# Oral History of Ian Ross

Interviewed by:
Lewis Terman

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**Lewis Terman:** Good day. It is August 19, 2009. I am here with Ian Ross, President Emeritus of the Bell Laboratories. He joined Bell Labs in 1952 becoming involved in the development of a wide variety of semiconductor devices. In 1959, he became Director of the semiconductor laboratory at Murray Hill, New Jersey, and three years later was named Director of the semiconductor device and electron tube laboratory in Allentown, Pennsylvania. In 1964, he was appointed Managing Director of Bellcom, a Bell Systems subsidiary, and became President of Bellcom in 1968. In 1971 he returned to Bell Labs as Executive Director of the network planning division and was promoted to Bell Labs Vice President in 1973, Executive Vice President in 1976 and President in 1979. He retired in 1992. He was elected to the U.S. National Academy of Engineering in 1973, the U.S. National Academy of Sciences in 1982, the Engineering Academy of Japan in 1988 and the Royal Academy of Engineering in 1990. I am Lewis Terman. I joined IBM research division in 1961 and worked there in semiconductor devices, technology, circuits and design retiring in 2006. I was elected to the U.S. National Academy of Engineering in 1996 and served as IEEE President in 2008. Ian, let's start with some background. Where were you born?

**Ian Ross:** I was born in Southport, England, which is about 20 miles north of Liverpool which is famous for having given birth to The Beatles.

**Terman:** Okay. And did you grow up there or did you move around?

**Ross:** No, we spent some time in Manchester which was 30 miles away but mostly I was there until I went to Cambridge when I was 18.

**Terman:** What did your parents do?

**Ross:** My mother was a housewife. And my father was in food processing business, canned foods and dried foods. But they were divorced when I was very, very young and I didn't, in fact, see my father from the time I was about six or eight until I was about 20. So it was not a good relationship during those times.

**Terman:** How did you meet your wife?

**Ross:** My wife is from Helsinki. And she was visiting her sister who was married to a Bell Labs employee and we met at a Bell Labs social function. We were married in Helsinki. And she came over here in 1955 and we've been here ever since, of course.

**Terman:** So how did you keep the romance going between Bell Laboratories and Finland, or was she living here at the time?

**Ross:** How did we do what?

**Terman:** Keep the romance going as she was here? And did she go back to Finland?

  

**Ross:** She went back to Finland and made up her mind whether she wanted to marry me and then explained that to her parents which they took very well. But, of course, in those days, if you wanted to make a telephone call to Finland it was all radio telephony which was very poor quality and you had to make an appointment. So mainly this was done by writing letters. It was not a very long period of time, as I recall. We met in maybe about the spring of 1955. We were married in August.

**Terman:** Wow. So I guess not only have you advanced technology, but you moved very fast in other matters also. How many children do you have and what are they doing?

**Ross:** We have three children, a boy and two girls. My son is in the software development for customer premises switching equipment. My elder daughter is in a company that provides software to residential construction companies. And my younger daughter is a systems engineer for AT&T.

**Terman:** Okay. As you grew up, where did you develop your interest in science and technology?

**Ross:** It began in what you would call high school in the science courses. And yes, that was really when I began to get interested in technology and science.

**Terman:** Were there any especially influential teachers or other persons who got you interested in science and technology?

**Ross:** The principle of the school, the headmaster as he was called, George Milward was a graduate of Cambridge University, had a degree in mathematics. And one of his interests was to try and develop some of his students to be able to get into Cambridge and go back to his old college. And I was lucky enough that he took me under his wing and by the time I was 18, I knew more mathematics than I do now or I ever did since. So he was a great inspiration.

**Terman:** Okay. So you went to Cambridge and he was the influence as to why you went to Cambridge for undergraduate.

**Ross:** Absolutely.

**Terman:** And then for graduate school, where did you go?

**Ross:** I stayed on in Cambridge and bear in mind that that term graduate school wasn’t used there at that time. I spent three years as an undergraduate in what we’d now call engineering, though at that time Cambridge called it mechanical sciences. And that led to a Bachelor’s degree. At that time, I decided or I was allowed to stay on for my Ph.D. which was in exactly the same faculty, the same university and that took three years.

**Terman:** Who was your thesis advisor? And what was the topic of your thesis?

  

**Ross:** My thesis advisor was a delightful Irish person by the name of Jim Yates. And his task was originally to propose a subject for me to investigate. And he would advise me as we went along. Now, bear in mind that when you took a study - studied for a Ph.D. in Cambridge in those days, you spend three years. You had an experimental project. You built the equipment. You did all of the work necessary. At the appropriate time you wrote a thesis. There was no course work. There was no graduate school in a sense that is used in this country.

**Terman:** So how did you pick the topic? Or did you pick the topic? Or did Jim Yates pick the topic or how was that decided?

**Ross:** It was picked by the faculty at that time and most of the work that was going on with the graduate students was in what we would now call electronics. And they picked a subject for me. One of my colleagues was doing very exciting work on early electron microscopes. I wish I had had that project.

**Terman:** You might not have come to Bell Laboratories though.

**Ross:** Correct.

**Terman:** I think you said the undergraduate topic was mechanical sciences?

**Ross:** At that time, the engineering faculty was called mechanical sciences. I think it probably was part of Cambridge academic snobbery. And the unusual thing was that when you studied for a degree in mechanical sciences, you studied all aspects of engineering. It wasn't just electrical or mechanical or civil engineering, it was the whole works. And that was very different.

**Terman:** Yes. I assume, therefore, you did mechanical drawing and all of these other things that you really didn't apply to your career.

**Ross:** Yes.

**Terman:** Because I remember, I was in physics [as an] undergraduate. I remember even the double E people doing mechanical drawing courses. I said, boy am I glad I'm a physicist.

**Ross:** We did a lot of mechanical drawing. We even did welding.

**Terman:** Yes, so life was a little bit different in those days. Okay. So how did you make your decision to get more into electronics and focus on…

**Ross:** Before I went to Cambridge, I had got in my mind that I'd like to become a civil engineer, build bridges and roads and things of that nature. And then, I think it was in the third year, I contracted

  

German measles and was put into a hospital for a week in isolation. And that gave me a chance to do some thinking. And for some reason or other I concluded at that time that I was more interested in the physical sciences end of engineering and I made the decision that when I graduated I would go into what we would now call electronics and, of course, later made the decision that I would stay on at Cambridge for another three years and get my Ph.D..This would have been much more awkward in an engineering faculty where you specialized in a particular discipline earlier on. I didn't have that problem.

**Terman:** So when you made that decision after coming out of the hospital, did that change the courses that you took?

**Ross:** No.

**Terman:** You were still tied into the fixed curriculum?

**Ross:** I was required, like everybody else, to take the exams that were set by the mechanical sciences department and they chose what they asked you to do but there were papers in all of these disciplines.

**Terman:** So are they more flexible nowadays? I don't know if you've gone back and talked to people?

**Ross:** You know, I don't know.

**Terman:** It was very interesting though to get an insight into what things were like. So you graduated and you ended up at Bell Labs in 1952, what was the transition? How did you get to Bell Labs? What was the cause?

**Ross:** I had spent six years in Cambridge, three years as an undergraduate, three in doing my research project which was rather a lonesome experience. No courses and all of that kind of thing. And I decided it would be a good idea to get as far as away as I could from Cambridge without learning a new language. I was interested in the United States. I let my professors know that I had that interest. And they arranged some interviews for me. I recall there was one with RCA. And they also had me meet with Bill Shockley who was visiting Cambridge and he interviewed me in his typical way and made me an offer, which of course **-** at that time the transistor invention had happened five years earlier **-** it was an irresistible offer. And as I recall I sort of told Bill that I would be delighted to work in his organization which was a research organization, but I was also interested in other factions of the engineering business. And that I probably didn't want to stay in the United States for more than a year. And I failed on that one.

**Terman:** You say he made an offer, a specific offer to work with him or was there something else involved in the offer?

**Ross:** No, no, this was an offer to work in his group, which I did.

**Terman:** So you moved from the U.K. to New Jersey, to the Bell Labs, this was Murray Hill, where he was located?

**Ross:** Yes, it was.

**Terman:** Okay. And that was 1952. What was the state of the semiconductor technology that was going on at that time? What kind of work was being done?

**Ross:** The state of the technology was roughly, let's see, point contact transistors had been in manufacture for several years. The junction transistor, grown junction transistor, was just going into manufacture. [John] Saby at GE had just invented and published the alloy-junction transistor which was a very promising thing. [Gordon] Teal, I believe, was beginning to grow pretty pure single crystals of silicon. That roughly was the state of the technology. The group of people who were involved in the original invention of the transistor mainly were still there. Bardeen had left, I think, just about six months ago but he was still a frequent visitor. Of course, Walter Brattain was still there as was [Dick] Haynes and [Gerald] Pearson, Morgan Sparks and Bert Moore. It was a very stimulating group of people to be with. They were still all enthused about what they were doing and very helpful to me as a new Ph.D.

**Terman:** So they interacted quite well, they interacted well with you then?

**Ross:** Oh yes.

**Terman:** What was it like to be suddenly almost magically, almost instantaneously transported to be in the presence of these people who had done this phenomenal work and were quite well known for the work?

**Ross:** It was, of course, very, very stimulating. And the whole technology was extremely exciting in those days. Things were moving very, very rapidly.

**Terman:** Was it one of those things where people would walk down the hall and say, "Look we've just done this?" I mean new things popping up every week or month or something that …

**Ross:** It seemed like that. And, of course, the communications were totally free. And as somebody once said, virtually all of the knowledge of semiconductor devices existed at one cross aisle in Bell Labs. .

**Terman:** Okay. But did they have monthly progress reports that they would fill out that you could read? Or was there any formal way that they cataloged what progress they had been making?

**Ross:** No. There wasn't [such] a project at that time, certainly not in the research organization. There were frequent meetings, either informal or formal at which people would report on what they were doing.

And of course, there was always the activity of developing patents with the patent attorneys and publishing.  And internal memoranda, of course, played a major role.

**Terman:**  So there were internal memoranda which would get circulated?

**Ross:**  Oh yes.

**Terman:**  You raised the question of patents, how much focus did Bell Labs have on patents?

**Ross:**  We had our own patent department. At one time, it had about 50 people. And it was a very important activity.  And I don't think it's any secret that when somebody had an invention and you started writing the patent, many of the ideas that got into the patent came from the attorney.

**Terman:**  So you had attorneys who actually…

**Ross:**  They were amazing people.

**Terman:**  They actually had technical understanding?

**Ross:**  Not really.  They were legal people, but they were very closely working with you.  I mean you had a patent attorney assigned to you or to your organization and he'd drop by every so often and say "what are you up to and what's in your notebook?" And he might even suggest things to you [which] you hadn't thought were possible for patenting.

**Terman:**  Okay.  That's very interesting.  Your initial technical work at Bell Labs, what did you do when you arrived?

**Ross:**  Immediately after I arrived there was a transistor symposium and this was the second of two symposia at which Bell Labs and Western Electric brought in their new licensees, the people who paid $25,000 to have rights to the technology.  And they brought them in for I think it was something like six days several of which were to Bell Labs, Murray Hill.  And the other days were in Allentown, Pennsylvania, where the Western Electric factory was making transistors.  And they were taught almost everything we knew.  They were supposed to go away from those meetings being able to make transistors themselves.  Now, that meeting, that second symposium which was for the international, non-United States, patent licensees was to take place just a few days or weeks after I was there.

My first assignment was to set up a sort of lab demonstration and course so these people could come in and actually see a transistor and measure its characteristics and my job was to design that little course, which I did.  But it had a rather startling result.  I got to Bell Labs in March, and the weather was typically what it should be.  It was very cold for England. And at the time we were going to have this symposium the weather in New Jersey did what I never expected, it switched from winter to the height of summer without any spring in between, I wasn't used to that. And all of a sudden one morning, it was terribly hot and awfully humid and I went into my lab at Bell Labs and there was no air conditioning at Murray Hill in those days and my transistors had no characteristics, absolutely none. And, of course, I had discovered what everybody else, a lot of people, already knew, that the transistor was very sensitive to the environment and particularly to humidity. So what do you do? There was 48 hours to go and I got some test tubes and some desiccant and some cotton wool and some rubber bungs and that's the way these people saw their first transistor and made the first measurements on it. It didn't seem to upset them at all. I was very embarrassed. That was my first project. It didn't take very long. And then as I mentioned, Saby had come up with the alloy-junction process. And Bill Shockley then re-analyzed the field effect transistor using the alloy technology to create a P-N junction which when back biased would create a layer that was non-conducting and that would close off the conduction.

**Terman:** So this is not the MOSFET [metal-oxide-semiconductor field-effect transistor].

**Ross:** No, this is not…

**Terman:** This is a junction FET.

**Ross:** A grown junction. And, in fact, it was called a unipolar device at that time because it only had one charge that was flowing. And Bill had reanalyzed this and he had also, by the way, made a drawing of it and it was an N-type channel with P-type gates in it. And they were put in there like the grids in an electron tube. It was amazing how the tube idea died rather slowly. He asked George Dacey, who was a new employee, and me to just make the field effect transistor. And we avoided the complexity of multiple grids. We just took a filament of germanium and surrounded it by indium and alloyed it in there and made the field effect transistor. And it worked as Bill Shockley's theory had predicted. But, of course, at that time the characteristics of a field effect transistor were inferior to the junction transistor. And for most applications, it was not attractive. So we wrote up the paper and the field effect transistor sort of went into quiescence.

It's interesting there the history of the field effect transistor. The concept was first proposed by somebody called Lilienfeld in Germany in 1925 [Editors note: Polish-American physicist Julius E. Lilienfeld filed his patent in the US in 1926]. And I believe Shockley pre-War and post-War tried to make a field effect transistor and mainly by putting a field from an external plate, and it never worked. But it was, when they assembled their group there to look for amplification in semiconductors they again tried to use the field effect concept which, of course, didn't work because of the surface states. But it had started in 1925. And I think it was only in the mid '60s that the field effect concept, the then MOS concept, came back into being important, and it became the mainstay of the integrated circuit business. So it had a long history. And eventually became one of the most important concepts in the business.

**Terman:** And the original field effect transistor did not use surfaces. It used the back bias P-N junction to pinch off conducting channel, whereas, of course, the MOSFET device was a surface device and that involved a whole new set of physics and understanding and control and the technology of that insulator silicon interface. It's quite a different device, actually, in the characteristics. But the MOSFET device was invented at Bell Laboratories by Atalla and Kahng, as I remember.

  

**Ross:** I wouldn't say it was invented by them. It was the first MOS device. It was first reduced to practice by them. John Atalla was chasing the problem of what to do about surface states. We still had in those days transistors that were subject to the environment. They were encapsulated just like vacuum tubes. Evacuated, heat treated, gettered, and all of that stuff. And John had the assignment with a group of people to see what could be done about this. And he had the concept or he was working on the concept that maybe you could deal with the surface states in silicon if you produced very pure silicon, very clean silicon,and grew a superbly clean oxide on top of this and that way you may be able to reduce the surface states to a level at which you would have stability from a reliability point of view. And they worked very hard at that. But then they got to the point where they had reduced the surface states to the point-- which by the way they did their measurements on what was going on by putting a metal on top of the oxide -- And they got to the point where indeed, the surface states were reduced to the level that they could get penetration of the space charge into the semiconductor. So they produced an MOS device and published on it. But, again, it was not attractive compared to the junction transistors that then could be made in silicon and went back into obscurity. Just to end that story, they were not able to produce stable surfaces. They got the surface states down to the point where you could penetrate but it was not a stable structure. And it took [Jean] Hoerni several years later to get back at that problem.

**Terman:** Was the bipolar device technology silicon when you were there or were they still doing germanium and then converted over to silicon later?

**Ross:** Oh, when I moved into the development organization there was activity in germanium for high frequency applications. This was headed up by Jim Early. Our thoughts in those days were that germanium would have application for the highest frequency applications because of its higher mobility. And that silicon would be appropriate for switching because of its higher insulating properties and resistance to high temperatures. So both were being actively pursued.

**Terman:** Okay. So somewhere here, you moved into Management Director of [the] Semiconductor Lab in Murray Hill. Could you sort of give us some insight into what changes that made into what you were doing and then what you directed.

**Ross:** Yes, I didn't move into management as a Director in the development area. What happened there was…

**Terman:** Management and then Director, sorry.

**Ross:** What happened there was that Shockley came to me and said they were proposing that it would be a good idea if I moved into the development area. And, of course I had implied that I was interested in things other than research. By the way, I was having so much fun in Bill's organization that I was quite reluctant to make that move, but I made that move. And I found when I got into the development organization -- which then was headed by Jack Morton. He had all of the development on active devices, tubes and semiconductors -- I found that I had a supervisory responsibility. I was suddenly at the first level of management which meant there were maybe four or five professionals [who] would report to me. But what I discovered was there weren't any professionals there and I had to start out and hire these people. So I started out at the lowest level of management.

  

**Terman:** Where did you go to hire? Where did you find people that you could hire, from other companies or from academia or some mixture?

**Ross:** Well there really weren’t other companies that were doing anything much themselves in the technology. And of course, they were hiring from us, rather than the other way around. No, these people came from universities. And Bell Labs had a very active, high quality activity to find people in the universities and pick out the ones that we wanted. And of course, in the semiconductor area, it was easy to attract them in those days.

**Terman:** Did Bell Labs actually go out and have technical people from Bell Labs visiting universities, understanding what was going on, talking to professors?

**Ross:** Oh yes. We had recruiters who would be assigned a particular university. I was assigned Stanford at one point. And we would go out regularly, find out what was going on in the important areas, talk to the professors. When I was doing that, Gerald Pearson was out there. He told me who all the best people were, this was just wonderful. But we would meet these people. And the ones that we thought were attractive, we'd invite them to come to Bell Labs and visit. And they would spend several days interviewing.

**Terman:** So it was actually the technical people who were visiting universities.

**Ross:** Oh yes.

**Terman:** Some companies have "recruiters" going, who are not particularly deep technical people.

**Ross:** We didn't make that mistake.

**Terman:** How many people did you manage in your first level of management? And then later on, you became director of the semiconductor laboratory?

**Ross:** Oh, I don't remember exactly. But [as a Director] it probably was in the range of 50 to 100 people.

**Terman:** Did you work with other Bell Labs groups, or was it basically self-contained within the group that you were in, and then when you went on to the semiconductor lab?

**Ross:** Well, when I first got there, as I think I suggested, we were assigned in different organizations, different major tasks. Of course, we worked very closely with the other people in the development area and equally closely with the people in research, who were still looking at the fundamental technologies.

**Terman:** You mentioned Jack Morton, a name that I remember from my early days, a pretty famous person at Bell Laboratories. Any insight into what made him so important and having such impact?

**Ross:** Well, he was head of the whole activity in device development, active device development, which included the work done at Murray Hill and the work done by the branch laboratory in the Allentown factory. And eventually, it became what was called a vice presidential area, which was probably about one-tenth of Bell Labs. Now, Jack had developed a close-space triode, very high-frequency triode, [a tube] which really made the microwave network, radio network, possible. And he'd done a superb job on that. He was an interesting guy. He had strong views.

**Terman:** As I understand.

**Ross:** Yeah. And a very talented fellow.

**Terman:** Was he open to new ideas, other people coming to him with ideas?

**Ross:** Oh yeah. He was open to other ideas. Not as much as he was to his own ideas.

**Terman:** This is an interesting period.

**Ross:** Oh, fascinating.

**Terman:** Could you go over the names of some of the people that you were working with, either in Bell Labs or outside of Bell Labs? Who was moving the technology forward at that time?

**Ross:** Well, of course, still, most of it was happening in Bell Labs. The industry was growing very slowly. We interacted with the other organizations that were outside of Bell Laboratories. But initially, it was mainly the RCAs and the GEs and the people who were already in that kind of business. But as time went on, it got to be more and more. And we had a lot of those relationships.

**Terman:** And it seems like, from the invention of the transistor to where it became sort of beginning to have some commercial impact, there was a fairly long period there, perhaps longer than people might be willing to put in at this time.

**Ross:** Yeah. It depends on what commercial impacts you're talking about. I mean, it was one thing to come up with a transistor radio, which Texas Instruments did. It was another thing to come out with an application that was important to the military, where they were always pioneering, regardless of the initial expense. And a totally different thing if you were talking about a component that had to go into a Bell Systems switching machine, which by the way, had a design requirement of no more than two hours down time 40 years. Now, I never understood why that wasn't one hour in 20 years, but it was a very challenging requirement. So things got into the Bell System very much later [particularly if] it was in switching . As I recall, earlier applications of point contact transistors were in telephone sets. But that was a long time.

**Terman:** So the Bell System requirements were more stringent than the MIL-SPEC requirements? Or were they just different requirements?

**Ross:** Well, they were different. But they were, I think at least as stringent as the military ones.

**Terman:** Although the military may have had a broader spec for environment, such as temperature and so on?

**Ross:** Yes. And of course, we didn't pick up switching machines and drop them.

**Terman:** I hope not.

**Ross:** It was different. Talking to the military, you had a much better conversation than talking to the people who were working consumer electronics.

**Terman:** Some names that I remember from that era are Ebers and Moll of course, and Jim Early. You've worked with all three of them. Any comments about their impact and working with them?

**Ross:** Well, when I moved into the development area, Ebers and Moll I think had just published their seminal paper on the engineering characterization of the transistor. Jim Ebers died very shortly after I got there, tragically. I think it was a brain tumor problem. John Moll had the silicon responsibility. And I worked closely with him for some time. And Jim Early was working very hard on the germanium end of it. And I had a couple of things I was working on, including the PNPN diode, which we need to talk about at some point.

**Terman:** Now would be a perfectly good time.

**Ross:** Well, when I moved into that organization, the assignment I got initially was to work on something that we in Bell Labs called functional devices. And other people had other names for it. I think the Air Force called it molecular electronics. And this by the way was remember, in 1954. There was the concern about the future for semiconductor devices in very large pieces of equipment, such as computers, where you needed to have hundreds of thousands of components, they had to be very close together in order to get the speeds that you could provide. They had to be interconnected with a myriad of interconnections. And the concern was, “How the heck would we make these things with a reasonable yield and with a reasonable lifetime?” And I think it was Jack Morton who coined the phrase "the tyranny of numbers." How do we deal with the tyranny of numbers? Which was eventually of course solved by the integrated circuit, but we'll get back to that I assume. But in those days, there were a number of approaches. And one of them was this idea that Jack liked and other people were pushing that maybe a way to go, rather than using multiple components to perform a particular function, [was] to look for something in the physics that would permit you to produce a single device that would perform a function that otherwise would take many components. The piezoelectric crystals were an example of that. Delay lines were another example of that. I think you could claim maybe that the CRT was an example of something like that. And by the way, I believe that the CCD, the charge coupled device, which was produced much later by Willard Boyle, was one good example of something you'd call a functional device that really worked. Well, I was given the assignment to try and dream up functional devices, and that I worked on. And eventually, we produced something which we called the stepping transistor. I think the computer people would call it a shift register, something where you put in pulses and the contacts were closed sequentially.

**Terman:** That was one transistor, not a circuit, but one transistor?

**Ross:** Well, what we produced was a stepping transistor with four steps on one piece of silicon. And they were in fact PNPN structures. And they were interconnected within the semiconductor such that the first device, when it was closed, leaked charge into the one on its right. So when the next pulse came in, it shifted from one to two. That was the way it worked. And it did work. And by the way, in fabricating that, which was quite a challenge, this was the first time within Bell Labs that we used oxide masking for diffusion. And we used photolithography to produce the structure itself. So as typically happens when you push something that doesn't work practically, you often develop some useful things in the meantime. Now, that work was-- we published on that stepping transistor. But it was not too long away from that that the integrated circuit revolution started. And of course, that was obviously the way to go. Producing these ideas that were based on new physical concepts, that was tough sledding. And when I think back on it, the best that we would hope to gain out of that was maybe a factor of five. You could produce one component that would replace five. Now, in what happened since [with ICs], I mean, you gained that in three months. It was challenging in what it caused us to do, but it was dead ended.

Now, the other project that I got assigned was the PNPN diode. And one of the things that Shockley analyzed in that miraculous Christmas-New Year's period in 1947 and '48 was the PNPN diode. And he included the triode too. But he recognized that a PNPN device, as distinct from a three-layer device, would have an alpha greater than unity. And therefore, it could be unstable, and it could have two operating conditions, one where it acted like a reverse-biased junction and the other where it acted like a forward-biased junction. So that was an interesting device that Bill had analyzed. And it was recognized that it could have a very important role in AT&T switching equipment. And the way that went, let me describe this rather simply. If you have 100 telephone customers you want to connect to 100 others, you can take 100 wires vertically and 100 wires horizontally. And where they would intersect, you put a switch that can be open or closed. And that was called a cross-point switch.

**Terman:** Right.

**Ross:** And of course, if you had 100 by 100, then you had 10,000 of those switches. And as you got bigger, you had even larger numbers. Well, the way this was originally done with relays, you put a relay device at that point. And you goosed it and closed the switch. But the PNPN device, you could put at that point and just by putting on a voltage greater than the breakdown voltage you could make it close. And then later, if you reduced the current below the current at which it would stay in the high-current condition, it would turn off. So this was very much simpler, it was what was called end marking. All you had to do was say "I want to connect that one to that one," put some voltage on there, and you had an elegant switch. So that was a very important project. And I had the chore with my organization at that time to try and develop that. And of course this was now a four-layer device. And it was pushing the technology somewhat. We worked very closely with the people who were developing the electronic switching systems at the time, and they did the analysis. And of course, when you got to not just hundreds, but thousands of things, very, very many devices in there, and the margin analysis led you to [exacting] requirements, precision on the breakdown voltage of the diode, the current at which it would switch off, which was determined by alpha, and the series resistance. And we were just struggling up against that brick wall. It required precision in diffusion which was way beyond what existed. And Jim Goldey and I worked long hours improving the diffusion precision, which we did. We made fine contributions there.

And eventually, somebody, [Alec Feiner], came up with a totally different device, a ferreed switch. And that was just much more practical than the PNPN. We abandoned the project. But there again, it was productive. We really improved, made contributions, to the state of the art on diffusion. And the other thing that was important, we ran into this age old problem in semiconductors that you were either trying to deal with the very small difference of large numbers, which you do in an alloy transistor. We can come back to that. Or you were stuck with a capacitance and series resistance in the collector that was limiting your frequency response. And a number of people were wrestling with that problem. We were wrestling with it for the PNPN diode. Jim Early was struggling with it to produce a high-frequency germanium transistor. And Jim came up with the PNIP transistor, which would create a collector area in which the capacitance was determined by the i-region, and the series resistance by the heavily-doped p-region. So that would solve the problem, if you could make it. And by the way, Jim has the distinction, I believe, that he was the only person beyond Shockley who came up with a basically new transistor structure. He did that. But we were all struggling with how on earth we would make these things. And there were the basic problems of dealing with these small differences of large numbers. And the problem that in all the processes we had, the processing to go from n to p, was always additive. Whether it was alloying or whether it was diffusion, you always overcame what was already in there, so your layers got steadily higher conductivity. But the stuff you start out with was the highest resistivity, and that was the bugaboo. Now this problem of small differences of large numbers was the reason the alloy transistor couldn't compete with the diffused junction transistor. We needed a new process ..

And I was reminded the other day of the first time that I met Bob Noyce. This was in the early '50s. He was working in a company called Philco Ford, which was somewhere just north of Philadelphia. And I and some other people visited Bob Noyce, first time I'd met him. And he had an extremely ingenious idea for how to get at this problem of getting a thin base in an alloy transistor. And he showed us an equipment in which he was trying to do this, and it was amazing. He had a piece of n-type germanium, which was going to be the base. And then impinging on this from either side were two jets of electrolyte. And they were biased such that they were etching into the germanium. At the same time, he was monitoring the resistance in the remaining n-type region, which of course increased with time. And at the appropriate point, when he was satisfied, he would change the process from etching to depositing. And he would deposit an acceptor-type material, maybe indium. And that's the way it would go. Well, you know, this was a brilliant idea. And I came away from there with just two thoughts. Firstly, I was convinced it would never be practical to manufacture. But secondly, here was an extremely inventive character. And I turned out to be right on both scores. But here was somebody who was looking for a totally new process in order to solve a fundamental problem. And it seemed to me that in this problem we were having in silicon transistors, PNPN and in germanium and silicon transistors, that we needed a new process. We needed one where you could somehow or other create a thin layer at the surface with a process that wasn't additive. And this was a nice idea. And one morning, it was very early, it suddenly occurred to me that we probably had such a process already available to us. Because I knew at that time that Henry Theurer was growing single crystals of silicon by depositing from a vapor of silicon tetrachloride. And he was growing single crystals, and he was of course growing doped crystals. Well, it seemed to me it was pretty obvious that if he could do that, he ought to be able to switch the doping mechanism and produce a high-resistivity layer on top of the heavily-doped layer. Well, very soon after that, I think it was the same day, a couple of us went over to see Henry. And I'm sure Howard Loar was a member of that, I don't know who else. And I said to Henry, "Can you produce a thin intrinsic layer, high-resistivity layer, on top of a heavily-doped n-type crystal?" And he said, "Yes, I can," which wasn't a surprise, I thought he could do that. And within about three days, he produced such a sample. And Howard Loar and his people in the lab diffused a base and an emitter into it. And lo and behold, we had a perfectly good working transistor, which removed one of the uncertainties there. The material was excellent semiconductor material. Of course the next step was to see could we take Henry's chemistry and move it into the development lab. And could we then deposit on a slice of material as prepared - a silicon slice as prepared for diffusion processes - and grow a layer - , which at that time I didn't know was called an epitaxial layer - , into which we could then diffuse a transistor and it would work. And that's what happened. The guys went back into the lab, and they did exactly that. And they produced a working silicon transistor with all of the nice characteristics, the right capacity and low-series resistance, and did the same thing in germanium. And we published on that. You know, the reaction I think in the industry was a sigh of relief that you could hear from coast to coast. And quite a number of cries, "Why the heck didn't I think of that?" Both of which, I think are reasonably true. But that was a very exciting development that came out of trying to make [the PNPN diode] ,something that was damned difficult to make.

I've talked longer about this than I should have.

**Terman:** No, not at all. Believe me, not at all. The only thought I would have had when presented with that was well, I've got a very heavily-doped substrate, I'm trying to put an intrinsic or very lightly-doped layer on top, how about the auto doping?

**Ross:** How about the what?

**Terman:** The doping from the n-plus material. Wouldn't that diffuse out into the base material, the thin layer?

**Ross:** Yeah. That could have been a problem.

**Terman:** But apparently, the temperatures were low enough.

**Ross:** Apparently, yeah.

**Terman:** So it does work, as we know.

**Ross:** There were a number of reasons it couldn't have worked.

  

**Terman:** But I was wondering if that argument came up. I can't put an intrinsic layer on top of a heavily-doped layer because it'll just get doped. The temperature was low enough, I guess that didn't happen.

**Ross:** But presumably, you're right. Had you chose to, you could have diffused from the heavily-doped layer back into the surface.

**Terman:** Which you didn’t want.

**Ross:** But you didn't want to do.

**Terman:** Okay. Now, you mentioned the stepping device that was the first use of oxide masking and photolithography. Was that oxide masking and photolithography a Bell Labs creation? Or was that things that were being played around in the industry which Bell Labs picked up?

**Ross:** Oh no, this was something that was-- the oxide masking was done by Derick and Frosch. I think they were struggling with the pitting of the silicon surface when you went through the diffusion process. And they determined that it was due to silicon monoxide that somehow was interacting. And if they switched to silicon dioxide, which they did by putting water vapor into the atmosphere, they got a nice clean surface. So they had solved the problem of pitting. But in the process, they discovered that donors and acceptors wouldn't diffuse through that oxide. So they discovered oxide masking, which was just fine. And then not too long after that, Andrus and Bond discovered that you could use photoresist to etch away silicon dioxide where you didn't want it. And then you could diffuse with that. So those two things were made very much about the same time by those four guys. And by the way, this was four people in a few weeks who made these two major breakthroughs. You can't do that these days.

**Terman:** I guess the question [is] the lithography, but somebody has to come up with the photoresist.

**Ross:** Yeah.

**Terman:** And you would think it would take a lot of effort to find the right material that would adhere to the silicon oxide, that would hold up to the etching for the silicon oxide, and all these things that you can just, again, envision that "well this isn't possibly going to work, so we'll have to look someplace else."

**Ross:** Well, you're absolutely right. The photoresist was not made by the Bell Labs people. Photoresists were being used in various industries. So they took from the other industries. But I was not involved in this. But I think it took a long time before they found a photoresist that kept pace with the miniaturization process.

**Terman:** And that was a constant fight, as I remember, improving the photoresist, because you had to go with thinner layers too. Now, you've mentioned the MOS, or the MOSFET device. I guess I was interested in the effort that was done to clean the surfaces there. Because the MOSFET doesn't work unless you get the surface states down far enough that you can actually invert the surface to create the channel. And initially, unless you really know what you're doing, I think a lot of work went into getting those surfaces clean enough so that you could actually make the MOSFET device work. Not just merely penetrate into the surface, but in fact invert the surface for the channel region.

**Ross:** Well, as I said, Atalla was working to get as clean a silicon as he possibly could. And as pure a silicon dioxide as he possibly could. And he didn't really get there. And of course, we all knew at that time that you had to have-- look, if you used a dirty oxide like the one you had in diffusion, you wouldn't even get a junction, let alone get passivation. So we didn't proceed with that. Now Jean Hoerni didn't have that prohibition, and he apparently at Fairchild, not apparently, he did at Fairchild, diffused a junction through an oxide and left that dirty oxide on the surface. And what he found was he had a perfectly good PN junction, which amazed everybody else. And it seemed to be pretty stable at times. So he had produced some passivation we thought. Well, it wasn't that simple but the main point was he led us in the right direction, which led very soon to the planar transistor. An important building block in the integrated circuit. There are a lot of examples I think, of things that we knew wouldn't work but somebody went ahead anyway and tried them and they did work. Not deprecating anything that Kilby did, but we knew that if you put multiple germanium devices bonded altogether, that wasn't going to get you any place. But he did it. And then Noyce came up with a technology that they'd produced and said, "You know, I can solve that interconnection problem." And the pair of them produced the integrated circuit.

**Terman:** It was interesting that it was sort of the combination of Noyce's insight to what Hoerni had done with the planar process. That "I can do this". You mentioned patent attorneys. Kilby also had a very good patent attorney. He wrote a patent that could be interpreted- first claim probably, that could be interpreted broadly enough that--

**Ross:** Who? Kilby?

**Terman:** Kilby, yeah.

**Ross:** Well, Kilby did say- he told me this - that in his patent application that he had described the concept that you could use an insulating film and deposit over it. But he didn't do it. It was Noyce who did it.

**Terman:** That's the importance of getting a patent really written right. Patents through history have been used in the technology far in advance of what was envisioned by the person who got the patent originally.

**Ross:** But those guys did a wonderful job of just breaking this thing open. But we still all knew in the industry that it wouldn't work. If you put 100 transistors on a slice of silicon and the yield of a single transistor was 90%, the yield of 100 was down to 1% and that wouldn't be good. And if the reliability was such and such, then you were in trouble. And we all knew you couldn't do that. Well, some people, and I'm sure Fairchild was some of them, ignored that. They made a huge investment, a huge gamble and just kept trying it. And it kept working. And you know what emerged eventually was that we were all wrong in that these defects were not randomly distributed. In the processing we had, particularly the batch processing, there were parts of a silicon slice that were perfectly good and there were parts that were no good. And if your integrated circuit was small compared to the parts that were good, you could get reasonable yields and that's the way it turned out. So, the courage of whoever did it to make those investments and just bull this thing through, which took quite a while, was a very important part of that innovation.

**Terman:** I also remember in the '60s and '70s, people talking about the next chip, the next microprocessor, the next DRAM. They would run these things through and the first yield would be one out of 100. If they had one good chip on the wafer, they were really happy. That's presumably because that was not in an area that had no defects. But the next step was so that you cleaned up the process by one or two orders of magnitude; a phenomenal amount of effort that was done by people in the manufacturing area making things cleaner and cleaner and cleaner. People ignore that but it was just an amazing ability to make things cleaner.

**Ross:** Also in defense of we who didn't- - who believed in this yield problem -- the way we made transistors in those days with individual chips, individual alloys, bonded leads; that would never have worked. But it was the batch processing that Fairchild pioneered in the planar transistor with the planar contacts that gave us this batch processing that had the chance if you cleaned it up, if you controlled it meticulously. And if you invested enough money it would work.

**Terman:** Which people did in trillions of dollars actually. We've talked about transistors and MOSFET devices, and PNPN devices. How about the silicon solar cell, which was one that was invented by Pearson at that time, and which has had potential immense impact looking forward now as one of the ways that can get power to rural areas around the world without developing a grid?

**Ross:** Of course in the 1950s, oil was cheap and we had lots of it. But Gerald Pearson did invent the solar cell. He knew the physics and he knew it would work and he made a few and of course they worked.

**Terman:** Did he do it from understanding the physics? What was the thinking that went into it?

**Ross:** I don't know. I would assume that Gerald understood the generation of charge and what would happen--

**Terman:** When light comes in.

**Ross:** Yeah, because people have been shining light on junctions for many, many years; so they knew the photoelectric effect. I think the real question was, was it 10% efficient or 90% efficient? And it was about 10 as I recall. But he made some of these. As I recall, there was a field trial done somewhere in Atlanta, or in Georgia, with some solar cells on the top of a telephone pole to see if it had an application in the telephony. And it was concluded at that time, the weather problems and the birds defecating on the solar cells were a problem and it was not pursued. So, as far as I was concerned, the next time I ran into the solar cell was when we were going to design and launch Telestar. And remember, this was the Bell System, AT&T launching of a communications satellite that could carry TV. And Bell Labs had the audacity to say we could design one of those things when we'd never designed any spacecraft before.

  

So, we had an extremely tight schedule. NASA agreed to launch it. We had to produce this thing in I think in a couple of years. I got the responsibility- I was the project manager - for all of the active components that went into Telestar, which included the traveling wave tube and the telemetry circuits' or the transistors that went into telemetry, and they included the solar cells. And I had the responsibility in my group to design and put into manufacture or fabrication, the solar cells that would go on Telestar. And in fact, we designed them. It was not that difficult to design. And we put them directly into small scale manufacture at Western Electric and they went on the satellite.

The problem though, believe it or not, was we didn't know how bright the sunlight was in earth orbit. Nobody knew that. So we didn't know how much power we could get out of a solar cell. Eventually, Friedolf Smits, who you've met, went out to California to Table Mountain where the Smithsonian had a research facility. What they were doing was monitoring and analyzing the intensity of the sun that reached Table Mountain as the sun rose from the horizon up to the zenith. And they were analyzing the intensity and they were analyzing the spectrum. And Friedolf went up there- and with one other - . Hermann Gummel I think it was - . and they took one of our solar cells there and it was tracked exactly the same way. They measured it. And this way they were able to deduce, as the Smithsonian could, what indeed the sunlight intensity should be when you got right out of the atmosphere. And that's what we used and, by the way, Friedolf's calculations turned out to be right to within a few percent. It was a very fine achievement. Incidentally, when I got his expense account for his trip to Los Angeles, he charged me for snow tires for his car. I had to question that and he told me about Table Mountain [snow.]

But there was one other problem that Friedhoff helped with and he had a remarkable way of thinking about things. The next question was how many solar cells should we put on Telestar? And we were doing the analysis the right way. We were trying to find out- we had antennas and all the rest of it - how much power would the individual components use? And from that, try and determine how many solar cells to put there. Well, this was turning out to be very difficult and in that kind of project, you didn't really know accurately what the power consumption was going to be. Friedolf came into my office one day when I told him I was wrestling with this problem. He said, "You know, the solution is very simple." He said, "The spacecraft is a sphere. The diameter's determined by NASA, by the shroud on the launch vehicle so you know the area. You've got the transmitting and receiving antennas. You've got the telemetry things. You've got certain sensors. You know what the area of those are." He said, "Everywhere else we put solar cells." And that's what we did. A very simple solution.

But talking of Morton and talking of the satellite, I got myself into trouble because having done all of that, we concluded that we didn't have the power to run the traveling wave tube at night. We just had to turn the traveling wave tube on and off. And I knew that one of the prime accelerators of failure in tubes, including traveling wave tubes, was turning them on and off; particularly the heater. And I had the chore of going up to Morton's office and saying, "Jack, we're going to have to turn off the traveling wave tube." Well, he chewed me out regally. I stood there and he explained to me why I didn't know anything about traveling wave tubes, which was more or less correct. And we couldn't possibly do anything like that. And I stood there and stood there until eventually he quieted down and I said, "Jack, we don't have the power. We have to turn it off." And talking about accepting other people's ideas, he reluctantly agreed. And that's the way we did it. Telestar didn't fail because of traveling waves tubes. It got into trouble with the Van Allen Belt but that was another story. So much for solar cells.

  

**Terman:** The original ones were 6%. The theoretical maximum was about 21% because of the band gap. There are things that eliminate reflections and other things that can do better. There were some very basic physical things. The band gap for silicon is this and here is the spectrum and you're not going to be able to do any better. People have been able to work their way around that.

**Ross:** The question of solar cells have found lots of applications before we got into the oil problem. My desktop computer is solar powered. But not as critical [an] application.

**Terman:** So at Bell Labs, what was it like to work with Shockley?

**Ross:** It was very exhilarating. He was an extremely smart physicist and analyst. He had bright ideas. He was very good at hiring people. If you look at the group of talent he accumulated when he moved to California; a beautiful example of the way he could do that. Bill had his strengths and he had his weaknesses. His technical strengths were great but he was not a program manager. He was not a project manager. He was not a business type. When I got to Bell Labs and worked in his organization, he had a full time PhD physicist assistant. John Hornbeck by name. And John took care of the budgets and the personnel things and the equipment; all of those things that Bill didn't have to deal with including some of the personnel things. And that worked extremely well. I think what must of happened there is the management of Bell Labs appreciated what Bill's strengths were and what his weaknesses were and they set up an organization there that could capitalize on his strengths and handle his weaknesses. Unfortunately, when he moved to California, he didn't have that kind of organization. I think also one of the reasons that Bill was so unsuccessful in hiring people out to California from Bell Labs is that those who knew him, really didn't believe that he was the kind of person who could run that kind of business. So, we just didn't go. He was as a personality, he was an interesting guy. He had lots of interests. He was an amateur magician, which he used when he was giving presentations. Had a wonderful sense of humor. Some of his jokes that I can't repeat I still tell. He was very fond of English sports cars at that time. What affected me, he was also a rock climber and mountain climber and he took me rock climbing in downstate New York. A 300 foot cliff straight up with foot holes and hand holes you could hardly see. And I wasn't so sure I enjoyed all of that but after about three of those trips, Bill decided to put up a new climb. He was first on the rope. I was second on the rope. There was no third on the rope. Now, it amazes me that I had the courage to go on that trip. It amazes me that Bill put his trust in me to be on that rope. By the way, if you look up the records, it's a climb that's called Shockley's Ceiling. You'll find my name in there.

But the another thing about Bill that was interesting, shortly after I got there, there was an American Physical Society meeting in Washington and Bill said, "Why don't you go down there and learn something about what's going on?" And a group of us went down. Walter Brattain was in the crew and so was Dick Haynes. And we went by train from New Jersey to Washington. I sat in the club car with Walter and I asked him, "Tell me, how did this all come about? This invention of the transistor?" Of course I couldn't have asked him a better question and Walter talked for probably most of the way to Washington and told me about this in great detail. He also discussed the relationship among the three of them. He described Bill as being very outgoing, sometimes dominating person. And he described Bardeen as being a retiring shy person, which he certainly was. And Walter said that he was very concerned that Bardeen would not get the credit he was due and he went out of his way, made it his task, to assure that Bardeen got the credit he was due. And he was very proud of the fact that he accomplished that. And that has stayed in my mind ever since when this issue cropped up as to did everybody get the right credit and there was no question that they did.

Now one of Bill's weaknesses was that he didn't like people to reject his ideas. It didn't sit well with him. Before I got there, he was pushing for the idea that Bell Labs should use IQ tests when it was recruiting people. I gather his interest in that kind of thing followed him to California. Well, he got no where with Bell Labs management so he decided to do an experiment; typical of Bill. He had the whole of his group IQ tested, including himself. When it turned out he didn't have the highest IQ in the group, that's the last they heard of it. But I think this stubbornness when he was rejected on ideas, later got him into considerable trouble.

I make the observation that it really was a pity from the point of view of society that Bill left Bell Labs because I think it's fair to say that after doing that, he really made no major contribution to the technology. Whereas I think had he stayed in that environment where people understood him, it might have been totally different.

**Terman:** He seemed to have gotten hung-up on the PNPN device at the Beckman Instruments. From my understanding from the few things I've read, that was one of the reasons that people left and went to form Fairchild because he didn't want to push the transistor which they could see had a market, but wanted to push the PNPN device, which they thought was questionable.

**Ross:** Yes, that is true. Bill- he of course knew about the PNPN device. He knew about the important role it could play in AT&T and in Western Electric equipment. He was in a sense right. [ If Beckman ] had been able to produce a PNPN diode which would have been put in a Western Electric switching machine, it would have been an enormous market. And he pushed to go ahead on that. As I mentioned earlier, I had the benefit of the analysis that had been done by the systems people as to the very stringent requirements you had and Bill knew nothing about this. In fact, at one time, he submitted, they submitted some samples of PNPN diodes. And they came to me or my organization to test them and they were just absolutely no use in the application any more than the ones that we were able to produce, which in fact were much better but still they were inadequate. So, I think you were absolutely right. He was stubborn about that and clearly Noyce and crew were also right that the real future was with what they could do with a diffused silicon transistor and that's the way it went.

**Terman:** What was the reaction at Bell Laboratories when people like Shockley and Atalla, Moll, and Pearson left?

**Ross:** I was there when Bill left. The reason that he said he left, and I think it was probably true, was that he felt he was not getting the proper recognition for the contributions he had made. He felt that he deserved a higher salary and he deserved a more prestigious position in Bell Labs and the management wouldn't give him those things. And he went off to California to make a million dollars in industry. I think we were disappointed that that was the way it went but we didn't know what we could do about it. And as I said before, most of us didn't want to go with him. Now on the other ones, I don't remember specifically. I think John Moll left after I had moved into the space program. But bear in mind that in the mid '50s, my organization or the organizations in semiconductor development at Bell Labs, we were losing about 25% of our employees, technical professionals, per year, to the emerging industry. So we were very used to seeing people leave; good people leave. And for a while, I thought I was running a recruiting organization rather than a development organization. So, it was not a surprise. I think in Gerald Pearson's case, it was an excellent pre-retirement opportunity for him to do something different and something he enjoyed doing. I don't remember any great chagrin over that.

**Terman:** Do you know of any attempt to retain people?

**Ross:** Some of us, believe it or not, felt that if the salaries were raised for the people who were in device development, we could deal with that problem. And the Bell Labs management said, "You know, if we do this with every new technology coming through, we'll be in trouble." And they didn't do it. But other than that, there was not much you could do.

**Terman:** Bell Labs and the integrated circuit. Bell Labs had as you've pointed out a lot of technology, but didn't seem to be a leader in integrated circuits. Was there a reason for that? Was that by choice or happenstance?

**Ross:** Of course I think we discussed earlier, why we in company with most other people didn't believe that the multiple silicon components, on a chip of silicon, would work for the reasons of yield and reasons of reliability. After Noyce and crew did their work, it was still several years when many other companies continued to pursue the other approaches. We weren't the only ones. It was only after much investment that people were pushing the silicon integrated circuits that people were able to demonstrate that these problems really were not there. So, I mean I wish we had thought of that but we didn't. But we were not alone.

**Terman:** Did Bell System have a big enough internal market to justify the development of integrated circuits? The design of the individual chip and then – what Intel has done is design one chip and sell millions of parts. Did Bell have that size of internal market?

**Ross:** Not really. The manufacturing in Western Electric was for components, equipment that could not be bought on the open market. So if you were looking for a very high reliability vacuum tube, they were really the only game in town. But when we got into the semiconductor business, and it became a fairly large volume quality operation, the regulators of AT&T said that we could only manufacture equipment of a kind that was used in the telephone network. And of course, that came along later than applications like transistor radios and it certainly didn't have the mass volume that you got in memories and you got in microprocessors. And I can recall I was involved in the decision that really we should stop developing memories because we really couldn't compete with people like Intel and the market that they had. And we did a nice job of designing microprocessors and digital signal processes but reluctantly reached the same decision. So in fact, the Western Electric did not have the size of market that would put them in the first ten of U.S. manufacturers. I remember at one time recommending- we better make a decision. We either get into semiconductor business or we get out of it because at the moment, we're not big enough to prosper.

**Terman:** And that would require selling to an outside market.

**Ross:** Of course after the divestiture, when what was left of AT&T, including Western Electric, was in our unregulated business. We could do whatever we liked. But at that point, we didn't have the [volume]- it was too late in my view.

**Terman:** It was interesting that the Bell System licensed anybody who wanted the transistor technology for $25,000, I think you said. So they were in fact pushing this out to the world rather than trying to keep it as a proprietary thing they held.

**Ross:** Yeah, this goes back of course, to the original composition and mission of AT&T. It was a regulated utility. Albeit privately owned with stockholders but its business was providing telephone service to not most of the, but a large number of the, customers in the United States. And AT&T funded technology development and research because they believed that the future of the telecommunications business did depend on new technology. The company was born out of a patent and all that sort of thing. So, they were interested in getting new technology, which would benefit their provision of service. But if they could get somebody else to manufacture it, then the probability was that they'd get a better product and a cheaper product than just pushing it into Western Electric. So, they purposely pushed it out that way for those reasons.

**Terman:** Bell was the research side and Western Electric was the manufacturing side. How did ideas get transferred across at Bell Labs at that time?

**Ross:** You're talking about technology transfer?

**Terman:** Yeah.

**Ross:** Most of the time, the exploratory development people, the front-end of the development process were in the same building as the research people. This was typically true at Murray Hill when the research work was being [done]- and semiconductors were being done at Murray Hill. The development work of the early devices was done at Murray Hill and it was just a matter of walking down the hall. That transfer was relatively easy. Now after World War II, Mervin Kelly, who was the president of Bell Labs at that time, and he was the man who set up the group that invented the transistor. But he also concluded that with vacuum tube technology and things of that nature, you could no longer design a product in one place and then send a specification to another. We used to call it mailing the tube drawing to Western Electric. He said, "So, this is not going to work." It was very astute of him. He said we've got to take some of the development activity, the development for manufacture, and put that in the same building as the factories. So we did that. And where Western Electric was in Allentown manufacturing tubes and then transistors, there was a development lab there doing that transfer into Western. So what that left of the transfer of technology, the difficult part of it, was between Murray Hill and Allentown but it was within the development organization. Now that didn't completely solve the problem. But that's the way we tried to handle it.

**Terman:** Did you end up with duplicate processing capabilities? One in the manufacturing area where they're doing the development and one in Bell Labs?

  

**Ross:** Between manufacturing and development?

**Terman:** Yeah.

**Ross:** It was heading that way. And when I got down to Allentown, one of the big problems that I saw, and my predecessors had seen, was that it was no longer possible to separate the processes by which you made something; from the specifications on what it was supposed to do. The Bell System was set up such that we in Bell Labs did the design. Now we turned it over to Western and their job was to manufacture it as cheaply as they possibly could consistent with our design. And a very unique characteristic, Bell Labs had the quality assurance responsibility. We were responsible for checking the product at the end of the line, which is the way you put in quality in those days,.and approving whether it would be shipped or whether it wouldn't. That was the atmosphere there. Now, when I got down to Allentown, we were beginning to bring silicon diffusion into the factory floor. High temperature stuff where you were worried about particles and contamination and all the rest of it. And I, as a Bell Labs employee, was not allowed to go on the Western Electric factory floor without an escort. And the quality control charts were covered and I wasn't allowed to see them. I'm not criticizing Western Electric. This is the way they did it. And I'm sure other companies did it a similar way. I had taken it on as a difficult chore to persuade these people that you just had to integrate the processes with the manufacturing.

And as one example, I found when I got down there, they were setting up silicon diffusion with plastic tubing that was feeding the gases into the ovens, which was an absolutely no-no. It was Tygon tubing. And I bitched about that and nagged about that and eventually they got so upset with me, when I got back to my office after lunch I couldn't get into my coffee table because it had a big pile of Tygon tubing. They'd taken it all out.

We were going through that kind of process. Western was struggling with particles in the air. It was a normal factory and they had young ladies working in the open atmosphere. And what could you do about that? They didn't have clean rooms in those days. And one thing I was able to contribute when I went out to [visit] the Sandia Laboratories in Albuquerque. We ran that lab. They did the development of nuclear weapons. And they had a thing called the Sandia hood. It was a lamina flow hood inside which they were doing things with-- they were sensitive to particles. I thought that was wonderful. Brought it back to tell the Western people about it. They rejoiced and they put it on the factory floor and they avoided having to build clean rooms for a while. But when they built them, they put the Sandia hoods in. But this was I think something that was going on in the industry as a whole. And not unrelated by the way, to the change in the approach to quality. Test quality at the end of the line rather than build it in at the beginning of the development and through the whole process. That's what I was referring to.

**Terman:** Some time you will have to tell me how do you test for two down hours in 40 years. What that some kind of accelerated testing?

**Ross:** Bear in mind the ESS switch, the electronic switching machines, which were stored-program,controlled machines; we had two processors. Completely redundant. Running side by side. That's one way you got at it.

  

**Terman:** Redundancy is a very good thing.

**Ross:** Yeah, expensive.

**Terman:** In 1962, you went to Allentown and became Director of semiconductor device and electronic tube laboratory. Why was that move made?

**Ross:** The reason it was made was that the gentleman John Hornbeck I mentioned, who had been an aide to Shockley, was transferred to Washington to establish Bellcom, which we'll get to. And this opened up [the] need to move people around within the device area. And somebody thought it would be good for me, or maybe good for them, to move me down to Allentown. That's how that happened and I was there for two years. At the end of which somebody left Bellcom and John Hornbeck grabbed me to go down there, so that's how that occurred.

**Terman:** Did Bell Labs have a policy to move people around to broaden their experience?

**Ross:** Yeah. It wasn't too formal but it was done. I benefited or suffered from enough of it. By the way, just to characterize this, when I left Allentown, the Western factory for the first time was producing a silicon transistor for a dollar. And we thought that was absolutely wonderful.

**Terman:** Why was Bell Labs Allentown created?

**Ross:** For the reason that I described that Kelly felt that you just could not with modern technology, transfer it 100 miles by mail. You just had to have something closer.

**Terman:** What was the relationship between Bell Labs in Allentown and Murray Hill?

**Ross:** The development organization in both areas, in both locations, reported into the same management. That was the relationship. Now it wasn't perfect.

**Terman:** There must have been some division of responsibility.

**Ross:** Roughly of course, the Murray Hill development people did the exploratory development; the first stage of development. And that was when it was decided that there was a semiconductor component that would have an application in systems - switching systems, transmission systems - that might be manufactured. So then the group at Murray Hill would take the technology and develop it to the point where you could demonstrate that it could be made and you could produce a specification that would satisfy the system's needs. Then at that point, you transfer it hopefully, all of the know-how and the specifications and the Allentown people worked then with the manufacturing people to see how they could translate that into things that were manufacturable without the baby going out with the bathwater. That was the broad responsibility.

  

**Terman:** Somebody has an idea saying maybe some new semiconductor or use of semiconductor technology could solve a problem. Did that come from the Western Electric people back in saying if you guys could do this for a problem we have or is it that you knew enough about what Western Electric was doing that you could see something you were doing here would solve a problem? Which way did that go?

**Ross:** It went at least both ways. And it also went back and forth with AT&T. They would be concerned about what the telephone companies needed and what could we produce that they could use.

**Terman:** Was there a formalized way of getting that kind of interaction done?

**Ross:** There was I think pragmatically. The process was in where the money was. What did the Western Electric or AT&T fund in the development areas? I think that's where it really hit the road.

**Terman:** At least partially the funding came from outside Bell Labs.

**Ross:** That's another issue. The research funding came from AT&T. It was recovered from the money that AT&T gleaned from the telephone companies. The development work was funded by Western Electric. We were very strict about that because we were afraid if we didn't do that right, we might get into an antitrust suit. So, this was a major concern. Now, in Bell Labs at that time, 80% of the funding was from Western Electric; 20% or less was elsewhere. We were mainly a development organization.

**Terman:** You spent two years in Allentown. Any fond memories that you have from that period? Any major challenges that made you want to tear your hair out?

**Ross:** I think I really discussed those. Despite all this conflict, some of which was very productive, we were all good friends in there. So, I have good memories of that but it was only two years. It was mainly relocation, one after the other.

**Terman:** You were involved with Bellcom from '64 to '70.

**Ross:** Correct.

**Terman:** What was Bellcom?

**Ross:** Bellcom was established at the request of Jim Webb, who was the head of NASA. And when the president committed the nation to the Apollo Program, Jim Webb had the concern that this was an enormous system. He was concerned as to whether NASA would have the capability- had enough capability to design a system of that size. He looked at AT&T and said, "Well you've got this great big telephone network that's similar to that. Will you help us?" And what AT&T agreed to do was to set up a separate company totally contracted to NASA headquarters to do the top level systems engineering.

  

Now the ‘com’ has confused people. We were not there just to do the communications. We did the whole thing at the very top level. And we reported directly into the head of the Office of Manned Space Flight and into the project manager, General Phillips for the Apollo Program. And when we produced a spec, the first specification for Apollo, he approved it. So, we just did the work. Another way to think about this is that we were a technical resource for the NASA headquarters people. And as such, by the way, we were looked upon by the field people in Houston and Huntsville as being just that. And particularly a bunch of telephone technicians in there trying to tell us how to run a space program. So, we had some fun with that.

**Terman:** How many people did you manage?

**Ross:** I believe something like 40 or 50 technical professionals transferred from Bell Labs in there. And I think we ended up with about 400 people by the time we dissolved the corporation.

**Terman:** The transfer of 40 technical people leaves a pretty large hole behind at Bell. There must have been some adjustment when they transferred the people out.

**Ross:** It was 40 out of 10,000.

**Terman:** At the end it was 400 people. You must have done a fair amount of hiring for the program.

**Ross:** Oh yeah. We needed to hire people that you didn’t have in AT&T. We needed geologists and we needed people who knew how launch vehicles worked. So we hired aerospace technology.

**Terman:** Was it easy to hire people at that time for the Space Program? Was that something people were really interested in working on?

**Ross:** Oh yes. That was a very glamorous, exciting time.

**Terman:** What were the major contributions that Bellcom made to Apollo?

**Ross:** I think the major contribution was that we were good team players with the headquarters. We did get the respect of the professionals out in the field. Remember, Werner von Braun was sitting there in Huntsville. He knew about launching vehicles. But I think we did provide the program with this top level specification as to how things- what the equipment should do. We did what we called Mission Assignments. What would each NASA launch do. We wrote a document on the environment in which it all had to work. There was no single outstanding contribution. We were part of the team.

**Terman:** Between Bellcom and AT&T Bell, there was no link at all? Bellcom was a separate entity?

  

**Ross:** Oh yeah, it was a separate corporation. It was owned by AT&T. Our relationships with Bell Labs were just friendly relationships with our old colleagues. They did no work for us.

**Terman:** Do you remember any of the other people at Bellcom who stand out in your mind as far as contributions?

**Ross:** We had one lunar geologist who I think is still working with the Smithsonian called Farouk El-Baz. He used to train the astronauts on what they should do on the moon and how they should handle themselves. They referred to him as "The King" because Farouk was the King of Egypt. He was very much appreciated.

**Terman:** You mentioned lunar surface strength specification and reliability and so on. Can you go down some of those things we talked about earlier?

**Ross:** Another year-- By the way, I was very impressed by the quality of engineers that NASA had in those days, both at Houston…. They'd come out of an organization that was founded in the '20s because the United States was falling behind on aeronautics technology. Does it sound familiar? And they were very, very smart people. They've been hired during the Depression when government salaries were very attractive. Then there were the Werner von Braun's crew down at Huntsville. They were another bunch of experts.

But I remember when I got down there, there was talk about what reliability we should have. What was the probability of things happening. And of course, the reliability experts were throwing these nines around; 99.999. And it was ridiculous. I remember Werner one day sat there and he said, "Are you meaning to tell me that when an astronaut leaves on Monday morning, he'll say to the wife, 'Darling, I'm going to the moon and there's a 99.99% [chance]I'll see you a week from Tuesday'." He said, "You know this has got to stop." And somebody came up with the idea, it was certainly not us; a very simple way of dealing with this. They came up with a guideline that no single failure would cause a loss of a mission and no two failures would cause a loss of crew. And this was a remarkably potent thing to come out of headquarters, not necessarily appropriate with the contractors, but it got us out of all those tyranny of nines.

**Terman:** You mentioned the lunar surface strength spec?

**Ross:** Of course, you know this business of uncertainty about space. How bright is the sun? I wasn't down there very long when suddenly NASA came to me and said, "Look, we need you to specify the surface strength of the moon." I said, "Well, you know nobody's been there. We don't know." They said, "That's all very well but you have the responsibility for the spec and we have to contract with somebody." And it wasn't that easy it turned out because the natural thing to say is look at it. It looks like a desert, therefore-- But there were people who were saying the moon has a depth of crystals that are likely welded together and they can be hundreds of feet deep. A sort of fairy castle that has no tensile strength whatsoever. And if you land on the moon, you'll go straight down. So, it wasn't a trivial question but we concluded that the most likely thing was that it was like a desert sand. So, we specified a desert sand. And then the opportunity came up not too long after that -- a missile was going to crash into the moon.

  

At the time there was an orbiter taking pictures and you could get it to photograph the impact of the crater. Now you turn that over to a geologist and they said yes, it looks like any other desert. We were in good shape.

**Terman:** Of course there are sand deserts and there are hard packed deserts.

**Ross:** Oh yeah. But once you take it that above a certain level and the size of the footpad on the LEM [Lunar Excursion Module] was I think more a stability issue.

**Terman:** You mentioned a typo.

**Ross:** Oh Lord. When we got down there, as I said, the people in Huntsville and in Houston looked cross-eyed at us. We wrote the specification and they were always teasing us. And we had to specify shutting down the engine on the LEM when it came down and you would think the normal thing you do is you wait until it touches the surface and then you automatically turn it off and all is well. Well, it's not that simple. The concern was that as you approach the surface, you might get a whole pile of dust. If you kept the engine on too long, you'd get more dust than you wanted and that would impede the vision of the astronauts. If you cut it off too soon, you would <makes crashing sound>. It'd be very uncomfortable. So, we wrote a specification that basically said turn off the engine just before you get to the surface and before the dust becomes too high. A simple statement. It was under the title of Shutting Down the Engine. Well, we had a typo. The 'u' was an 'i'. The Houston people thought this was hugely funny.

**Terman:** Today that would be on YouTube like that.

**Ross:** Not necessarily.

**Terman:** Don't bet on it. So, Apollo you left in 1970 from Bellcom.

**Ross:** Yeah.

**Terman:** You went back to Bell Labs first as Executive Director of Network Planning Division and then as Executive VP from '71 to '79. What was the difference in the responsibilities you had when you went back to Bell Labs in this position?

**Ross:** Well, I went back to Bell Labs in charge of a systems engineering division, the Network Planning Division. So I've gone from systems engineering Apollo to systems engineering AT&T Network. To me, this was the first time that I really had to know something about the telephone business. I mean, I knew a lot about the things that went into the telephone business but how did they work - I really didn't understand. It was about the time of course, when digital technology was becoming more prevalent. And we realized that if the Bell System was going to go from a principally analog network to eventually a totally digital network and do the transition gracefully, we needed a plan for doing that. So, one of the exciting things that we did in that organization while I was running it, we put together a plan for the all digital network; working of course heavily with the AT&T people, the people who ran the long distance network. That was a very challenging operation. I enjoyed that.

**Terman:** It also sounds like you had to come out of your comfort zone because you had to learn all sorts of new things.

**Ross:** Well, I had gone from transistors to lunar surfaces to the network. What the heck? I was trained as an engineer for all fields. Sorry, educated.

**Terman:** What was the semiconductor technology being worked on at Bell at this point?

**Ross:** You know, I really can't answer that question. I was preoccupied with the--

**Terman:** You developed the plan for the all digital system. There are a lot of components in there. How did you impose the reliability requirements, get what you wanted and so forth? How did you go from the plan to the actual realization?

**Ross:** This is a little bit like doing systems engineering for Apollo, working for the Apollo Program in headquarters. In this, we were working for AT&T. They had to approve what we did and their job of course was to implement that through their operating arms. This would lead to requirements for equipment, which would then get into the various systems development areas through to their manufacturing. That's the kind of way it went.

**Terman:** Did you work with individual suppliers for specific things on reliability, for instance?

**Ross:** I don't believe so. Not at that level.

**Terman:** Okay.

**Ross:** See, it would be the responsibility of the transmission development people to decide, do we manufacture this? Do we specify it? What do we need? The problems were problems of interfaces that worked, protocols, and those kinds of things. We had the reliability objective.

**Terman:** As long as you could get somebody to supply it of course. But the semiconductor industry was doing military specs, which would certainly be pretty good, better than the standard products that they were turning out. So you became Bell Labs president in 1979, and then through 1991. So what was the difference between your job as president of Bell Labs and that of your previous jobs as head of a research or development in a large company?

  

**Ross:** Two things. The president of Bell Labs had all of the research and the development for the Bell System reporting to him. Now, that's very different from being the head of a research organization, say at IBM, or being the head of a development group within a business unit manufacturing product. We had the whole thing. And there were engineers out in the operating companies and in branches of AT&T, but we were the whole development thing. So as far as research is concerned, if I had a good vice president of research, it was his job to direct that. And whoever was in charge of semiconductors, which was Johnny Mayo at the time, he could take care of that. My job really was to interface with the managers of AT&T. I felt that in a sense, I was the technical conscience of AT&T. I had to make sure that they didn't try and violate the laws of physics, which they frequently, particularly the marketing people, wanted to do. And my main interface was working with the chairman, working with the people who reported to him, working with the president of Western Electric and the president of the Long Lines Organization, which ran all of the intrastate, international networks. So that was my main job.

And as far as Bell Labs was concerned, I mean, I had the top-level responsibility for the budgets, but more importantly, the ambiance that would retain a creative, cost-effective R&D organization. Now, I'm sure my job was different from prior presidents because almost simultaneously with getting that responsibility, the Department of Justice decided to run an antitrust suit against AT&T. And that really preoccupied the top management. And incidentally, I was, for some reason or other, the lead witness in the defense of AT&T in the DOJ antitrust suit. So I was heavily involved in that. And partly as a result of [the response to ] my testimony, Charlie Brown, who was then the chairman and his chief legal advisor, kind of concluded that the way the judge was heading, he would probably recommend splitting off the manufacturing from the telecommunications management. And Charlie was concerned as to whether that was the right way to go. And I got involved as other people got involved, very small groups, talking very privately about what were the alternatives, which was the best way to go. And as you may recall, we ended up with the conclusion that AT&T did, that we should make a settlement with the Department of Justice outside of court. And the settlement should be along the lines that you separate the regulated businesses from the competitive businesses. And that seemed to make eminent sense. And by the way, I think it still does today. Having regulated and competitive businesses within the [same] corporate shell, very, very difficult, and you're always trying to prove you aren't cross-subsidizing. So that was the settlement that we recommended. And we split off the telephone companies. We could have done it as one telephone company, but we decided let's make it eight, because bigness in those days was bad. And then we kept the long distance, [and the] international business and the manufacturing business, which by then were all competitive, in the so-called AT&T. So that was a very different job from my predecessor.

And then of course, the problem was having gotten the judge to agree, Judge Green, that it was in the national interest to have this settlement, then the question was, how did we divest the telephone companies? And then, how do we reorganize AT&T? And by the way, in divesting the telephone companies, I recommended that they needed a systems engineering organization. And they in fact set up an engineering organization called Bellcore, which no longer exists. But that was a common resource to the telephone companies to do the systems engineering of their networks. And bless their souls, they also wanted a research organization, which I never understood, but it was their money. But then we had, how do you reorganize AT&T? And it was sitting there as AT&T headquarters populated by people who were trained to run telephone companies, and we didn't have any telephone companies. We had a Western Electric president with all of the manufacturing separate from what businesses we were in. And of course, we had Bell Labs, with all of their R&D. And in a competitive environment, that just did not make sense. So we had to reorganize that gracefully. [For Bell Labs] what we did was let the assignment of responsibility follow where the funding was. And we took whatever was funded in Bell Labs by a certain business unit, at some point or other, they took over, those employees were transferred to them. And this worked pretty smoothly. The question was what do we do with research? And the chairman of the board said, "I will fund that." Now, that was a major transition for Bell Labs. We kept the Bell Labs oversight to make sure that we kept the ambiance, we kept the environment, we did the right kind of recruiting. But of course, since then, all of these things have splintered into bits and pieces. And in a sense, the fact that we had done that made it possible for Lucent to be split off. It made it possible for Lucent to sell the semiconductor business. And I don't rejoice about that, but it was an accomplishment. But that's how I spent quite a bit of time, really from the time I took over to the time I retired.

**Terman:** Quite a bit different than working with Shockley also, I'm sure. How about the interoperability question? You divested, there was a lot of companies, you had to get specifications for interoperability. Did those specifications already exist? Or were they formalized well enough? Or did you have to come up and start really generating specifications so that [what works for] a company down in Georgia in the network can also work for somebody up in Montana?

**Ross:** You're talking about the specifications for the telephone network run by the operating companies?

**Terman:** Yeah.

**Ross:** That's a very interesting question. Because if you ask me was this decision to divest a right decision, and I think it was, I mean, separating those two things made a lot of sense. Have we suffered because of it? And I think in general, the answer is no, except for one thing. AT&T, when it existed as a whole company, was responsible for the long-term planning of the United States telephone network, by default, by de facto. The FCC didn't do it, AT&T did it. We set up the standards, we negotiated with the PTTs in various other places. And after divestiture, there was no such organization. The marketplace determined where things should go. However, the other nations kept their PTTs, in Asia and in Europe. And I think as a result, they have had much more forward-looking planning of networks like the cellular network, which I think has put those countries in a better situation as far as cellular service than we are. But other than that, and there were other similar specifications, I think as far as the customer is concerned, the prices have come down, the services are prolific. Competition has moved in with cell phones and with cable. I'm afraid it was a right decision.

**Terman:** And somewhere along this time, you were on the President's Commission for Industrial Competitiveness.

**Ross:** Yeah.

**Terman:** Do you remember anything about that, how that worked out?

**Ross:** This was a Presidential commission set up by Reagan, or by the Reagan Administration. It was chaired by John Young of HP. And it had chairmen and presidents of major corporations throughout the United States. And we were beginning to be concerned at that time about how the nation was dealing with competition from overseas, particularly with the Japanese. And we went through all of that and made a lot of recommendations. We were concerned about cost of capital in the United States versus subsidized capital so-called elsewhere. We were concerned about fair trade laws and all of those good things. But the one thing that stuck in my mind from that is we began to realize that there was something different about quality. That quality wasn't just something that you added to the product as an extra cost at the end in order to satisfy a customer who needed it. What the Japanese were teaching us was that quality put into the process was a cost reduction during the whole process. And this was really a revelation. I mean, we believed that Western Electric was the highest quality manufacturer in the world. And we found out there was somebody in Tokyo who was looking pretty better than us. So that was a very big change. And that was one that I remember as a result of that.

**Terman:** And then you were on the National Advisory Committee on Semiconductors, and chaired it 1989 to 1992.

**Ross:** Yeah. And that was a very interesting operation. Again, there was serious concern at that time that we would lose our semiconductor industry, to the Asians, mainly. And that committee was set up to address the question, what can we do about it? And it was populated by, again, the heads of the semiconductor industry, computer industry, IBM, TI, Motorola, National Semiconductor. And we had representatives of all branches of government. And we looked at that problem for three years. And by the way, one of the things that worried us greatly at that time was the continued loss of the consumer electronics business, because there was large volume semiconductor business that was going overseas. And we made recommendations to try and hold some of them back, which I don't think got any place. There was, again, a broad spectrum of recommendations, including things to do to lower the cost of capital. Make the investment tax credit return, get that back in. Make the R&D tax credit permanent. We're still wrestling with that. A suggestion that the depreciation rate on semiconductor equipment should be reduced to three years, a number of things of that nature. That the government should enforce fair trade laws. I still don't think we're doing that. And maybe the thing that maybe had the most effect was the idea of a semiconductor industry technical roadmap. John Armstrong [from IBM] was much a leader of that. And we, in fact, while the committee still existed, we put together with the industry the first technology roadmap. And it was amazing. This was all U.S. industry, but they all came together and they talked to each other, and they shared pre-competitive ideas and put together this roadmap. And one of our recommendations, and it was accepted, was that the SIA should take over the responsibility for that, and they have done that. And I think they're still producing them, except now they have international cooperation. So that was, I think, a very effective thing. It again demonstrated the continued willingness of that industry to cooperate in precompetitive things, which I think is quite unusual. But the other thing that came through was that group of people, the Intels and the Motorolas, recognizing they'd better get their act together, get the quality into the product, get the cost of production down. And probably, the thing that has saved the industry is that recognition that you shouldn't rely on the government, you've got to do it yourself. But that was a nice time.

**Terman:** As I remember, one of things that's striking about that time in the semiconductor progress, we had the Moore's law sort of stuff. And all of a sudden, there's a kink. Which means that people, when they saw the roadmap said "Oh, that's where everybody else is going to be in three to five years, I'd better be ahead of that."

**Ross:** Yeah.

  

**Terman:** And so the whole industry moved ahead. And I found that really astonishing, because they had been struggling to keep up with the Moore's law. And all of a sudden, they exceed Moore's law. And that was something that I found very interesting. But also, the whole industry did it together, and so it was I think something rather amazing. Looking back, are there any major opportunities that you think that Bell missed, or maybe the industry has missed?

**Ross:** That Bell missed?

**Terman:** That Bell Labs, AT&T missed. Some opportunity back there that if they'd done it, boy, things would have been a lot better. Or the semiconductor industry?

**Ross:** Well, let's take the Bell System. You know, sometime in the '70s, it was realized that the cost of transmission was a minor portion of the total cost of providing service. And we could have provided universal service at a fixed cost independent of [time and] distance, the way we did locally. And there was a major study done, mainly by AT&T, I don't think we were much involved in that, as to whether we should do that. And we decided not to. And when you see what's happened with the internet since, that probably was a mistake. And the other thing of course we didn't realize is that if you gave people unlimited service for so many bucks a month, they would compromise on quality. You know, two hours down time in 40 years didn't worry them. We hadn't realized that. We still felt that quality was of the highest importance. So we missed that. I don't think, and it's easy for me to say this, I don't think the telephone industry moved as rapidly as it should into the internet business. I think it's maybe still struggling with that.

**Terman:** So you look back on over 50 years in the industry, what projects--

**Ross:** Well, 40 years in the industry.

**Terman:** Well, something like that, okay. What projects or accomplishments are you most proud of?

**Ross:** I think there's no question the breakthrough that we accomplished on that epitaxy thing was the most satisfying. One way or another, that was a major problem that got solved, moved out of the way. And I have great satisfaction in believing that I stimulated that. The National Advisory Committee on Semiconductors I think was very helpful. And, you know, in preparation for [ this interview ]what we're doing now, I revisited an invited paper I wrote, published in the proceedings of IEEE on the transistor [50 year] anniversary. And I look back at that paper, I'm proud of that one. I think it's a good piece.

And there aren't any things that I particularly regret.

**Terman:** Well that's good. That alone I think is something that's very good. Any general thoughts on the history of semiconductor technology, looking back?

  

**Ross:** Well, yeah. The semiconductor industry was born out of AT&T, Bell Labs. And the unusual structure of the corporation that led it to create new technology and to propagate it to the rest of industry. And that, as I have explained to you, was the result of the regulated aspect of AT&T. But the whole industry was brought up with people who were used to that kind of cooperation, and people like Gordon Moore, I'm sure I can call tomorrow and he'll return the call. We all learned that it was good to cooperate in the precompetitive things. And to my amazement, and we discussed this with the roadmap, that has persisted. And I think that's unusual in industry.

The other thing that also started there, and sometimes it got us into trouble, was the insistence on trying to understand what was going on. The insistence on understanding the physics of the thing, and understanding what could be done and what couldn’t be done. And as we mentioned, sometimes thinking we understood what couldn't be done. But I think the industry is still benefiting from top management decision that we'd better understand this as best we can. Those two things stick with me as being characteristic of the semiconductor industry.

The other thing that I worry about, says he, this loss of manufacturing in the nation I consider to be very, very serious. I don't know what you do about it. But I don't know how you continue to maintain a superior standard of living if you aren't making things. On the other hand, you've got to face up to the fact that, as somebody once stupidly said, it's a global world. And you've got to deal with the cost of labor in different parts of the world. But anything we can do to retain good quality manufacture is, I think, terribly important.

**Terman:** I think we pretty much have gone through things. Let's see if there's any other questions which we want to go through. Final comments on your 40 years in the industry during an era of phenomenal growth, innovation, and advancement. Any sort of last comments you'd like to make on the industry or the telephone system, or life in general.

**Ross:** Oh heavens.

**Terman:** That's a whole other interview, I think.

**Ross:** I think we save that for another interview.

**Terman:** Okay. I want to thank you very much for taking the time to do this oral history. It's been fascinating listening and talking to you. My own career parallels yours. It started a little bit later and went a little bit beyond in computers as opposed to Bell telephone system and the telephone system, but it's been very, very fascinating listening to your talk, seeing a lot of things that you're talking about reflected in computers and seeing where the differences were also. So I'd like to thank you once again for taking the time and doing this.

**Ross:** Lew, I'd like to thank you for this interview or [I think it was more like a] discussion, And I very much enjoyed the discussions we had prior to this that were most helpful. Thank you.

  

**Terman:** It's been terrific, thank you very much.

END OF INTERVIEW