Oral History of Arthur D'Asaro

Interviewed by:
David Brock

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David Brock: Great. Well, this is an oral history interview with Art D'Asaro on July the 21st, 2009, in Madison, New Jersey, and Art, as we discussed I thought we could start at the very beginning and ask you about when and where you were born and where you grew up.

Arthur D'Asaro: Well, I was born in Buffalo, New York. My mother was a basically German extraction person, a blonde, blue-eyed woman, and she married a guy who was a Sicilian from a family which was basically in some sort of a grain-milling operation in Italy. Because the Austrians were in charge of the Italian government at that time, around 1910, they were concerned- the family was concerned that their oldest son who became my father would be forced into the Austrian army. Of course, that was prior to the first World War so they decided to leave and come to the United States. My grandfather decided to go into business making pasta or spaghetti. And that went on for a while and then he died and then my father married and had me as a child. My parents didn't get along. They came from two different cultures. My mother a Protestant, my father was Catholic, and they had different temperaments. Each of them were poorly adjusted to a lot of stress in marriage, so they divorced and my mother came to live in a house which she had built with her own money earlier when she had been working as what we would call today an executive secretary in New York City. But this house was in Illinois and her mother was living there and it was built actually for her mother to live there. So that was the house that I grew up in, in a small town. It's called Verona, Illinois. There were about maybe 100 and some odd people, a very small place. I went to grammar school there, a two-room school. Some schools in those times were actually one-room schools, but I got all the basic things that you get in grammar school. What we call grammar school now is called elementary school I think, and then I graduated from school there and tried to go to the nearby high school but the nearby high school was totally devoted to the interest of farming people. This was a farming community and I was very pleased when my mother was able to get a job in a larger town called Ottawa. She was trained—My mother was trained to be a telegraph operator and actually her father had taught her that. Her entire family was involved in that kind of business at that time, the Western Union Telegraph Company, which was a big deal in those days. So she got a job in Ottawa, Illinois, a small- well, not small, medium-sized town on the Illinois River, and they had a much bigger high school and more diversified. There I could take chemistry and physics and- which I loved. I was so delighted to be able to do that. I was really thrilled, and so I did and of course I got very good grades and ingratiated myself totally to my teacher and he said, "Well, you're so good you'd better learn a little more." So he gave me a course in qualitative chemistry on the side because that was not a listed course. We just sort of did it separately so I had a fine experience there, and when I was getting ready to think about college I didn't know anything about colleges. Nobody in my family had gone to college. I was the only one who wanted to do that, and I didn't have any money. My mother said, "Oh, you can't do that. You don't have any money. We don't have any money. We can't send you to college." Well, I said, "We'll figure it out somehow" so I worked that summer before I went to school- to college. I worked in a shipyard. This was during the war- during the Second World War and they were building what were called landing ship tanks, LSTs, and I worked in the shipyard. They had this interesting method of working there which was called cost plus. The more people they hired, the more money they made so it was easy to get a job, and so I made—I don't know, a few hundred dollars at that time was a lot of money and off I went to Northwestern University. That was the place which had advertised itself to me and to the adviser at my high school as being the equivalent to MIT. Well, it turned out it was not at all the equivalent to MIT. They did have a nice building and it was called the Technological Institute. It was very handsome but the teachers came from a different era. They weren't up to date although I got fundamentals and basic things in physics but I wasn't very happy with that kind of an education. That was a school where the highest achievement of the girls was to get married and the highest achievement of the boys was to play on the football team, and guys like me were a little out of place, although I did meet a very good friend who did the same thing that I did and wound up at Bell Laboratories as I did. Well, when I finished with Northwestern I was looking around to try to get a good school I could go to, and I found that Cornell University would give me a teaching assistantship and with the money I got from that I could pay my tuition. And then I figured out that if I was at Northwestern I could get a job and they had a very strong sorority and fraternity system which I detested, but at the sorority that I got a job I could work serving meals to these spoiled brats who were the girls there and that way I got my dinner and my breakfast so- and sometimes even my lunch. So I did that all the time that I was at Northwestern so between those two things—Also I forgot to say that when I was at Northwestern University for the first quarter—they were on the quarter system—I turned 18. Well, the day before I turned 18 I enlisted in the Navy because I would be drafted otherwise and I didn't want to go in the Army, so I enlisted and got in to the electronics training program which was called radio technology at that time and got some training in electronics that way. When I was in the Navy I was in the Navy for a year and a half, and when I got out of the Navy then I qualified for the GI Bill of Rights which you know was quite generous and they paid oh, essentially all of my tuition. So there wasn't enough money to live on, but I appealed to my father who was living in Buffalo and had become pretty successful as a realtor and he owned a series of apartment houses. So he felt a little bit I guess guilty about not having to do anything to do with raising me but as I was going to college he felt this was very admirable and much better than doing something else that I might have done. So he did pay extra money for me to go to college so I went to college there, and when I finished up at Northwestern where I got a bachelor's and a master's degrees in four years because I went in the summers and then I looked around for what schools I could get in to and I got in to Cornell. As I said before, I got a teaching assistantship there and I loved it. Oh, Cornell is just wonderful.

Brock: Before we get in to your experience at Cornell, would it be okay if we went back? And I had a couple of follow-up questions to some of what you've just been talking about. When you were living in—When did you move to Illinois? What—

D'Asaro: I was four years old when my mother decided to leave and split up the marriage so I remember a little bit about when I was four and—

Brock: What year was that?

D'Asaro: What year was that? Well, I was born in 1927 so it would have been 1931 in the heart of the Depression, and we had terrible times in the Depression. When we were living in Verona and we didn't have any money. My mother wrote repeatedly to my father saying, "Oh we don't- we can't manage. We don't have much money and you should really pay for the upkeep of your children" so he sent a very small amount of money. He said, "Well, business is terrible." He said, "I can't afford very much." In the thirties people were out of work and- but we had a house to live in. My mother had actually as I said before paid for the construction of the house. So we did own a house and so we got a little bit of money that way so- but basically I felt poverty stricken. I—My mother's sister was employed all the time during that period and also was a telegraph operator and she helped us out with castoff clothing and other goodies, so that's how we managed.

Brock: You mentioned that by the time you moved to the larger town, Ottawa, Illinois, that you were thrilled to be able to take chemistry and physics in the high school. How had your interest in technical subjects and math and science —How had that developed before getting to high school?

D'Asaro: It was always in me from the very earliest time when I learned to read. Actually, I learned to read in kindergarten and there was no kindergarten in that town. The reason that we were- for one year—we lived in a larger town because my mother's brother had been killed in an accident when he was working as a delivery man delivering milk and his delivery truck was hit by a train. He left four children behind and had no money- had no income and nobody could take care of the children because his- the mother of those children was mentally disturbed. She was emotionally disturbed and she could not collect her wits well enough to take care of children. My mother went and we lived for a year in a Chicago suburb and I went to kindergarten. In kindergarten I learned my ABCs and the whole idea of reading thrilled me immensely so when I got to —Then they- that lady left. The children were farmed out. Those children were sent out to various foster homes because my mother had no money and couldn't take care of them. They were brought up in foster homes and I went back to, as I said, Verona. When I went in to school there, first grade, the teacher said, "Oh, this child already knows how to read. He's too—He knows more than the other children do. We'll put him in second grade" and so they put me in second grade so ever since then I was a year ahead of everybody. So that's how I happened when I was at Northwestern to be 17 when I was a freshman there whereas the usual age is 18 so I was 17 when I was in Northwestern and then volunteered as I said before to be in the Navy so I didn't have to get killed in the army so—Well, I'd gotten as far as Cornell. Was there another question?

Brock: Yes. Were there any hobbies that you had that connected to your interest in science?

D'Asaro: Oh, well, of course in this little town, as I said, opportunities were extremely limited. There was no library. We had no books to speak of. It was a very constrained environment. My mother and my grandmother were very religious. The church was right across the street. We were very attentive churchgoers. It was the center of our lives and read the Bible. Garrison Keillor talks about people like that living in small towns. We were like that, singing hymns and all of this. We had an organ which my grandmother had inherited, played hymns. I would learn to play hymns on the organ and my aunt gave us a piano and I learned to play on the piano, and my mother had a violin which she had purchased when she was living in Manhattan years earlier and she said, "Oh, you could learn to play the violin" so we found a teacher who would give us lessons for very little money and came once a week and I learned to play the violin. And then when I was in Ottawa there was a school orchestra so I played in the school orchestra, and that was an interesting thing, but as far as science was concerned when I was in Verona I had to amuse myself with science. One of the things I could do was make kites and—Oh, I forgot to tell you that I have a brother and my mother had an attempt to reconcile with her husband and that produced my brother, but the reconciliation didn't stick, so he’s five years younger. My father helped him in school also and sent him through school to be an MD. He became an MD and so we both got good educations by one way or another. Anyway, as far as scientific interest in that little town there wasn't anything that nurtured my interest. I found one day in a old shed that was in the back of the property a device that I couldn't understand what it was. If you held it up next to a piece of iron, it stuck but it didn't seem to be made out of glue. I was amazed by this thing. What is this? And "Well," my grandmother said, "your grandfather had gotten that from somewhere." Well, it turned out my grandfather, her husband, who was dead, of course, by that time had been very interested in technology and science and he dabbled in all kinds of things such as photography and tried to become a photographer. And there was an old camera we had of his and some of the development tanks that he left behind, but none of this stuff I could use really but it was interesting that he had these things. And so I felt somewhat inspired by the fact that here my grandfather had been interested in technology and I knew I was, but I didn't know what it was. I was able to get a hold of the Chicago Herald -Examiner, which was a newspaper, one of the Hearst papers at that time, and every week they had a section on science, and I was thrilled one day when they said, "Atomic energy can power all our needs" it said in the Herald-Examiner. What is this? I was amazed and a scientist had come up with the idea that you could change matter into energy and that could be used for making power. What an exciting thing, and that was way before the atomic bomb. That was in the thirties that that was published. That was when Einstein had first published his paper on the equivalence of mass and energy. Of course, they didn't have- they couldn't put it that way in the newspaper, but that was a thrilling thing for me at that time, but I really didn't have an opportunity to do much science in that little town. It wasn't until I got to Ottawa that my horizons' border were able to expand somewhat and I was able to get hold of old radios and play with them and take them apart and put them together again, and so I made opportunities for myself and my science teacher was very helpful. His name was Mr. Bohannen. [What a wonderful man. He was so good to me and I- after I got in to Cornell I went back to visit him, told him I was planning to get a PhD. Well, of course he was pleased when one of his students did that. I was the only one.

Brock: What years were you in high school? Was this from—

D'Asaro: I always get these years not quite straight. I—What was it? I have to look because I always have a tendency to get the exact- not to get the exact dates. It was—Oh, and that's not it. Do you have it?

Brock: Forty-one? I don't have it.

D'Asaro: You have it.

Brock: No.

D'Asaro: Well, I gave it to you.

Brock: Let's see. You—

D'Asaro: It was—It might have been '41.

Brock: —got your bachelor's degree in physics from Northwestern in 1949 but you had roughly a year and a half in the Navy in there so maybe 1941 to 1944. Does that sound approximately—

D'Asaro: That sounds about right, 1944 and then in 1945 I would have been 18 so it was 1944. It was 1940 or 1941 to '44.

Brock: Right. Certainly during the war—

D'Asaro: It was during the war, yes.

Brock: —coincided with your—That's a very formative time, high school. How did the second world war sort of shape your experience of high school?

D'Asaro: Well, of course we were all- everybody in high school was quite astonished at those events. When the Japanese bombed Pearl Harbor, it was so astounding. By the time I got to high school, I- as I told you my mother was working. She said, "Oh, Arthur, you can deliver telegrams. We don't have to pay someone else. You can be the delivery boy" so- and I had a bicycle. I had- rode all over town delivering telegrams and that was an interesting job and so I was pretty active and mobile. I made some friends there at the high school who were interested in science and doing things like making model trains and things like that, but I didn't really know much beyond what I learned in school.

Brock: When you did get to Northwestern and you had that first period of time and then you enlisted in the Navy you said you elected to go in to a radio electronics training program—

D'Asaro: Yes. That was pretty famous, that program—

Brock: Could you talk about your experience with that?

D'Asaro: Well, they talk to you about it and do things like soldering, which of course I already knew how to do, and how to- and what resistors and capacitors were but I already knew that because when I was at the shipyard I- as I mentioned to you before that summer I was at the shipyard. I had oh, so much spare time. I was working in a warehouse and they- there was very little structure. If somebody came in, "Oh, we want this from the warehouse," and I would get what they wanted and then I'd have three hours before somebody wanted something else. And meanwhile I got a hold of this American Radio Relay League handbook. Do you know what the American Radio Relay League is?

Brock: No.

D'Asaro: It's a organization of amateur radio people, people interested in amateur radio, and they put out a book and in this- beginning of this book there is a list- there is a description of the fundamentals of electronics and of course it was all tubes. And so I learned what an inductor was and what a capacitor was and what RC meant and all that, sort of stuff from the American Radio Relay League handbook. If somebody had said, "Oh, I've got a physics textbook here on electronics," I would have been thrilled beyond belief but I didn't know anybody that would have had such a thing. So I did the best I could but at least I knew something at that time.

Brock: You said you were in the Navy for a year and a half. Where were you stationed?

D'Asaro: Well, the war was just ending and I was stationed on a decommissioned destroyer in the middle of Charleston Harbor, and again talk about having time on your hands. I was the only person on this vessel. My job was to report back to headquarters every four hours that all was well. Beyond that, I didn't have any duties. Well, the equipment on the destroyer was all of course left from the world war and it had radar and all these kinds of things on there which I could look at but I didn't destroy them or anything like that. And I—And there was a library at the base- at the Navy base and I could get on a boat which went up and down the harbor taking things to Navy people on these various boats or ships, and with that I could go to the library in- at the Navy base and I could get a meal and I could get a book. Oh, I got six or eight books out of that library. I got one by Eddington on astronomy and cosmology. That was a very interesting book to read. I got others of that sort and so I was very appreciative of having an opportunity to go to that library, and also they showed movies which were interesting. And the most important movie that I saw there was "It Happened at Sun Valley." That was the first time I ever saw anybody ski. I saw people skiing. That was so great. I loved it. When I later had time and money I took up skiing pretty viciously. I took the whole family skiing as much as we could possibly do so everybody became a skier. I was a good skier, an expert skier, so that was important for my future to do that, but Charleston itself, Charleston, South Carolina, was unimpressive to me. Of course, during the war things were perhaps not in the way they would have been before or after the war. I don't know. Some people say they like it but I thought it was a pretty dull town, and so I took advantage of my opportunities.

Brock: When you returned to Northwestern then you were there for approximately four more years and you earned both a bachelor's and a master's degree in physics?

D'Asaro: Yes. I felt that I was not very well prepared. I felt that to go to a really good school like Cornell I needed as much preparation as I could get and I- although I actually got a bachelor's degree in three years I stuck it out and I got a master's degree which I got a little more, something, whatever they could give, which wasn't a great deal. The faculty was mediocre, I'm afraid. I did make a good friend there at that time in the faculty. The United States Army had a policy of bringing German scientists to the United States because they were very concerned that the Russians might find a way to get the better German scientists out of Germany and in to Russia and they did do that with many German scientists. And so as many of the Germans as could took advantage of that opportunity, and so there was one of those German scientists there at Northwestern and I- and he was interested in cadmium sulfide and cadmium selenide, which are photoconductors, and I loved to talk to him. And then I read that a device called the transistor had been made at Bell Laboratories. "Oh," I said to my friend, this German guy, "do you think we could make that, cadmium sulfide?" Really I didn't know a thing about it and he was game to try it. We tried but it didn't work. We could do stuff like that and it was fun to talk to people that were knowledgeable. There was one good faculty member that I- and I said to him, "Well, look. I don't feel very well prepared to go to- to go on to graduate school. What do you think I could take or could you give me a course just not on the regular curriculum?" And so I took a course in mechanics on the higher level which was very useful, and it should have been in the undergraduate program but it wasn't because of the way they were badly organized there so I was glad I did something good.

Brock: As your coursework and your interest in physics developed, was electronics a theme in what you were interested in physics or were you drifting over toward electrical engineering at all? What was your—

D'Asaro: Well, I—No, I was not drifting toward electrical engineering. I was drifting toward atomic physics because that's what seemed to me to be the future. Everybody said, "Oh, atomic power is the future" so that's what I should I thought so that's- when I got to Cornell that's kind of the way I thought I would organize my courses, but the- for the early phases, usually the first two years of a graduate student's life, is devoted to a lot of basic courses like—Well, of course now the people learn much more advanced things at an earlier age, but at that time I hadn't had quantum mechanics and I took quantum mechanics and electromagnetic theory. They had such terrific people. Hans Bethe was there. Here comes Hans Bethe down the hall. He's got a book in his hand. It's Sommerfeld's book on electromagnetic theory in German. He's walking down the hall and he's reading this book. He comes in and he gives the most marvelous lecture, Hans Bethe, a very famous scientist, most marvelous lecture you could believe on electromagnetic theory. You went out of there. Everything was absolutely clear, as clear as could be, a great guy, a wonderful guy. I got to know him well enough he'd say hello to me.

Brock: Just in thinking about the transition between Northwestern and Cornell, did you do a master's thesis while at Northwestern or—

D'Asaro: I did but it was a very trivial thing and I can't even remember what it was now. I was sort of ashamed of it, but I got the degree.

Brock: How did you make then the connection to Cornell? Was that through connections from the Northwestern faculty—

D'Asaro: No. They didn't know anything about what I should do or really- or they were not adequately tuned in to know what I should do. I did it by reading "The Physical Review" and "The Journal of Applied Physics", and what I was- what I decided to do is I would go through the best technical literature I could find and see where it was published and I found there was more good publication coming from Cornell than from any other school including MIT, including Cal Tech, including whatever you name. They were the top and I said, "That's where I want to go," and I applied to some other schools but Cornell gave me the best deal so, as I said before, I was teaching undergraduates physics and I didn't do the lecture course. I did the part of it which was for the students to ask questions and we'd take a quiz and try to see what they needed to learn. They were the most disinterested bunch of students you could believe. They were all candidates for an MD degree and they didn't care beans about physics. They just wanted to get through the course somehow and- but I gave them a regular course in elementary physics and went through what I was supposed to do. There were other students there also doing exactly what I was doing in physics. It was a pretty active department.

Brock: Could you describe the department, what its main emphases were, the facilities—

D'Asaro: Well, as I sort of implied before, the main emphasis was on nuclear physics and they had a- I guess a synchrotron you'd call it, a synchrotron to accelerate positively charged particles and look at the products of the collisions, and then they were publishing a lot of papers. That was- at that time was so exciting. Hans Bethe and another guy named Salpeter worked out the theory for the carbon cycle in the sun which accounts for the production of energy in the sun and that was the quality of the people who were there, amazing. I had wonderful lectures from a guy named Phil Morrison who was so personable. It was just a pleasure to take a course from him. I don't know if that answers your question.

Brock: It does. Could you describe then how your graduate career unfolded, how you came upon—

D'Asaro: Well, I came to the point where I realized that when I compared my abilities with those of other students who were preparing for a career in nuclear physics I wasn't as good as they were. I knew that I was not as good as they were and I thought I'd better change fields; I am not going to be a success in this field. There was something about nuclear physics which seemed so abstract to me because you don't- you- there's very little you can hold in your hand. You do these experiments with radiation and then you interpret the experiments, but it's not really hands-on; it's hands-off because radiation is very dangerous, and that didn't appeal to me. I found that I- when it came down to being closer to what was really done it didn't appeal to me and when I tried to get involved with the synchrotron I- there were so many people involved with it that I couldn't make a dent in the structure and I couldn't have any effect on what was going on. I had some ideas about what might be done. Nobody wanted to listen to my ideas because I was just a- one of the early-stage graduate students. I decided to switch fields and go to solid state physics, and I was really fortunate at that time. Luck was with me. There was a chairman of the department. His name was Lloyd Smith and he had started a new curriculum called engineering physics in which a person could study engineering and physics and get a degree in this field, and so I took that sort of courses and luck was with me again. I—It happened that at that time at the [Bell} Laboratories they had decided to have a course for professors or teachers at various universities in order to get them up to grade on their understanding of semiconductors because at that time, 19—what is that? Fifty something or other—transistors were known at Bell Laboratories but not very many people understood the basics of semiconductors, and at the universities where they should have been teaching courses they didn't have any faculty people who understood them, who understood how semiconductors worked or how you would prepare a course. So Shockley initiated a course at Bell Laboratories where faculty members from all over the United States—I think they were all from the United States—came to Bell Laboratories and they were there for—I don't know—two months or something in the summer and took lectures on semiconductor physics and they had all kinds of supplementary material. And then they came back to their universities and set up courses, and the first year at Cornell University they had a course of this type. I took it and it was a great course. It was terrific. We did experiments in mobility and all about holes and electrons and recombination and all of the things that we should really understand, how to make P-N junctions and what's a field-effect transistor and all of these things that we did the experiments on. We had the materials. It was all germanium because that- silicon hadn't yet been brought in to a situation where it could be used.

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Brock: You were at Cornell roughly the period of 1950 or 1951 to 1955, somewhere in there, and we were just talking about your experience of this Bell Labs semiconductor physics course at Cornell. What was the place or the image of Bell Labs that you had at this time and among sort of the technical community at this time?

D'Asaro: Well, Bell Labs as you know was a preeminent industrial laboratory in the world. Maybe the Philips Lab in Europe was a good place, but nobody came up to what Bell Labs had and what they could do so we had enormous respect for the kind of things that they did, which were usually first class, top rate stuff and so- but if I was continuing with my work as a graduate student at Cornell I—One day Lloyd Smith, who was my advisor, said to me, "Oh, I have some money" he said "from RCA for a graduate student to do some work on field emission and would you like to do that?" And I said, "Yes" and—Well, I knew what he meant because there was a device called a field emission microscope which a fellow named Muller had developed in Germany, actually at an earlier point, and then he came to the United States and then he was at Penn State, not the University of Pennsylvania, Penn State. And he had been publishing stuff on field emission microscopy and what it was, and I thought yeah, I can do this, and so I did and I set up the equipment and Lloyd Smith was strictly hands off. He was the head of the department. I did it myself. Whenever there was a problem to solve I solved it myself. I set up this equipment and I had a high vacuum system. I got the parts made and we had a glass shop where they could make glass parts according to what you wanted, and I even made the screens myself for the luminescence. You have to have a luminescence screen. I made that myself, and so I got all of it together and put- and it worked and then I thought "Now what can we do that's different and new?" and I said to Lloyd Smith, "What about looking at the field emission from an alloy. Nobody's done that" I said and so he said, "Okay. That's good." And so I did that. I got some zinc or something, tungsten alloys, and looked at the migration of the ions in the metals and how that affected this pattern, and I took tons of pictures and it was easily done for me. I could schedule my own time. I went in and I did as much as I liked and then I left, and so- and that was the time when I took up horseback riding and you probably saw some pictures here on the counter of me riding a horse and horseback riding became another hobby that I did later quite a lot. At that time also I met Barbara who was a student- a graduate student in nutrition and we met, I guess, at a dance and I thought that's a pretty cute girl and then I met her again at a party. I was a member of a graduate fraternity, which is a pretty unusual thing to have. There was a graduate fraternity at Cornell and we had parties and girls and dancing and Coca-Cola and a good time was had by all. So

Barbara came with some guy who was from nutrition where she was doing a thesis. And so I danced with her and the other guy kind of got disregarded, and so from then on we dated each other and I said to her one day, "Let's get married" and she said, "Fine. I'll set the date." And we got married at Cornell University and my father came to the party and my mother came to the party, and we had a band and it was really quite astonishing. I had gotten to be very close friends with the student adviser for the Presbyterian youth organization at Cornell and he was the officiating minister at our wedding at Cornell in a very nice chapel there. Nine months later our first child was born and Barbara's parents were thrilled. I was thrilled. My father said, "Okay. I'll help you out financially." He was doing better now. He had gotten in to the stock market. He was a natural talent at the stock market. He was far cleverer than I ever turned out to be, and so he had enough money he could support me well beyond the level that most graduate students could be supported. We had a little house we lived in right there in Ithaca and it was very idyllic. I finished up my thesis, handed it in, and they said, "Fine. That's okay." It wasn't a great thesis but it was something, and I looked around for places to get a job. At that time—That was 1955 I think. Was it? Yeah.

Brock: Uh huh.

D'Asaro: Nineteen fifty-five. I sent out resumes to various places that I thought might be interested, and every place I sent a resume asked me to come and visit them. There was a great demand for scientifically trained people at that time and they had gotten such a good reputation the- because the United States won the war against Japan with a nuclear bomb and I called the atomic bomb. It was really a nuclear bomb. The reputation of scientists at that time skyrocketed. Scientists could do no wrong. They were wonderful. They saved the United States in the Second World War, and so every place I went wanted me including Bell Laboratories. Bell Laboratories invited me to come and visit and I came to Bell Laboratories. I think I had two days and I said- the guy who recruited me actually at Cornell I said to him, "I'd like to see some stuff about semiconductors and semiconductor devices here." Okay. So he took me to see this guy. His name was Ian Ross. Ian Ross said to me, "Well, Shockley has asked us to do some work here to make a device called a functional device and so if you'd like to work with us here that would be okay with us," and so I took the job at Bell Laboratories. It was oh, so smooth.

Brock: Was there any sort of direct connection between the field emission work that you had done at Cornell and what was going on with this project?

D'Asaro: Well, there was a guy at Bell Labs at Murray Hill there who did work on field emission and I got to know him pretty well, but I did not intend to do that. I intended to do what was suggested to me that I should do, which was to make what they called a functional device, and Ian Ross had some ideas about how it should be done. So that's what I did, but actually very soon after I started working on that, I became very knowledgeable about the properties of silicon. Silicon at that time was a novelty. The material that was used for making transistors was germanium and germanium was going through a crisis because it wasn't very reliable. The devices died quickly and didn't have a- not a good reputation and so the fact that I was working on silicon was a plus although silicon was in no condition at that time yet to be commercialized, but I thought this silicon could be used in a field emission microscope. And I said to Ian, "Look. I have some equipment at Cornell. I'd like to take a piece of silicon and put it in that equipment and look at the field emission," and he said, "Well, I'm not so sure that you should do that" and then he said, "I'll have to talk to someone else and get their opinion." He asked George Dacy. "Oh, no," George said. "That's not what we're supposed to be doing. You're not supposed to do it," and I said, "Well, I'd really like to do that, so I'll take some of my vacation time and go do it." So I took some vacation time. I took a piece of silicon. I worked out a technique for doing it and the equipment I had- the same equipment was still sitting there that I had worked on before and I put it in and I did the field emission experiment and showed the results, good enough to publish and I published it, but it was not well received at Bell Labs. They said, "No. You're not supposed to be doing that. Why are you doing that?" Well, I thought if a guy wants to do more than he's asked to do he ought to be given extra credit, but that was not the way it was. People felt annoyed that I didn't do what I was asked to do, but I felt good so if you can do more I thought well, that's better, so anyhow I continued with the work on the functional device.

Brock: May I ask what some of the other job opportunities you explored before deciding on Bell Labs?

D'Asaro: Well, there were lots of them in New Jersey. None of them were very appealing to me. They were mostly pedestrian or they didn't understand what my background was or they wanted me to do chemistry or maybe they wanted me to do computer at a very high level, which I also was not able to do. I didn't know computer programming. I didn't know any of that stuff at that time so I didn't find anything that was as suitable for me as the Bell Laboratories job. The fact that I had taken that course, you see, made me ideal for the job. I knew all these things about semiconductors and how they worked and what you could expect to do with them so I was the perfect person for the job so I stuck with it for the rest of my career at Bell Laboratories. I continued to do semiconductor device physics and fabrication and invention of a lot of the different stuff- different things, different devices at Bell Laboratories.

Brock: Could you describe Bell Labs as you found it in 1955?

D'Asaro: Well, it was full of marvelously productive and clever people. You just walked down the aisle and every name was a marvel to see and people doing all kinds of incredible things - the Hall effect transistor for example. The Hall effect device got the Nobel Prize for the fractional Hall effect as an example, and I knew a lot of these people. I could go talk to Bardeen or Shockley, who had left Bell Labs by the time I got there and started up his new idea on the West Coast. After I'd been at Bell Labs for a while, I thought I'd kind of like to see what Shockley's doing out there, and so I took some vacation time and I went out to see Shockley in his new lab, which was actually the first of the Silicon Valley labs was the one that Shockley had established already. I don't know what year it was, 1957 or something like that, and he showed me around. He wanted to give me a job out there but I didn't think I really wanted to do that, and then another marvelous thing happened. I had published a couple of papers on diffusion in silicon and Gordon Moore came up to me at a device research conference meeting and he said to me, "Oh, I think you might like to come and work on silicon devices in our new company," and I said, "Well, what's this new company you're talking—" I knew Gordon Moore. He had done really good stuff already. I said, "Well, what's the name of your company and what are you trying to do?" "Well," he said, "we're trying to make integrated circuits and sell them and the name of the company is Intel and we're—" And he said, "Why don't you come and join this company? And they got- we got about five or six guys here." They had guys like Hoerni and all these people and they became world famous and incredibly rich, all of them. And I said, "Well, I'll- I've got a good job at Bell Labs" so I turned him down. How about that? Later he came to me again. Three or four years later he came to me again after I'd published that paper on the stepping transistor. "Oh, would you like to come again and see us?" So we went. Barbara came, and I said, "They're serious. They really want me to come and work at Intel." We looked around and she said, "Well, no, I don't like California. I don't like it. The houses have no basements," she said, "or, [they have] these little lots." And everything was so much more expensive there too so we turned them down again. How about that?

Brock: Returning then to Bell Labs when you first joined it, I was wondering if you could comment about just sort of its organization and then particularly for the efforts in solid state electronics. How was it organized? Could you describe the leadership?

D'Asaro: There were two sections in Bell Laboratories. Most people didn't realize that. In device work there was a section called research and then there was a section called development, and research was supposed to look at the basic properties of materials and find out new things in physics. Development was supposed to take these new ideas and make them into something practical which could be manufactured so- and I was in development. I really didn't think that was quite what it should have been, but anyhow that was what I was. And in the development area the constraints on the individual choice were much narrower than in the research area, so I often found myself hemmed in like I did when I was trying to do the experiment on the silicon field emission because I had to stick to stuff that the manager wanted to do and not do something different than that and I was not very clever. I was not sophisticated in my dealing with people, so I really didn't understand that you really have to please the manager. I tended to be a little rough sometimes I think in retrospect, but I was productive and because I turned out things that worked. They liked me there. And I got promoted to be a supervisor which meant other members of staff worked for me, and that was really quite delightful. I—Well, skipping over a few things, I had mentioned to you I guess earlier but not on tape that this guy, Zhores Alferov showed up at Bell Laboratories and he was the guy that initiated- that worked on the semiconductor laser at- running it continuously at room temperature, but earlier I had taken an interest in semiconductor lasers at a much earlier phase and I had found at the device research conference, which was the premier conference of the year, there was a paper given on gallium arsenide, and the luminescence of gallium arsenide, in which the paper showed that at low temperatures, liquid nitrogen temperature, the luminescence was a hundred percent efficient, hard to believe. That means for- if you did a photo excitation experiment on this device every photon that went in was converted to a photon which went out; a high-energy photon came out as a low-energy photon. If you put a P-N junction in the liquid nitrogen, forward-biased it, the luminescence was extremely high efficiency. I thought wow, I bet you could make a laser out of that, and I came back and I said to Bob Ryder, who was my department head then, "I think I can make a semiconductor laser because I've heard this talk at low temperatures." And he said, "Well, that's not your job. Come on." That was the point of view. If this wasn't given to you as your job—I was supposed to be making light-emitting diodes and I knew all about how to make P-N junctions and gallium arsenide. I was making light-emitting diodes. That was my job, but nobody wanted me to make lasers. I went to my friend, Dawon Kahng, who later became very famous as- for the MOS transistor, and I said, "You know, Dawon, I know how I think we could make a semiconductor laser" and I told him and he said, "You're right. We could do that," and he said, "All we have to do is find a guy with a pulser and a spectrometer." I didn't have a pulser and I didn't have a spectrometer so we went around and we talked to various people. We found a guy in research who had a spectrometer and a pulser and he said, "Well, yeah, I understand what you're saying but I'm very busy" and he said, "Well—" So I went to my department head. "I think I can do this experiment if I had a spectrometer and a pulser." He said, "Well, no, it's not your job to do that" so we didn't do it. Then about three months later at General Electric and also at the University of Illinois and at Lincoln Labs—They all three made semiconductor lasers exactly the way I said they should be made, and then Bob Ryder came to me and he said, "You know you were telling me about this. Why don't you work on that?" So I worked on semiconductor lasers from then on, but they were pretty cloddish in the development area.

Brock: When was this whole episode that you just described?

D'Asaro: I can't remember the date exactly but it was well along, in 1960 maybe. I don't even remember the date for the exact date of the semiconductor laser. So I—They said, "There's a guy named—" Somebody told me there's a guy named Ray Gershensen in the research department. He would like to write a paper. He'd like to work with you and write a paper." And we wrote a paper on band filling as- in gallium arsenide and that was a paper which dealt with what happened just at the threshold of the semiconductor laser so we did get something published right away which was- actually used the exact same material that I was trying to get somebody to do earlier, the same stuff, and they were—I had that kind of situation happen again and again at Bell Laboratories where I said, "Let's do this" and then it wasn't until much later that I got it done and sometimes it was too late; sometimes it wasn't too late. The thing—The same thing happened with the avalanche photodiode. Shall I tell you that?

Brock: Sure. Why don't you tell us that and then maybe we can return to the functional devices.

D'Asaro: Oh, yeah, we kind of skipped over that, didn't I? Well, I was so proud of this project called the avalanche photodiode. I had—When Shockley's lab collapsed because he didn't know how to run a business— He was very good at research and wonderful at doing innovative experiments and as I told you I had visited there earlier and seen some of the things he was doing. He was very, very clever. He was doing plasma depositions of silicon nitride which didn't become well known until very much later, and he had published a paper together with a couple of people there that were working for him on guard ring photodiodes which showed that if you make a certain structure called a guard ring photodiode structure you could avoid the surface effects of leakage. And then I saw that paper and I'd say at about that same time the lab collapsed and a whole bunch of people came to Bell Laboratories from his lab and they were very good people, very, very high-quality people. And one of them came to work for me- with me and I said to him, "Look. I—" And I said, "At Shockley's lab you work on making on this guard ring photodiode. I think if we do that and put the device into reverse bias you'll have this wide space charge region and we could put it into avalanche multiplication," which was the mechanism by which diodes carry current when there's a high electric field. So you get them up to breakdown and then there'll be an avalanche multiplication current, but there's also a surface leakage component which makes it useless as a device, but I said to him, "Let's make it with a guard ring and then we won't have these leakage currents and we can make a good avalanche photo detector out of it" and he said, "Great." So I went again to my famous Bob Ryder who, was a stick-in-the mud, and said, "I got an idea for how to make a very new kind of a photodiode which would be very efficient and very sensitive." "Oh," he said. "Well—" You know what he said already. "We're not working on that" and I guess I- by that time I was working on lasers but this was a photodiode so I said, "Well, why don't I just put it in the shop?" We had what was called a model shop that was a structure by which—That was a arrangement by which if someone had a device which he made— He didn't have to make it himself. He could describe exactly the steps necessary to make it and it would be done in this facility which was set up to carry out all of the processing steps which would be needed to make a semiconductor device. So I put my plan, my program, my processing steps and my photolithography mask all in to the model shop and I said, "Look. I'm trying to get this thing done," and he said, "Well, you have to get in line. You have to wait until you- until there's a time for your work to be done." And I said, "Well, can't you give me a little faster than that?" And he said, "Nope. You don't have any high priority. You just have a very low priority," so I had to wait. We waited six months. During that time, a guy at—oh, dear me. What was the name of it? Let's see. It was Hughes Laboratory—did an experiment which was not anywhere near as good as what we did. What he did was that he looked at a very high frequency at the avalanche photo multiplication in a photodiode and found that he could get gain at a very high frequency and it wasn't as degraded by this- by the surface currents as it would have been at a lower frequency. Well, he published his paper. I'll be darned if the IEEE didn't give him a medal or an award for innovator of the year or some stupid thing. I thought that's dumb, and meanwhile we got the results and it worked great; it worked perfectly. We got this terrific avalanche multiplication photodiode out of it and it did just what we wanted. We measured the frequency response. It was wonderful, published the paper. What did I get out of it? Not a lot, but I certainly got a lot of satisfaction with the first avalanche photodiode to have photo multiplication photo detector. They're widely used now. They're widely used everywhere. They're used in fiber optics transmission systems. They're used in astronomy and they're widely used so I can say to myself, "Okay. So you did a good job. So what do you get out of it?" There used to be an old joke. When the Staten Island ferry cost a nickel you could say, "Oh, with that a nickel you could a ride on the Staten Island ferry." So one of many experiences I had at Bell Laboratories.

Brock: If we return to when you first joined Bell Labs and this group who were on this project that Ian Ross had going on to create a functional device, could you talk about just the concept of functional devices as you learned about it in 1955? Could you describe the group working on it and where it fit in to the sort of development organization?

D'Asaro: Well, it was really lucky in a way that I came to Bell Laboratories when I did because at that time all kinds of technology for processing silicon was being developed. For example, the first photolithographically controlled masks were made by a guy named Jules Andrus who was in the research area, and the first oxidized silicon wafers with controlled thickness of the oxide were made at that time. And the combination of those two things made it possible to control diffusion by oxide layers so what one could do is use a mask which was made out of a photolithographic material, control the pattern, etch this- etch the oxide, the silicon dioxide, away with dilute hydrochloric acid, and then diffuse into the material- into the silicon in a very controlled way. And I developed a technique for diffusing arsenic into silicon which was very fast and very reproducible. It was all new at that time. Those are commonplace technology things nowadays but they were very new then, and I used those things then in further development of the stepping transistor, but the idea of the stepping transistor did not originate with me. It originated with Ian Ross and Shockley. They—It —Shockley had said, "Okay. You guys want to make something which I'll call a functional device. It means it should do something all in one piece of silicon, not putting different things together but all in one piece of silicon so we'll make a counter." And Ian Ross said, "Okay. We'll make a counter. It'll count to four." So we wrote a couple of patents describing it, but hadn't made it, and another guy named Howard Lohr [ph?] was there before I was there and they tried to make this thing in germanium but it didn't work, and they even gave a talk at the device research conference but it was all pie in the sky. They actually had never made it work, and so I was given the job of doing it in silicon and that was great. That was just wonderful so I could get silicon from—Silicon was being grown by two techniques, either vertical floating zone silicon or horizontal Bridgman silicon was being grown at Bell Laboratories and you could get them, and you could get them cut at a crystallographic direction that you wanted and you could have them polished. And I developed a technique for polishing them very smoothly. All of this was new stuff, and then I was able to make as I said patterns with well-defined p-n junctions just in a local region, which is commonplace of course today for integrated circuits. The—So the first ones, however, that I made that worked were not made with those technologies. They were made just with plain, old etching but they were made using the silicon dioxide mask so what was done in the very first ones that I made was that the silicon was oxidized and then we had a pattern and with the pattern as I said before you could remove the oxide selectively, and then you could use that oxide as a mask for etching and we could etch the structures. And that- there was a picture of that that I gave in that- the talk I gave at the Computer History Museum [May 6, 2009]. And so that was a completely etched structure which- where their dimensions were controlled by oxide masking, and that one worked as I said earlier in 1955 at the end of the year, and as you know the first integrated circuit which was made at Hughes—no, not Hughes. I mean Texas Instruments—was 1958, but I- neither Ian Ross nor I nor others who looked at this thought of it as an integrated circuit although in fact it was an integrated circuit because the devices were- each of them were separate P-N-P-N switches. And they were connected in such a way that if the device turned on then the current would flow in a non-uniform way so that if you had another device adjacent to it, the voltage would be transmitted to the next device and then it would switch if you put a voltage on that device. And so you could actually move the on location from one device to another, and Ian had set up all kinds of fancy pictures in his patent on that, but somehow it didn't occur to us that it was an integrated circuit at that time. So when I wrote the paper then later about using oxide masking to make the exact same structure it still didn't dawn on us that it was in fact an integrated circuit, and Nick Holonyak, whom you may know the name at the University of Illinois wrote a letter saying, "Look. You made an integrated circuit." And as I had mentioned in the talk I gave at the Computer History Museum, Jack Morton, who was the vice president, had taken the point of view that integrated circuits were a bad idea. He took the point of view that integrated circuits could not be made to work in any large volume, that it was a waste of time to try. He called the people who were trying to make integrated circuits—He called them large-scale idiots, and when I said to Ian Ross- I said, "Look, Ian. You could make circuits on a single piece of silicon using these techniques. You could make resistors and you could make capacitors and you can make transistors. We know how to do that," and Ian said, "Bell Laboratories—" He said, I quote, "Bell Laboratories does not want to make itty-bitty circuits." He had a kind of humor which is characteristic of Cambridge and Oxford, and anyway he came from Cambridge. In England at the universities there's a game that's played. It's called one-upmanship. One-upmanship means I'm smarter than you and I can prove it because I'm more clever than you are, and Ian played this game constantly so saying itty-bitty circuits that's demeaning the thing by just the way you say it. So I was so taken aback by all this negative stuff that I didn't pursue it, much to my regret later, from this point of view because if I'd even written one more paper, which I easily could have done with the information I had, I could have gotten the Nobel Prize with invention of the integrated circuit because I was way ahead, way, way, way, far ahead, but we didn't. So there you are.

Brock: Bell Labs did though have some of a commitment to this concept of the functional device.

D'Asaro: Well, it was because of Shockley that they did, but when Shockley left that kind of push evaporated and then it was up to Bell Laboratories to make their own decisions as to what they wanted to push so what they pushed was what they called right scale integration. What does that mean? That means if you have some information about the rate at which devices die, degrade or become non-useful after they've been operating for a while, then you can design a circuit which would put the devices together but at the scale or at the number of devices which would not fail. Now the statistics of this were faulty. They had gained- had gotten statistics on the rate of failure of silicon devices and they found that that rate of failure was too big to allow them to all be put together on one chip. The failure rate would go to an extremely high value and it wouldn't work according to their statistics. The statistics were faulty. The reason that it would work anyway is that failures in devices are always due to processing changes. There's a little piece of dirt or some kind of a process irregularity which causes a device to have an imperfection and it fails. Now these kinds of imperfections are- come in patterns or certain areas so a certain area if you have a wafer, which is a little circle thing, a circular thing like a button, it will have certain areas which are a good one- good areas and other areas which are bad areas and they're clustered, the good areas and the bad areas. So at Intel and at Fairchild, what they did was just go right ahead and make it anyhow, never mind their statistics, and they found that they could get a yield and the yield was not zero and they could make them work. And they put together things with a hundred transistors or a thousand transistors and they worked so that decision that it should not- that Bell Laboratories should not work on integrated circuits put Bell Laboratories and their manufacturing division, which was Western Electric at that time, way behind. We were five years behind. They didn't wake up until a long time after everybody else started making silicon integrated circuits, and by that time it was too late. They never caught up. They closed it all down now. They decided to go completely out of silicon manufacturing. That's the way it is now today so it was a great shame that they did that.

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START OF TAPE 3

D'Asaro: ...maybe I didn't mention that Jack Morton, who was the Vice President at that time, was very much opposed to the idea of integrated circuits, and didn't like it. And the reason that he got the idea that they could never be made to work was that they were using some statistics, but it turned out later that the statistics were faulty and did not apply to the real world. So the decision not to pursue it was that way. But Jack Morton was a pretty stupid man, and he was not very broad in his understanding, which I would like to tell you a different story and that had to do with when I was working on what was called the stripe geometry laser in my group—because by that time I was a supervisor of my group. One of the guys come up with the idea of making a stripe contact instead of a uniform contact. Now so the idea was that if you had a narrow stripe contact, you confine all the current to a stripe, and that would change the properties of the laser. And you could get control of the modes and what were called filaments, which are local regions where the lasing would concentrate. And so we tried that, and it worked great. And we used different techniques to make the narrow stripe. We used photolithography. We used diffusion, and we got some devices which had very narrow stripes, maybe 10 microns wide or something like that, and they had very well-defined Hermite-Gaussian modes, which were well identifiable by theory instead of being what was true before of semiconductor laser, very irregular and not clearly understandable. So with this stripe geometry contact, we could understand exactly what was going on in the mode structure in the device, and we got some that were sufficiently narrow that they had a zero order mode, which is the most desirable one, and that's used now all the time in fiber optics communication as most desirable mode. So I gave a talk at the Device Research Conference in Europe, it was a European device research conference on zero order mode semiconductor lasers, and Jack Morton was in the audience. I gave this, and I thought it was a pretty good talk. It was the first time anybody had made such a thing, a zero order mode semiconductor laser. And that impressed some people in the audience, but Jack Morton was not impressed. Instead he said later, "Oh this other talk by a guy who's got a new microwave oscillator is what I really like." And I think I said, "Did you hear my talk about the zero order mode?" And he wouldn't even talk to me. So it turned out later that the guy with the microwave oscillator had talked about stuff he hadn't really done and that the device never really worked at all. <laughs> But that was kind of a situation, you know, was out of control. So we published something on the zero order mode semiconductor laser, and it became very important. So that was an unfortunate influence that Jack Morton had.

Brock: In this period that you were working on the stepping transistor element, the functional device, that was from when you joined Bell Labs until into 1959?

D'Asaro: Yes, about 1959. And then I was told, "Well you've done enough of this and nobody wants to pursue it anymore." You know, that was when I had the conversation before with Ian that we could make this, we could make that. And he said, "No, no more. No more on this topic." So I went to work on tunnel junctions, the Esaki diodes, which was a dead end. Nobody got anything out of that. After that I worked on photodiodes. I'm sorry not photodiodes, light-emitting diodes rather, and Bell Laboratories didn't get anywhere with light-emitting diodes and neither did I. <laughs> The good work was done by Nick Holonyak at the University of Illinois.

Brock: I just had a few other questions about this period of 1955 to 1960. In particular, the context for this counter seemed to me to be more a context of this was a potential device for computing rather than for a communications type of application. Was that the case?

D'Asaro: Oh yes. It was definitely oriented toward digital calculations. It was not oriented toward an amplifier at all. The p-n-p-n device is a switch, and it's not suitable for anything else except a switch. But it's a pretty good switch in a way, although it's inherently slow because it won't turn off very easily. But for what we did with it, you know, it was suitable for what we did. If we'd have gone further with the technology we would have used transistors, at least if I'd had my way. I saw very clearly when I was working on that device that it would be possible to use the oxide as a mask and also with the same oxide to do interconnections between different devices on the same chip. And that became called planar integration, planar integrated circuit. I tried to do that. My oxide was leaky and I couldn't get good operation because there was something at the interface between the silicon and the silicon dioxide which was interfering with the operation and causing a lot of leakage. I thought, "Well, you know, this must be a question of cleaning." And I didn't really know what to do. I knew there was a lot of sodium floating around, but I wasn't given any opportunity to have a program to do something about it because, you know, Ian said, "Well no. Let's not work on this anymore." Even though I thought it could be done. When I was at the celebration of the 50th anniversary of the invention of the integrated circuit [Computer history Museum, May 2009], I asked Gordon Moore directly, I said to him, "When you were there with Hoerni and others and they made the first planar integrated circuits, how did they control the interface so as to prevent leakage?" He said, "We didn't. We were just lucky." He said, "Some of them worked, and some of them didn't." "And we didn't know why," he said. <laughs> Of course he didn't want to tell me what they did later in order to get control of it because that would have been, you know, proprietary information. Or at least he didn't—I don't know why it would have been proprietary at this late date, but anyway that's what he said. So that kind of fitted in. If I'd given, you know, had the opportunity to do some cleaning of some sort and some chemistry, do some analysis and some analytical chemistry, I probably could have figured out how to do it. I could have changed all my water to be distilled water on site instead of as it was. It was water which is ion exchange water, you know, which isn't as clean as I could have gotten by distillation. But I don't know what else was at the interface. I never found out. <laughs> That was left for others to do. But a vast industry grew up on exactly that.

Brock: How many other people worked on this stepping transistor element project in those four or five years?

D'Asaro: Nobody worked on it except myself and my assistant. We were the whole show. Howard Lore had worked on it earlier in a failed attempt to make them out of germanium. And Ian Ross became a department head, and as you may know, ultimately President of Bell Laboratories. So I was the whole show at that time.

Brock: Ian Ross, at what time did he leave the project? He went on to get interested in these PNP sorts these structures with intrinsic layers of silicon in them, is that right?

D'Asaro: Well I think one of the critical things that happened when he became a department head was that the epitaxial growth of undoped silicon became recognized as a way to make high-speed silicon transistors and could reduce the capacitance by making the device on a very pure silicon, whereas previously they had to use silicon which had some kind of doping on it. But by going through epitaxy in such a way that the layer was undoped, then you could build your device on an undoped substrate so the substrate didn't have any affect on the device. That was a step forward and really improved the yield of silicon transistors. Yeah, that technology was also used in photodiodes, as you said.

Brock: So he was kind of—but that epitaxy effort seems to have really picked up at the time when the stepping transistor element project was shut down.

D'Asaro: Oh yeah. Yeah. Yes. Yeah. It did, and as you know, MBE was invented about that time, molecular beam epitaxy, which became the dominant way for making very pure silicon or other things.

Brock: Well how would you—in terms of the size of projects that were going on in Bell Labs at this time, the stepping transistor element project in terms of resources and manpower, was it a—what scale of an effort was it compared to other efforts in semiconductor electronics around it?

D'Asaro: Well nobody was working with me except for, as I said, I had a technical assistant who was willing to do whatever I said and did it reasonably well. You know, so it was just a one-man band. That wasn't unusual in Bell Laboratories. If you had a device which was headed for production, then you would get more people involved, but this thing was clearly not headed for production. It had gotten a negative signal from the management, so it was not going on production. I was given permission to carry it over to a certain point, and then they said, "That's all you're going to do."

Brock: Were there other people working on functional devices?

D'Asaro: At other companies there were. Sometimes it was called other names. Molecular electronics was a name that was given to some similar technologies, and in cases where there was military support from the Army or from the Navy there was a lot of enthusiasm about things which would be integrated. And for example in the, as you probably know since this it the 40th anniversary of the moon walk, on the equipment which was used they had planar integrated circuits in their computers, so they'd gotten that far by that time.

Brock: But there weren't other researchers at Bell Labs while you working on the stepping transistor element that were working on different forms of functional devices?

D'Asaro: No. It became an unpopular subject <laughs> at Bell Labs.

Brock: This period also, '55 to '60, as you were saying, and a little bit before, was an incredible time for people coming up with new technologies for fabricating silicon devices or devices in general, you know, oxide masking, diffusion, photo etching. How did people at Bell Labs sort of come up to speed on what had been created at Bell Labs? Was there a formal method, or was it from just interacting with other people? How did it work?

D'Asaro: Well if somebody came up with a new idea, usually they'd give a talk and anybody who wanted to come was invited, usually. Although there was this supercilious idea that people in research were in another universe, and they oftentimes didn't invite the people that were in development, which I thought was really ridiculous. But that was often the case that there was this social division. I don't know, it was very—that would have had a negative influence. But for things that were done in the development area, yes, everybody gave a talk if they went. For example if they went to a meeting and heard somebody talk at a meeting, then they would give a talk at Bell Laboratories to explain what they had learned at this meeting or how they would evaluate what they had learned. And that was always very useful to see what other people were doing. And usually we would say, "Well that's pretty terrible. We put out much better work than that." <laughs> That was the usual thing to say. But usually it was true.

Brock: Was there any sort of group formation around people who were working on silicon? Was there an informal group defined by that interest would you say?

D'Asaro: No. There was not an informal group defined by that interest. It was pretty much do what you want. But of course, at Bell Laboratories, if you wanted to know something about something, and you didn't know about it yourself and you didn't know, you know, what would be the best way to find out about this, you would go ask someone else who had worked on that subject. And usually it was just a short walk down the hall and you'd find the world's expert on that subject right down the hall from you. And so it was a very fine place to work for that reason. So you could find somebody who knew whatever you wanted to know. And usually people were very proud and very happy to tell you things.

Brock: Also in this period, let's say up to 1960 or so, for device development projects in thinking about the major areas of application, were there device development efforts aimed at sort of communications or telephony and then another group of device developments aimed toward digital computing? How did that shake out?

D'Asaro: I don't think anybody thought of it that way. We usually worked on whatever we thought was interesting and tried to persuade our management to go along with something that we thought was interesting or novel or useful whether it was digital or whether it was not digital, maybe called it analog. Whatever it was, I later worked on some devices which were called varactors, variable capacitance devices which could be used in microwave amplifiers and there was an interest. Also those devices turned out to not be very interesting because they were basically two terminal, and two-terminal devices are not as useful as three-terminal devices. So they were also called parametric amplifiers. They didn't really go anywhere. But there were a lot of projects that didn't go anywhere, you know. But those that went somewhere <laughs> there were some but not all.

Brock: So around 1960 when you were switching to start to work on tunnel diodes, how did you and your other colleagues react to some of the developments that were being announced around that time? For example, planar silicon transistors and semiconductor integrated circuits from places like TI and Fairchild?

D'Asaro: I always went to all the conferences I could get to, sometimes on the West Coast but sometimes on the East Coast, and I heard a talk I think it was on the west coast from some guys at Intel, or maybe it was Fairchild at that time. And they said, "Oh we've made this planar integrated circuit and here it is. And here's what we've done and here's how we did it and so forth." And I tell you what. I couldn't believe it I was the only person from Bell Laboratories who heard the talk. Nobody from Bell Laboratories came except I came. And I thought, "You know, this is great. These guys did a good job. They've got a product. They can sell this product." And when I came back to Murray Hill I started telling people, "You know, these guys at Fairchild have made these planar integrated circuits and they're selling them." Nobody cared. Nobody was interested. Nobody was excited about it. I thought, "What a bunch of clods." <laughs> Really. The people had so little understanding of, I guess you could call it marketing, that they knew what would get them ahead personally and that was to make their boss happy.

And to try to think of the future, there was not that much interest, which I thought was really sad, you know, because I was always enthusiastic about stuff that could be done, that might open up new areas of interest. And that's the way I felt, as I said before, about the strike geometry laser, you know, it would open up new fields. I said to my boss at that time, whose name was Gene Gordon, I said, "You know, we could do an experiment showing transmission of optical fiber for communications." And he said, "Well there's a group coming," he said, "from investment companies and Wall Street and they want to see something and maybe you could show them that." And so I set up a demonstration using fiber optics and semiconductor lasers and photodiodes so I could talk into one end of this thing and you could hear me at the other end and this fiber. And these guys stood there, you know, "Blah." They didn't know what they were looking at. They had no idea what it was. This is a prototype for the fiber optic communication system they were looking at. They had no idea, bunch of clods. <laughs> Yeah, I talk about people on Wall Street, they don't understand anything. <laughs> So then Jack Fleming and Gene Gordon, my boss, said, "Well don't pursue that anymore. That's not your job." So I didn't pursue it anymore. <laughs> But it just proceeded to become the best thing, you know, that was done in electronics to make the fiber optic communication system.

Brock: In your switch from the stepping transistor element to tunnel diodes, it seems like you were going from one area that was very sort of cutting edge at the time, to another. I believe at this time, and around 1960 or so, that the tunnel diodes were of great interest for making very high-speed digital computers, et cetera.

D'Asaro: Yeah, that's right. The tunnel diodes were of great interest for making high-speed digital computers. But I think I perceived, and others at Bell Laboratories perceived, that they would have a very limited future because it's a two-terminal device—it's not a three-terminal device, it's a two-terminal device—it's a very difficult device to make something useful out of it. I mean, an amplifier out of it. I think some people at RCA managed to put something together that worked, but it was so delicate and so tricky, you know, it could not be made really reproducibly in a manufacturing situation. So it never went anywhere.

Brock: For how long did you work on tunnel diodes?

D'Asaro: I think about a year maybe or two years at the most. I didn't work on it very long. I saw right away that it couldn't be. I couldn't think of any way to make it any better. The technology was rather primitive, and I couldn't think of anything I could do. We didn't have MBE at that time. Later when molecular beam epitaxy became possible, then it was possible to make well-controlled tunnel junctions, but we didn't have that then. So it wasn't done. I mean nothing else was done.

Brock: Was this work in silicon?

D'Asaro: No it was done in gallium arsenide.

Brock: So this was a switch for you from silicon devices to compound semiconductor devices?

D'Asaro: Yeah, that's exactly right. I left silicon and I didn't come back really at all. <laughs> I spent the rest of my life at Bell Labs on gallium arsenide and related matters.

Brock: I think from the perspective of today where silicon devices have achieved such predominance, I'm wondering if at the time, 1960, it was at all clear that silicon would have come to have such a dominant place in electronics? Or did it seem to you perhaps just as likely that compound semiconductors like gallium arsenide could become the most important semiconductor material?

D'Asaro: Yeah. Well many people thought that gallium arsenide might be used in computers. That turned out to be an incorrect idea. You probably know that some people tried to make large computers using gallium arsenide, but their efforts were unsuccessful. They couldn't really do it in a reproducible and reliable way and they couldn't do any better than what silicon did and the reason that they could not, as everybody knows who's taken courses in electronics nowadays, is that the interelectrode capacitance, that is the capacitance not to be interelectrode, interdevice capacitance which is due to the connections between devices in an integrated circuit, those properties of that interconnection, just a piece of gold usually or maybe it's aluminum. There is a resistance in capacitance and inductance properties of that connection that govern the speed of the integrated circuit. It's not the switching speed of the device. Gallium arsenide has a faster switching speed than silicon, but that's not what's needed. What's needed is to put the devices so close together that the RC time constant is reduced so the whole thing boils down to a question of lithography <laughs> and nothing at all about the fancy compound semiconductors.

Brock: Maybe one other question before—well maybe a few questions before moving on to sort of your gallium arsenide career, if you will. Just thinking about this period of the late '50s and very early '60s, the status of interest in the P-N-P-N structure at Bell Labs as a switch for both telephony and for computing applications, can you talk about that? It's, I think, often the four-layer diode, whatever you want to call this P-N-P-N structure, historians often characterize it as some sort of red herring, marginal sideline, but it strikes me that there was a lot of—that it was more a part of mainstream semiconductor research in this period.

D'Asaro: Well all of that was well understood at the time that I was making these study transistors, and there was some analysis which showed the speed that you could achieve with a P-N-P-N, but because of the fact that the P-N junction floods the two regions, the N and the P regions are full of minority carriers when it's in the on state, to get them off, you have to pull them out through the p-n junctions and it's rather slow. It's a slow device to turn off. And it's difficult to do anything about it, to turn it off. Shockley himself fell into this trap and thought that it would be possible to make practical integrated circuits using P-N-P-N devices. He was totally in error, and it was partly because of that that people abandoned him, you know, the people he had hired from the best universities in the world said, "You're wrong." But they couldn't tell him to his face because he was a very difficult man to deal with. I got to know him very well later, and he was a tough guy to talk to because he felt that he was always right, you know, and that's a terrible flaw. So you couldn't turn them off fast. But Nick Holonyak, whose name keeps coming up, had left Bell laboratories. He knew me there when I was making these stepping transistor devices and he recognized exactly what they were, that they were integrated circuits. Well he went off to General Electric. At General Electric he perceived that the p-n-p-n device would make a great power switch, it had a very low loss when it's in the on condition, which is ideal for a power device. And so General Electric is very big on power, and so they manufactured them for that reason and it's called the silicon controlled rectifier, it's sold all the time for that and everybody's dimmer, you know, for your light, it's all using silicon p-n-p-ns, silicon controlled rectifier. And they're used in very large power facilities like substations, you know, for power. They can be made in huge sizes. So that was the destiny for the p-n-p-n.

Brock: But it was the case, was it not, that there were several other individuals and groups working on p-n-p-n structures at Bell Laboratories on the development side?

D'Asaro: I didn't know of any. I didn't know of anybody except me. I didn't know of others who were doing anything. Everything was centered around what I did.

Brock: Let me just—I'm just looking at my question list here. Pardon me for a moment. When you were, in the late '50s and early '60s, as you were working on the functional device, were other approaches to sort of micro circuitry being investigated at Bell Labs, like thin film circuits or hybrid circuitry or circuitry at cryogenic temperatures, or were any of these things involved?

D'Asaro: Yeah, well we knew of all those efforts. People that wanted to make low temperature computers for example, and I think everybody at Bell Laboratories thought it was pretty impractical. IBM did a lot of work in that area. But when you talk about other approaches, the one that comes to my mind is the MOS transistor, you know, which was invented at Bell Laboratories by Dawon Kahng and Mohamed Atalla, they who made the first one. And Dawon Kahng very correctly recognized that the MOS field effect transistor would be a very practical device to use in a computer and he said to me and to others, "It could be made easily in large numbers because it's quite simple." And he'd gotten a hold of some high purity silicon dioxide and showed that, in fact, you could make a nice FET, field effect transistor, switch out of silicon using this method and he got a patent on it. He gave a talk at the device research conference and he said to Jim Early, who was the Department Head, he said, "Look, this thing has a lot of advantages. It can be made cheaply in large numbers. It's perfect for large scale integration." And Jim Early said, "No, you're wrong. Large scale integration can't work. And bipolars are the thing to make if you were going to make large scale integration you'd want to make bipolars because they're inherently faster." And Dawon Kahng was a very stubborn guy, and he said, "Yes, but cost is predominant in manufacturing of large numbers of devices. The least costly device is the one that's preferred in there." So he couldn't get anywhere. Nobody would agree with him, and they put it on the shelf. And then later, later at RCA they published a paper saying, "Look, we've invented this great thing. It's an MOS field effect transistor." Then <laughs> then Jim Early got himself in gear and wrote a letter saying, "Look, we've already patented this thing." And there was a talk already given at the Device Research Conference on this, I don't know, three or four years previously to that. And so Dawon Kahng got the credit, and he got a bunch of prizes, you know, for that invention. I was in Korea many years later and I said to my Korean host, "Who is the best—who is the most famous and most important Korean scientist?" They said, "Dawon Kahng." <laughs> Well, okay, so he's vindicated. <laughs> He went on to be head of the DuPont Electric Company Laboratory which was set up here in New Jersey and flourished for a while. But he got sick and died very young, very young, unfortunate. The guy died at a fairly young age.

Brock: If we go to this period of around 1962 when you move over to gallium arsenide, and in particular, looking at light-emitting diodes, could you just talk a little bit about the state of affairs in compound semiconductors at this time, 1961, 1962. What were they mainly used for? Who were the main, sort of, customers or clients for compound semiconductor devices? And then also, you know, how you got the material. How the material was prepared, the differences in processing, these sorts of issues, a general introduction to compound semiconductors.

D'Asaro: <laughs> A general introduction coming to you. It was a lot of fun. I mean, gallium arsenide is fascinating stuff. Very early on I recognized that aluminum gallium arsenide is a very close lattice match to gallium arsenide that enables one to have a change in band gap and that was the trick. Using the change in band gap introduced by growing aluminum gallium arsenide was the thing that made it possible to confine the minority carriers and get the device to run at room temperature. That was what Zhores Alferov had accomplished in his new growth technique that I mentioned to you earlier. So the advent of liquid phase epitaxy introduced, made it possible to make some very efficient devices particularly semiconductor lasers, but you could also make photodiodes and light-emitting diodes that way. Unfortunately they were in the infrared and so you couldn't see them very—at all. And if you wanted to make them visible, you would have had to introduce some other material. Well, at Bell Laboratories what they did they decided to try gallium phosphide and gallium phosphide is luminescent in the yellow and in the green, so you can make visible light-emitting diodes that way, but they're not very efficient. You can also make them out of gallium arsenide phosphide which gives a larger range of colors, and many people pursued that, but again, the famous Nick Holonyak comes into view and he showed that if you use nitrogen doping in your gallium arsenide phosphide, then you can greatly increase the efficiency of the device and that became the practical light-emitting diode. So that was one of the big success stories. That's one of the first successful light-emitting diodes which you may remember were red, always red. It wasn't until very much later when nitrides were used that we got them into the blue. So those matters interested me greatly and I, again with the famous Dawon Kahng, we <laughs> took a look at Schottky barrier diodes. Dawon said to me, "Look, with using molecular beam epitaxy, you can make undoped layers. And with these undoped layers you could make a Schottky barrier diode, and it would be very effective. It would be a high breakdown voltage and could be used as a high-speed switch because you could put the lightly doped epitaxial layer on a highly conducting substrate." That was the same concept I mentioned to you earlier about using that same idea on transistors, but to use it on a switch was something new and so we tried it. We evaporated gold on epitaxially grown, undoped layer of gallium arsenide grown on a heavily doped substrate and were able to get good diode characteristics and could demonstrate that this device would switch very fast. You could also do it in silicon. You could do the same thing in silicon. These kinds of high-speed P-N diodes using gallium arsenide became the dominant microwave mixer diode that was used everywhere. Nowadays, you know, they're used in all the things like the satellite communications diodes or any of the <laughs> other devices that run at high speed. They use those.

Brock: Well maybe we should pause here and change the [tape].

END OF TAPE 3

START OF TAPE 4

Brock: Perhaps just to continue this line on compound semiconductors, you just mentioned a phrase, which is “liquid phase epitaxy,” as being an important growth technique that was central to some of these device developments, and I was wondering if you could talk about what liquid phase epitaxy is and how that differs from the crystal growing used to make germanium single crystals or to pull silicon single crystals. How does liquid phase epitaxy differ from those earlier established crystal growing techniques?

D’Asaro: Liquid phase epitaxy was actually invented at RCA. It’s a technique of growing a layer of a material, such as gallium arsenide, on something that you want to grow it on. The way you do is you have some sort of a metallic solvent, typically gallium, and you might dissolve gallium arsenide in the gallium, and then you would have a tipping arrangement so that the wafer that you want it to grow on is at one end of this tube, and the other end is the liquid, which I mentioned, which is doped. So then you tip the tube and the liquid runs down to the other end. You have to have enough arsenic present, and then if you lower the temperature, the material which is in solution will come out and grow epitaxially that’s matching the crystals together on this substrate that you have there, and then you can tip it back again, cool it down, and take it out. And that’s what we’ve got. Well, usually in that case, you could make a P-N junction or whatever you wanted to do. Usually the metallurgy was pretty miserable. Ordinarily, the surface was irregular; it wasn’t well controlled. But you would always find a few good spots, and these few good spots would produce very highly efficient devices and they were really superb. But because the yields were low, it was really only used in research. It wasn’t really practical. Some people tried to use it in manufacturing, but the problem of making a high yield process was insurmountable, and it never really was continued after molecular beam epitaxy was invented. Of course, molecular beam epitaxy is a fancy name for evaporation. It just means you have a substrate; you have a very high vacuum. You evaporate something. Maybe it’s gallium arsenide and maybe it’s a dopant, and it grows epitaxially on a substrate in a very high vacuum. That whole concept of doing that in a vacuum is very different than the way crystals are grown out of melt, let’s say, by the Bridgman technique, which is a horizontal tube, and you cool one end and the phase front advances from the substrate at the cooler end onward as the hotter end cools down. That’s called the Bridgman technique. That’s really the technique that’s used now for making most gallium arsenide wafers. It’s usually that way. Czochralski is another way, where you pull it out of molten material. That is also widely used. But these ideas about crystal growing have nothing at all to do with molecular beam epitaxy, as I said before, which is basically an evaporation technique. So the advent of molecular beam epitaxy did away with the use of liquid phase epitaxy. So as soon as everybody could get layers of gallium arsenide doped with whatever they wanted to dope them with, then they chose to do that. I had good working relationships with Al Cho and others at Bell Laboratories who had molecular beam epitaxy machines. They’re big machines. Have you seen one?

Brock: No.

D’Asaro: They’re quite big. They look like a space age monstrosity. They got things sticking out in all directions. They work great if you know how to use them. Making them work right is somewhere between an art form and a scientific study. But when you get them to work right, they do great. You make hundreds and hundreds of devices—thousands of devices—that way, and they work fine. In some commercial enterprises, they’d churn out thousands of these a day, thousands of the wafers, and chop them up into devices. So that’s a very practical scheme. So having the advent of that available at Bell Laboratories was a great help for new ideas, such as what I mentioned before about this high-speed diode made on gallium arsenide. The same technique can be made also with silicon, using a dope layer, which can grow by molecular beam epitaxy and put a Schottky barrier diode on it, and make a high-speed diode. Although the one made with gallium arsenide is faster than the one made with silicon. Another device I invented, which used gallium arsenide, was what is called today an opto-isolator. What that is, it is a light source, such as a light-emitting diode or a laser, and a photodiode contained in a package. The photodiode has to see the light coming from the light-emitting diode, let’s say. It suddenly occurred to me that this would be a practical thing, and nobody seemed to have made it or discussed it. I had a friend who worked on glass, and he was an especially authoritative and knowledgeable about high-refractive index glass. These are glasses which are made out of things like selenium, sulfur, arsenic—other unpleasant stuff. They have a property of having a very high refractive index. So I said to David Pearson [ph?], “Hey, could you give me some of this glass? I’m going to take a photodiode and I’m going to take a gallium arsenide light-emitting diode, and I’m going to embed them in this glass so the refractive index is similar to the refractive index of gallium arsenide, then the light will be coupled into photodiodes.” So I tried it, and it worked, and I gave a talk about that at the Device Research Conference. That enables the user to separate direct current signals from alternating current signals, because the alternating current can be used to modulate the photodiode, and you’ll get the alternating current out on the detector. But if you had a voltage difference between the light-emitting diode and the photodetector, you wouldn’t get the voltage across the output. That device is widely used. It’s called an opto-isolator, and I made the first one. I gave a talk at the Device Research Conference. That’s the same story about the ride on the

Staten Island Ferry. <inaudible> There things generally aren't very well appreciated at the time. They get more appreciated as time goes by.

Brock: In the earlier 1960s, when you first investigating light-emitting diodes in compound semiconductors, what was the context for that? We talked about—or I keep trying to impose on our conversation—these different contexts of maybe computing applications and communications applications, and it seems like these two realms converge over time, computing and communications. Does that happen around these ideas of fiber optics and these sorts of applications for compound semiconductor devices? Could you just put your work into a larger context?

D'Asaro: Well, it's very difficult for me to do exactly what you say because you kind of imply that there was a choice, and often there was no choice. Things like this opto-isolator, that was bootlegged. I did it totally without anybody telling me to do it, and that's part of the reason that it didn't get much applause, because nobody told me to do it, and I did it just as an extra thing I did. Some things were like that. Some things we were told that we should do. We should thus and so, for example, the high-speed microwave field effect transistor, which was a communications device. This question came to Bell Laboratories because sometime in the 1960s—I can't remember, or maybe it was even the '70s—the AT&T company get pretty annoyed because of their microwave transmission system, which was the system that you're familiar with by looking at the towers that you frequently see. There's a characteristic antenna, which was developed by Bell Laboratories just for this application. It looks like a segment of a horn. It's mounted up on these towers. It's not a satellite dish; it's a kind of a horn-looking thing. And those were designed for use in the microwave communications transmission system. They had also in them a thing called the traveling wave tube. You might have mentioned that earlier. The traveling wave tube was, as the name implies, a tube, and it had a beam of electrons emitted from one end, and there was some modulation, and you could transfer information from one end of this tube to the other end of the tube and get amplification that way. It wasn't a triode tube; it was a traveling wave tube. These were developed at Bell Laboratories and brought up to a pretty high state of perfection. But they had the characteristic that they died quickly. Tubes are notorious for having a limited life, as you know, if you've ever had a radio with tubes. There's a certain life, and then they go bad. These microwave traveling wave tubes went bad after maybe a year or a year and a half, or something like that, and they had to be replaced. They were quite expensive, and there were thousands of them in the microwave communications system. So somebody came to Bell Labs and said, "Could you guys make a high-speed transistor that we could use to replace these traveling wave tubes?" There was a guy named Jim DeLorenzo. This incident, when I got involved in it, was when I got caught in a political struggle, and I, like many, many people with a scientific background, I was terrible at politics. I never understood politics. I never understood how you make yourself look good. All I could ever do was invent things and make things, but I never knew how to play the game of politics well. So other people did. I was going great guns on these lasers, as I was telling you about before, about the stripe geometry lasers, and I got caught by a cabal of people who said, "We're going to get this guy out of there and we're going to do it ourselves, and get him out of there." So they told me, "You've done enough. You go work on something else now. We don't want you here anymore." Basically they threw me out. I was peeved—I mean, to say the least. I was worse than peeved. I really wanted to quit. It was about that time that I enrolled in a course at Fairleigh Dickinson University on management, and I got an MBA degree by going nights at Fairleigh Dickinson. I thought, "I have enough stuff, I could start a company. They push me around like this; I could have my own company." So I got an MBA degree from Fairleigh. It wasn't very hard. It was sort of fun in a way. But then when I was asked to go work on something else, they said, "Well, go work with Jim DeLorenzo on the question of making these high-speed transistors." Okay, so I went. The guys that stole the project from me did a terrible job, which should have made me happy. But it didn't make me happy; it made me sad. They messed it up so badly. The devices that they made, the lasers that they made, were expected to have a high life, a high reliability. The company wanted to put those devices into the submarine cable and have a fiber optic submarine cable. Everything was going great guns. They had the fiber. They needed the lasers, and they needed to have a long expected lifetime, like 20 years. So there was an effort to try and do that. They mostly worked on the wrong things, and there was a lot of time spent in arguing, and the project was to be transferred to the manufacturing facility, which was still run by Western Electric. But the guy in charge of the project, he decided to spend his time fighting and quarreling and arguing. Well, working with Western Electric, as I later found, is a skill all of its own. Trying to transfer technology from one place to another requires a lot of diplomacy. So the diplomacy wasn't there. The project got stuck, and they didn't get the devices made. Instead, all the devices were bought from Fujitsu. And the guy, who was the key guy, was asked to go work at some other company. Well, I thought, "Well, okay. You messed it up, and the labs lost an opportunity of doing the job." So I went to work for making this high-speed transistor, and it was a MOSFET that had the epitaxial layer in it, and it had to carry a lot of power, the equivalent—I think it was a couple of watts or something like that. I forget the frequency. It was something like a gigahertz or something like that. The question was, how can we get them to go faster? So one of the people said to me, "Oh, the inductance in the source is one of the components that governs the speed. How about making a structure where you can reduce the path length from the source to the ground?" And I said, "Okay, let's connect it directly right through the chip from one side to the other." So I got the job, since I volunteered the idea of doing that, and I worked up a bunch of techniques, different plating techniques and etching techniques to do it, and eventually I got it done, and the device met the requirement. There were many other things that had to be done correctly as well, but that was one of them that got the speed up, but reducing the inductance. So then I was giving the job of transferring the technology. Talk about technology transfer. "Okay, you made it. Now you're going to transfer the technology to the manufacturing facility." All right, fine. So I had to go to Pennsylvania; there's a facility there. I had prepared a long description of all the process steps which had to be followed. One of the key process steps, which I described, was a technique called crystallographic cleaving. Crystallographic cleaving means that if you want to separate the chips from each other in gallium arsenide, you arrange to cleave the material—cleaving is breaking—and it breaks along crystallographic planes—1-1-0 planes. I worked with a manufacturer of diamond inscribing equipment to modify the equipment so that we could do a whole wafer and cleave it. We actually used that technique in making the lasers before I got booted out of that project. Then I said, "Well, this is a very good technique to use in the microwave devices," because there's no damage when you do sawing—talk about practical questions in manufacturing—when you do sawing and saw the chips with a diamond saw, there's a lot of damage. Gallium arsenide is a very fragile material, as compared with silicon. Silicon is much stronger mechanically. So when you saw the gallium arsenide, you get a lot of damage and it spreads out from the saw. That's called the kerf, the region where the saw cuts. If you do cleaving instead, you get no kerf at all—zero kerf—because it splits atomically. Also there's no damage because you have an atomically perfect edge if you do it by cleaving. So I tried to introduce that process, and they said, "No, no, no." They told me, "You can't make it that way. We never made anything like that," they told me at the Western Electric plant. I said, "Yeah, but you're getting a very bad yield." I said, "Look, you sat out with these chips, and sometimes you get 10 percent yield, sometimes you get zero percent yield. It's because you're damaging the chips by sawing them," I said. "No, no, no." They didn't want to do it the way I said. Finally—I guess it went on for a couple of months and they didn't get anything done—they said, "All right. We're going to let you work with a guy here in the factory and tell him how to do it." So I got a hold of a guy who did it the way I said to do it, and they made some devices that way, and it got about 95 percent yield. "Oh, great. Well, the guy from Western Electric solved the problem, so you go back to Bell Labs." I said, "Harvey [last name??], come on. Wait a minute. He didn't solve the problem; I told him what to do." Talk about diplomacy, huh? You got to be patient. There's a higher judgment up above. I worked with the people there in factory on the procedures and the process steps, and I introduced some new technology for fast processing. For example, if you did this cleaving that I mentioned, you could arrange it so that the devices were all arranged neatly on a plastic tape. I don’t know, like if you had a big piece of Scotch tape, and you had these semiconductor devices all sticking to it, there’s some special tape which is made just for that purpose, which is somewhat sticky but not all that very sticky. You can put your wafer on that and then do the cleaving. It’s on the tape. Then you take it off that tape and you can have an equivalent which will take the devices off the tape and allow them to be moved with the aid of a little vacuum needle. Well, I got some money, and now I had some diplomatic leverage because I had already showed them how to make things work. So I hired a company which was good at manufacturing equipment, and they made a device—a manufacturing tool—according to my needs, which would push the device off the tape and apply a vacuum, which was a needle with a flat bottom—a hollow needle—and pick up the chip, and move it, and put it down again wherever you wanted them to put it down. I wanted to put them down in a package. So we made that transfer tool, and it worked fine, and we could pick up the chips and put them all in packaging according to what kind of chips they were or where they came from in the wafer, or if they looked good or they looked bad. So that went along fine. There were other critical steps also in making this high-speed transistor, which I was transferring the technology from Bell Laboratories into the manufacturing environment. I found that in the manufacturing environment, the Western Electric Company hired the lowest cost labor that they could find which would meet the minimum specifications for their knowledge. These were all women. And they were very dedicated. They tried really hard to do their job properly. There were some critical steps that had to be done in a hood—a hood is a vacuum arrangement where you can work in there with chemicals—and the step required the use of hydrogen peroxide and a little bit of HCl to adjust the pH in the hydrogen chloride, and it was to be sprayed. Well, it had to be done in a hood because you didn’t want the people spraying this. I got a story back from the Western Electric Company that said, “You can’t spray these dangerous chemicals. Those are terrible. It’s going to kill the operators.” I said, “Look, the pH is 7, the same as water. This stuff is less hazardous to you than Coca-Cola.” I finally got a chemist who worked there. I said, “Look, what do you think of this material?” He said, “You’re right. It’s totally innocuous.” So he wrote a little letter explaining that this material was innocuous and wouldn’t hurt anybody. You could put it on your hands; it wouldn’t do anything. I had all these interesting experiences that I had in the manufacturing environment. I got so I sort of liked the people. They would use a few words that I thought were pretty unusual for a woman to use. It was fun. I made it a rule to go in the morning and come back in the evening, and I never would stay overnight, because I knew that they wanted me to transfer my permanent place of employment to Pennsylvania and work there, and I didn’t want to do that. So I would never stay overnight. Drive back and forth every day that I was there. It turned out to be a good decision. I stayed at Murray Hill. So there were other things I did. Actually, I wanted to mention before I worked on the high-speed microwave transistor, I did something when I was still working on the laser, which I didn’t mention before. We were trying to get the lasers to operate with as much power as possible, and I became aware that the conductivity of the substrate where we were mounting the lasers was a limiting factor, because the laser the would heat up and the thermoconductivity of the material underneath it would limit the amount of heat that you could extract. So I discovered that the thermoconductivity of diamond is higher than any other substance that you can get. I said to one of the guys that was working with me, who’s name, coincidentally, was Jack Diamond, “Look, let’s get some diamond and try this.” So we went down on Route 22—if you know where Route 22 is; it’s a very busy commercial route—and there’s a company which was an importer of diamonds for industrial use. We went in and we talked to the guy who was kind of the owner of this facility. We said, “Look, we’re from Bell Labs. We’d like to get some diamonds which have a flat surface.” He said, “Oh yeah. I can supply you with those.” They’re naturally growing diamonds, which have a flat surface. If they grow in a certain crystallographic direction, they’re going to have a flat surface. Later, I got them especially polished for my purposes, but the first ones we got were that way—naturally occurring diamonds. So we tried mounting some lasers on those, and they did run at a higher power level. We got some that were specially fabricated for us. It’s not easy in diamonds, you know. It’s a very tough material to fabricate. I got some that were specially made for us. I think they made them in Holland. We metalized them and mounted the lasers on those, and we were able to mount them in a very nice, convenient way, and that became a standard method for heat-sinking of semiconductor lasers and any other devices that had to have a good heat sink. I got a little award for that, and I got my nickel for the Staten Island Ferry. So that was a fun thing to do. I published a paper on that. It was the first use of diamond in a semiconductor device. The people at DeBeers were very grateful. They wanted me to come and visit their laboratory in South Africa. I think they would have paid my way. I didn't think it would be worthwhile to do it.

Brock: In the Bell Labs work on semiconductor lasers, was this in the general framework of sort of the optimal transmission of data and voice information?

D'Asaro: Well, as you know, with fiber optics—if you're talking about using a semiconductor laser with fiber optics—the bandwidth is enormous. It's a huge bandwidth, compared to, let's say, an ordinary telephone line, like a twisted pair telephone line. On a telephone, it's hard to high-fidelity transmitted over the telephone. With fiber optics, you got probably 100,000 channels of high-quality transmission available to you in one fiber. If you have another laser, so that you have another wavelength, then you get the same thing again, and you get put ten devices on the same thing. It becomes astronomical, the channels you can get. That's why you can call China, and it probably costs you five cents or something nowadays. I mean, you could get a bulk rate to call anywhere in the world for a monthly fee, which is quite modest. So all that came about because of the advent of the fiber optics. Once you have this, you can have enormous bandwidth. So devices that could be used for that are very desirable. We knew all that, of course, but the Western Electric Company was very slow, plodding, ineffective, and they never did develop that market. They could have developed the market for selling semiconductor lasers—they became very widely used—but they kind of decided not to pursue it. I don't know why. They could have also done another thing, which we did when I was working on this high-speed microwave transistor that I mentioned before. We became aware that by using these techniques of the grounding of the source through the wafer I mentioned before, you can make a very high-quality amplifier for low power, for a field-effect transistor, which would be used as the input to a microwave amplifier. I had talked to Huck Fakui [ph?], who had made the measurements. I said, "Look, don't you think that the Western Electric Company could manufacture this and sell it as a high-quality microwave amplifier? It's a very low-noise—high signal to noise ratio." "Oh yes," he said, "that would work great." I tried to talk to various people about it, and they never did it. Nowadays, you have satellite communications; they're all using a gallium arsenide field-effect transistor as their input stage. It's a very low-noise and high-gain device and can be run at high frequencies. So it's widely used.

Brock: I'm just wondering when the whole vision of optical communications arose. Was that after the appearance of the laser, or did the concept of fiber optics and optical communications, did that predate the laser? When did that emerge?

D'Asaro: Yeah, the fiber technology predated the laser. But at the time that it was first discussed in a publication that I read, the devices that could utilize the bandwidth were not available. So later things like that—field-effect transistor that I mentioned and the lasers, the single mode lasers—later they became available. So then it took off. But a lot of work was done at Bell Labs. They had a very experienced, knowledgeable group of people at Bell Laboratories making fiber optics, and it was a very hot field. There's a funny story connected with it, because there was an optimum wavelength associated with the fiber, and if you wanted to get the best possible transmission—that's the lowest loss transmission through the fiber—you had to work at that wavelength. I think it was 106 or something like that—microns. But it was not a very convenient wavelength. It would have been better if it had been closer to the gallium arsenide luminescent wavelength, around 980 nanometers. Well, one of the people in this group suddenly realized what needed to be done. They had already started to manufacture this fiber in a big fiber manufacturing facility in Georgia. They had a big factory making fiber. So there was already a market for it. But it would have been a better market if the bandwidth in the fiber had been wider. So this guy realized that what they were doing is they were pulling the fiber—you know you can pull glass, go into a very thin strand—but they do this in a high-speed manner, and pulling the fiber very fast. But they were heating the fiber with a hydrogen flame in order to heat it up to the temperature at which it would become soft enough to pull. There was water vapor being created in this hydrogen flame, and the water was being incorporated in the fiber, and that was responsible for the extreme absorption. So once they got rid of that source of water, then the bandwidth became wider.

Brock: So the sort of semiconductor/laser devices that you were working on, the idea was to use them for optical communications?

D’Asaro: Yes.

Brock: And does that imply sort of a digital mode of transmission, if it’s optical?

D’Asaro: Well, yeah, because you’re always impressing a modulation on the light, and the modulation is digital.

Brock: So it seemed like—and what I was looking at in this area—this is also the beginning of the ideas for optical interconnections for devices. Does that emerge around this time?

D’Asaro: Oh, absolutely. It does. And in fact, I worked on that, interestingly enough. After we finished up this project I was telling you about, about the high-speed microwave transistors, the Western Electric company and the AT&T company said, “Well, now we’ve made 10,000 of these transistors and we’ve replaced all of the traveling wave tubes, and we don’t need any more transistors, so you guys can work on something else now.” So we didn’t have a job. So there had to be some other project that we worked on, and there was a lot of thrashing around. The thrashing around produced very little in the way of useful ideas. There was some interest in making monolithic microwave integrated circuits, which means that you make a microwave amplifier and then you put some other components on the same chip in order to do some data processing on the same chip. But I don’t think that was ever really done, so I mean, I wasn’t involved anymore in it, and I went off to do other things. The next thing that I did was that a group had been formed to look at the question of whether the central office in the telephone system could be replaced or modified with an optically interconnected system. You mentioned the possibility for optical interconnects. An optical interconnect would mean something like what I said before about the opto-isolator, where you have a light source and you have a photo detector. But if you were going to put this in a central office, you’d have to have many, many channels—like thousands of channels that would be switching in the central office—and you might do that with an optically interconnected system. The reason that you might want to do it with an optically interconnected system is that you can put the channels physically very close to each other in an optically interconnected system. Whereas if you want to do the equivalent switching, either in the traditional way with relays or in another way with silicon microelectronics, there’s mode space used up than there is if you make channels of optical communication. We went through various schemes and wound up—we tried to do this. We could demonstrate that it would work, but it really wasn’t satisfactory, what we had made, which used light-emitting diode arrays and photodetector arrays for this integration and switching. Then we decided to make pixel arrays.

Brock: Maybe we should pause and switch the tape before we get into it. Pardon me. We’re just running to the end of this tape. Thanks.

END OF TAPE 4

START OF TAPE 5

Brock: We were talking about optical interconnection schemes for central offices and various approaches that you were working on. Is this the late 1980s or early—

D'Asaro: Yeah, that was the last project actually I was involved in was that project doing optical interconnections in a central office. It seemed rather fantastic at first but we could write a good story and make a good story out of it and it sounded sort of impressive and a lot of people were impressed. So we were adequately funded and we had quite a lot of people involved in doing this, and we started out as I said with light-emitting diode arrays and arrays of photodiodes that- but you might say that it would not be obvious to the casual observer how the switching was really done. And it was actually— Actually, what was done was that all of the photodiodes were illuminated but one of those would be selected by some circuitry, which was silicon integrated circuitry, to select one channel. So then you'd say, "Well, if you're using silicon why do you want to bother with the optics anyway?" Yeah. And ultimately that skeptical point of view turned out to be correct, and that whole idea of optical interconnection turned out to be a useless endeavor, but we worked on it pretty hard for several years and we went- we progressed from making- using light-emitting diode arrays to using VCSEL arrays, and the VCSEL arrays were great fun. VCSEL, if you don't know what that means, it means a vertical-cavity surface-emitting laser. In the usual kind of laser the light is emitted from a cleaved edge parallel to the surface of the device so it's like sort of a rectangle of material- block of material and light comes out one edge of it parallel to the top surface. And it comes out in a region which is defined by the stripe that I talked about so that device has inherent problems because mounting it on the diamond heat sink that I mentioned requires a lot of skill and accuracy of the operator to do it by hand. And it's extremely difficult to automate it so it never has been properly automated. Usually, the devices are assembled by hand. I always considered that to be kind of an antiquated way of doing things, and when I became aware that it would be possible to make a laser- a semiconductor laser where the light would come out perpendicular to the surface instead of parallel to it then I was really enthusiastic about doing that. Making this kind of a device is really hard. It's far from trivial. You have to have mirrors on both the top and the bottom surface. The mirrors have to be low loss. They're made out of gallium arsenide and had aluminum gallium arsenide and it has to be done in a very accurate manner, and then there's a P-N junction in the middle which emits the light and a region of active light emission has to be defined by something. In some way or other it has to be defined, maybe by etching or by diffusion or implantation or some procedure. So we [who is “we”?] decided we would make pixels and we would make not just one pixel; we would make ten thousand or a hundred thousand at a same- in the same process steps. So we worked on that and eventually we did make those, and you got very beautiful arrays of light spots each of which is a laser and each of which has a well-defined mode. And the processing was straightforward after you learned how to do it but it had to be done right, and getting the MBE, molecular beam epitaxy, done correctly was difficult for the crystal grower. Everything was in the crystal grower's hands. He had to grow a lot of layers of gallium arsenide- aluminum gallium arsenide with the correct doping. He had to grow another region which had a p-n junction with the correct doping. He had to grow another mirror on the- and then again with all the correct doping. It was a big stack of epitaxy. I don't know how many layers. Maybe a hundred layers or something well of epitaxy, but- and then it had to be etched or fabricated and all of that was done and VCSEL arrays were made, and then because of the fact that they had a lot more power in the light than the light-emitting diodes had, a thousand times as much power, then the photodiode could switch much faster because this rate of switching is determined by the rate at which carriers are inserted into the photodiode. If you can insert them at a higher power level, they will switch faster so we got it to work at a—Well, we got it to work at a few hundred megabits or something like that. We demonstrated that, and then one day I met somebody in the hall at Bell Laboratories where every important thing always happens, in the hall, and he said, "Did you know that we're going to cancel this project, that it's been decided that optical switching in the central office is useless?" And I said, "No, I hadn't heard that yet" but I soon did hear it, yes, and in fact it was useless, and with improvements in silicon photolithography they could actually do a better job with just silicon and forget the optics. So that whole idea, although we published ever so many interesting papers on this subject and did a lot of work making the arrays, it didn't really lead to any great practical utilization. Well, that's the way life is. You win some; you lose some. Right? And one fine day somebody came in and said, "Hey, you know Bell Laboratories is contracting. We don't have enough money. We don't- can't keep all the people we have so we're having a lot of layoffs" and I got laid off in 1961, 19—What did I say? Sixty-one?

Brock: Yeah.

D'Asaro: Sixty-one, yeah.

Brock: No. It must have been in the nineties.

D'Asaro: I'm sorry. 1991. I beg your pardon. I'm sorry. 1991, yeah. Then I thought well, gosh, this is a bad spot I'm in, and so I said, "Well, look. Can I stay on as a consultant?" and my department said, "Okay. Fine. You can stay on as a consultant." So I stayed on as a consultant and continued to work on the same thing for another six years or something like that, five years or so at Bell Laboratories, but it was a dying subject. We made more VCSEL arrays and tried to improve it but it was a dying subject, and eventually everybody in my organization was laid off, my laboratory director, my department head, all of the MTS, and I was left like Ahab was in the story of the great white whale. I was left to tell the tale. I still had a pass. I could come in and use the library so my friend, Leo Chirovsky, said, "Look. I've been approached by a company in Princeton called Princeton Optronics to take a job there and I've got another job so why don't you take the job?" So I went down to Princeton Optronics and said, "Look. I'd like to have a job here," and they said, "Fine. You're hired." So I went to work there, and what do you know? We were making exactly the same thing, VCSEL arrays, exactly the same thing, and we also made high-power VCSELs for trying to maximize the amount of power and got some very good people to work with. And we did a really good job and we made some high-power devices and single mode devices and some large arrays and they're for sale today and—

Brock: For what are they used?

D'Asaro: It's a hard question for me to answer because the costs of manufacturing them are borne by the Department of Defense and they have visions of high-power lasers connected with weapons, and I am unable to tell you exactly how they would be used in weapons. One way they might be used is as illuminators at night. As you may know, the United States Army has a system involving a infrared photo receiver which will give an image in infrared at night, and with an illuminator which is also in the infrared then the person who is being observed doesn't know he's being observed because he can't see the light; it's in the infrared. And they can be used in that kind of an application. That's been a very successful thing in Iraq. It got so the Iraq dissidents wouldn't fight at night. They wouldn't fight anymore because they always got picked off by sharpshooters who could see them but they couldn't see who was shooting them. They work pretty well so they'll probably be used for that but they don't want to tell us much about what they use them for but they do support the effort 'cause there's quite a bit of effort at Princeton Optronics now to develop other kinds of customers, and there is an application to printing. In your Xerox copier there is a light source which illuminates the paper and puts a pattern of charge on the paper and then that charge is used to pick up the toner- what's called the toner in the Xerox copier which adheres to the region which is charged and doesn't adhere to other regions. Astonishingly enough, this works quite well and the Xerox copier is a great success, and they would like better and more powerful lasers to be used in their copiers. That's one application but there are others so it's- I sort of enjoyed working there, but one day they came and they said, "Look. The board has found out—" The board—They're a bunch of idiots. "The board has found out that you're 82 years old and we think you're too old to work here. There's nothing wrong with your work. Your work is fine. We just think you're too old; that's all. We don't want to find you dead in here. It'd be very inconvenient." So I was laid off. That was only been less than a year ago so this gave me an opportunity to—-there's always a plus side—to go to the conference on the 50th anniversary of the integrated circuit, and not having to get permission from anybody—I'm sure it's certainly a great conference.

Brock: I have a few final questions about compound semiconductors and Bell Labs and then we have a series of sort of standard closing questions that we ask so if it's okay I'll just maybe ask these last questions about—Okay. Great. My first question is kind of very specific, which was going back early in our conversation where you're talking about the importance of these deposition techniques I think we called them or growth techniques and we had talked about the liquid phase epitaxy and molecular beam epitaxy. I was wondering. Was there a technical materials reason that one couldn't do the same sort of gas phase epitaxy or chemical vapor deposition type of epitaxy that was going on for silicon and do that with the compound semiconductors?

D'Asaro: I don't think I understand your question.

Brock: I don't understand why you didn't just do the same sort of epitaxy that was going on for silicon in 1960, why you didn't do that same sort of processing for compound semiconductors.

D'Asaro: Are you talking about liquid phase epitaxy?

Brock: No. I'm talking about how you would do silicon epitaxy, which would be to use gases and have basically chemical vapor deposition—

D'Asaro: Well, the chemical vapor deposition is always done on oxidized layers. It's never done on pure silicon. I think it's not clean enough to do for that purpose. The dominant epitaxial technology is molecular beam epitaxy.

Brock: But for silicon it's certainly not molecular beam epitaxy and it certainly wasn't in 1960 kind of using silane gases and things like that to do—

D'Asaro: Well, you're getting mixed up between the deposition of the oxides and the deposition of the layers in the semiconductor device. The oxides are put down by plasma deposition and it's a very effective way to do it. The layers of the semiconductor material itself are done by molecular beam epitaxy typically.

Brock: Right. Right around 1960 people at Bell Labs like Henry Theurer, etc., come up with a means of doing silicon epitaxy, of growing these doped layers of silicon on top of intrinsic silicon.

D'Asaro: Yeah. They were evaporative techniques.

Brock: They were—

D'Asaro: and now—They were evaporative techniques and now it goes to molecular beam epitaxy.

Brock: They were.

D'Asaro: The initial efforts weren't called molecular. That name was created—and I think it's a very bad name—was created to describe the system using an ultra-high vacuum and evaporation in that ultra-high vacuum to grow the silicon. The advantage of it is that it gives a very clean silicon epitaxial layer free of impurities and other imperfections like dislocations. They are minimized so that's the way most epitaxy is done whether it's in gallium arsenide or whether it's in silicon. It's—I'm not aware that there is a practical gaseous technique, plasma technique, for growing silicon epitaxial layers. I don't know of one.

Brock: I thought it was the decomposition of a gas was the initial way that silicon epitaxy happened.

D'Asaro: No, I don't think so. I think that's the way that the oxides are deposited or the nitrides are deposited.

Brock: I'll have to look into that more. It seems that over the time that you were working with gallium arsenide or compound semiconductors the real two big applications for those became wireless communications and optical communications. Is that a fair characterization?

D'Asaro: Well, wireless—And I—I don't know if you mean cellular when you say wireless or whether you mean the kind of application we have where you can pick up the telephone and walk around the house with it but it doesn't go any further than that, inside your house. That's what's usually called wireless as far as I know.

Brock: Cellular telephony.

D'Asaro: Cellular is different. Huh?

Brock: I meant cellular telephony.

D'Asaro: When you said—Oh, when- you mean when that's cellular. Cellular is a totally different animal as you know. It—The name cellular means that you divide up the countryside into cells each of which has a communication to your cell phone and when you go from one cell to the other it automatically switches so that the limitations that there are on the distance that the high frequency will carry are not violated because you go from one cell to another automatically. Gallium arsenide has become the dominant technology for making the microwave circuitry in cell phones. It cannot be used in an analogous way in computers because the- as I said before the RC continuity [ph?] of the connections inside the chip are the governing factor for the speed so gallium arsenide doesn't offer any advantage over silicon, but in a cell phone the signals are carried really by analog signals and there is a high-speed high frequency being amplified, and the gallium arsenide can do that just fine and it doesn't need to be switched. It's just change—It's just an amplifier at those high frequencies and it's inexpensive and very reliable and so gallium arsenide has that field.

Brock: Would you say that molecular beam epitaxy was really the key technology or the key development for all of this sort of compound semiconductor work?

D'Asaro: Oh, yes, definitely. It was the advent as you said of molecular beam epitaxy which made it possible to make very complex devices with multiple layers, and some of the devices are astoundingly complex that can be made out of that molecular beam epitaxy, thousands of layers backed up on each other. So this is all possible because of the very accurate reproducible production of layers, and you can automatically- you can program the molecular beam epitaxy machine to grow automatically. The operator doesn't have to do it. The— All of the switching of the layers between one layer and another is done-controlled by computer programs. That's lovely.

Brock: To switch gears then and to ask you some sort of reflections on Bell Labs, you spent 41 years there and really it seems were there when it went through its absolute zenith to frankly its decline at Bell Labs in the later part of your stay there. I was just wondering what thoughts you have about the evolution of Bell Labs during your time there, the leadership, key decisions, the impact of the AT&T breakup if you could talk about the sort of story—

D'Asaro: As you said, the AT&T breakup had a very critical role to play in the future of Bell Laboratories, and I think when those discussions were being held in the Congress of the United States the idea that Bell Laboratories was a very important national resource was never mentioned. And the breakup was something that I believe—it's my opinion—that it was engineered by a bunch of brokers or investment managers who profited immensely from the breakup. You take a big thing and make it into a bunch of little things you get a big commission for all of this breaking. I think that had a strong role to play in this breakup. Unfortunately, nobody cared what happened with Bell Laboratories so Bell Laboratories tried to survive on the money that came from the manufacturing but the manufacturing was so inefficient and so ineffectual that they didn't get any profit from it. And they had to—The company had to sell itself to another French company, Alcatel, to be able to survive, which is what it is today, and it's surviving at a rather low level as you may- as you probably know so it's tragic. I think it's very sad that the laboratory's almost disappeared. It's still there but the level is low. There is very little device work done. I know of one person there that's doing device work now compared to thousands who were doing it when I was there, and there's no manufacturing at all now so it's at a very low level. And when I look in technical publications there's nothing really from Bell Laboratories. When you look at the technical publications in the fields that I'm familiar with they are done at universities or in other countries. Lots and lots are done in Taiwan and Korea and in mainland China and in India and in Europe too, and the majority of the names that you see when you look at the names on the publications they're mostly Asian names. So it's sad that the United States and its educational procedures and policies have been not- been- has not been able to inspire young people to take up a career in science or technology. I guess the appeal that they can make more money doing something else like being on Wall Street, but I'm not sure that's very convincing anymore so the Asians on the other hand as you know are very hard working. They believe their work is its own reward and they are pretty well educated. Some of them are innovative. Most of them are less innovative, but I got to know quite a few Asians. There were a number of Chinese when I was at Princeton Optronics. Usually they were hard working but not innovative. I think that's a characteristic of Asians in my opinion. So as you said I was resident at Bell Laboratories as it went through its best years. I think the influence of Jack Morton was fatal. He really ruined it. Everything he touched turned to mud or worse. He pushed resources on to a technology which involved magnetic bubbles, which is kind of cute but led no place, a tremendous amount of effort put on technology which didn't go anywhere and was meaningless, and he refused to participate in the leading technology, which was silicon integrated circuits. So he was murdered as you probably know or you don't know. He was a closet alcoholic. One night he stopped at a bar in Myersville late at night. The story that I got which I believe is the correct one: There were two people in the bar, two- a black guy and a white guy. They were unemployed automobile mechanics. Jack Morton sat down and had something to drink. It was very late. The bar was going to close in a short time. The—Jack Morton started explaining to these guys why black people are not doing well in the United States. It's because they're lazy; they don't apply themselves. The guys were friends and they didn't particularly like his attitude. He could have gotten away with that kind of stuff at Bell Labs because he was the boss, but in that situation it didn't work very well and they told him, "Well, we're going out in the parking lot. Would you like to come out there and talk to us?" So he said yes. He thought he could fight with these two guys. They picked up an iron bar which is used in changing tires. I think it's called a tire iron. They hit him so hard he died right there, died on the spot, and they threw him in his vehicle which was a small sports vehicle. They threw gasoline on him and burned and then they stood around and watched until the police came. They—I can hardly believe that it's possible that somebody could be carried to such a state of fury that they would commit a crime and just stand around and watch. Naturally, they were convicted and put into prison. The official story from Bell Labs is a little different than that but I think my version is the right one, sad, very sad. I guess that's the point at which Bell Laboratories went down instead of up.

Brock: Would you say the key then though was the breakup of AT&T?

D'Asaro: Oh, yes. The breakup of AT&T meant that their source of income was vastly reduced and there was no way they could get it back. When AT&T was in charge of all of the communications in the United States there was a very small amount of money, like a tenth of one percent, spent on Bell Laboratories, which was actually a huge amount of money, maybe in today's values—I don't know—maybe $500 million a year or something like that. They hired the most intelligent people in the world. Whoever was the brightest was who they had there, and some people that were said not to be good enough went out and became leaders in their field at various other institutions so even their rejects were wonderful, wonderfully productive people, and so it was fatal, the breakup.

Brock: Gardner, unless you have some questions I thought I would move on to the closing questions or if there's something that you wanted to touch on that we haven't touched on yet.

D'Asaro: Well, everybody that I know who has been at Bell Labs, and I know quite a lot, have such a sense of sadness about it.

Brock: About what's happened to it in the recent past.

D'Asaro: It's hard for me to—

Brock: Sure.

D'Asaro: —even talk about it it's so bad.

Gardner: Should we ask Art to tell that story about the Russian—

Brock: Oh, yeah, that- we didn't catch that on tape fully where earlier off camera we had a discussion about the Soviet scientist who had come up with the room temperature continuously operating semiconductor laser. Can we maybe review that story again—

D'Asaro: Well, yes. This guy whose name was Zhores Alferov had done in Russia at the Ioffe Institute together with some other people there had made the first semiconductor laser which ran at room temperature. That had been something I'd been trying to do for a long time, and I really didn't quite have the right approach but he did, and he made a heterostructure. That's a structure in which aluminum gallium arsenide and gallium arsenide were used in combination to make layers which confined the minority carriers right around the p-n junction, prevented them from diffusing away and that way they recombined more effectively. This structure was made by liquid phase epitaxy and he arranged that a sample which was being grown would be exposed to several cycles of heating and cooling while it was immersed in a couple of different solutions all of which are molten. It was a very clever idea and technology that he had and he had made semiconductor lasers which ran continuously at room temperature and he got a Russian patent for them, and about that same time he came to Bell Laboratories to visit. Of course, he knew what I had done and also another guy named Mort Panish had been working in this area, but we didn't have the semiconductor laser at room temperature. We had it at other temperatures but not that high a temperature so he was very polite and came and saw what we were doing and we showed him, and then we decided to take him to dinner and he said, "Fine." So we went to dinner at a nearby restaurant and we were enjoying each other's company and Mort Panish said, "Well, if you show us how you made this device, how did you make the room temperature laser," he said, "Oh, yes. Fine." So he took a napkin and drew a picture of how the liquid phase epitaxy was done and that was key to how to make it so Morton naturally took the napkin and saved it, and I felt that it was very wonderful of him to do that because he knew that we would go ahead and make them but he was fully conscious of that and did not try to hide what he had done or make it secret. You might wonder why the Russian government let him come. He was—That was at the time when Stalin was in power and to let a Russian scientist come to the United States was quite amazing, but he did and he must have been a very good diplomat to get permission. Well, he stayed overnight at my house a couple of nights in fact and gave me a gift of a- made with—It's a typically Russian picture of a birch tree made in wood inlay, quite nice, quite pretty, so he was certainly a wonderful guy, still is in fact a wonderful guy. Mort Panish followed his directions and made the device and it worked fine. The ones that were shown to the later press conference were made—I made them by putting them on the diamond heat sinks that I mentioned before and they ran for a long time whereas the ones that Mort made died very soon after he managed to get a spectrum of them, but the ones that I put on the diamond lasted a long time and were demonstrated to the various press people who came to hear a talk on what we had done. I have a great deal of respect for Zhores Alferov. He became President of the Russian Academy of Sciences and President of the Ioffe Institute, the most prestigious university in Russia, and he's still there today.

Brock: Great. Maybe we'll move on to these sort of closing reflective questions the first one of which asks what was the most exciting, rewarding, and satisfying period or project in your career?

D'Asaro: It's a hard question to answer because there were so many times when I felt really a great deal of satisfaction. I did feel a great deal of satisfaction when we made the stepping transistor operate correctly, and of course I felt a sense of regret that I hadn't been able to pursue it further to try to get the- what they would call the plainer type of structure to work. I was very happy when I made the first avalanche photodiode as I mentioned to you before. I think that was a great achievement. It was also lovely and marvelous that Dawon Kahng and I made the first high-speed mixer diodes or photo detector diodes which ran at microwave speeds using gallium arsenide, that they were so widely accepted. I felt very pleased when I made the first opto-isolator despite the fact that nobody remembers it anymore, and so those were some of the things that I felt very satisfied with. Some of the projects didn't go anywhere such as the optical arrays that didn't lead to anything, but it was certainly a lot of fun and later they- pixel arrays became a product for Princeton Optronics so I guess you could say it was successful in that sense.

Brock: Despite the fact that you spent the vast majority of your career within Bell Labs, is there a way that you consider yourself an entrepreneur of sorts or maybe an intrapreneur would be the better term? It seems like you were extremely sort of self-directed in the projects you wanted to—

D'Asaro: Yes. Well, I was as you said self-directed quite a lot but one has to be cautious as I learned over the years and being self-directed in such a way that your collaborator feels happy and doesn't feel imposed upon, which was sometimes a hard lesson for me to learn. Yes, I still consider myself an entrepreneur. God in some way has put me here and why or how is mysterious, but I think I've left the world a better place and I think all the juice has not gone out of me by any means. I can see my path to making better things. I tend to think in—I tend to think of the highest goal instead of the minimum goal. I try to—I like to think of what the most wonderful thing is that I could do. Right now I think the most wonderful thing I could do is to provide fusion energy. We got involved at Princeton Optronics with a project at Lawrence Livermore Laboratory who were making- who were doing experiments on fusion using pulsed lasers and concentrating the light from a hundred or maybe more lasers of the highest power they could get onto a tiny, little speck maybe ten microns in diameter of deuterium-tritium gas and saying, "Oh, yes, we can get this thing to show fusion. Well, so what?" I say, "There's nothing useful about it. You can't plug it in to your electrical system by any means and there's no way that it's going to lead to anything plugging it in. You've got to have a practical way of making power. I think I can do that," but as you know how it is with time, time flits away, doesn't it? And I defy time. My wife is a nutritionist. I'm pretty healthy at my age and I can do a lot at my age so I'd be willing to be an entrepreneur on fusion energy. I think there are ways to do it which have not been explored in my opinion. I started out in nuclear physics at Cornell and so I'm not afraid of nuclear physics although I decided I didn't want to do it as a career. It was a good thing I didn't because it really was not a good career at all. People who started in that field weren't able to get jobs and I did get a great job so that's what I see.

END OF TAPE 5

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D’Asaro: And if he knew what my son has accomplished, my son, whose name is Eric, is a physical oceanographer, he lives in Seattle, Washington, he does very imaginative work on measurement of the temperatures, solidities, dividites and currents in the ocean and has made astoundingly complex devices, which are put in the ocean and radio their results continuously to satellites so he can sit in his office and monitor his equipment. He can make hundreds of these things. So he’s doing wonderful work. His son, my grandson, is extremely talented in science and engineering, beyond belief he’s talented. So there’s no electrical problem that he can’t solve. I mean, it’s just beyond belief; it’s amazing what he does. So, for a summer job, he’s employed at a company which manufacturers many of the devices, which my son uses in his oceanographic equipment. So, this is not an accident this cozy relationship came about because my son Eric said, “Look,” my grandson, who’s name is Matthew, “he needs a summer job” and so, he got a job, you know with the company in Seattle doing that. This is his last year in college at the University of Washington and he’ll be looking for another place to go to graduate school. I’m really overjoyed to have these kids.

Brock: What advice would you give to young people about their potential to make a contribution to innovation?

D’Asaro: Well first of all, they have to want to, you know. There’s no point in trying to impose your opinions on people unless they really want to. We had a neighbor, a couple of years ago, who lived across the street and he had two sons. We got to know them quite well, his older son I believe he was Phi Beta —not Phi Beta Kappa, what do you call it, when the person graduates from high school?

Brock: Valedictorian?

D’Asaro: Valedictorian of his class and has gone off to college somewhere, I don’t know where. But, you know, a guy like that, if he’s inspired to do something in technology can make a difference for sure. But a person of medium abilities may be discouraged because there’s so much competition for jobs in technology now, so you may not be the smartest guy and you’ll probably have trouble getting a job. Unfortunately, that’s the world we live in. I lived through the depression in the 1930’s and nobody could get a job then, it was unusual to find any kind of a job at all. And it seems to me that people in college are looking forward to that same kind of future. Where’s the job? What did I —why did I go to school and study all this and now what am I going to do with my life? And after that, I have no answer. It’s really too bad. Maybe we’ve got too much in the way of education, you know, maybe we should be thinking less about education and more about other questions.

Brock: Why do you think that preserving history is important?

D’Asaro: Well, the history of museum for the computer is a wonderful thing because it put together in one place all of the major computer achievements and it is wonderful to see these and to go through the museum and to have a guide to tell you what they all are like the Cray computer, for example. Cray is such an interesting story to tell about what he did. I’d love to bring my grandson there and hear what he has to say about all these things, because he always has something astonishing to say about something technical. So, there’s no other place like this computer history museum, it’s unique. I’m sure that they’re planning to do stuff on the integrated circuit as well as on the computers, probably that will be part of their program as they go on. I was very flattered that one of the historians said, well he wanted a copy of my notebook showing the pages in which I recorded the first operation of this stepping transistor. So he said, “Well, maybe we’ll put that in the display case along with Jack Kilby’s,” and others that they have, you know, special place to, so I’m really delighted that people have come to that point that they think it’s important. They think it’s important. Of course, I think it’s important.

Brock: Those are all the questions that I have, unless there’s any additional thing that —

D’Asaro: Thank you so much for giving me this opportunity to talk. It’s been very fine indeed. Thank you.

Brock: Well, we really appreciate it.

D’Asaro: Thank you.

END OF INTERVIEW