that was very good indeed.

BROCK: You must have had some coursework that you were asked to do. How early on did you have to settle on who you were going to work with?

GOLDEY: Probably in the second year, and possibly the third. I'm not sure. I took Wayne Nottingham's course on physical electronics. He was a great experimentalist. He was also the thesis professor of Bob [Robert N.] Noyce. Bob was there when I was there—I knew him then.

BROCK: Could you tell me about that course?

GOLDEY: It was primarily experimental, although it did things from physical electronics. It had a lot on things like thermionic emission and so forth. But it also got into solid state [electronics]. That was my first experience in solid state. J. C. Slater also was teaching a course, much more theoretical, and between those two courses. I was just fascinated with semiconductors.

BROCK: Really? From that coursework?

GOLDEY: Yes.

BROCK: Do you have an impression of what it was about the topic of semiconductors that really grabbed you?

GOLDEY: It was something completely new to me. I'd had an undergraduate course in quantum mechanics but it wasn't much more than solving Schrödinger's equation. The whole concept of electrons and holes and the things that could happen. You see the transistor had not been announced very long at that point, so it was new, exciting and something quite different. I thought it could be very important. Of course, I didn't think it could turn out to be what it is, not at that point. [laughter]

BROCK: You say that you knew Bob Noyce slightly.

GOLDEY: I knew him. I asked him later, "Do you remember me from MIT, Bob?" He said, "No." [laughter] Both he and Sandy Hudson, who was in that group too, who spent his career at Bell [Telephone] Labs [Laboratories, Inc.] were both Nottingham's students.

BROCK: Was Sanborn [C.] Brown your advisor?

GOLDEY: It was interesting because Nottingham didn't have very much going on as far as graduate students in the area of semiconductors. Somebody had the idea that you could get electrons, ions and so forth, in a gas discharge or a plasma and the electrons and the semiconductor were a plasma of sorts. I know that [John C.] Slater was behind it, but probably Will [William P.] Allis, and probably Sandy [Sanborn C. Brown]. There was Sandy's gas discharge laboratory, and Allis was the theoretician. We developed all these microwave techniques for studying gaseous plasmas, maybe they could be used for semiconductors. That's how they were like, kind of an interesting thing. I was the only solid state guy in Sandy's lab.

BROCK: What were the other grad students like in the lab?

GOLDEY: They were doing the gas discharges. A number of them ended up at Bell Labs. Saul [Solomon J.] Buchsbaum became an executive VP, actually. Gene [Eugene I.] Gordon is still knocking around, I think maybe at Seton Hall [University], I'm not sure where he is.

BROCK: Was that the context of this research laboratory of electronics? Was that what the group was called?

GOLDEY: That wasn't Sandy's. The RLE [research laboratory of electronics], had a full time director, Al [Albert G.] Hill. Jerry [Jerome B.] Wiesner had that job for a while, before he became president or before he went to Washington's President Services Administration. I forget which he did first. That was probably a staff of ten, twelve professors there. It was an interdisciplinary lab, with physicists and double-Es [electrical engineers]. That was long before the days of computer science.

BROCK: Was the whole lab directed toward solid state?

GOLDEY: There were lots of things going on there. There was magnetic stuff, though I'm not sure that [Francis] Bitter was in that lab, but there was some magnetic stuff. There was lots gaseous electronics, physical electronics.

BROCK: It had a pretty broad front.

GOLDEY: I think that lab still exists.

BROCK: All right. Was Brown's group that you were in one of the groups in the research laboratory of electronics?

GOLDEY: Yes.

BROCK: I understand now. Was it really taking the microwave setup and equipment that they were using for these gas discharge studies and applying it now to the solid state phenomena?

GOLDEY: That's it, applying it.

BROCK: That was how you got your project on—sort of shining that light on germanium, is that right?

GOLDEY: Yes.

BROCK: All right. Could you tell me a little bit more about how your dissertation research unfolded?

GOLDEY: There was a young Chinese lady, Hilda [T.] Hsieh, who later became chancellor of Fudan University in China. She visited us back in Bell Labs about twenty years ago, maybe twenty-five. She was a theoretician and was trying to calculate all the multiple important equations on putting a semiconductor in there, and how we could determine certain properties. We wrote that one paper, the *JAP* [*Journal of Applied Physics*] (1). It was a very interesting thing because I was learning about these things and trying to understand solid state physics and developing my lab techniques with microwaves. There were a lot of very helpful people there, good mechanics and machinists who would help. I was able to get my germanium samples from Bell Labs. I wrote a letter to [William B.] Shockley, and John [A.] Hornbeck turned out to be my chief correspondent. He would supply samples and so forth.

BROCK: Was he someone in Shockley's group?

GOLDEY: He was at that time.

BROCK: Was he a chemist?

GOLDEY: I think John was a physicist, but I'm not sure.

BROCK: Was he the one who was growing crystals of germanium?

GOLDEY: No. There was a Haynes-Hornbeck experiment measuring the drift mobility of the germanium. No, he was more experimental, looking at the properties. He was one of the recruiters. Bell Labs did their graduate school recruiting in a very interesting way. They had staff members who had some relation with a professor usually visit twice a year and then call on maybe just four or five professors. They [the recruiters] got to know their graduate students, followed them all the way through. John, and Julius [P.] Molnar, who later became executive VP at Bell Labs, were the people that would come by. Julius was actually my recruiter. I later got that job at [the University of] Illinois. The reason I got it was because I was a friend of Nick [Nicholas Holonyak, Jr.].

BROCK: Were they MIT men themselves or did they have a relationship with [Sanborn C.] Brown?

GOLDEY: I think they were both MIT guys but they also knew Sandy pretty well.

BROCK: All right. A question about getting your material from Bell Labs in you grad [graduate] research. Was there no local supply of suitable materials from people at MIT or in Boston-Cambridge?

GOLDEY: No. The industry was just getting started. This was 1956-1957. No, it was before that.

BROCK: Was it your idea to get in touch with Shockley about getting some samples?

GOLDEY: I think maybe Sandy or Slater or somebody suggested it.

BROCK: Was he [William B. Shockley] helpful to you?

GOLDEY: Yes, because I got the samples! [laughter] Actually, [John A.] Hornbeck was the guy who became the contact.

BROCK: Did you do any teaching while you were a graduate student?

GOLDEY: A little bit.

BROCK: Can you tell me about that experience?

GOLDEY: I don't remember an awful lot about it. It's something I did, maybe, only for two or three weeks. It was really more of a chore than a challenge.

BROCK: As you were going through your graduate school experience, were you becoming more and more convinced that you wanted to be a researcher rather than an educator?

GOLDEY: I think so. With this interest in semiconductors, it became clear to me certainly by the end of my second year. If I could make it I wanted to get to Bell Labs.

BROCK: Because they had the reputation of second to none.

GOLDEY: Yes.

BROCK: How would you characterize how your dissertation research went? Did you get good results? Did it go relatively smoothly?

GOLDEY: Yes and no. We had a little slab of germanium and a cylindrical wave guide, and measured both the attenuation and the phase shift of the microwave signal going through that. From that we could get the complex dielectric constant—we did at several temperatures. From the complex dielectric constant we could get the effective mass. At first I got results but they were very erratic. I couldn't figure out what was going on. Before I was finished, a guy by the name of Ted [S.] Benedict had done the measurement, but a different way, at Bell Labs; and he was in Shockley's group. It was published as [Ted S.] Benedict and [William B.] Shockley on the effective mass of electrons and holes in germanium (2).

Finally, I think it was one of the technicians who helped. I built the box and the wave divide through the box where the thing was, and I had liquid nitrogen in there to get the low temperature measurement. Cambridge in the summertime can be as humid as Pennsylvania. One of these technicians or machinists said to me, "I think maybe the reason you're getting erratic results is you got that waveguide in there and just ordinary air in, and there's probably all kinds of humidity in there, and you're getting ice formed." I modified the setup and we put in just a couple of little holes and bled the dry nitrogen in, and then we got consistent results, but they were not consistent with Benedict and Shockley. We went through it over and over and over again and, you know, had it critiqued many ways. We stood by our results and published them, eventually. They turned out to be the right results. There have been lots of other techniques developed since then, and those numbers still stand up.

BROCK: Just looking out, then, from that time forward, what about this technique of microwave approaches to studying the properties of semiconducting materials? What was the story of that approach over time?

GOLDEY: I'm not sure that it has really been pursued. To tell you the truth, once I got out of grad school and into Bell Labs, my focus shifted more to PN junctions and devices. There would be an occasional paper or something. There are so many ways you can study semiconductors that I don't think it's the primary way.

BROCK: But one among many sort of secondary—

GOLDEY: Yes.

BROCK: Just then, two other questions. I was interested in about your dissertation research. What kind of source did you use for the microwave radiation?

GOLDEY: It was a klystron.

BROCK: Was that something that the laboratory had already as a Varian tube?

GOLDEY: I don't know who made them. Some of the people in the gas discharge group use magnetrons, but I used klystrons.

BROCK: Were the samples of germanium that you were working with high purity germanium?

GOLDEY: I had asked for several different resistivities, as we weren't quite sure how this was going to go and we wanted to be sure we got enough signals through that we could measure at the other end. I couldn't quote you the number.

BROCK: That's all right. Maybe then we could move to the story of your recruitment to Bell Telephone.

GOLDEY: Bell Labs had staff members come by and stop in every six months, typically. The team that was in our part of the physics department was, as I said, Julius Molnar, John Hornbeck, and John [K.] Galt. Julius was the primary recruiter, so to speak.

BROCK: How did that work? Did he mention it to you in conversation during one of these visits?

GOLDEY: We all knew what was going on. We knew that they were following us and we knew that he was a recruiter. Just because he stopped and talked it didn't necessarily guarantee you an interview. I was getting various feedback. I was pretty sure I would get one when the time became appropriate. When I finished my thesis or was about to finish, he invited me to come down. So I did.

BROCK: Was that the first time you'd been there?

GOLDEY: I think there had been an opportunity earlier but for some reason that had gotten messed up; whether there was a storm or some other problem. Professor Nottingham, I think, would take his students down there every two or three years, but I never did.

BROCK: Who did you talk to during that visit and what impression did the laboratory make on you?

GOLDEY: The way that it worked at Bell Labs was this: you went in and you gave what's called a thesis review. You gave a half-hour presentation and there were people from a number of different departments there. Then I talked to the people in the transistor development department at that time, Jack [Robert A.] Morton's organization. I didn't see Jack [Jack A. Morton] , but I saw Bob [Robert M.] Ryder, Sparky Waltz, Ken [Kenneth D.] Smith. I don't think I saw Ian [M. Ross, president of Bell Telephone Laboratories, Inc., 1979-1991] that day. I saw a few people up in research, and saw some people in the systems area. I saw Hutch [Duncan Hutchings] Looney, who was a former graduate student of Sandy's, but that was primarily, I think, to make me feel good, to see a familiar face.

BROCK: Were you recruited specifically for the development side of the laboratory?

GOLDEY: The way Bell Labs recruiting worked then was that you would give the thesis review and after the interview the various groups of the departments that you interviewed with would let the recruiter know whether they were interested.

BROCK: All right.

GOLDEY: They didn't want to make you bid. But they also asked you, of the things you saw, if you had a bid, what was your order of preference? I chose the development department and that was all I needed because they were interested in me.

BROCK: What did you see there that interested you more than the research side?

GOLDEY: I think it was more focused on interesting, exciting new things.

BROCK: Within Bell Labs, what was the division between the research and the development sides? When you started there in 1954, how much of a boundary was there between these areas and what were the relative sizes in terms of people?

GOLDEY: The boundary, at least at the working level, was almost nonexistent. We worked back and forth all the time, it didn't matter whether this guy was in research or he was in a different supervisory group in development or even in your own group. Bell Labs was always very good at that. They had no walls, at least in those days. In fact, when you went to work there they gave you a dollar, and that was for all your future patents. The whole idea was, don't offer big rewards for patents because that would teach people to keep it to themselves. They wanted to foster open communication. Boy, certainly in our area of the company it was great, always was.

Morton was in charge of what today we would call a laboratory with two or three technical department heads, and then there was a whole lot of other people. Each of the departments had four groups, and each group maybe had eight to ten people, maybe four or five professionals. There was the Shockley Lab, which by that time, I think he was still in Washington, but came back shortly thereafter. He didn't stay long because he had been told he would not become a vice president in research. As he said, if he wasn't going to do that, he'd seen his name and the name of his company enough in the *Physical Review*, he wanted to see it in the *Wall Street Journal* so he left.

There was that activity, which we, the people within, interacted very closely. Then there was the chemical area. Cal [Calvin S.] Fuller, who was one of the early guys on diffusion—very important contribution—was there. Earl Shumacher's another name, do you know that name?

BROCK: I don't.

GOLDEY: I don't know if he's still alive or not. He was the materials guy. The head of the metallurgical lab or something of that type.

Then, Henry , who later had a big role in the epitaxial transistor, and Bill [William G.] Pfann who did the zone melting, were over in Shumacher's lab. [Carl J.] Frosch was in our end of the building so he might have been in the Lacy, Shockley, [Morris] Tanenbaum. [Addison

H.] White took over from Shockley first. He retired in Paris, but I know he came back. He would be somebody you might want to talk to.

BROCK: All right.

GOLDEY: I don't really know the overall size of the research effort.

BROCK: Was there a physical separation of where the development activity was and the research activities, or one building versus another?

GOLDEY: Not really. The chemical guys and metallurgical guys were over in building one. The Shockley group was with us in building two. In fact, Nick actually went and worked with Morris [Tanenbaum], you know, that's how close we were. He actually worked up there for a while.

BROCK: It was really just down the hall, sort of.

GOLDEY: We would be back and forth there all the time. It was a great atmosphere. As a new kid, it was just like that experience at MIT of walking into the professors. Most people that I would approach at Bell Labs and ask them for information or help were more than willing.

BROCK: That really is an unusual atmosphere.

GOLDEY: Oh yes. The management gets a lot of credit for that.

BROCK: In you early weeks and months at Bell Labs, did you have an opportunity to meet John [A.] Moll?

GOLDEY: Oh yes, first day.

BROCK: I wanted to ask you about meeting him and what he was like, how he introduced you to what his group or team was going after?

GOLDEY: Very interesting. John, I love him like a brother. But I'll never forget when I got through the personnel department and they said, "We'll get your supervisor." Most of the other people I had met during that day wore shirts and ties, even in the lab. Up comes this guy, a pair of pants, an open shirt, big broad suspenders, had socks on but then flip-flops like he was at the beach, very soft-spoken. Do you know John?

BROCK: No.

GOLDEY: You ought to get to him if you can. He was really a key player. Soft-spoken and not a real outgoing personality. We became very good friends, but he was sure different than most people that I had met at that stage in my life. He was straight—he was never playing any games with you or anything like that. I think Nick and I went to work on the same day and we both joined John's group.

I think [C.] Harry Knowles, George Benski, Nick and I were the members of the staff, I don't know whether there was anybody else in the group. We had a number of technicians. It was clear that we were becoming the first silicon device group.

There were people doing solar cells and that kind of stuff, but we were trying to make some silicon transistors. That was our role. Harry Knowles was trying to make NPN transistors using alloy technology where you put a little balls of gold antimony in a jig of carbon, throw it into a furnace. The first thing that Nick and I both did was to work on aluminum, evaporating aluminum to think in terms of PNPs or contacts. We worked on it from slightly different perspectives, and he was up with Morry much of the time. That was the first thing of any significance that I did.

BROCK: Could you tell me a little bit more about that?

GOLDEY: There's a question in there later someplace about the paper by Morry, Nick, and myself in the *Electrochemical Society Journal* (3). I actually wrote it but they did much of the work. The technique being used initially was, to evaporate aluminum onto a hot substrate. The trouble was, the aluminum would ball up and you wanted to get a nice uniform junction, and boy it was very tough. Nick and Morry together then discovered that if the substrate were held between the eutectic temperature and the melting temperature of aluminum, and there is about 100 degrees in there if I remember correctly, then you didn't get a severe balling up. My contribution was this: I was trying to evaporate it on to a cold substrate and subsequently alloy, but the trouble is, you would get a little spot over here, but aluminum would dissolve the silicon, then as you cool down it re-grows, as we say, back out as a single crystal, but now it's doped with aluminum. If you put it on cold, you get a penetration here, and one there, so you did not get a uniform junction. Then one afternoon, I remember going over to the library, and I found an article on how they make mirrors. The technique they make to have the silver is to either heat it up ahead of time, to pre-bake it so that you get rid of all the organic, or to bombard it with a gas discharge. I tried that, and lo and behold, we pre-baked it which was the easiest thing to do, and then it evaporated, and then, later alloyed it and you got a nice, uniform junction. That made a lot of things possible. We were developing those alloy techniques, and I'll tell you the truth, I don't remember all the sequences properly, but sometime along the way, the diffusion became obvious that it was going to work, even though maybe it hadn't worked yet. Jack Morton had been involved, in his earlier life, with vacuum tubes working closely with Western Electric [Company] in Allentown. He understood something about manufacturing—the concept of "batch processing" was the term that was used—diffused the whole wafer.

BROCK: Right.

GOLDEY: Though we made some PNP transistors and Harry made some NPNs alloy, once it became clear about diffusion—. I can still remember meeting in Jack's office with John, Nick, Harry, and Gene [A. Eugene] Anderson. There was the decision that silicon device was going to be made by diffusion. I don't know whether that was before or after Morry had made the first transistor.

BROCK: From what I read, it was soon thereafter. My understanding is that after Morry Tanenbaum made his diffused transistor, that that was a real confirmatory thing to set the direction for diffusion.

GOLDEY: I think that's probably right. Charley [Charles A.] Lee made the diffused based germanium transistor.

BROCK: Was that before you had arrived?

GOLDEY: No. Those things were announced publicly at the device research conferences down in Philly [Philadelphia, Pennsylvania], at Penn [University of Pennsylvania] (4). Morry and Charley gave the two talks. If memory serves me right, that was the 1955 Device Research Conference.

BROCK: I think that's right. Could you talk about John Moll's role in contributions to the work? Maybe what he did particularly technically, or maybe how he helped you.

GOLDEY: It was just tremendous. First of all he wrote a number of papers himself—there's the famous [James J.] Ebers and [John L.] Moll switching transistor paper (5). John was in with us every day, going over our day with us, making suggestions. He was very much a hands-on supervisor, but not, in any sense, a dominant one. He wasn't there like a foreman telling you what to do. He was making suggestions and asking questions. Very, very good. Exceedingly important in his contributions. He was a very thoughtful, very bright guy, not flashy at all. I can't say enough good things about John.

BROCK: To the extent that his group had more or less explicit goals, was the goal when you joined to make silicon transistors or was it to make this PNPN switch?

GOLDEY: When I first joined it was the silicon transistors, but it switched over. Now I think Nick might have gone off into another group that was going to be the developer of the silicon transistor, and the Moll group remained then, on the PNPN. Morry also made the first PNPN by accident. I think he had an NPN diffused transistor, and the way you would contact the base—this was before photoresist—was by putting a dot of aluminum on and alloy it right through the N-type layer, because the N-type wasn't degenerate so there was a junction there and you could pick up the base. One time, he didn't alloy it all the way through, so he had PNPN. I looked at it, and here was this PNPN characteristic. That became our assignment: the PNPN.

BROCK: The Moll group that you were in?

GOLDEY: Yes.

BROCK: That would have been in the 1955 time frame?

GOLDEY: Yes. Somewhere in there.

BROCK: Could you tell me a little bit about where the idea for making this PNPN switch device came from?

GOLDEY: It's in Shockley's book, actually (6). They had actually mocked up a PNP and an NPN and they connected the base of the PNP to the collector of the NPN and *vice versa*—and you can get that characteristic. Jim [James J.] Ebers was the first guy to show it. The concept was understood. That came, to the best of my knowledge, from Bill [William B.] Shockley. The idea was that that would make what they call a "cross-point switch" for electronic switching. The way they talked in those days about so-called "end-marked switching network" is that, here's the caller and there's lots of lines and so forth—there's the one you want to call to—put a voltage on both and let [the signal] pick its own path through the network. <u>End</u>-

marked. With this characteristic, that would work. That was the whole idea of it, developing the prime cross-point switch. We used to call it the "cross-point," but "cross-point" switch is really a term from switching systems. People who understood the telephone business knew that would be great. To the best of my knowledge, it came out of Shockley, but that may or may not be true. I bet you John Moll would know.

BROCK: Silicon would be particularly appropriate for this kind of PNPN device?

GOLDEY: When you reverse biased the junction, the leakage current was too high in germanium and there would be all kinds of crosstalk getting through. That's why silicon was the material of choice there.

BROCK: We've talked a little bit about Jack Morton, but maybe we can talk about him directly, as you got to know him in these early days and then subsequently. Can you just talk about his role in the silicon work at Bell Labs?

GOLDEY: He was a driving force. Jack was the classic Type-A [personality]. He was hard working, hard playing, hard drinking. He was a driven man, no question about it. He was a man of great vision. He understood the impact the diffusion and enjoyed the things he did too. I guess everybody did, but Jack drove it. He was very good at getting resources. He would get into trouble occasionally because he would tell what we would refer to as our customers, really the systems development area, what they should be doing. He could be a very tough guy too. It was indeed unfortunate that he met his untimely death. He made some wrong bets later on, like magnetic bubbles and things like that, but he sure made some right ones earlier on, both his microwave triod tube and the whole silicon business. I think he may be underappreciated because of some of his bad calls, but I think he was very important to that whole effort. He and Shockley got along very well together, as far as I could tell. They were three levels above me in that stage and I got to know Shockley very well later, after he went to California. I think I got to know Jack pretty well too.

BROCK: Maybe we could spend a moment and talk a little bit about Bill Shockley and your sense of, in the early days of the laboratory, how closely Shockley was following the silicon work?

GOLDEY: He was in Washington finishing up that Defense Department assignment, came back and didn't stay long. I didn't really interact with Bill much because I was just a little too late for that, though I got to know him quite well later. You'd hear his name mentioned a lot, and so forth. There had been an awful lot written about him, of course.

BROCK: Oh yes.

GOLDEY: It had to do with the fall of AT&T, when SBC bought them. I can't tell you very much about Shockley in the Bell Labs days.

BROCK: What about later on? Maybe we could follow that Shockley story a little bit.

GOLDEY: I had actually forgotten these telephone calls that are mentioned in here, but I guess we did do that. I don't have much of a memory of that. It certainly is true, I think you quoted John there, saying that the Bell Labs management wanted Shockley to succeed. I don't think there was any question that that was true. They recognized his contributions and in fact, after his company folded, he came back on staff as a consultant for a while.

BROCK: I didn't realize that.

GOLDEY: I got to know him through those calls. One thing we did at Bell Labs a lot was make what we called "licensee visits" to people who had bought those twenty-five thousand dollar licenses. Most people did not regard us as a major competitor, and we could visit TI [Texas Instruments, Inc.], Fairchild [Semiconductor Corporation] after they got started. After Bill was going there we visited him a number of times. I visited him four or five times.

BROCK: During the Shockley Semiconductor period?

GOLDEY: I saw him back at Murray Hill [New Jersey, Bell Telephone Laboratories, Inc. research facility] too. He tried to hire me a couple of times. Well, you know the story of Fairchild I'm sure. You talked to Gordon [E. Moore].

BROCK: Was this in the 1956 to 1960 period that you were making these license visits?

GOLDEY: Probably.

BROCK: Was it just you going out to—?

GOLDEY: We always went out in two or three people.

BROCK: Do you remember any of the other people who would go out to the Shockley operation with you?

GOLDEY: Well, we went with different teams, usually; I think Howard [H.] Loar—we probably went with John Atalla at one time. He had a couple guys on his staff who later came to work at Bell Labs: Adolf Goetzberger, Hans Queisser. We made a number of those trips and it's hard to remember who went on which one.

BROCK: As you got to know him through the context of those phone calls in 1955 and then these visits—do you have a recollection of what you talked about in those series of telephone consultations? Was it technical issues?

GOLDEY: Oh yes, it was. I don't have much of a recollection to tell you the truth. There were always problems with the PNPN, both in the technology and in the design as well. "How do you do this? How do you do that?" and so forth. That kind of stuff.

BROCK: As you had that interaction with him and then you were making those licensee visits, what sort of impression did you form of the Shockley Semiconductor [Corporation] lab [laboratory], the people there, and what they were doing with the silicon technology as compared to how you were developing the silicon technology back in New Jersey?

GOLDEY: They were fairly close. When I made those visits, that was after the Fairchild guys had left. Not the first time, but most of the time because the guys I remember interacting with out there were Adolf Goetzberger and Hans Queisser. It was very similar; they were a much smaller operation. They were also focusing on PNPNs because Bill [William B. Shockley] thought that would be a big device in the future. Well it turned out to be a fairly important device as a three terminal device for silicon thyratron but not what we all thought it might be as a telephone switch. That was, I think, one of the reasons—there were several—why the Fairchild guys left. You've talked to Gordon so you got that story better from him.

BROCK: There's this piece of lore surrounding their departure to form Fairchild, where, the group of dissidents, if you wanted to call them that, were having some meetings with Arnold [O.] Beckman, explaining to him some of the issues they were having about the direction and the management of the Shockley Semiconductor. Arnold Beckman was trying to decide how he should handle this. The piece of lore is that somebody from Bell Labs got in touch with Arnold Beckman, or Arnold Beckman got in touch with somebody at Bell Labs who said, "You really need to go with Shockley rather than this other group," for a number of reasons. That it would be damaging to Shockley's career, just coming off the Nobel Prize, for this to happen. Do you know anything about that?

GOLDEY: All second and third hand. I know one of the emissaries was Bill [William C.] Hittinger. He later became the executive vice-president of RCA [Radio Corporation of America] but he lives over here in Bethlehem, now. He retired over there.

BROCK: Who was acting on Shockley's behalf?

GOLDEY: He was sent out by Jack. Bill was a very even-headed, calm, cool, collected guy. I remember, at the fiftieth anniversary of the transistor, over at Murray Hill he and Gordon had a brief conversation about that. Gordon was one of the two outsides invited to that meeting, Gordon and Jack [S.] Kilby. I don't really know anything firsthand, so I don't think I should go on further.

BROCK: Maybe I'll try and talk to Bill about that. In this period of 1954-1955, the early period, I'm trying to get a sense of how many different people there were working on making silicon devices. There was Tanenbaum's group and your group. Were there others?

GOLDEY: In Morton's organization, there was the Ian [M.] Ross group first, which was called Functional Devices. You talked about Art [L. Arthur] D'Asaro in there, in that stepping switch. There was the [John L.] Moll group. I think it was Bill Hittinger's group that was trying to make the first transistor. There was the Mason [A.] Clark group, now this is a specific device for switching application. Mason Clark was doing some power transistors. There were those four groups at least. Then there was Sparky Woltz, but he was doing some microwave devices. Ken Smith was doing diodes. Jim Ebers' became the Hittinger group. There were at least four groups, probably, by the end of 1955—early 1956.

BROCK: That sounds to me like a significant fraction of the activity under Morton.

GOLDEY: It was at least one full sub-department, as we called them in those days.

BROCK: Do you think that would have represented back-of-the-envelope, a quarter of the activity under his direction, or more?

GOLDEY: Yes, because there was Jim [James M.] Early's group on germanium diffused base transistors. The Allentown [Bell Telephone Laboratories, Inc. manufacturing facility] people reported to Jack in those days. [William J.] Pietenpol was the department head out here. I would say, a quarter to a third doing silicon transistor-type things. Then there were all the diode activities, which were silicon too.

BROCK: So about the collaborations, between people within these different development groups, with people over in research, like Tanenbaum or in the chemistry materials metallurgy at the working level—was that just very loosely organized between individuals or was there any sort of more formalized process for that?

GOLDEY: We had joint seminars. Certainly, the supervisors, like John [L. Moll] and people like that greased the skids, suggested you go see so-and-so. A number of the people who were in the development, Ian Ross was a good example, had come out of research, there were others, [John A.] Hornbeck. I don't know who designed the grand plan, to be quite honest, but it was a good effort. I always felt very comfortable and very much at ease in talking to any of the people in other organizations.

BROCK: Were these joint seminars, were they mainly presentations by members of the technical staff?

GOLDEY: Yes.

BROCK: All right. Did you ever bring in outsiders?

GOLDEY: All on a more formal basis, yes. They would have some of those.

BROCK: Thinking about, again, this 1955 time frame, were there any technicians that you worked closely with, or with whom others worked closely, who played particularly important parts in the silicon work? Do any names stand out there?

GOLDEY: There were a lot who did things. Ralph Heffelwhite was one of ours. Phil [Philip W.] Foy, over in research, and Link Derick. Derick worked with [Carl J.] Frosch, and Foy worked somewhere in the Tanenbaum operation.

BROCK: In your early work with the alloying metallization, from where were you getting your materials? Were you getting silicon slices from somebody in particular?

GOLDEY: We usually got ingots from research, usually. Ernie Buehler, Ernie [Ernest D.] Kolb. They would, on occasion, grow things to our specifications, not a formal specification. Yes, that's where we got our stuff in the early days.

BROCK: Once you got those ingots, were you doing all the subsequent processing work yourself?

GOLDEY: Certainly somebody sliced them up, and then lapped them and polished them. We had a model shop downstairs. I don't really remember who did what. That was part of the development process, how to polish, how to lap, how to etch. A lot of chemistry in that.

BROCK: In the polishing?

GOLDEY: The etching, yes.

BROCK: Were these etching processes where you would put black wax on it as a mask for the etch?

GOLDEY: We needed to get rid of the mechanical damage from the mapping. Yes, we certainly did that too with the black wax.

BROCK: I guess it was in early 1955 that Tanenbaum made his diffused silicon transistor.

GOLDEY: I think so, because I think it was reported at that June meeting.

BROCK: Did that create a stir at all in the laboratory or for those working on the silicon side?

GOLDEY: Yes, both in the labs and at the device research conference and in the fledgling industry. To the first diffusion symposium, actually.

BROCK: Was that in 1956?

GOLDEY: I think so.

BROCK: Could you tell me a little bit about that, or your involvement in it?

GOLDEY: I think I gave a talk on aluminum alloying then, the evaporation. I think all licensees, both domestic and foreign, were invited to that and invited to send two or three people. I think it was a three-day symposium, certainly, two. I remember we had a dinner in Newark one night. I can't imagine anybody going to Newark for dinner now! [laughter] But there wasn't that much out in the Murray Hill area. It was certainly well attended. It was lots of questions. It was clear that the people from outside thought it was something major, realized it was something major.

BROCK: Were you talking about diffusion technology broadly, that is to say, in both germanium and silicon, or was it really focused on silicon by that point?

GOLDEY: No. It was both as I recall. Again, we had speakers from research and speakers from development. I'm sure Cal Fuller gave a talk, I'm sure Carl [J.] Frosch gave a talk, I'm sure Morry did. Yes, and we talked about the metallization as well.

BROCK: Do any of the licensee attendees standout in your mind as having been there, people who later made the most of what they learned at the conference?

GOLDEY: I remember Willis Adcock was there. Bob Noyce was there. I think , from Philips International, Inc., was there. I don't know whether those guys from Fairchild that had split off at that time or not, but Bob was there, I know. A fellow from Siemens [AG] that I knew quite well, I can't think of his name at the moment. I remember the British guys. Bill [William] Webster and probably Ed [Edward] Johnson, from RCA. Somebody from Westinghouse Electric Company. GE [General Electric Company] probably, the guy that did the alloy transistor, I think it begins with a "D," the last name [Crant and Dunlap].

BROCK: Oh gosh. I'm sure that I don't know.

GOLDEY: Earl Thomas was probably there. Whether Earl was with Osram Sylvania Inc. then, or still with Lincoln Lab at MIT. Tom [Joseph T.] Longo might have been there but I'm not sure. Maybe Arnie [I. Arnold] Lesk from GE, too. He was from GE then, but I mentioned him later as from Motorola Inc.

BROCK: Just thinking about that diffusion conference, about these licensee visits, it strikes me that while Bell Labs had to diffuse this technology because of the consent decree, it's my impression that it wasn't done begrudgingly, that people were enthusiastic, in a way, about doing it.

GOLDEY: You're right. It was even done before the consent decree, wasn't it?

BROCK: I think so.

GOLDEY: I think the Bell Lab management recognized that this was in the national interest, even in the world interest. There are obviously important defense implications in the technology, but the last thing in the world we wanted was for the government to get into it and classify it. I wasn't in the management at that time, but I heard it. Jack and others recognized the potential importance of it and for it to grow, it had to grow freely. The fact that it was only twenty-five thousand bucks, which was a lot more then than it is now—but still. TI got into that and made their first transistors in a hurry. It was done willingly and openly. I was not in that loop at that time, but still, I heard enough about it.

BROCK: I guess the next thing I wanted to ask you about was your own efforts to create this PNP diffused base transistor with the alloyed emitter that you were working on in 1955. Was that a big proportion of your work?

GOLDEY: No. We got into the PNPN pretty quickly, though we did do a little bit of that other as I recall. The focus shifted to the PNPN.

BROCK: All right. In some things that I have read, in thinking about Carl [J.] Frosch's work on his discoveries about oxidation and oxide masking—did he come up with that in the context of doing diffusion work to support your work with the PNPN switch?

GOLDEY: He was supporting, I think, in a broad sense, all of the people trying to develop devices. He was a very friendly, very helpful guy. He was interested in that problem and he was very anxious to have people interact with him who could make use of that too. He and Nick were particularly close there for a while. He was certainly supporting that work, but others as well.

BROCK: Could we talk a little bit about George Binski and where he fits into the story during this period?

GOLDEY: An important thing, in those days, was the lifetime of a minority carrier. Initially, in transistors, it was important just so you could get a high alpha [current transmission factor]. But in PNPNs, the whole idea was, "How does this thing really work?" It was clear that you had to have an alpha, which varied with injection current. That implied something about traps. George was the guy who did major work on identifying gold and controlling gold. I think he probably looked at copper as well. By the way, I forgot Herman Perry, when we were talking about technicians. George did that work and wrote several papers, as I recall, on particularly the gold traps, the gold levels (8).

BROCK: Was it later that he was involved in identifying this gettering process?

GOLDEY: There was gettering with metals and also gettering with glass. Phosphorus glass was very important. That glass reminds me of Stu [Stuart] Flaeschen, who later went to Motorola. He was always kind of on the fringe, but he was a factor out there.

BROCK: In looking at diagrams of stepping transistor that were published, it appears to have a planar structure. Do you have any recollections of that development?

GOLDEY: That was Art [L. Arthur D'Asaro], and Ian was the supervisor of that group. Howard Loar was in it. It was basically, as I recall, some PNPNs adjacent to each other and there was a way that the carriers would drift from one to the other, and then turn on the next one. That's why it was "step around:" you could form a shift register. I was so bold in one of the histories of integrated circuits of the transistor you said, to put it in, that that was, perhaps the world's first integrated circuit. I don't think we should claim that as a major deal because we weren't thinking in the way that Jack Kilby was or Bob Noyce was—this was an interesting device. Semiconductors are such wonderful things, and Jack Morton would talk about functional devices all the time. The idea of a functional device is, "Let's make use of new phenomena and we don't have to design circuits in the classic way, building them up out of components and maybe we can achieve a function, directly." That was a dream of his, which never really came about except in very special cases.

BROCK: I'm not sure that I completely understand his concept there. Would it be to craft a piece of material so that it performed an electronic function?

GOLDEY: In fact, the example that he always used to use was the quartz crystal. He says, "You can't find the inductors or the capacitors in the quartz crystal, but it will do the same thing as an LC circuit." That was what he was thinking of.

The Air Force out in the Wright-Patterson Air Force Base—Dick [Richard] Albert. He put money into Westinghouse for molecular electronics and they were growing ribbons of silicon, for what purpose, God only knows! Then, somehow they did something in TI as well. It's exactly the same thing that Jack was talking about.

BROCK: Who was Dick Albert?

GOLDEY: He was a contract administrator. He was a technical guy. I don't know his exact title.

BROCK: Was he the one behind molecular electronics?

GOLDEY: Yes.

BROCK: Did you look at it at Bell Labs, as something—

GOLDEY: That's a boondoggle. [laughter]

BROCK: Could you tell me about that?

GOLDEY: Since Jack was pushing us the same way—if you get a long, thin strip of silicon, what is that? They would have things, they said, and so forth. I think it's fair to say we didn't get it. [laughter] There were some people in Bell Labs in Morton's organization, who were really pushing integrated circuits as a fact. Jack grudgingly came along. But when LSI [large scale integrated circuits] came, his expression was, "large scale idiocy."

BROCK: If you don't mind, maybe we could talk a little bit about what his attitude toward integrated circuits as they came on the scene around 1960-1961.

GOLDEY: My understanding is he probably thought that he was still on this idea of functional devices and if we could do it much simpler in code with the fundamental properties of matter to achieve a function, that that was the way to do it, rather than making transistors and other components smaller and smaller and pack in more silicate. He just didn't buy into that. As I say, he was a brilliant guy and he made many right calls earlier and into mid-career. Later, his calls were not as good. It happens to a lot of people.

BROCK: That must've been an interesting situation for you. I'm thinking in the sort of mid-1960s when you really got into the integrated circuit work.

GOLDEY: I was promoted to department head in 1961. I came to Allentown in 1966. We began some stuff back at Murray Hill then. The most important part of that whole thing. I mentioned Atalla's name earlier, still not properly credited, in my view, with the invention of what is now called the MOS [metal oxide semiconductor] transistor, which we called the IGFET, for insulated gate field effect transistor, by Atalla and [Dawon] Kahng. Dawon has passed on now, but John is still very much alive. I believe that's a real injustice.

BROCK: The broad recognition for that—

GOLDEY: No prizes, no big prizes at least. There are people who would say, "Well, it's true that they invented it, but Bell Labs didn't capitalize on it." I accept that, certainly we weren't the first to capitalize on it. But, it seems to me that that's got nothing to do with the credit for who invented it.

In a sense, sometimes you're victims of your own success. That was announced at the device research conference at Carnegie Mellon in 1959, 1960, or 1961 (9). The same year that we announced the epitaxial transistor, and that was the thing that blew the roof off of everything. So that MOS paper, though recognized as important, was not the big event at that conference (10).

BROCK: I see. It was overshadowed by the epitaxy. To think about your personal work in the period from 1956 through 1959, how did the PNPN switch work develop over those years? Was that the major direction of your work across that time?

GOLDEY: 1956 to 1959, I'm sure of it. I think I became a supervisor in 1956. There was actually an internal competition in Bell Labs for the so-called cross-point switch, between our device and the gas tube. It turned out that they both lost when Alex Feiner came up and invented the ferreed switch. The ferreed was where they just took an old mechanical relay, wrapped the ferrite around it, so they could drive it electronically, and go on, and even though the switch took a few milliseconds, or whatever it takes to close, the circuit could be getting faster. We had a problem with the PNPN device, called dynamic breakdown, which meant, if you tried to fire it several times in rapid succession, the breakdown voltage would drop down. We kept thinking it was leakage current and everything else. Finally Ian analyzed the problem and realized that it was just the charge buildup. The way to go around it was to make a complex junction where you had a small portion, which would determine the breakdown voltage, and then the rest of the junction was much higher resistivity. Therefore, it would be lower capacity because the space change layer would widen out and that worked. By that time the ferreed had come along. Then Ian Macintosh, who was in the group, got interested in the three terminal device and wrote his paper on that.

BROCK: Could you say some words about Macintosh as a person and his contributions?

GOLDEY: Oh yes. He was a very interesting guy. A Scotsman, I guess. Quite diligent. That was basically his baby, the three terminal device. He really characterized it and did the thing. Very diligent. We didn't have too many applications at that time in the Bell system for it, but his name stands out as the guy who really did that. That device is widely used now. GE gave it another name, the thyristor. It's in all your dimmer switches, plus major power things like theater lights and wherever used to use rheostats or thyrotrons. Then Ian Macintosh came out to the Reading [Pennsylvania] lab Bell Telephone Laboratories, Inc. manufacturing facility] as a supervisor on some diode group after that.

BROCK: The Reading lab, was that a Bell Labs branch laboratory in a Western Electric operation?

GOLDEY: Just like Allentown, Pennsylvania, Western Electric Company manufacturing facility. It was originally setup for military devices, but that changed over the years.

BROCK: Was 1956 to 1959 the period in which the Allentown, Western Electric manufacturing facility was being constructed, or was that later?

GOLDEY: It was earlier. If you could catch up with Bill Hittinger, he, probably more than anyone else, can tell you that story. Jim Ebers had a very early death from a brain tumor, and Bill came out and took over the Allentown lab.

BROCK: Was the Allentown facility originally manufacturing vacuum tubes?

GOLDEY: Yes.

BROCK: Then it also added semiconductor lines.

GOLDEY: Yes.

BROCK: In thinking about the sitting of these facilities in Allentown, in Reading—it really does seem that there's this straight shot on this route 78 corridor from the lab's other AT&T system facilities. Was that intentional?

GOLDEY: I think so. The whole concept began when Mervin [J.] Kelly, who was president of Bell Labs [1951-1959], wrote a memo talking about how we are going to get high technology into manufacture. He came up with the concept of a branch lab because it would be too difficult, in his view, to transfer a technology from Murray Hill to a factory which turned out to be in Pennsylvania. That was the branch-lab concept, which later spread all around the Bell system. The first branch lab was Allentown.

There was a man by the name of Warren Diefer, who was an engineer for Western Electric, in New Jersey, but he was an Allentonian. He always came back to Allentown for his dental work. He was talking to his dentist, and he told him that Western Electric was going to build a new factory somewhere for the electron tubes. The dentist said, "Why don't you do it here?" That, somehow, got it started when Warren went back. Then the initial question was, would it be in Allentown or would it be over on that circle or in that general area—alpha New Jersey. It's where 78 and 22 separate. They decided that they didn't want to do it there because that would be downwind from Bethlehem Steel. So it came to Allentown.

BROCK: In this time frame, Bell Labs was already out in Murray Hill?

GOLDEY: I believe so. This was before my time. Then Reading was kind of an offshoot of Allentown.

BROCK: In thinking about the branch laboratory, it seems that it would be possible for transferring the technology to the manufacturing facility.

GOLDEY: Yes.

BROCK: It also seemed that that siting would allow the possibility for people from the Allentown branch lab to go to Murray Hill in a day, or vice versa.

GOLDEY: All the time.

BROCK: That was a big circulation. Interesting. Was the theory that production lines for new devices that got perfected in Allentown could, once they were engineered, could be moved somewhere else to even a larger place, or did they stay?

GOLDEY: They tended to stay. Now just twenty years ago they built a factory in Orlando [Florida] and they put a few devices out in Kansas City [Missouri] at one point, not so much semiconductors as vacuum tubes. Once they got into solid manufacturing, it was then Western Electric's decision to do what they felt was appropriate.

BROCK: All right. Going back yet again to this 1956 to 1959 time frame, it seems like many of the basic silicon technologies are really getting developed at Bell Labs during that period. If not a little bit before, we have the oxidation, diffusion, metallization, gettering going on in this period. Can you talk about when the photolithography begins to emerge as a technique?

GOLDEY: It's hard for me to put dates on these things. A fellow by the name of Jules Andrus started it. He got to talking initially to [L. Arthur] D'Asaro, Ian and [Howard H.] Loar, I think that was the first application through a working device. I'm a little hazy on that but you have to talk to Ian, he could probably confirm it. That developed and we all immediately saw the benefits of that because by this time. The ease that could come with getting contacts in, because in the past they had used shadow masks and everything else for evaporating gold from one side and aluminum from another. You've probably heard Gordon [E.] Moore's interesting story about using aluminum to make the contact. He tried everything else and he talked to Bob one day, and Bob said, "Try aluminum," and it worked! Because it was so degenerate.

It was immediately recognized that that was going to be powerful, though there were a lot of problems with photoresists at that time. We were getting them primarily from Kodak Eastman Company, but there were some other players there. Semiconductor type purity and everything else applies to photoresist as well as all the other things. We were playing in a different ball park than anybody else who was using that stuff. I think Kodak got convinced and others and really put some major efforts into it.

I think it was Ian, Morry, and Bill Hittinger, who went out to visit Fairchild just at the birth of the planar transistor. I don't have those dates, but I think the planar transistor came along very close to the time of epitaxy.

BROCK: Around 1960.

GOLDEY: Around 1960, give or take a year. Jean [A.] Hoerni's contribution there was major, but I think because of the work that D'Asaro and [Gordon E.] Moore did. That was all done at about the same time.

BROCK: Was the Allentown branch laboratory out here in this time?

GOLDEY: Oh yes. It was out here when I came to work in 1954. [William J.] Pietenpol, who later took over Morton's job when Morton got promoted, was the department head out here, and he reported back to Jack .

BROCK: When was the first time that silicon devices really got into any sort of volume production or into manufacturing in the Bell system?

GOLDEY: I should know the answer to that. I think it was probably in some of the transmission systems [land line communication systems], the L-4 maybe, or even the L-3, or L-5, certainly coaxial cables. Silicon became a big deal in the switching systems and ESS [electronic switching system] but those were big, longer term developments. I would imagine that they first got into the transmission systems.

BROCK: In terms of just producing the devices themselves. In a lot of things you can find to read about this period, the focus is on Bell Labs developing the central silicon technologies, and then they focus on the relationships with the licensees and how it diffuses out that way. Then people develop it and start manufacturing things in Fairchild and TI. What about the diffusion of the silicon technology developed at Bell Labs to Western Electric, and when Western Electric first starts producing things like silicon transistors?

GOLDEY: Western wouldn't do it until the systems were developed. They didn't just make them and put them on the shelf. All these early transistors were custom designed rather than general purpose. The systems were built in the Merrimack Valley [Works, Massachusetts, Bell Telephone System manufacturing] plant, and in a few other places, but that was the lead plant, there was a big Bell Labs up there, too. They were usually the first. There had been trials, of course. There was a P-carrier system down in Georgia, and it was actually using a germanium transistor. I don't think that ever became a production system. It was when the system would be ready, that's when Allentown would gear up. The big users became the switching systems, but that was a little later.

BROCK: It seems like the development for a silicon device that's going to be used in the Bell System, is in essence tied into the development of a much larger technology like the new transmission system, etc.

GOLDEY: Exactly.

BROCK: It seems there were many components in simultaneous development in the overall system. As if it was a much longer time process than, as you say, "Make them and put them on the shelf."

GOLDEY: One of the things that Bell Labs didn't do was get things into production fast, but that was because the things were going into big systems, which had relatively long development time.

BROCK: That's interesting. That's the first time I've heard it put as clearly is that.

GOLDEY: There were transmission systems and switching systems. There were also what we called then, EPBXs: Electronic Private Branch Exchange. That might have been the first active switching application.

BROCK: That would be, for example, the telephone system for accompany or something along those lines. Was it in the 1959 time period that you had a change in role with your job? Did you begin the move into the more management track at that time?

GOLDEY: It was 1956 when I became the supervisor. I think I became a department head in 1961.

BROCK: Was that unusually quick? You had only been there for roughly a year and a half.

GOLDEY: The supervisor was fairly quick, yes. The others were more standard. Then, maybe I became a department head in 1959.

BROCK: I think you took on a new role in directing transistor development in 1959 to 1961. In 1961, you became a department head for the silicon transistor and the integrated circuit department.

GOLDEY: All right. I thought that epitaxy was in 1960.

BROCK: I think it is.

GOLDEY: I might have been a department head at that time.

BROCK: We can turn to that. In thinking about this period from 1959 to 1961, in that time frame, what changes did your supervisory role entail and what were your responsibilities? Did it change a lot for you?

GOLDEY: We had been working on the PNPNs, when I became a department head, we still had a PNPN group. We also had a silicon transistor group, and then the Atalla-Kahng effort, which led to the MOS. I might have been promoted in 1959. The big thing that happened, and I didn't play very much of a role in it, was epitaxy.

BROCK: Could you describe that development?

GOLDEY: I don't know if Henry Theuerer gave a talk or if Ian just happened to be over there talking to him, but Henry showed how he could take a rod of silicon and, by vapor, grow additional silicon on it. Ian recognized immediately that if you put that on a plain wafer, you can have a heavily doped material there and put a lightly doped base on it. Normally, if you're diffusing it from the same side you've got to go heavier, heavier, heavier, and that's what we wanted. He had one of the best imaginations of anybody I have ever met in my life. They tried it on wafers. Howard [H. Loar] brought all the stuff in to make it into a transistor.

BROCK: Howard?

GOLDEY: Howard Loar. Also, there was a guy by the name of Howard Christensen, who had toyed with the concept in germanium earlier, and he got back into the game on germanium. That was a major breakthrough: epitaxy. Epitaxy and [the] planar [process] together are basically the two thing that really made the integrated circuit possible.

BROCK: What was the context in which Theuerer came up with this vapor deposition of the silicon? Different ways to make a crystal?

GOLDEY: He was over in the metallurgy department. I knew Henry, but I didn't interact with him that much unless something happened. I don't really know what was in the back of his mind. He was one of these guys who was hired as a messenger or something like that. Very creative guy. It happened more than once in Bell Labs. You think of all this high-powered recruiting and all these Ph.D.s who do fine, but Henry, I think, was a genius, and most of the management could recognize talent.

BROCK: Wherever it might be.

GOLDEY: It was a conscious effort in Bell Labs, the only people called "doctor," were the president and the medical staff.

BROCK: Really? I didn't realize that.

GOLDEY: Oh, yes.

BROCK: Was it pretty much a first name—

GOLDEY: Oh, yes. Breaking down barriers. Interesting because, having grown up in Wilmington, I know a fair amount about DuPont. I actually interviewed there, but just to keep my wife happy. [laughter] I knew I was going to Bell Labs. Anyway, there, everybody, sometimes including the janitor, was called doctor because at least in those days, they were afraid of offending somebody who had a Ph.D. by not calling them doctor. It was just the opposite of Bell Labs.

BROCK: People just referred to one another by first name?

GOLDEY: Yes.

BROCK: What's your sense of how that differed from other East Coast Laboratories? Was it different from other East Coast Laboratories?

GOLDEY: Not that I know of. I think, at RCA, they were all on a first name basis. At GE, I think the same was true.

BROCK: All right. In this period, important things are being developed. We've talked a little bit about the various conferences that happened every year that everybody was participating in. I know from talking to others that they had a sense of this healthy, technological competition that really could get expressed at these conferences. Of course, everybody outside Bell Labs really felt like they were competing with Bell Labs, that they were sort of the pace setter or mark of where things were. How did it feel from inside Bell Labs? Was there a sense of a competition with groups in academe and in these other companies?

GOLDEY: I think the answer is, "Yes, but—." There was a competition not in that we wanted to take on GE or RCA or TI, it was just that we wanted to be there first. I think most of the people, at least, were friendly and approachable, and outsiders as well. We probably were pretty arrogant. We knew we were pretty good as an organization.

Those conferences were great. What used to be called informally the Device Conference became the Solid State Research Conference. That, for many years, did not allow people from *Electronic News* and other places in. It was basically invitation only, even though there were three-hundred people there. If that conference is still going on, they'd probably let them in. It's different than the solid state circuits conference, which was a more formal thing. That device conference was where people really wanted to give their hot stuff because all your peers were there.

BROCK: That was really where the community was.

GOLDEY: That was always a two-and-a-half day conference. There was usually good stuff there, not only from us, but a lot of other people. IBM [International Business Machines Corporation] had good stuff, and RCA, too.

BROCK: Just to say it in a slightly different way. In this silicon technology competition, it sounds like there wasn't a business motivation to the competition with Bell Labs, but rather, a typical science technology competition that you'll find even in academic contexts.

GOLDEY: That was certainly in those days. That's a good description.

BROCK: Is that because at the end of the day, you'd have to license the technology anyway?

GOLDEY: We didn't "have to" as much as "want to" I think. We were frequently criticized internally by not being close enough and responsive enough to systems customers. Some of that was justified, but not all of it. Yes, we got out kicks by being out there first—no question about it! [laughter] We also recognized that there were a lot of other good outfits out there, and there were a lot of good people out there, and particularly later on, as the whole Silicon Valley thing got going.

BROCK: One other question that I wanted to ask you about this general period up to 1961, was, in the silicon technology area, where there many academics or academic groups that stood out as people who would participate in the device conference or the circuits conference?

GOLDEY: There were certainly some. Lincoln Lab [Laboratory], if you want to call that an academic lab. It's not quite the same as RLE, but it is under the same MIT umbrella. It was one of the earliest. There were papers from time to time, certainly, out of the Fan group at Purdue [University]. Of course, once Stanford [University] got going a little later, there were important papers there. Also, when Carver [A. Mead] got going at Caltech [California Institute of Technology]. Initially, he was the only guy there, at least that I remember. There was a guy at the University of Arizona, who came up with a charge control concept. Berkeley got involved—more on the circuit and applications area—but then the whole scanning electron microscope came out.

BROCK: I thought the scanning electron microscope came out of RCA, initially.

GOLDEY: There was a regular electron microscope, but the SEM [scanning electron microscope]—

BROCK: Oh yes. Didn't that originally come out of IBM?

GOLDEY: It might have. I remember the papers. There was, of course, Van Winkle, up at [the University of] Minnesota. Of course, the Illinois group got going a little bit later. Much later, Florida State [University]. Maybe there was some out of Penn, at the Moore School [of Electrical Engineering], more at the circuits conference than the device area. There was Jim [James H.] Mulligan [Jr.], at NYU [New York University]. There was a little work coming out of Brooklyn Poly [Brooklyn Polytechnic Institute]. Those are the major ones that ring a bell.

BROCK: If you think about that academic component in this silicon, solid state community, were they the minority group compared to the people working in industrial contexts?

GOLDEY: I think so. There was a lot of activity at Murray Hill and at Princeton with RCA, and even GE, Westinghouse initially. The only big player, initially, was Lincoln Lab. In later years, Stanford became a major force.

BROCK: What was the context for Lincoln Labs getting into the semiconductor work?

GOLDEY: I don't know. I know those guys well, Earl Thomas and Bob [Robert H. Kingston]. What their justification was, I don't know. Lincoln Lab was basically an Air Force program.

BROCK: For temperature, and things like that, of silicon? That's what I would guess.

GOLDEY: I can't really tell you. Bob Kingston. Bob Kingston or Earl Thomas, if you happened to run into them, if they're still around.

BROCK: Maybe the last thing we could talk about before we break would be this period in 1961, in which you became the head of the silicon transistor and integrated circuit department at Bell Labs. I think you served in that role from around 1961, maybe a little earlier, up until 1965. Could you describe that change for you?

GOLDEY: We did a lot of crazy things then. We were thinking about multiple-chip integrated circuits. Putting a chip all on a substrate or even inside a header, connecting them with wires. We were heavily focused on getting the maximum speed, primarily, and we were deluded into thinking that if you put a lot of things in the same substrate the parasitic capacitance and everything else, it would be slower and wouldn't be what we were interested in doing. We made circuits like that for Nike-X [missile defense system], and tried to do things for ESS, electronic switching systems, the stuff out at Indian Hill [Illinois, Bell Telephone Laboratories, Inc. research facility].

I think it's fair to say—and how much of it was Morton's influence and how much of it was that we missed the boat—we didn't really see the way that integrated circuits were going to go, the way that people like Gordon and Bob [Noyce] did. I think we have to admit that. As a matter of fact, maybe the reason we didn't run with that—as we called it IGFET [insulated gate field effect transistor], the MOS transistor—was we knew that that was an inferior device from the point of view of speed; performance through a bipolar transistor. Yet we didn't have applications. I think that others really did pick up the ball there, though we were doing some good stuff.

We were also concerned on passivation. We were worried that the header, the metal package we used was expensive. There was a big drive on to get rid of that. Silicon nitride work, that was a very important development. Interestingly, I don't know whether Gordon remembers it. The first paper on silicon nitride was given by some IBM people and they didn't recognize what they had. We came back and we seized on that silicon nitride. That became the passivating layer, because the silicon dioxide was a good diffusion mask, but it was not a passiviting layer. Andy [Andrew S.] Grove and Bruce [E.] Deal and [Edward H.] Snow did work with the sodium at Fairchild, but silicon nitride solved that problem. We were doing a lot on that.

Again, there are our chemical friends. Chemists were developing the techniques for putting it down, and there was the whole question of the etching and how could you do this. That was a major effort during that time period. It wasn't only in my group but we were players there.

I guess what you could only say was another distraction came along, that was the whole idea of beam leads. Do you know what I'm talking about?

BROCK: I've heard the term a number of times, but I don't know that I fully grasp it.

GOLDEY: The idea was, you've got lots of circuits on a wafer on an integrated circuit. At the edge you put up fingers of primarily gold. Then as you're continuing to scribe and break, you etch out this and leave these free, hanging fingers. Then you turn the thing upside down, and bond it down. Another concern was the high cost of wafer bondage. That was before all these very high-speed bonders came on. That was the whole concept that Morton had of diffusion being batch processing. This was batch processing of the lead attachment process. A lot of people, particularly the West Coast guys, saw that that was a huge waste of the silicon. That you had half of these beams and then you had to etch all that silicon away, and you'd probably cut down the number of circuits on a wafer buy a factor of three or four.

We pushed that for a while because Jack Morton and Bell Labs had, and there were systems areas using the thin film technology. We were going to hybrid, and then we were going to put semiconductor components onto that. The thin film technology was tantalum-based for our in-house stuff. Again that was following our own path, satisfying our own needs, rather than doing something for the industry as a whole. We called that beam lead sealed junction technology. The fact is, it worked, and you didn't need any more encapsulation. They put a little wax or some rubber over the silicon, just to give mechanical protection. But that was not the way the industry went, but it was useful for the Bell system at the time.

BROCK: The last question I would ask this morning would be about that multi-chip assembly that went into the Nike missile. Could you tell us a little bit more about that project, that technology?

GOLDEY: I think we probably had a little ceramic disk with metallization on it and bonded chips to it and interconnected them with bonded wires and then encapsulated it, maybe had a few gates in there.

BROCK: What was it doing in the missile?

GOLDEY: I don't really know. That system never got deployed. That, of course, was a political decision. However it was a way of getting very high performance, there's no question of that, combining some of the benefits of integrated circuits. It was smaller than using a bunch of individual headers and so forth, but it was nothing like what we know today.

BROCK: I was curious about it because if it was adopted for that program, than certainly it must have passed considerable muster for reliability and performance.

GOLDEY: I think there was no question about that because it was basically using known technology in a slightly different way. I don't think there was any concern about that.

BROCK: Maybe this is a good point for us to break.

GOLDEY: All right.

[END OF INTERVIEW]

INTERVIEWEE: **James M. Goldey**

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

LOCATION: **Allentown, Pennsylvania**

DATE: **8 April 2005**

BROCK: This is a second oral history interview session with Jim Goldey, taking place on 8 April 2005, in Allentown, Pennsylvania, with David Brock and Christophe Lécuyer as the interviewers.

We had left off last time at a transitional period for you, right around 1961, when you became the head of the silicon transistor and integrated circuit department of the Bell Telephone Laboratories Inc. Could you describe that change in role for you?

GOLDEY: It was really a promotion, to move up to very similar work in what had been my former group. The new supervisor then reported to me, as did several others. That was the PNPN group. In addition there was the silicon transistor group, the beginnings of an integrated circuit group. We were still developing devices for both new generic devices, in some cases, and in other cases, specific for use in, or potential use in Bell Telephone System applications.

BROCK: This was still in the development side of Bell [Telephone] Labs [Laboratories, Inc.].

GOLDEY: Very much so. During the five years or so I was there, before transferring to Allentown [Bell Telephone Laboratories, Inc.], a number of notable things happened, most of which I can't claim any credit for, but which occurred in the organization and may be worth mentioning.

Probably the single most important one was of the epitaxial transistor, which was conceived by Ian [M.] Ross, who was my boss, and who later became president of Bell Labs. It was carried out, from the point of view of the device, by Howard [H.] Loar, one of the supervisors. That made a big splash at that solid state device research conference, the June conference at Carnegie [Institute of Technology] (11).

There was another piece of big news that year coming out of Bell Labs which got overshadowed by that. That was the invention by Dawon Kahng and John Atalla, of what we then called the IGFET, for insulated gate field effect transistor, commonly known as the MOS [metal oxide semiconductor] transistor. That was also in the department, in a separate supervisory group.

There was also important work done on my old device, the PNPN, by Ian Macintosh. This was the three terminal device, the triode. We began our foray into integrated circuits during this five-year period, and we did some things which, looking back on, were pretty stupid, I guess. [laughter]

BROCK: In what sense?

GOLDEY: The whole idea in the Bell Labs at that time was that the thing that really matters is performance, which mainly is speed, in our case; high voltage wasn't a big deal. As the work of the Fairchild guys, Bob [Robert N.] Noyce and company, was going along, as was that of Jack [S.] Kilby down in Dallas [Texas], we saw the great accomplishment for Jack [S. Kilby], but we didn't see that as a viable commercial thing. We recognized that the planar technology and so forth would be, but we also knew it would never perform as fast because of the added parasitics as maybe discrete devices. We came up with this idea of multiple-chip, as we called it, integrated circuits, and then putting them in a common package, one of the old TO-5 [transistor], and clearly we were wrong on that. But it did take up a substantial effort.

LÉCUYER: Were these the hybrid circuits?

GOLDEY: No, this was before the hybrid circuits, because the thin film operation was beginning with the Henry [A.] Stone people, Dave [David] MacLean, Charlie [Charles] Goddard, and David Feldman, but those hybrid circuits became a reality later in that time period. Marty [Martin P.] Lepselter was the principal guy in the invention of the beam lead transistor.

Originally, we made a lot of transistors in the same wafer and then etched them apart. But there were beams, they called them—those large contacts extending out from the device—and when we etched in a part, we turned them over, and then we could bond those to the hybrid circuit, or to the thin-film circuit, which made it a hybrid. Though that was not a real big deal in the industry, it was a fairly big deal in the Bell System, and found lots of applications in things from telephones up to ESS [electronic switching systems]. It was used in the ESS-1A, I guess, which was developed out at Indian Hill [Illinois, Bell Telephone Laboratories, Inc. research facility].

That whole hybrid technology was thought to be a major future technology for the Bell System. Jack [A.] Morton, who was our vice president and driving force behind much of this, had set up what he called PCLs, for process capability laboratories, at Indianapolis, which was not a device plant, but it was where Western [Electric Company] manufactured telephones; at Merrimack Valley [Works, Massachusetts Bell Telephone System manufacturing facility] which was the main transmission plant; and at Lyle [Illinois, Bell Telephone Laboratories, Inc.

manufacturing facility], which was the introduction to manufacturing for ESS. It's right near Indian Hill.

If I'm saying things that you don't know what I'm talking about, stop me. But I know you know an awful lot.

There was a Bell Labs department head and a few Bell Labs people at each of those locations, but most of the staff there were Western Electric engineers on loan. The whole idea was to ease the transition, design for manufacture, into manufacture. That technology remained important for a long time.

BROCK: What characteristics did that hybrid circuit technology have, if you're comparing it to, for instance, a planar integrated circuit?

GOLDEY: The passive components, the resistors and the capacitors, were tantalum based, and they were very precise. They could be controlled by anodization processes, and you could get a very good classical type component. The capacitors and resistors you make out of silicon aren't good standalone components. However, obviously, people have learned how to make good circuits with those. The active components, which initially were discrete transistors, had all the high performance. They were free of the parasitics that you get in the others. That was the drive to performance.

LÉCUYER: It seems to me that many system houses made the same choice?

GOLDEY: Some followed our lead, that's right.

LÉCUYER: CDC [Control Data Corporation] and all these choices about technology—

GOLDEY: There certainly were a number, and they had their own conferences. I didn't go to many of their conferences. The mainstream silicon company, the TI [Texas Instruments, Inc.], Fairchild [Semiconductor Corporation], later Intel [Corporation], didn't pick it up in any big way. TI had a little bit, but I'm not even sure of that. We knew we were in on something important, and doing something important, but none of us foresaw what was really going to happen in the semiconductor business. Not at that time, not in the early sixties. Even though we always thought of ourselves as long-range thinkers, we were solving the problems of the next decade, not the next twenty-five years. Some people might take exception to that, but that's the way I look at it.

BROCK: I wanted to loop you back a little bit and ask about some general features about the silicon transistor and IC [integrated circuit] department in this period, the general size in terms of the professional personnel.

GOLDEY: By the way, when I was promoted, Ian was the director, and then Jim [James M.] Early became that, we were part of what was called the Transistor Development Laboratory. There were other departments with different titles, which were also doing quite similar work, but on microwave stuff. Jim Early was still involved in germanium transistors, the whole thing in the Barney [Bernard C.] DeLoach [Jr.] and company, the varactors [diodes], and then later the IMPATT [impact ionization avalanche transit-time] diodes. That was all part of Ross's organization.

As far as my department was concerned, there were four supervisory groups, and each supervisory group probably had five to seven MTS [member of technical staff] members, technical staff, and an equivalent number of technicians, approximately one for one. The department was maybe whatever that multiplies out to be.

BROCK: Fifty people?

GOLDEY: Something like that. Most of the staff were Ph.D.s, a good sprinkling of Europeans, particularly English, and Germans, primarily. The rest tended to be Americans. A few Asians were coming in, but not at the level that we have today.

BROCK: Each one of those supervisory groups were device centered, and it was to really develop a particular device?

GOLDEY: In part. There was the PNPN work, for example, which had been my old group. When MacIntosh did that work, he was not the supervisor. He later became the supervisor in Reading [Pennsylvania, Bell Telephone Laboratories, Inc.]. He was characterizing the device. He wrote a classic paper in the Proceedings, on the impact of the base current and how it affects all the other characteristics, and so forth. There was the general work on advancing the technology in the device understanding of various classes of devices, but there was also the development of a specific transistor, which would work closely with what we called the systems people at Indian Hill. They were probably still at Whippany [New Jersey, Bell Telephone Laboratories, Inc. research facility] at the time. I don't remember whether there was a Holmdel. There were a lot of systems areas in Murray Hill [New Jersey, Bell Telephone Laboratories, Inc. research facility] at that time.

Most of what we called "final development," the specific design and the hammering out of detailed characteristics, and negotiating those with Western in a way that they could make, was done by the people in Allentown. Sometime in that period Reading got a start, but initially as a developer on the military side. It was called Laureldale [Pennsylvania, Bell Telephone Laboratories, Inc.] at that time, for military work, most of it in the Bell System. Whippany at that time was primarily involved in Nike [missile defense system], underwater sound, and various defense systems.

Jim Early continued his work on germanium transistors and other high-speed devices, and they saw use in military systems, but I'm a little foggy on that. It was pretty well recognized because, back to this old question of performance, germanium outperformed silicon because of higher mobilities. Because of the oxides and all that kind of stuff, we recognized that the future of the world was clearly in silicon. That's where the effort was, but there were still one or two groups doing germanium.

Martin Atalla was the supervisor, and Atalla and Dawon Kahng had a separate group out of which came that MOS transistor. They were primarily trying to understand how to passivate the surfaces, because it was well recognized by then that there was an awful lot of cost in the package, the hermetically sealed TO-5 and the successor packages. The goal to get these surfaces passivated was set fairly early on by Jack Morton. We knew there was some passivation from the oxide work already, and that's what Atalla's group were really working on. They came up with the MOS.

They would probably be offended by this, they were focusing on the chemistry, the oxide surface, so I don't think they understood devices at the detailed level that many of the other people did. There was a major breakthrough in the understanding of what was causing the failure at Fairchild, and that was Andy [Andrew S.] Grove and Bruce [E.] Deal and [Edward H.] Snow.

I was still at Murray Hill when the big break came which really did lead to devices which would be adequate for Bell [Telephone] System and even for military application. It came with the development of silicon nitride, which was done in the Allentown Lab [laboratory]. It was interesting that they were well ahead of Murray Hill on that.

There's an interesting story, you might ask Gordon [Moore] if he remembers it. In the history of silicon nitride, there was a paper given by IBM at the PGED Conference in Washington, and I was sitting next to Gordon chatting about it afterwards, said it sounded good, and so forth. They were talking about the masking, and I think they said that it kept the sodium out.

Anyway, a chemist, Arpahd Berg, has a somewhat parallel history to Andy Grove. He escaped from Hungary. There was Harold Nish, Howard Loar, who by that time had come to Allentown and been promoted, and others. They developed the idea of silicon nitride and the sandwich with silicon oxide, and verified that it was a good sodium barrier as well as a water barrier. That, for many years, was a standard. We called it the sealed junction, and with that we convinced ourselves that we did not need to use the hermetic packages anymore, so that was a big thing.

LÉCUYER: What were the steps in the process?

GOLDEY: It was tricky because of how they did the etching. They tried lots of things. You could not put silicon nitride, at least at that time, directly onto silicon. It was done by one of these CVD-type [chemical vapor deposition] depositions. What happened is they put the silicon dioxide on, next to the silicon, and then the silicon nitride on top of that, and then etched a hole for the metallization.

LÉCUYER: Did it act as a getterer?

GOLDEY: No, as a barrier. P-glass, phosphorous oxide glass, was a good getterer, and that had been used in various device lines along the way. A lot of people were doing that, and I wouldn't know who got the major credit for that. It was widely used.

I think it might have been while I was still in Murray Hill that some people in Allentown began looking at MOS transistors and trying to apply the silicon nitride to MOS. It did not work very well, because the surface is much more sensitive, surface states and so forth. Harold Nish was involved, and Arpahd Berg was involved, and they found that aluminum oxide, Al_2O_3, also served as a barrier and did not have the deleterious effects on the MOS. That became at least the Bell System standard, and a lot of other people picked up the junction sealer, as we called it, as well.

I think they're the main things that I remember from that period of time. It's interesting, sometimes old technology triumphs. We were originally developing the PNPN as a diode for what is called a cross point switch. Do you know what I'm talking about?

BROCK: Yes.

GOLDEY: The whole idea initially was to use what they called "end marking." If I'm calling you on [one line] and on yours coming out the other side, putting voltage through maybe a six-stage network, and let the cross points fire that wanted to fire, it would find its own path through the thing. It's a cross-point switch, major application.

We frequently held competitions in the labs, but we still cooperated. This was another development group, and they were still using gas tubes. There was a huge advantage of PNPN's, because once they turned only that appeared across the devices, whereas a gas tube had a high what we called "sustained voltage," and we thought we would just knock their socks off.

We were winning, but then Alec [Feiner] who was actually over in the systems area at Whippany for ESS, had a great idea. He said, "We want the control circuitry to be able to be high-speed, but in terms of closing a switch, that doesn't have to be high-speed. That can take microseconds, even milliseconds," as long as the control was done and the controller can go on to its next task. He invented what was called the ferreed switch, which was a reed relay. Do you know what I'm talking about?

BROCK: No.

GOLDEY: It's two little pieces of metal inside of a glass bulb, and it's a plain old mechanical switch. What he did to make all this work is he put a piece of ferrite around it, and so the high-speed electronic controller would come and switch the ferrite and then the controller would go about its business. It may take a few milliseconds for this to close, but no problem. There you've got a real metallic connection and everything else, and so that's what was used in the analog versions of ESS that were deployed.

BROCK: Wow.

LÉCUYER: These seem to be very inexpensive to make.

GOLDEY: They were pretty inexpensive, too, and pretty reliable. I think the key was that he recognized that you wanted the high-speed in the control circuitry, but the switches are closing, in those days at least, just to benefit humans, and a human can't tell what a millisecond is.

BROCK: Right.

GOLDEY: Even if it was ten. That was part of the fun of Bell Labs. There were so many bright people all over the place, and we all communicated well, and it was a great, great experience. That was the main thing for that period in my department.

BROCK: Just to stay in this period of time, and going back to that comment that you just made about the ease of communications, from your side on development, what were the interactions like with any groups that might have been doing related work on the research side in that period?

GOLDEY: We talked a little bit about this last time. There was always free flow of information. Back in the very early days of modern silicon, not the grown junction stuff. Nick [Nicholas] Holonyak [Jr.], who went to work the same day I did, in the same group, John [A.] Moll's group, was doing most of his work up with Morry Tanenbaum and George [C. George] Dacey. Carl [J.] Frosch was in the research department. Nick was really interacting with Carl, but I probably had two or three conversations a day with him.

BROCK: That just continued?

GOLDEY: That just continued.

BROCK: I was interested to hear a little bit more about the epitaxial transistor work that happened in this period.

GOLDEY: Oh yes. Ian Ross is one of the most innovative, creative, imaginative people that I've ever known. He was over at a seminar in research. By the way, all the seminars were open to anybody in the labs. But you couldn't go to all of them. Ian was over there, and Henry [C.] Theuerer, who was a metallurgist up from the ranks. I think he was hired as a technician, maybe even as a mail boy originally. He gave a talk on an epitaxial process where you could deposit silicon onto a rod of single-crystal silicon. I think it was some silane in those days, but there were two or three basic processes, one was silane, one was silicon tet, tetrachloride. Henry was interested in growing crystals, and this deposited silicon would, in fact, deposit as a single crystal. It was pencil-shaped. I think it was either hexagonal or octagonal in cross section.

Ian heard that, and then he got to thinking, because at that time we were well into the era of diffused transistors, and we made diffused transistors is by starting with a piece of fairly high resistance silicon, and then diffusing in NPNs with some boron and then some phosphorous. The wafer had to be so thick, but the wafer was high resistance silicon.

That caused a number of problems. One is the plain old resistance, but the other is that in the case of the saturating transistor, when you drove both junctions forward bias, of course the emitter junction would go forward bias in the emitter. We're generally talking maybe base rings and so forth, but the collector would go forward bias under the base, and the most forward bias base would be there. This happened because otherwise the base current with that voltage drops. Then there was this space between the base ring and the emitter where the mobile carriers were.

There was a base-resistance problem. In particular, this was a problem for switching transistors. When you turned that thing off you had all these carriers, some of which were fairly far from the junction, and you had to wait. You had a long storage time, and you had added voltage when the transistor was on, VCE and so forth, and that was a major problem. The competitor to silicon diffused-base transistors in those days was probably still the Philco [Corporation] MADT [transistor].

BROCK: Yes.

GOLDEY: The etched and then plated device. Ian came back and thought about this. He says, "If Henry can put that on a rod, I wonder if he could put it on a highly conductive low-resistance substrate." The answer is yes, and out of that came the epitaxial transistor, because then you had a collector region on which the thing was built of heavily doped phosphorous. You then epitaxially deposited a very thin layer of high resistance silicon, and then you did the diffusions on top of that, and that basically solved the problem.

That paper was presented at the device conference at Carnegie Mellon, which was the same year that Atalla and Kahng's paper was presented, but it was overshadowed by the epitaxy, because that was the mainstream technology (9). I remember walking out of the session during a break, or maybe it was lunchtime, and as we were walking out of that meeting, Bernie [Bernard J.] Rothlein turned to me and said, "What you guys just did to Philco." [laughter] It's true. I don't know whether you're familiar with that name—with Ed [Edward N.] Clarke, he was one of the guys who started National Semiconductor [Corporation], which was then a small operation up in Connecticut. They eventually, of course, sold it. Epitaxy was a big deal; still is.

There was an effort started at Bell Labs for epitaxial germanium transistor by Jim Early's group, and a fellow by the name of Howard Christensen, who had done some earlier work on epitaxy, not related with devices. It was in Early's department, and he had done something about it, so they did the work on the germanium epitaxial transistor. That was really quite a story.

BROCK: Did that work require new forms of equipment?

GOLDEY: Yes, because you had to develop the process for growing the epitaxial layers, and both silane depositions and silicon tetrachloride were used. Silane was a little bit more hazardous, but silane became the favored thing.

I talked about [Martin P.] Lepselter's beam lead transistor. He had a four-metal system, and that's when sputtering came in and got applied to the semiconductor.

BROCK: In this same period?

GOLDEY: Yes.

BROCK: In this period of time we've talked about what a lot of people in your area were doing. What was your day like? What were you concentrating on?

GOLDEY: I tried to keep my hand in. I didn't actually have a lab at that point. I would spend a couple of hours a day, at least, wandering about the lab. We would have seminars and just internal project reviews. There was a little more money in that job, but it wasn't quite as much fun. [laughter]

We also did a lot of talking, interacting with the systems area people, like the PNPN people, with the ESS. ESS was <u>the</u> big project at that time. There was the mainstream ESS, which was originally 1 and 1A, then 4. But there were other versions. There was an electronic PBX [private branch exchange]. That's a real side issue, but interesting. The whole idea was, it became possible that for a PBX you could have two parts, the switch out on, say, the customer's premises, and you could have the control back in the telephone company office. You could share that control among a number of customers. Everybody thought that was a great idea. In the end, telephone companies never bought it. The reason was: say Staples builds a new store over here, and they want their PBX, and so if the telephone company doesn't have it, they've got to buy a whole new central processor only to serve one customer, until the others came in. Anyway, they were still important.

Somewhere in the background, too, in this period was Telstar [satellite communications system]. I would never call myself a bureaucrat, because I always thought of myself as a technical guy, but it was reading memos, interacting with the people, getting updates, talking to our so-called customers, going to seminars, lots of different things, as opposed to mainly working on one or two projects. When you're a supervisor you could still do a lot. At that time Ian Ross, as director, he still did a lot, but Ian is a very special guy. You ought to try and talk to him if you can.

BROCK: I'd like to.

GOLDEY: He does winters in Florida, and I don't remember whether they still come back to New Jersey in the summer or not.

BROCK: I think the address that the folks at Bell Labs had for him was in northern New Jersey.

GOLDEY: In Shrewsbury, I think.

BROCK: I can't remember off the top of my head.

GOLDEY: It's in that Red Bank area.

BROCK: That's right.

GOLDEY: I was also involved in IEEE [Institute of Electrical and Electronic Engineers] affairs. It's not every day, of course, but it was an important part of my activity. I chaired a task force on the development of standards for PNPNs. I was on a couple of other subcommittees as well. I was active in the Device Research Conference, and going through all the chairs during the early sixties period, as program chairman and then general chairman of that conference. I was an active participant, but to a little lesser extent, in the Solid State Device Research Conference. I was also in the PGED, the Professional Group on Electron Devices, now called IEDM [IEDS, IEEE Electron Devices Society

BROCK: Were these the three big conferences for this community?

GOLDEY: That's right. There was the device research conference, which later became the SSDRC [Solid State Device Research Conference]. It was, in many respects, an illegal conference, because if it's an IRE-sponsored event, anybody from the—IRE, which it was in those days—IEEE, could go, but before I was even there, the attempt was always to keep it private and small; broad. People from any company of interest, or university could come, but they didn't want the marketers, the press, and anything, because they really wanted to foster the interaction.

We never published anything at that conference. It was not regarded as publication, so everybody could keep their patent people happy. It was not disclosure. It very well did an awful lot of good stuff there. I think it was the year I was the technical program chairman, or the general chairman, which would have been 1960 or 1961—that we made some changes and we did publish abstracts in the *Transactions* [*on Electron Devices*] of the Electron Device Society. That was all right at that time, because we were over those initial phases. That was a big part of my life, Jim Early the same.

By the way, I think the letter on the epitaxial transistor was authored by [Howard H.] Loar, Joe Kleimack, [Henry C.] Theuerer, and possibly [Howard] Christensen (12). Ian's name wasn't even on the paper, but he later received, I guess it was the [Morris N.] Leibmann Award for it, because everybody recognized that he was really the key guy.

LÉCUYER: Why is it that his name wasn't on the paper?

GOLDEY: I don't remember. They even acknowledged me in that paper. [laughter] They acknowledged him and they acknowledged Jim Early. He probably said, "Well, you guys made the transistor." He was always willing to share things.

BROCK: What was the informal social life like at the meetings?

GOLDEY: I don't know whether I should give you an honest answer, but I will. You said I could always edit this.

BROCK: That's right.

GOLDEY: It was a two-and-a-half-day conference, and there were rump sessions in the evening, and then there was a night that there was a banquet. The banquet was pretty informal, and I could best describe the social life by telling you that most times we were told by the university where we had the party, "Don't come back." [laughter] There are stories that I heard before I even got there, about things at Penn State and other places, but we had some corkers. [laughter]

BROCK: I've heard a couple of people talk about the Penn State incident, where the university said, "Please don't return."

GOLDEY: I think they told us that at New Hampshire, too. But the answer is that everybody regarded themselves as a fraternity. I remember one of the first years up at Boulder [Colorado] in 1957. Do you know that mountain right behind Boulder? We had a cookout and there were steaks up there. They said, "There's a few extra steaks, and you can have another steak, but you've got to give your name."

Well, Jim [James J.] Ebers had been one of the early players at Bell Labs, with [the] Ebers-Moll [model], and so forth, and who, unfortunately, died very young of a brain tumor. We all went through that line to get the extra steak, "Name?" "Jim Ebers." [laughter] We did all sorts of crazy things. They were pretty wild. I don't think there were any women at the banquet night, unless maybe Esther [M.] Conwell, who was a contributor, or some families if it was a nice place. It was pretty much a stag affair.

BROCK: Was she one of the only women who were part of the group?

GOLDEY: Well, she's certainly the most notable. She's still kicking. I saw her recently in some article.

BROCK: Does she live in Rochester?

GOLDEY: I don't know. She had been with Sylvania for a long time. We had a woman, Eileen Tanenbaum, no relation to Morry.

BROCK: Was she from Bell?

GOLDEY: She was in Atalla's group in metal-oxide operation. I forget whether she was Eileen Tanenbaum who married a man by the name of Handelman, or whether she was Eileen Handelman, who married somebody by the name of Tanenbaum. That was one of the interesting things we used to laugh about. During, I guess it was one of the Egyptian-Israeli wars in the early sixties, the department head at that time was Ian Ross, who was a Brit. The supervisor was John Atalla, who was Egyptian, and the MTS was Eileen, who was Jewish. [laughter]

LÉCUYER: Could we go back for a moment to integrated circuits? I was wondering, was there any work done in your group on the semi-integrated circuits at that time?

GOLDEY: We were just beginning on some of those things, but I was still at Murray Hill. At our instigation, a group to begin integrated circuit development was set up in Allentown. To show you how these things do arise, the creation of that group was concocted over a bar in Philadelphia at the circuits conference (12), on a dull afternoon. Ian Ross, who by that time had become director at Allentown, Jim Early, who had been promoted to director at Murray Hill, and I, as department head, were out drinking a few beers in the middle of the afternoon. We decided, "We've really got to get something going here," and that was how that got started. Probably everybody else will deny it.

We were doing studies, and one of the big questions was if we could do these integrated circuits. The answer is, we were doing a few things at Murray Hill, but they never really got going. How can you get it into a system? The big system at that time, either in or about to go into manufacture, was ESS, and it didn't make sense to completely redesign something to put these integrated circuits in.

We had a fellow by the name of Dick [Richard] Lindner, who was looking at the idea, what we called board replacement. In other words, can we redesign a circuit board that was used in ESS and make an integrated circuit that has the same function, the same terminals, but that never took off.

The thing that really got things going out here came along when John Forester was the first department head. I think it was the Nike-Xproject, because I know when I came we were pretty heavily involved in that. Also, one of the first things was to try to get a semiconductor memory activity started here, which we did.

I think I'm jumping back a little bit, but I just want to talk about the big advances. In a sense, that old stepper of Ross, [L. Arthur.] D'Asaro, and Loar was an integrated circuit. It had several devices on one chip, but we never called that, never recognized. The work of Jean [A.] Hoerni at Fairchild was fundamental, as was the bringing together of epitaxy and the planar technology.

Then, of course, Bernie [Bernard T.] Murphy, who had been around the circuit a little bit and then rejoined us, he's the guy that came up with the so-called buried layer, the buried collector in epitaxy, which really finally solved all those problems. You had a region and just had the collector diffused in a small area on this heavily doped substrate with the rest of it, and then grew the epitaxied layer again. That was a major contribution. Most of this was bipolar, of course.

BROCK: Before you made the move out to Allentown, in the 1961 to 1966 period, I was interested in where your sources were for electronic chemicals and your materials in your group, your department. Where would you get your crystals, wafers, chemicals, resists, and things like that?

GOLDEY: We got most of the crystals from research—homegrown—and then as Allentown got set up, we got most of them from Allentown. I know a lot of it came from Merck [& Company, Inc.]. There were European sources, not much out of Japan in those days. I think we probably got our silicon from Merck.

BROCK: Merck or Monsanto [Company]?

GOLDEY: Monsanto came in later, certainly, but I believe Merck was in the silicon business.

BROCK: That's interesting.

GOLDEY: I'm pretty sure of that. That's the way I remember it, but you'd better verify it. Interestingly, one of my golfing buddies is a retired guy from Monsanto, and he headed up the gallium business there. Kodak [Eastman Company] was the major supplier of the photoresist, but we were always looking for better resist. Pechenay was a French company that—maybe that was just germanium.

LÉCUYER: I think it was making aluminum then, Pechenay.

GOLDEY: All right, but weren't they in the semiconductor business for a while?

LÉCUYER: Not that I know of.

GOLDEY: I thought they were. Germanium never became the big deal that silicon did. I think a lot of our organic materials came from across the river here. Is it J.T. Baker [Chemical Company]?

BROCK: Yes.

GOLDEY: Are they part of Pfizer [Inc.]?

BROCK: No. Now they're Mallinckrodt Baker [Inc.].

GOLDEY: All right. Was there was a [E. I.] DuPont [de Nemours and Company, Inc.] role? DuPont was in the film business. I don't think were a major factor in photoresist. This is the Chemical Heritage Foundation. I should have thought about this ahead of time. Well, the gases were Linde [Gas LLC], and Air Reduction [Company, Inc.]. Later, Air Products became a major factor. They're based right here.

BROCK: Do you think that proximity, between the Allentown activities of Western, helped Air Products move in that direction? I believe they were here before.

GOLDEY: I don't know, but I could probably find out pretty easily, because I've got a lot of friends at Air Products. I do remember being at a dinner one night when Bell Labs Allentown upped its organizational level to what was called a division, and Bill [Willard S.] Boyle came from Murray Hill to be the first executive director. There was a dinner at Lehigh Country Club [Lehigh Valley Club], to introduce him to the various business leaders.

I happened to be at a table with Gene [A. Eugene] Anderson, who was the director of engineering for Western here, he just died a year or so ago; Leonard [P.] Pool, who was the founder of Air Products; Don [Donald J.] Blickwede, who was the research vice president for Bethlehem Steel [Company]; a guy who ran a department store, and others who just sat in wonder all night listening to the rest of us.

Don Blickwede said, "If it weren't for that guy over there," pointing specifically to Leonard Pool, he said, "BOF, basic oxygen furnace, never would have been a process were it not for Air Products." Air Products had a concept of building plants onsite, and so forth. Leonard Pool is quite an innovator, as you know. In fact, if you noticed this big hospital complex, that's Leonard Pool's baby. He started that.

But I don't know how Air Products got interested in that. I don't know what it's like now, but in those earlier days, in the fifties and sixties, there were only half of a dozen major companies in the area, and the people who were heading them up all knew each other.

BROCK: Maybe we should go then to your change in location and job when you came out here in 1966 to head the device technology department here in Allentown. Could you talk about the conversations leading up to that move?

GOLDEY: That's an interesting one, on a personal level, because one of my many friends through these meetings that we would all go to, and the department head of EE [electrical engineering] at Berkeley, Don [Donald] Peterson, said, "We like to bring people in from industry. Why don't you come out for six months? We'll pick your brain and give you the chance to do a project, anything you want to do." Well, we had been through all the so-called big things. I thought, "That doesn't sound bad," and so talked to my boss.

He said, "It's been done before. It might end up three months, rather than six," and so forth. Anyway, it got kicked up the line, and it reached Bill [William C.] Hittinger, whose name I mentioned before, by then back at Murray Hill as the executive director, my boss' boss.

We were good friends, and Bill called me. He said, "Can you come up to my office, Jim?"

I said, "Sure."

He said, "I've got two pieces of news for you. Julius [P.] Molnar," referring to Julius Molnar, the executive vice president of the labs, "has signed off on your going to Berkeley, but he said it will be for three months." He said, "I have an alternate proposition for you, that Howard Loar," who headed that department, and was moving over to Western, down at the engineering research center in Princeton [Hopewell Township, New Jersey Western Electric Company research facility], "needs a replacement. It would be good for your career to get some Allentown experience." He said, "This is not a promotion but a lateral," and laterals were widely practiced. "So instead of going to Berkeley, how would you like to go to Allentown? You can go to Berkeley if you want, but I think as far as your career is concerned—." [laughter] That was the conversation that led up to that. [laughter] I could see it in the long term. I was looking forward to going to Berkeley, but I came here.

BROCK: What was your charge?

GOLDEY: There was a transistor group headed by Dick [Richard M.] Jacobs, who was the supervisor. I think Arpahd Berg, the fellow that I mentioned earlier, was more technology oriented, and I think he was probably a physical chemist in a group. The main charge was doing some specific development for devices to go into manufacture. There was all that negotiation back and forth between the manufacturing guys, who want a product and a process that they can manufacture with reasonable yields and so forth, and the design guys, who want to have every last a little faster so that was a lot of interaction.

The whole idea of branch labs was originally conceived by Mervyn [J.] Kelly, the former president of Bell Labs, because he recognized the difficulty of transferring something, say, from Murray Hill to some Western factory, which might be seventy miles away, or [it] might be a thousand. In those days they were talking about the vacuum tubes, but really high technology came into being. Allentown was the first branch lab. The Allentown lab people looked back to Murray Hill and looked forward to Western, and they had the specific design responsibility, but they got a lot of their technology out of Murray Hill.

Initially, the Allentown lab had a much smaller proportion of Ph.D.s on their staff. There were certainly some, but much a smaller proportion than Murray Hill, which was almost 100 percent. [Mervyn J.] Kelly wanted to upgrade the first lab. In fact, when Ian [M. Ross] was sent out here, that was one of the main things to do. A lot of the subsequent technology was developed right here but the main function was to help developed technology for manufacture, and to develop specific devices for that.

One of the proposals I made to my boss was that, "We ought to get an IGFET [insulated gate field effect transistor]" as we called it, "memory group started here." Jim Early had also come out here a little before I did. He thought that was a good idea, and we set up a group within a group under Dick Jacobs. Glen [T.] Cheney was what you would call the lead MTS, and later became a supervisor. That was one thing we got going, because the integrated circuit activity that was here was all bipolar.

Another thing that was in my department was the Nike-X development. We had a little ceramic substrate, and numbers of transistors and so forth inside that, and then wired them together, but at least you got a number of devices in one can. We still had the TO-5, even though we could have done it with just the silicon nitride passive [process]. Later we did do it without the package. So some [of the focus] was continuing technology development, and some new device things, like the memory thing we got started, and specific device development. There was a lot of support of the manufacturing people.

The concept was there long before I came, but we tended to use a lab facility called DCL, for device capability line. It was out on the Western floor. It's what you might call a pilot line, except it was under Bell Labs' control with Western Electric operators trying to see how it would go as all the bugs were worked out. You'd put new technology and new devices in through this DCL.

BROCK: Interesting. We've been working on an oral history interview with Harry [Harold] Sello.

GOLDEY: You mentioned that Harry's coming to—

BROCK: Yes, to Philadelphia. In this same period of time he was at Fairchild. He was running a group that had responsibilities similar to your group in Allentown. He described some of the difficulties that he faced in getting someone who was a Ph.D. trained scientist or engineer to take seriously that this manufacturing support was an important, valid, central thing that they should work on. Did you have that experience?

GOLDEY: Certainly, yes. How much of it was personal, I'm not sure. That's why we originally didn't have many Ph.D.s out here. We gradually did it, and Allentown gradually got to do some technology development, so that some of those people could do original work and so forth, but we wanted to raise the caliber of the staff, if you will, and try and make people understand the importance of this. I would agree with Harry on that.

Then our Reading lab was kind of spun off from Allentown.

BROCK: All right. Reading was more military?

GOLDEY: It was the same problem one more level down. Most of the talented people from Allentown didn't want to go to Reading. It was farther from Murray Hill. That was a real problem. It's not today, but at the birth of a new technology, where the exciting things are happening, the really sharp people and well trained people want to be in where the exciting things are happening.

Was Harry was one of the original eight Fairchild—

BROCK: No. He joined very soon, maybe a year into it.

LÉCUYER: It seems to me that as Allentown became more competent and moved more into product development, its relations with Murray Hill changed a bit over time.

GOLDEY: They did change over time. That's right. There's no question that by the eighties, the center of device development was at Allentown, not in Murray Hill. Murray Hill was going off on their own stuff. Murray Hill was one of the major places where ion implantation techniques were developed. Of course, they did it at Stanford and other places as well. It was interesting that in the Bell System, Allentown was initially too busy doing other things, and Reading got into the ion implantation business before Allentown could.

Murray Hill was looking to try to get its technology. They knew that getting it into a manufacturing process was the real payoff, and Reading was quite willing to take all the help they could get. Allentown became the focus. I think when Bill [Willard S.] Boyle came out here probably in maybe 1971 or 1972, that really [got it going]. Bill [Willard S. Boyle] was originally a researcher, and invented the CCD [charge coupled device], with George [E.] Smith.

LÉCUYER: That's interesting, because Fairchild experienced a somewhat similar pattern as well. There was research and development for product engineering and the products were moved to the plant. Afterwards the manufacturing organization developed its own engineering group that did an increasing amount of the product engineering and design.

GOLDEY: All right.

LÉCUYER: Was it the same process?

GOLDEY: Fairchild had at that time something similar to a DCL [device capability line] or model shop, one of Andy's early jobs. He was in charge of that.

BROCK: I did want to ask about starting the semiconductor memory group, because this is roughly in the same time period that Noyce and [Gordon E.] Moore and others are starting up Intel, to try and get into the semiconductor memory business.

GOLDEY: That's right. I think that was their first product.

BROCK: Were there many other people looking to get into semiconductor memory around this time, late sixties?

GOLDEY: We certainly never heard of Micron Technology then. I don't recall whether the Japanese were into that kind of stuff. I don't really know, or at least I don't remember. TI might have been and probably was. There was the National in Connecticut, and an outfit called Rheem [Semiconductor Corporation] in Silicon Valley, but I'm not quite sure whether they got into that memory business or not.

BROCK: I don't know.

GOLDEY: Certainly, Intel were the guys that made it big.

BROCK: If we could go back to your personal thought processes, why did you think it would be a good thing to start to go after in Allentown?

GOLDEY: Nobody was doing any at Murray Hill. I think that we certainly recognized that the cost of big machines was really in the memory; and the technology, which was maybe ferrite sheet or ferrite core, or various other things.

BROCK: Magnetic cores?

GOLDEY: We could talk to our system designers. That was a great advantage in Bell Labs, that we didn't have to worry about proprietary information and all that. Memory was clearly a major factor, and I think people recognized then that more memory is better. I had suggested that because I was just looking around. I was aware of what Intel was doing.

There was a lot of the memory used randomly in ESS-4. I didn't stay with that too long, because shortly thereafter I got promoted again, and moved out of semiconductors for a while. The two big topics were the whole question of memory and logic. Looking back on it now, it was an obvious thing to do. I don't claim any great divine inspiration.

BROCK: All right.

GOLDEY: By the way, I just wanted to go back. You were talking about Harry [Harold Sello] and these problems. Ed [Edward O.] Johnson was with the labs at RCA, and then moved up to Somerville. He was the chief engineer. Ed was a good friend of mine, and we were having a drink someplace one time and got to talking about proprietary information and so forth. Ed [Edward O. Johnson] said something to the effect, "Worrying about that," he says, "that's a bunch of bull. We can't even transfer something from our pilot facility 300 feet from our own factory." [laughter] He said, "You're not going to steal anything." [laughter] I think everybody who was operating on the interface between developments in a new technology saw this same thing.

BROCK: It's interesting.

GOLDEY: All right. Sorry for the diversion.

BROCK: Not at all. We had used people as a measurement of the size of the operation that you were running right before you came to Allentown. What was the size of the device technology department in Allentown?

GOLDEY: It was about the same. There generally was a hierarchy in Bell Labs, not rigidly followed, but still what you might say typical of a span of control of four: a department head had four supervisors, a supervisor had four to seven maybe MTS, and the director had four department heads. They were certainly comparable in size.

BROCK: During those years, we described the range of activities that the department was involved with. What stand out in your memory as the big accomplishments?

GOLDEY: The Nike-X project. Those were multichip integrated circuits, but on a small ceramic substrate inside maybe a TO-5, TO-18 package. If I'm not mistaken, that was probably the first application of integrated circuits in the Bell System, even though it wasn't a Bell System application, but a project. We had those hybrid circuits in ESS. I think the timing was about the same. We were very pleased with having the high performance of the Nike-X project, because it was a highly reliable high-performance project, and getting it into manufacturing. That was certainly one of the important things.

We continued the development of lots of specific applications, both in integrated circuits and of specific transistors. This was long before the days of fiber [optics], but there were always very special transistors needed for transmission applications up at Merrimac Valley [Works, Massachusetts, Bell Telephone System manufacturing facility]. There was a big interaction between us and the Merrimac Valley systems designers, as well as ESS work. It became much more specific kind of thing, rather than the big breakthroughs that we used to make five to ten years earlier.

BROCK: I have one other question, which would be about your suppliers for the materials, for instance, the wafers or the silicon, and also about suppliers for manufacturing equipment, such as crystal pullers or other process machines.

GOLDEY: Most of that was done by Western. K&S [Electronics Company] supplied us with the bonders. I couldn't tell you where the crystal machines, the CVD-type apparatus, came from. We got a lot of special stuff from Varian [Inc.], maybe the CVD apparatus, and I guess Applied Materials [Inc.]was beginning to become an important player in those days.

BROCK: Early on you get this picture of Western Electric doing everything for themselves, building their own crystal pullers, "We're going to do it all ourselves." When we're getting into the late sixties there is this sort of support industry out there, where you can buy your crystal puller, or buy these machines that are doing the chemistry. Did Western Electric start to switch over to using some of these outside vendors?

GOLDEY: The answer is yes and no. I'll tell you what by that. There were a lot of people in Western, particularly the people in the factories and controlling the engineering aspects of the factory. If there was a piece of equipment available commercially that would do the job, and with some support behind it for maintenance and so forth, they favored that. The reason these other things happened in the beginning were that we, and our colleagues, were way ahead of the support industry.

I'll never forget coming out of MIT [Massachusetts Institute of Technology], and going to work at Bell Labs, and Bell Labs was way ahead, so you always had to do everything yourself. Western Electric had this interesting facility called ERC, the engineering research center, over at Princeton [Hopewell Township, New Jersey, Western Electric Company research facility], outside of Princeton a little bit, but we would refer to it as Princeton, and one of their challenges and *raison d'être*s was to develop new manufacturing equipment, with the thought that they could probably do it better, because they understood all aspects of it, not a specific application. Some of our ion implantation machines were built there.

There was a tantalum deposition machine that some of us referred to as a "monster," that was developed there and actually was brought to Allentown and used in production. There were those at ERC [engineering research center] who wanted to do it, and I think it's fair to say that on balance, the people in the Allentown and Reading factories in particular, would cooperate fully with ERC. We would have frequent meetings between Bell Labs engineers from the factories and ERC. There was no holding back of information, or anything else. It was also true that anything that came out of ERC had to be worth its salt—had to be as good as or better than what you could buy.

BROCK: All right. Tanenbaum was running that ERC at that time.

GOLDEY: At one time Morry was there, yes.

BROCK: That must have been helpful, to have a semiconductor—

GOLDEY: Then Howard Loar went from here [Allentown, Pennsylvania, manufacturing facility] to there [Hopewell Township, New Jersey, research facility], and then went into New York into a staff job. But he came back for a while and ran ERC.

BROCK: Interesting.

LÉCUYER: To go back to the Nike project, were the circuits they put up for the project mostly bipolar circuits?

GOLDEY: Yes, because the emphasis was on speed.

LÉCUYER: I see. What kind of logic would you use for these?

GOLDEY: At one time in the Bell System, LLL, low level logic, was in use. Then there was RTL, resistor-transistor logic, DCTL [direct-coupled transistor logic], and one other. Nike was probably DCTL [direct-coupled transistor logic], but I'm not sure, because TTL [transistor-transistor logic] had its problems, of one transistor robbing the current from another.

LÉCUYER: Did you develop linear circuits for that project as well?

GOLDEY: The linear work was always done out of Reading, and I just don't know the answer to that question.

BROCK: Why was that? Because they had more military application, or because that's where the experts were?

GOLDEY: I'm not sure I can answer that question specifically. Reading started primarily as a military thing. Then as the military work, the diodes, wound down, they wanted to get into some higher-tech stuff. Then there were linear applications in the telephones, in particular, and in various other things. I don't know the basis for the decision.

BROCK: Should we move on then to that next promotion that happened for you in the 1969 timeframe, to be director of the materials and process technology laboratory? How did that come about, and what was that shift like?

GOLDEY: There was a major reorganization in what we called Area 20, the overall device development area. Jack Morton by that time had been killed, and I guess John [A.] Hornbeck was the vice president. One of the things that Morton had always understood and recognized and wanted was the importance of the materials and process activity. He wanted to have his own materials and process operation, rather than have to depend on research for all of it. Even though on the working level our relationships were very good, I'm sure that particularly up at the top levels, there were always people worrying about their fiefdoms and things.

There was some justification for that, because research people were always looking at the leadership level, the next things that are coming along, and device development people wanted to have the thing solve their problems. Jack [John A. Morton] won the battle. He set up four divisions.

One was the main device development activity, which included the Allentown silicon lab. By that time there were several: the Reading lab, and one or two labs at Murray Hill. The one that I became a part of was the materials and process division, and Bill Boyle was promoted to executive director of that. I was promoted to the Allentown M&P [materials and processing] guy, and there were three laboratories at Murray Hill.

Then there was another thing at Murray Hill, Morry might have been in it. Derrick Scorill making bubbles. There was some non-silicon outdoor electronic work. Then you have the fourth one, Charlie Hoover, things like power systems, which we never thought of as part of the device area, but in its restructuring it came in.

BROCK: So you ran, then, the materials and process technology here in Allentown.

GOLDEY: Actually, it was in Pennsylvania, because it had one department at Reading and three at Allentown, and the Allentown branch that was basically the silicon materials and process. There was a mask lab and other mechanical things, but the main thing was the mask work. In Reading there was support of that, with a little bit of silicon technology there, and a ton of ion implantation. We also were beginning to get into three-fives, and particularly gallium phosphide. There was another department in Allentown that should have part of that other division, but they didn't want it sitting off by itself, so they put it in my lab. That had ferrites and later got into ceramics for ceramic substrates, the thin-film people. There was still some quartz work going on. That's the main stuff.

BROCK: Who was guiding things in the materials and process technology lab or divisions at Murray Hill?

GOLDEY: Bill Boyle was the executive director, and I reported back to Bill.

BROCK: All right. So he was running the relevant groups at Murray Hill.

GOLDEY: He had three directors there, at least one on silicon. Carl [D.] Thurmond was one of them. These organizations kept changing. It's hard to remember who did what when. But he had groups developing various new CVD technologies and things of that type, and I think there was some work on three-fives there.

BROCK: What did you think of that change, and what did you think of the challenges that this materials and process work represented?

GOLDEY: I had mixed thoughts. I was obviously glad to get the promotion, but in my earlier days as an MTS and maybe first-level supervisor at Murray Hill, we had done all of these things there ourselves. We recognized that things do change, but still we thought that that kind of worked, and if we needed help we would get it, but I thought there might be problems, and there were some problems. The concept of spreading us apart, so to speak, arose. There was still a lot of good interaction, but it happened a year later that Jim Early left. Morgan Sparks was promoted up to vice president, but then Jim didn't get promoted to be executive director, and he thought he should have been.

Jim left, and Marty Lepselter, who had been the inventor of the beam lead—very creative fellow—he came out to Allentown. He was really a process oriented guy, and he became head of the device lab. Marty had worked for me, and we were good friends. Still, we had lots of issues. I don't know whether it worked or it didn't. It probably didn't, because the next major reorganization we changed it back.

BROCK: Changed it back to having those things integrated together?

GOLDEY: Yes, at least at the laboratory level.

BROCK: We talked about some of the areas like CVD and ion implantation. What were the biggest issues that you were grappling with in that period?

GOLDEY: Well, certainly one of the biggest was an operational procedure. The Bell Labs Council—that was the president and executive V.P.s [vice presidents] and the vice president—went on the road. I think they wanted in particular to let the branch labs realize they were not forgotten. "Even though you're out there in a factory, you're part of Bell Labs. We know you and we love you, and we're interested in what you're doing."

They were coming to Reading, and in M&P operation in Reading, one of the groups was trying to grow liquid-encapsulated Czochralski crystals of gallium phosphide. Gallium phosphide has a high vapor pressure. Well, we had the first crystal actually grown about 2:00 am. I had been out to dinner with these guys, and so my guys were working all night long. I walked in there at eight o'clock the next morning; they were like this [demonstrates], showed it to me. [laughter]

But that was really not just because we did it for the council visit, but that was a major accomplishment, certainly one of the ones, getting that LEC [liquid-encapsulated Czochralski] grower online. We were the first people in the U.S. to do it, building on the technology from RRE [radar research establishment] in England. In fact, the supervisor had been over there an extended period of time. That was certainly one of the important things.

The mask lab got out of the horse and buggy days, and into the computer days, even though we weren't doing too much integrated circuit work still, but the mask generation for discrete devices.

There's ion implantation, and we were beginning to get some devices made. I think they were not our devices, but I think IMPATT [impact ionization avalanche transit-time] diodes might have been one of the early ones, as well as various transistors. There was a lot of support work for that out of Murray Hill, too, but we had a group doing ion implantation in Reading.

Then I had this other non-semiconductor group, which did the ferrites, ceramics, and things like that. The ceramics were in support of the tantalum technology hybrid circuits. Electronic-grade ceramic is not quite the same thing as making a new face bowl or something.

Western was always the manufacturer of last resort. If they couldn't get the quality and so forth out of other people, they would do it. We were kind of far from semiconductors. We were given a charge to set up the ceramic facility, to make ceramic substrates using a doctorbladed process, and we actually set up a whole pilot line over in a separate building over at Union Boulevard. It's a building that had previously been used for ferrite sheet memory, so it was always called the Ferrite Sheet House. [laughter]

We got that and we made very good ceramics. This was at a time when the principal outside suppliers, American Lava [Corporation] and Coors [Brewing Company] were trying, but not really succeeding to the degree that designers and manufacturers in that side of the house thought they should. We got that thing up and got it going, and American Lava people came in, and the Coors people were in here all the time. One day we had Joe Coors himself come by. He was the guy who they always refer to as one of Reagan's kitchen cabinet, informal advisor. A wonderful guy, great guy. We showed Joe what we had going there, and he became convinced. We never had to put those substrates into manufacture, because between Coors and American Lava, they responded. It's going to be a fairly big business, because as you said, there were other people using this technology, too. They did.

There's a funny little story there. We went to dinner at what was then called the Lehigh Valley Club, no longer operating. It was a Masonic club of some sort. I was sitting across the table from Joe, and I got to waxing enthusiastic about Coors beer—which you could not buy in the East [Coast] in those days—and told him how I would always bring it home. I said, "Last week I was at the University of Illinois doing some recruiting, and I had one of the professors who was an internationally known Swiss fellow, name of Ted [Theodore] Pqspelbaum there. We were talking over dinner, and we both had ordered a Heineken's beer." I'm telling this story to Joe. "There we were out in Champaign-Urbana, having a Heineken's beer." He's from Switzerland; I'm from Pennsylvania. I said, "Here's to you, Ted." I said, "World's second-best beer."

He says, "Yes, right after Coors." [laughter] At that point Joe kind of nodded to one of his fellows on his staff, who disappeared and came back with two six packs that they had in the trunk of their car. That was the first time that Coors beer was served in Lehigh Valley Club in the Lehigh Valley. [laughter] Those good times, things like that, were all part of the early days.

They're the main things that I can think of, until it was changed again. I left out one whole part of it, what we called the microwave integrated circuit. It was microwave subsystems, for instance amplifiers, some switches, various things which had semiconductor devices in, but which were really subsystems. That was part of that lab, too, and we did a lot of good stuff with respect to Merrimack Valley people.

BROCK: In the materials and processes, or anywhere else in this era, were there groups who were dealing with environmental and safety issues connected to the semiconductor manufacturing processes?

GOLDEY: There were some overview people who were probably Bell Labs people, Max [Maxwell] Weiss. But, the details of that usually were carried out by the Western people, as I recall.

BROCK: That was probably on a plant by plant basis.

GOLDEY: With some corporate oversight, both in Bell Labs and also by a fellow by the name of Bob [Robert] Peters. I don't know whether it was at ERC or at engineering headquarters of Western, which was in New York. There was concern about it, but nothing I knew much about, except who to call if I had a problem.

BROCK: You served in that role for five years, and then in 1974, there was the reorganization that you were talking about, the second reorganization, where you became director of the Solid State Device and Materials Lab.

GOLDEY: The kinds of things that changed there were in Reading: instead of having materials and process supporting silicon and gallium phosphide, they brought together all the three-fives in the department that reported to me, both from a materials process and device point of view, the principal device being LEDs [light emitting diodes] in manufacture. Then my silicon process work went over to the other larger lab, which was fully based in Reading.

In Allentown we began to get into what we referred to as lightwave work, which was really following from the earlier microwave stuff, the subsystem work on lightwave designing and transmitters, and the receivers, principally. Later it became other things, working with the people at Murray Hill and the people at Holmdel [New Jersey, Bell Telephone System research facility], where the systems areas were, for developing terrestrial applications.

In the Bell System, they used to refer to the original application as exchange plant. There was the local loop, which was from the office to my house. The first application was the exchange from telephone exchange to telephone exchange, long distance, which was big business in cities. That and we were also developing the technology for TAT-8, the first LED, lightwave transatlantic cable.

BROCK: Were those silicon devices that you were making to do that work?

GOLDEY: No. There were silicon photo detectors in the receiver. But our job was the subsystem work, putting the devices together and designing them. Most of our lasers initially came from Japan, and our photodetectors were homegrown, I think.

We actually had somebody go to Japan for three or four months, and then we set up the fax link, which we thought was something wonderful. With that twelve-hour time difference, that was pretty important stuff, particularly for the sub-cable part of it, to TAT-8, where the reliability requirements were exceedingly high. Most people don't realize it, because they haven't thought about it for the most part, "If something's wrong with that cable, you've got to send a ship out, find it, haul up the cable and repair it. That must cost a hell of a lot of money," which it does, but that's not the main problem. The main problem is the loss of revenue, at least it used to be in those days. The loss of revenue is much larger than the actual cost to repair. Both were important; it was the need for very high reliability.

BROCK: Were those systems deployed along the length of the cable, or just at either ends, so to speak?

GOLDEY: They had repeaters, so they were deployed under the ocean.

BROCK: Wow. So the lightwave work—

GOLDEY: The LEDs were as discrete devices, we used them in telephones and various other places.

BROCK: Were those the two major forces in your—

GOLDEY: Certainly the lightwave was the exciting part of that. We still had some continuing work on ferrites, because linear ferrites, as opposed to cores for switching memory, were very important in the transmission system for making inductors and transformers and so forth. I guess the main supplier was TDK [Corporation]. I'm not sure it was justified, but Western always had the fear that once TDK got to the point where they were the leading manufacturer of ferrites in the world, that they would quit supplying us ferrites and want to supply us the amplifiers. Very much in the same line as the way we got into the ceramic support business, we mounted up this whole technology. We had ferrites in for switching, but we developed major activities for transmission.

We began a little work on optical memory, but it was a fairly small activity, what's grown into CDs [compact disks].

BROCK: Some of these must have involved applications of a laser development coming out of Murray Hill.

GOLDEY: There was a lot of laser development activity in Bell Labs, yes, we were users of them. Most of that was at Murray Hill. There's a large complex out here now, in Breinigsville [Pennsylvania, Bell Telephone System research facility]. Are you aware of it?

BROCK: No.

GOLDEY: It's a third of the way towards Reading, which became a major lightwave lab. That was long after I was out of that particular job.

BROCK: The ferrites example and the ceramics example are interesting to me. A while ago I participated in an oral history interview with William O. Baker, who was talking about looking at the telephone system, as a consumer of materials, of copper, plastics, and what an enormous issue it was, he felt. My impression of what he was saying is that they're little recognized, these questions about materials, their quality, their production, their supply, and how central they were to the telephone company.

GOLDEY: Absolutely. That's why Morton wanted to have his own [materials] for the devices. Baker's work was in plastics when he was a working scientist, and he made some significant contributions, as I later heard. A marvelous man, by the way.

BROCK: A very impressive guy.

GOLDEY: I'll tell you a funny little story. I first met Bill when I was out in Allentown with one of these council visits. I think I was department head at the time. I had sat next to him someplace, or been a tour guide or something, I don't remember exactly.

A year and a half later was the next time I saw him up close, and by that time I was a director. Jack Morton would bring all his directors and executive directors into Murray Hill once a month for a three-level staff meeting. They always had it in what they would call in other companies the board room, in the executive area, building three. We were taking a break and I was in at the urinal, and somebody came into the next urinal, said, "Oh, hi, Jim. How are things at Allentown?" It was Bill. [laughter] I hadn't seen him in a year and a half to talk to. He was that way, not only with people but with science and engineering. The mind is—golly.

BROCK: He seemed to have an amazing memory.

GOLDEY: He always had something interesting to talk about.

BROCK: It strikes me that you're running something called a Solid State Device and Materials Lab, but it just emphasizes the materials side of the telephone system as a whole, and then you can think about all these materials issues in the device area.

GOLDEY: Oh yes. Going way back to very early in my career—getting a good silicon crystal was hard to come by. I've got them from research, and then there would be differences in different crystals, making these complicated PNPN devices. Some would work and some wouldn't, and you could diffuse them side by side. Absolutely no question about the importance of that.

BROCK: It's very interesting to me.

GOLDEY: Bill came out of it. He was a physical chemist, I believe. At the end of his career, [N.] Bruce Hannay was V.P. of research, a chemist. So chemists and metallurgists have always been an important part.

BROCK: Continuing to move forward in time, in 1981 you become the director of something called the Integrated Circuit Customer Service Lab. I was wondering if you could describe where that lab was and what its function was.

GOLDEY: That again was one of these unique inventions originally of Jack Morton, because there was this strong interaction at the working level, and first four levels of management. There were always conflicts at the higher levels, not necessarily bad, but people had their own things. The device area, as we called it, Morton's empire, was frequently criticized about not listening enough to the systems designers. We would all be involved in meetings in which V.P.'s said lots of times, "You guys, we never see you. But," they said, "there's a TI guy on my doorstep every month, and a Fairchild guy, and so forth."

So Jack set up this thing called a customer service laboratory, which was basically an internal marketing organization. It was an interface between the device developers and the user, and we had full staff. It was a smaller laboratory in one sense, and its role was to stay in close touch with the needs of the systems people. When I got that job, true for my predecessors and successors as well, I spent a lot of time on the road.

Even if we had no other reason to do it, we would try and schedule, at least once yearly a trip to Indian Hill or Holmdel or to one systems laboratory, and let them tell us what was going on and what their needs were. They didn't have to work through us. We were there as a facilitator, but not as a roadblock, and I think it helped, because there were a number of us that went through that chair. I was probably about fifth in line, and there were at least two or three more after me.

After Jack was killed, Morgan Sparks was the V.P., and then Hornbeck. Morry took over temporarily. He was back in Bell Labs as executive V.P. Then John [S.] Mayo, and then Klaus [D.] Bowers. Klaus had lived in Allentown, actually set up his office in Allentown. That was something. But we were located right over across the street from the hospital, in those buildings. There were some design laboratories there: computer technology, but no process. When Klaus was the vice president every week the department head who was specifically responsible for that, and I, would spend an hour or two with Klaus. It was scheduled on his calendar, and he wanted to know, "What are you learning from the system people?"

It was very useful. In a way it was a little awkward, because if there were complaints we almost would appear to be ratting on our buddies in the device area. I never held anything back from Klaus, but I would never go in there and say, "We got a complaint about so-and-so," without telling so-and-so ahead of time, and then I think the other people that sat in that chair did it, too.

BROCK: I was just wondering if this is an accurate sort of interpretation. If you think about Bell Labs, the major customer for Bell Labs is the systems designers. Is that true?

GOLDEY: Well, the system designers were part of Bell Labs.

BROCK: All right, within the development side of Bell Labs?

GOLDEY: Again, the overall structure kept changing, but the answer is yes. There was the research area, headed by the research vice president, and the rest of it, except for staff, personnel, treasurer, so forth, was development. Development was headed by an executive V.P., but the research vice president reported directly to the president. He was on the level of the other V.P.'s, but the other V.P.'s had an executive V.P. in between.

BROCK: I see.

GOLDEY: The customers, if you will, of the device area, were the systems development areas, the people originally at Murray Hill but not much lately, at Holmdel, Merrimack Valley, Indian Hill, Indianapolis [Indiana, Western Electric Company manufacturing facility],].

BROCK: I see. I didn't realize that they were part of the Bell Labs structure also.

GOLDEY: Most people don't, because the Bell Labs that people in the scientific side generally hear about, is research and a lot of the device development.

BROCK: That's interesting. It was sort of sales within the same organization.

GOLDEY: In a way it was. We had to give them our things, because they would have input about our budget. It was interesting, at the director level and up, your own performance review was heavily influenced by what the people in the systems areas thought of your organization.

BROCK: What was the system designers' relationship like to the Western Electric people who manufactured the systems?

GOLDEY: It varied a little bit, mainly on the type of the technology and the stage of the technology. At Indianapolis, which was a huge plant where they used to make telephones, probably the people who made black and simple color telephones didn't have much interaction, because Western figured they knew how to do that. In some cases I mentioned those so-called PCLs where we had the hybrid circuit people in those factories, but they were part of our organization.

When you get into some of the most critical thing, maybe it would be like TAT-8, the submarine cables, there the relationship was very special. Western had a very small stand-alone factory for doing all of that work in Clark, New Jersey. But you want to go back to the old-fashioned [facilities] like Hawthorne, where sometimes you had trouble getting in the building if you had a Bell Labs pass. [laughter]

LÉCUYER: If we go back to the customer service labs, did your lab also help the system designers design their own systems?

GOLDEY: No, because we wanted to find their needs, but that was really the role of the device development department. Our role was really one of oversight, but we also tried to look ahead a little bit. "Do you guys know what you're going to be wanting next year, or next generation?" Something that maybe the people that they were working with on this generation didn't know.

LÉCUYER: Did that information go to the device people?

GOLDEY: We just filled a role if it needed filling. We tried not to get in the way, because that's not why we were created. We were created to [help out?]; not to become another roadblock, but to remove roadblocks.

We also had a generic reliability department in that customer service lab.

BROCK: If you had to characterize the main issue that you were hearing back from the system designers, was it dissatisfaction with the performance of the devices that they were getting, or was it dissatisfaction that you weren't providing them some kind of device that they wanted?

GOLDEY: Every case was different. The themes that ran through were, even though the device area in Bell Labs was big, it didn't do everything, and sometimes people would say in some cases, "How come I can get this from TI or Intel or Fairchild or whoever, and I can't get it from you?"

Another complaint that we heard from time to time was, "Why should we use Allentown or Reading parts for this particular piece of apparatus, when we can buy those same devices from Radio Shack [Corporation] cheaper," even from the TIs and the Motorolas [Inc.] of the world.

In some cases, there would just be a problem where they said, "So-and-so's lab, they're not listening to us." That didn't happen too often, but it did happen.

BROCK: In thinking about that cost theme, was Allentown manufacturing products for anyone outside of the Bell System by this point?

GOLDEY: Between 1981 and 1984, certainly we weren't involved in that. That began to happen in the later eighties. Maybe there are some examples, but I'm probably not the right one to ask that.

BROCK: It wasn't certainly a big part of it.

GOLDEY: I think that's right.

BROCK: Would that have been a hindrance to competing on cost? If you're a Motorola or a TI, you have this economy of scale.

GOLDEY: We used to talk about that. We always said, "We can design a specific device for you or your application with the required reliability, as well as the performance." There was this whole other operation which was part of this reliability development, called the "KS Spec." Nobody knows where "KS" came from, but that was the specification that we put on outsiders' devices that a system developer wanted to use. We ran them through our reliability tests. It was part of the customer service lab, but not part of the customer service department. It was part of the reliability department. There were always lots of outside devices used, in non-critical applications, or maybe things that we didn't have, or maybe they had a very good product. There were a lot of very good products out there.

LÉCUYER: What was the share of outside products used at Bell [Telephone System], in the government side?

GOLDEY: I don't remember. In fact, I don't even know how to answer that question. If you're talking about an active component that's one thing. If you're talking about mud resistors, well, there are a lot of them. [laughter]

LÉCUYER: I mean, transistors and integrated circuits.

GOLDEY: I don't know, but my guess would be 25 percent or less.

LÉCUYER: All right. So 75 percent was supplied by—

GOLDEY: Some people thought this was very good business. AMD [Advanced Micro Devices, Inc.] for example, had an office right here in Allentown, which even though there are no systems people here, the fact that the KS [reliability] operation was here. I don't have any idea whether they still do. I remember being at one of the Dataquest [Gartner Dataquest Clusters] meetings once. What's the guy's name, is it Jerry [W. Sanders, III] ?

BROCK: Jerry Sanders?

GOLDEY: Jerry sought me out, because the whole KS operation reported to me, and we had a nice little chat for a while. He's almost as wild as they say. Nice guy, though. I enjoyed him. I didn't know him because he had come up through the marketing side, whereas all my Intel and Fairchild buddies, those guys were all technical guys.

BROCK: I think the next change for you was from 1984 to 1989, when you directed the Bell Labs linear and high-voltage integrated circuit lab.

GOLDEY: The silicon part of the Reading operation.

BROCK: Could you tell us about those years, working with the Reading operation?

GOLDEY: Reading had always operated a way in which the labs and the Western were more integrated. There was a little bit of a conflict situation in Allentown, not necessarily bad. It was set up that way primarily by Gene Anderson, who had been a Bell Labs guy and first or second director of the Allentown lab. The first director of the Reading lab, a fellow by the name of Ekstrand said, particularly when we were down in Holmdel or someplace, "I don't want to hear the use of the name Western or 'Labs [Bell Labs]. You say Reading." He instilled that spirit, and he was the dominant figure, even over the general manager for Western Electric, which is obviously a bigger operation. Ek really ran that thing for years.

When I got there, and I had had many interactions in Reading, and had a department there. I knew it would be different, and it was. I think it's fair to say that by the time I was there, we had a lot of Ph.D.s in the labs. Western Electric had been successful in upgrading their staff a lot, but the relationship had always been good.

We were concerned primarily with working with system designers, on the one hand, to get the design right, to get what they needed, and getting that thing into manufacture, and also introducing new technology as the need or the opportunity arose. Within the Bell System, ion implantation was first in manufacture at Reading. There were lots of linear circuits. There were some for use in telephones, for use in transmission, not many for switching; also in some key systems, such as a phone with buttons. They had linear circuits in some of them. That was the main activity, silicon linear integrated circuits.

As ESS evolved into number four and so forth, they used the high-voltage switch. GDX, gated diode crosspoint, it was called. It had some linear characteristics, but the application was for ESS, and that was a fairly big operation.

Since the focus had been linear, there began to appear a need or an opportunity for linear integrated circuits in MOS technology. I think I might have set that up. Certainly they had some circuits in development. We set up a full department for linear, but Reading did not have manufacturing capability for MOS. We had a Reading design department feeding devices into Allentown Western Electric.

It was particularly interesting for our members of staff and their management, who had been in this close working relationship at Reading, coming up into Allentown in this much more conflicting type of interaction, even though they knew people back and forth. But it worked, and there were a number of integrated MOS things. Again, lots of specific design, working closely with the "The Western," as they always used to say.

Then there were the housekeeping functions, the spec groups and so forth.

LÉCUYER: There's something we didn't talk about, namely, your historical work. You wrote a chapter on the history of integrated circuits.

GOLDEY: Oh yes.

LÉCUYER: How did it come about?

GOLDEY: There were these series of books. There were the earlier books, and then there was the *History of the Transistor*, and there was the *History of the Integrated Circuit*. I, along with Johnny [John] Forster, wrote that. Somebody, might have been Morton, thought that it would be a good idea to get this all down on paper, and I had been asked to do that. I don't know whether it survived the editor's cut or not, but I remember a comment I put in there, "Back to the stepping device, this is perhaps the world's first integrated circuit."

LÉCUYER: I've seen it. It's still there.

GOLDEY: It's still there? It was, in a way, but I think Fairchild were fully justified to say, "You didn't exploit it," which is true. I don't feel that that argument is equally valid when applied to Atalla and Kahng, and for a long time Atalla and Kahng did not get recognition, in terms of the IEEE awards and things like that, for the MOS transistor, though everybody knew it.

An IEEE prize is different than a commercial success. Somebody mentioned to me, however, not too long ago, that those guys did get some significant recognition. Unfortunately, Kahng is dead now.

It's funny. I have a condo in Hawaii, and it was last May—we have our annual meetings in May—I was sitting across the table from this guy that I'd just met, and he has his own business on identification things, eyeballs and fingerprints and everything else. He lives out in Silicon Valley. He said to me, "Did you ever know a guy by the name of John Atalla?" [laughter] He's a neighbor of his. John and I have reestablished an e-mail correspondence. I'll have to ask him if he got his just rewards.

BROCK: Then after your tour at Reading, is that when you retired?

GOLDEY: I retired, yes, at the end of 1989. The Bell System had a policy that people at the director level and above had to retire at sixty-five, and I went out a year and a half early, because they put some various generous offers on the table. I would have worked for eighteen more months for $10,000 total, so I decided that as much as I love my job, I know the end is coming anyway, I might as well get the hell out. A lot of people did the same thing.

BROCK: In your retirement, have you continued with any technical activities?

GOLDEY: Not really. I've been thinking about that, because I have a friend that lives across town, Freidolf Smits, who has been doing volunteer work, but still working hard, for the IEEE. I didn't because my wife and I spent a lot of time helping to raise our granddaughter, the one that wrecked the car last night. Also, my wife got sick, but that wasn't until the later nineties. I like it, but I'm just starting anew now. I have discovered the Teaching Company. Do you know about the Teaching Company?

BROCK: I don't.

GOLDEY: They advertise in *Science News* about once a month or so. They get professors, the cream of the crop, as they say, from around the country, and they put courses out on DVD. They have a rather strange marketing strategy. One course will cost, say, $450, but once a year it goes on sale for $50. [laughter] All the real advances in particle physics happened about twenty years after I got out of graduate school, and by that time I was all immersed in semiconductor. I wasn't worrying about that stuff.

I've just taken a course in particle thoery. Then I took one on relativity, which, of course, was around then, but I had never taken a course in it. Now I'm into one on astronomy, and every night I sit down and watch a half hour or forty-five-minute session, because science is still magic, and these things are very well put together. Not only science, they do arts and history and literature.

BROCK: I don't know about you, Christophe, but I just had a couple of very general questions, which just are basically reflections. One thing I was interested in your general reflections on Bell Labs' role in launching the silicon technology. How would you describe that in general terms?

GOLDEY: I may well be biased. I'll acknowledge that. But in launching it, it was paramount, in particular, from the whole idea of Shockley's work, and the first grown-junction transistor, [William B.] Shockley, [Morgan] Sparks, and [Gordon K.] Teal.

[END OF INTERVIEW]

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