David (Dave) Hodges: Good morning. My name is David Hodges. I'm an Emeritus Professor at The University of California at Berkeley, and I'm here to interview my old friend Dr. Lewis M. Terman as a part of The Computer History Museum Oral History Project. Lew?

Lewis (Lew) Terman: I'm Lew Terman. I had a career for 45 years plus exactly one day at IBM. I retired from IBM as a Research Emeritus and I was President of IEEE in 2008. And I'm delighted to have Dave here to talk with. Dave?

Hodges: Tell us about some of your early history, Lew, your family and your background.

Terman: Well, I was born in San Francisco, actually. Dad was a professor at Stanford. And I don't remember too much before the Second World War, a few minor things but nothing relevant. I do remember listening to the radio broadcast of the attack on Pearl Harbor. And that changed things very dramatically because my father was made head of the Radio Research Laboratory [RRL] outside of Boston in Cambridge in 1942. He had been President of the IEEE [during] 1941, at the end of that year, of course, was the attack.

He was well known, and I think perhaps through the influence of Vannevar Bush, who was his thesis advisor; he was tabbed as somebody who could lead this effort. And so we moved from California back to the Boston area. We actually were very fortunate where we ended up on 560 Concord Avenue in Belmont, up from Belmont Center on what was called Belmont Hill, and essentially literally surrounded by millionaires. And one of them was the treasurer of Harvard, Bill Claflin. Dad had a very warm and friendly relationship with him, and learned a lot, and mentioned him quite fondly in subsequent years. At that time he was running that laboratory (RRL), and it was somewhat different from his life as a professor at Stanford. .

My schooling at that point was first at a public school and then, a private school. My, two older brothers were in different schools, so we didn't overlap at school, and that [pattern] was something that followed me through my life. We did not at any time overlap in [grade] school or at universities. It was a very interesting period. Because I had been in the East and enjoyed the winter it's probably why we went back to IBM later on, my wife and I, in 1961.

So we [the family] came back [to Stanford] in 1945 at the end of the war, and [I] went to public schools in Palo Alto [where] I found a group of people that I [got to know]. I had a bubble of people around me of similar interests, although we've gone in different directions afterwards. Through junior high school and high school, I had a group of friends, got in involved in athletics. I wasn't a particularly good athlete, although there were two things that turned out had the same kind of motion, and also which I was pretty good at, and that was backstroke swimming where the arm goes up and comes down and fast pitch softball pitching where the arm comes up, goes around and delivers the ball. I was kind of pleased that I had this group of people and I also had some athletic ability. I swam on the [high school] varsity team, and we had the best high school swimming team in the nation in my senior year. Not because of me, but I was at least part of that effort. So it was great fun.
I had some very good teachers in high school, particularly in my junior year in senior high school. I had a really good chemistry teacher. You went into the laboratory, and you did these things, and these things bubbled up, and things changed and all. This was really great fun, so I was going to be a chemist. Well, senior year I had who -turned out to be the legendary Mr. Martin. Henry Martin was the physics teacher. You didn't teach physics in those days. You really taught engineering, sort of [the] physics principles behind engineering. But he was a tough person, a rough person, a rough exterior, but he was actually a very reasonable guy. He didn't get down on people. He enjoyed teaching physics. He had a sense of humor. He did a lot of experiments. And I just really loved physics. It was great fun. And so [I decided that] "I'm going to go out and be a physicist" I graduated and I went across the road to Stanford with the expectation of being a physics major.

Hodges: Now your father was a very prominent electrical engineer. Did he have any influence on your professional direction?

Terman: No. It came naturally. As a kid I had done things like fly model airplanes. I was interested in audio, music. I was interested in jazz at that time. Dad was writing books and later on doing his work at the university. Back in Boston he took me out to a place where they installed some of the radar counter - measure equipment on a, what I think was a B24 Liberator. He took me into the plane and showed me the equipment. I wasn't interested in that. I went up to the pilot's seat and pretended I was flying the airplane. Now okay, I was eight at the time, but I think it shows at first he wasn't pushing it on me. But this was a natural bent that I had. I just liked the science stuff that I took in school and just said, "Okay, this is what I'm really good at. This is the place that I find interesting and so I'll just go forward in this kind of an area."

Hodges: So you started off at Stanford intending to be a physics-major, but it seems that you changed...?

Terman: Yes. My freshman physics was interesting, but in fact I don't think I learned anything that Henry Martin hadn't already taught me. The second year we got into more real physics stuff. It was a [physical] mechanics course. And I think the third year we got into thermodynamics. And the professor for the third year was Professor Meyerhoff, whom, I believe, was a very good physicist, but he had this habit of writing with his right hand and erasing with his left hand, so you really had to be on your toes to get the material down. And I was getting into the more theoretical [side of] things and I said, "Wait a minute. This isn't for me." So I talked to my advisor, who was Robert Hofstadter, who later on got a Nobel Prize. And Dr. Hofstadter said, "Well, why don't you take a course over in EE?"

Hodges: Now he was a historian, isn't that right?

Terman: His son is the historian. I may have gotten the first name wrong, but I think they were both Robert [NOTE: the son is Douglas R. Hofstadter]. Anyway, so he said go ahead and take-- what's it called, the 150 series, and it was not taught out of my father's book. That came the next year. But I took the series of courses and really enjoyed them. This was great. But being a physics-major was really important because I'd gotten all of the math [courses] early. The engineers took three units of math in the first year. I took five units, so I was way ahead of them there. The second year I started taking vector mathematics and these other courses, which they [EE-majors] had not seen yet. And I noticed some of these guys who were engineering majors doing mechanical drawing. Now, in [junior] high school I had taken mechanical drawing in the eighth grade. I was one of the leading..... In fact, I think I was the leading mechanical drawer in the class, but I didn't want to spend my life doing that. And I felt very sorry
for these poor guys doing their mechanical drawing, but being a physics-major I had missed all that. And Stanford and Hofstadter said, "It's no problem. If you want to switch over and start taking EE courses, that's fine with us. It'll apply towards your physics major." I took the 150 series of courses, I found it interesting, really very good.

**Hodges:** Now that was an introductory electronics course is that right?

**Terman:** That's right. We did vacuum tubes, and simple circuits and that sort of thing. The next series of courses was my father's course [which I took] in my senior year in 1955-56. He [my father] had become provost so he was not teaching it. So I did not take the course from him, which is probably just as well. I think that would have been a little embarrassing, but I was actually taught by Rambo. Now that was Bill Rambo, not The Rambo [of movies], but I always thought it kind of amusing. My first really serious course in electronics was taught by [Dr.] Rambo. He had worked for dad at the RRL, Radio Research Laboratory, and had come to the Stanford faculty, and was a very good teacher. And so I really got into it. I took some other EE courses and that was it. I said, "This is it. This is where I'm going to do graduate work."

**Hodges:** You were also, I think, involved in the Stanford radio station.

**Terman:** Yes. As a freshman I'd gone over there and said, "I'd like to do a radio broadcast, be a disc jockey on jazz". Because in high school my junior year I got interested in listening to jazz. I play trumpet, but not very well, and so I wasn't performing, but I was listening. And I had a fairly reasonable record collection. I knew some friends who had really good record collections. So I went over there and they said, "Well, we're going to start our regular broadcasting next week. Can you do a half hour program?" I did a half hour program, and they said, "Okay, you're on. You can do this program in the afternoon"… not prime time [of course]. I did it from my freshman year until the year that I graduated. I think it was June of the year. I graduated in '61. I actually finished the thesis in '60. It was the June of 1960. So it was the longest running show, probably, on KZSU ---KZSU being the radio station at the university. But also they would broadcast Stanford athletic events. I had this interest when I was younger of being a sports announcer because you got to see the sports and you could describe it to the people. And so I had the chance of going out and announcing Stanford sports games - basketball, baseball, not the football, of course, that was done by real professional announcers. But it was a lot of fun. I realized I wasn't going to be a sports announcer. It was pretty nice, an interesting thing, but I just didn't have the talent for it, but it was fun. I did it. [I said] "I've been there done that," and I moved onto something new. Great fun.

**Hodges:** How about your social life at Stanford?

**Terman:** At Stanford I was very fortunate to be in Five-East, which is the fifth floor of Encina [Hall]. All the freshmen who lived on campus lived in Encina Hall. Five-East was the top floor, and should there ever have been a fire in Encina, which was pretty much of a firetrap, I would think, by current standards, we never would have gotten out alive. However, it was a great group of people, and fortunately nobody lit anything. I ended up living off campus with some people from that group in the next four years, five years actually, and I also met some people who became members of an Eating Club. Stanford had the only Eating Clubs west of the Mississippi. The Ivy League apparently has a number of Eating Clubs. The Eating Clubs are where people eat together, but they live wherever they want, unlike fraternities where people eat together and live together. And it suited my flexibility. And the group that I got into was called El Capitan, and it was mostly focused on people who were in Rams Head, which was the dramatic
organization, music people, actors and so forth, but it was a great group of people. Once again, it's life, meeting different people with different backgrounds and interacting with them. And so I stayed there until Bobbie and I got married in 1958, and then I came back to actually play softball. We had won the eating club championship six years in a row and went to the semifinals of the softball championship of the university three out of four times. I wasn't the best pitcher in the world, but I was good enough that we did pretty well. And it was just fun. You met different people. Members of El Capitan still are in contact. There's a group of people around [the class of] 1956 who graduated plus or minus a year or two who still get together 50 years and more later.

Hodges: Did you have some summer jobs or other experience during your undergrad years?

Terman: [As an] undergrad, I worked at Hewlett Packard, [but it was] not anything technical. They had me on the assembly line assembling signal generators. Again, met a bunch of people, different people, but I'll tell you, one of the things [I found] working for three summers at Hewlett Packard those people loved Hewlett and Packard. They thought it was a great company to work for. Hewlett and Packard would walk around and talk to people. [The employees] loved the company and they loved the treatment that they got. They loved the picnic that was put on every year where Hewlett and Packard would serve food. And it was just, it was a great experience. I didn't learn anything technically, but it was a great experience.

Hodges: it wasn't a very big company at that time was it?

Terman: At that time the first time I logged in was number 395 and the second summer it was 495. I don't remember what the third summer was.

Hodges: You didn't get to keep your number.

Terman: That's right.

Hodges: So did you go onto graduate study immediately after you graduated?

Terman: Yes. I was fairly tired….I was kind of burned out with courses, and so I had talked about going r [to HP]. I was saying, "I think what I'd like to do, really, is to work for Hewlett Packard and take half time." They had the work-study arrangement where you worked half time [and went to graduate school at Stanford half time] And a fellow named George Bahrs who was an associate professor, said, "No, I'll arrange that you can do research half time and take half time in courses." Now it turned out it didn't slow me up [getting a PhD] that much because I got the research going a little bit earlier and I still took enough courses, and I had enough courses from my physics background that I didn't have a lot of courses I had to take. So that really worked out quite well. Some people will ask, "Why didn't you go to another school?" I'd been in physics. I was now over in EE. I had changed [departments]. I felt that was enough of a change. I knew the place. I knew the people, and Stanford was a really up and coming electronics area.

Hodges: So was your bachelor's degree in physics or in electrical engineering?
Terman: It was in EE. I remembered, I actually graduated with exactly 180 credits, nothing extra. And the last six credits the last semester I took an independent study, which I read over the theses of a number of recent Stanford PhDs. And it was interesting because you started seeing their work and saying, "Look, that's what they're doing. That's what real leading research is."

Hodges: Did you take your quantum mechanics and solid state in the physics department or were they doing that in EE already?

Terman: I took solid state about four or five times, the introduction to semiconductors and transistors. The real quantum mechanics, the really serious quantum mechanics course I signed up for as a graduate student. I sat through two classes and I went over to John Moll, who was my thesis advisor, and said, "I don't understand what these guys are saying. These guys, the physics guys are sitting around nodding their heads and this is totally over my head." And John Moll said to me, "I don't understand quantum mechanics either. I never took it," and so we agreed I would just drop the course. And I never took quantum mechanics per se. The physics of semiconductors was another thing. I took that. That introduction was never a full course, but it was like two, three, or four weeks, and I took that three or four times.

Hodges: So you weren't ready for lasers when they came along…?

Terman: That's true. That's true.

Hodges: You always stayed on the electronics side…

Terman: On the electronics, the devices, and the processing, which is very interesting also, and the circuits.

Hodges: I see that your first project was looking at solar cells.

Terman: Yes. I had started out, as I said, working half time and Joe Pettit, who was my first thesis advisor and became the Dean of Engineering in the middle of the first year, I think. So he went off and I was sort of at loose ends and somebody, I think my father, but I'm not sure, had arranged for Gerald Pearson to come [to Stanford] on sort of a lend lease [sabbatical] from Bell Laboratories for six months. And he arrives, interesting guy, big cigar, and you'd say, "Oh boy." He never lit the cigar but he always had this big cigar. So we sat down and he said, "Well, nobody has measured"– oh, he was one of the inventors of the silicon solar cell. And he said nobody had measured the spectral response [of solar cells], the response to different wavelengths, so that would make a good project. And I said, "Hey, this is great."

I went over to the Physics Department and got a monochromator and I learned how to make solar cells, which were not very difficult – just a diffusion and a contact to it. And you make them [the diffusions with] different depths, and you would get different spectral responses depending on the depth [of the diffusion], [use] the monochromator [to] measure the response. Things were going very well, and then he [Dr. Pearson] went back to Bell Laboratories, and I finished the project up on my own. I was sort of at loose ends - he's gone. But, I went ahead and finished it up, did the measurements, sent them back to him, and he said, "Fine."
I worked out a rather simple [theoretical] model of what was happening that explained the response curves, and sent it off to *Solid State Electronics*, and they published it with minimal changes required. Now, that could have been a thesis. When I look back now I think, “Hey, wait a minute. I spent another two years working on a thesis. What was I doing?” But there’s nobody advising me to say, “Well, we should write this up and you get a thesis.” So I did it [and went on to do my actual thesis]

Then I went back into a little bit of work with Bob Scarlett, who was a circuit person. And then John Moll came [joined the faculty] from Bell Laboratories. And one fateful day, Moll was walking down the Stanford Electronic Laboratory’s [SEL] hallway in one direction and I was walking the other direction, and he said, "It's too bad that you're over in the circuit area because there's this interesting project that might make a good thesis." And I said, "Explain it to me.” So we talked about it for five minutes. I said, "That sounds really good. I like that." And Moll went to Scarlett and [he said that] there was no problem [for me to move over]. And so I went over and worked with John Moll. Moll had overlapped Pearson, I think, by a few months at Stanford. And so everything went perfectly well and [it] was a great thesis project.

**Hodges:** That's the one piece of work of yours that's actually known under your name. So tell us a little more about your thesis work.

**Terman:** Well, it was to measure the surface states at the oxide-silicon interface [using C-V curves of the MOS structure as a function of frequency]. And what [turned out to be really] important [is that] is that the MOSFET device, which is the key to the semiconductor VLSI high levels of integration that we have now, employed the MOS structure. And so that interface between the oxide and the silicon was really key to understanding the characteristics of that device. Now, at that time the MOS device had just been invented and it hadn't gotten into use. I didn't know it existed at the time I was working on it [the MOS capacitor]. But [Dawon] Kahang and I forget who the other guy was [it was John Atalla] [invented the MOSFET device].

**Hodges:** That is the MOS transistor?

**Terman:** The MOS transistor device.

**Hodges:** But you were really working with a capacitor….?

**Terman:** Just the capacitor of the MOS interface. It was the ideal thesis because you had a theoretical curve, and you had a measured curve, and the difference was due to surface states, and surface states were not reproducible [at that time]. So nobody could say, “Your measurements are wrong.” They had to be right because the difference was this characteristic, which was indigenous to that specific device that you were measuring. So I said, "This is a terrific thesis." Other people were designing amplifiers and they had to measure the characteristics of the amplifier and see if the characteristics matched with the theoretical characteristics. So I made a bunch of capacitors, [and] measured the capacitance characteristics.

**Hodges:** So you went in the lab yourself and fabricated devices?

**Terman:** Yes. Stanford had a [very primitive] semiconductor [process] laboratory. It was in the second floor of the SEL building and it was not a laboratory [clean room facility] like we have today. It had two
furnaces in it. You could do diffusions in one. You could grow oxides in [the] other. And it was on the second floor. And during the summer days when it got hot [with] a couple of furnaces running in there. It would get very hot. You'd open the window. This was not a clean room (laughs). So, yes, we got a lot of surface states, about ten to the twelfth, which is two orders of magnitude more than people get now, maybe three [orders of magnitude]. But I made them and measured them.

We had a theoretical curve. The theoretical curve did not match the actual curve, so Moll had me look at this paper by Garrett [and] Brattain," a couple of [Bell Labs] people, and it showed some theory. [I] Took that theory, applied it and then got a curve, which showed the right characteristic, except, of course, the surface states showing the difference. That was one important [aspect] where Moll pointed me in the right direction. Got that, and got the [theoretical surface-state free] characteristics, and now what you did is you measured the characteristics and you got a complex-impedance, which was a resistor and capacitor in series [in parallel with the] capacitance [of] the MOS capacitor. [In fact, there could be a number of series-connected RC equivalent circuits, each with its own time constant, in parallel with the MOS capacitor, each circuit representing a group of states with a time constant depending upon their location in the forbidden band. The total of the parallel-connected RC circuits were a complex impedance, measured over a range of frequencies].

Now we had the surface states. So what I did was to use circuit theory and go back and take that complex impedance, real and imaginary parts, and work back to actually decoding it down to the resistor and the capacitor [of the parallel connected RC equivalent circuits]. And came up that there were basically two large peaks of surface states, one fairly high up on the band and one a little bit lower towards the middle. And the rest of it [surface state densities] was within the error of measurements. It was pretty interesting to actually go back and say, "Yes, we can now look at this and understand where these states are, and that they're there, and their densities." And, of course, people would spend years after that getting those states down, getting rid of them.

Hodges: When you make those measurements, you're using a changing DC signal with a small AC signal as a probe?

Terman: Yes. It was an AC measurement and you would change the bias, DC, so it would change the depletion region if I wanted

Hodges: And would you also do it at different AC frequencies?

Terman: Yes, the frequency was important because surface states, depending on where they were [in the forbidden band], would have a different characteristic [time constant]. That's why, in fact, a given surface state population would have one capacitor and resistor. If you had another one you would put another in parallel and so forth, so you got this complex impedance and you had to work down, work back to determine the resistors and capacitors and thus the densities that the states were.

Hodges: Yes. That was pretty complex in those days because there were so many surface states and they were at different levels.

Terman: yes, but I was surprised. I got this big difference [between the theoretical state-free curve and the measured C-V curves] and I was surprised that in fact I could work back to where there just seemed
to be [basically only] two levels, and that was interesting. So that was my thesis. Moll said, "This is fine." I took about a month to write it up. It was a hundred and some odd pages. Went through my orals and justifications.

**Hodges:** Let's see, in those days you did that on a typewriter.

**Terman:** Yes, and it was great fun, let me tell you.

**Hodges:** You had to do your drawing that you hadn't ever...

**Terman:** They (the Stanford EE Department) did drawings. Stanford was very good about this. They treated their students well. You gave them a drawing and they did it for you. And the final [bound] version was typed up by [a] secretary [or] staff [person], but if they made a mistake they went in with a razor, scraped it out and retyped it. It was scary.

**Hodges:** Before they had invented whiteout.

**Terman:** That's right, and it was pretty scary because you had to get it right, and you had to go and change things if you didn't.

**Hodges:** So you wrote an article on that and submitted it to *Solid State Electronics*. You were already known by them as an author.

**Terman:** Yes, that was interesting. I wrote it up [in the East]. I went to IBM, and after about six months said, "I better get around to writing this up." So [in the] middle of '61 I went and wrote the article, sent it off to Crawford Dunlap at *Solid State Electronics*, [who] was their editor for decades, I think. And about a month later the first review comes back, and the first review says [in essence], "This is garbage. He's interpreting everything wrong. There is no value to this whatsoever, and you should reject it." Talk about depressed. Now first thing I wrote back to Crawford Dunlap saying, "Look- (I played the John Moll card) "Moll thinks this is good, therefore it must be good. Secondly these are what I measured." And I realized sometime afterwards that the reviewer - what he said was wrong with it was [in fact] not right.

There was a physics professor in Russia, Tamm, who in about 1920s, somewhere in the 1920s, showed that if there were semi-infinite lattice that terminated abruptly you had surface states at that interface. What we were observing were what at that time was known as Tamm states. The guy who was objecting was saying, "Well, the states could have gone into the silicon," but that's not what happens as we know now. Well, about a month or a month and a half after the negative review, in came another review saying [in essence], "This is terrific work, absolutely great. Publish it immediately."

The first guy had said the article was too long. I had reduced it by about a third. That was pretty embarrassing because, yes, there was a lot of [extra material] in there. And Atalla had seen the reduced version, and he said, "This is great and publish it." Well, I said Atalla. About five or ten years later John Atalla, who was at Bell Labs and went to HP, told me that he was the guy who had done the second review, and I thanked him very much because he saved me from that paper, perhaps, from being rejected.
I had literally hundreds of reprints. I had a hundred reprints from the *Solid State Electronics*. IBM ran off 200 more and I was through most of them before people stopped asking me for it. And so it was an important paper, and [later] some people at IBM said, "Hey, we're looking to the MOS device. Who knows about this measurement? Oh, we have Terman working down in the Research Center." So I had a chance to give talks on how to do it. So it was a landmark paper, and I owe it all to John Moll for suggesting it.

**Hodges:** So in those days there was quite a lot of action for new graduates in EE, and you did go to IBM, but did you have other options that you considered?

**Terman:** Yes, I interviewed at Hewlett Packard, Fairchild and then did a tour in the East at Philco-Ford, TI and IBM. They [all] seemed to be the reasonable. And I actually remember sitting down with Noyce, and Moore, and Jay Last, and Jean Hoerni from Fairchild. And looking back this was a pretty stellar group to be interviewed by. But Bobbie and I agreed, "Look, let's go back to IBM for a couple of years. We'll work there. We'll enjoy the snow and the change of seasons and what it's like back there, and then we'll come back and work for Hewlett Packard, which is the obvious thing to do." John Moll was at Hewlett Packard at the time. And so everything went exactly as we planned it except for the coming back [to the West Coast] bit.

**Hodges:** Right, so I think you should tell us how you met Bobbie and when that occurred.

**Terman:** Bobbie was the result of a summer job. The first year after I had graduated, I went to Sandia [Labs] and worked for a summer job down there. I had a little trouble with getting clearance so I didn't spend the full summer there. Not because of me, but you had to have a Q clearance which is pretty difficult to get. Then the second summer [1957] some of the people that I had lived with off campus, and their friends and [other] people that I knew, were working down in Los Angeles. They had graduated. So they said, "Why don't you come down and work here?" I said, "Hey, this would be great," and they had an extra room. So I applied for a couple of places down there and TRW was hiring so I got a job working at TRW, Thompson Ramo Wooldridge, at that time. So I went down there and Bobbie was going out with somebody that I knew from school [in this group], same class. And mysteriously, surprisingly, he got transferred up to the Bay Area, so he was gone. She started going out with another person there who got mysteriously, surprisingly, drafted into the Army.

**Hodges:** Had you arranged this?

**Terman:** Well, we don't say anything, but the third guy in the group decided he was going to volunteer to be drafted because he was going to get drafted anyway. So that left one guy who was going with another girl, and me. And so I called her up and she said, "Yes." And we just went out steadily from then on. The first date was the Hollywood Bowl. The second date was Disneyland. The third date was the Hollywood Bowl, and we just did our thing after that. And she went back to Northwestern [in the Fall] and we talked about her coming out, coming to Stanford, and she ended up going to Berkeley, which I'm not holding against her, for one semester [in early 1958], and then did summer school at Stanford to catch up for some requirements she needed, and we got married that summer. So it was the next summer in 1958. And that's been--[well,] the rest is history.

**Hodges:** Ever since. So when you went to IBM you said you made a decision to go into the Research Division. You were offered alternatives?
Terman: Yes. I had been there [to interview] and the people who were doing research on the MOS device and the [related] solid-state physics, Dick Rutz and John Marinace, well known people, were not there. So they [IBM] came up with some offers from Research, and also offers to be in the development division. And I said, "Look, I didn't have a chance to talk to these guys in Research who were doing what my thesis was on, so could I sort of accept the offer, but wait and make the decision [on where I would go] until I got there?" And they said, "Sure, that's fine." You don't do that kind of thing these days, but in those days they were happy to do it. So I came back[to IBM]. I had been recruited to visit IBM by Rex Rice who was really a computer guy.

Hodges: Systems guy.

Terman: Systems guy, yes. And I'm forgetting the other guy. The name will pop-up to me, who had been a Stanford graduate - Ed Davis, who was a Stanford [PhD] graduate and] who had gone back there [to IBM] and was in the development division. So I came back [on the original recruiting trip] and I interviewed Rex's group, I interviewed [someone] in the solid-state physics [group], I interviewed with Davis' group, and [with] some other people, and gave a talk about the MOS measurement. And then when I came back [when I joined IBM] I once again talked to these people [including Rutz and Marinace]. And I said [to Bobbie], "The guy that's really got the enthusiasm and has got a group that is enthusiastic is Rex Rice, and so this could be a changer, a real game changer to go into a systems group."

They were designing a computer that would directly implement a high-level language like FORTRAN in hardware. And I said, "This sounds really interesting, something different." I had not really gotten involved with computers before, but this [project] was interesting. And it was a very, very good decision because the first two or three months I spent just learning about computers, at that time they were quite primitive, but how they worked, how they were organized, why they did what they did, and there were a [very good] bunch of [computer] people that I worked with. And so that got me going in the right direction.

They got to the point where they had a logic outline [of the system] and I spent some time doing the logic design of how to implement the various decisions, instructions that were being done. And so that was very good, but the program died. You've never seen [a computer] that has FORTRAN in hardware. But one of the implementations was going to use what was then something really new, which [was] Read Only Memory (ROM), a stored program instruction set. And John Meggitt was the guy who was doing this. He was from the [IBM] British laboratory. I don't think he was the first person to do it [stored instruction program set], but at least in our group, in IBM [I think] he was.

There were several different competing technologies [for the ROM/ROS]. In a Read Only Store (ROS), [the information is stored in the memory, [and can only be read]; you can't electronically write the data. [A technology under consideration in Yorktown was] a thick [magnetic] film Read Only Store [TFROS]. [At the time,] people were trying to replace core memory with magnetic [film] memory, but this was a read only version. You had a thick magnetic film and if it switched it would give quite a good ONE signal out because it was quite thick and had a lot of [magnetic material switching]. FOR a ZERO, you had a little permanent magnet that was [over a given bit if you did not want it to switch], and to give an idea of how dense it was, the permanent magnets were on punch cards and you punched out the magnets [if you wanted the magnetic film to switch]. That was not very dense, but you had a simple way of putting information into it. Well, we were working on this technology.
Technically, we were a great success. I went over to the [IBM] British laboratories. They had a Capacitor Read Only Store [CCROS] and a Transformer Read Only Store [TROS] [as competing technologies]. And the transformer one [TROS] had been used for [an earlier] system and that was the technology that won out.

But looking at the TFROS technology, I gave a paper on it, and I pointed out that because it was a thick film you could drive it with a relatively small signal to sense it. It would switch for a ONE and it would give a signal that was pretty close to logic levels out. The logic levels here, of course, being ECL type levels, 500 millivolts or so. This [was the signal that] would come out for a ONE. And [the delay between driving the array and getting the output was only] 19 nanoseconds. Now, [in those days] you had to explain to people at that time what a nanosecond was. And I remember giving the talk and this gasp went through the audience at 19 ns access. And I said, "We have done the decoding, gone through the array and the sensing, and 19 nanoseconds later you have the output." But most of that, 14-15 nanoseconds of that, was lost in the array.

And so when the project was over I looked at this thing and I said, "We've got to figure out a way to reduce that array delay. Why don't we put a circuit there?" Integrated circuits were beginning to be good. There’s some hope that you would actually have an integrated circuit that would, in fact, have reasonable yields. Let's see what happens if you put an integrated circuit there [in the array as the information storage bit].

Hodges: Now you were thinking about supplanting, supplementing the magnetic device or replacing it completely?

Terman: Replacing it, yes. At that intersection between the word line and the bit line you would have the circuit.

Hodges: And what year was that?

Terman: This was '63 probably. Yes, that was pretty early. So we worked on it and we went through [possible] the cells and eventually Pete walked into my office and said, "This is the perfect cell" and it was. It was a six-device transistor cell. It has a cross coupled flip flop and then two devices, one coming from each node going out to the respective bit lines, They were MOS [devices]. That was a big thing. It had to be MOS. There was bipolar solid-state memory being done at the time but the density was just terrible. It clearly was not going to work [as a high density memory]. And then Pete said, "By the way, we’ve seen this cell before." It had been invented by Jack Schmidt of Fairchild and he had published [a] paper in [a short lived magazine] Solid State Design. I talked to him a few years later and he said they were sitting around [at Fairchild] and he came up with the cell and they decided not to patent it because it was just a cross coupled flip flop. [So] he didn’t patent the cell, thank heavens, because otherwise you would have gone through the whole bit with patents. [This cell became the standard static RAM cell in the industry.] Anyway, so we [used] the cell and that was one thing that was very important. The other thing is.... I think in '64 I was sitting in the Solid State Circuits Conference (ISSCC) in Philadelphia and a fellow named Kurt Goser from Siemens gave a paper.

Hodges: Karl.
Terman: Karl? It was Kurt Stein and Karl Goeser.

Hodges: Karl Stein and Kurt Goeser. You were right the first time.

Terman: His paper was on how fast MOSFET devices turn on... Now MOS [circuits] at that time were very slow....microseconds sometimes in switching. [But] he pointed out that when the [gate] voltage is applied on the gate and the field comes in vertically to the surface, the [source-drain] current was only limited by how fast the carriers would flow [into the channel] from the source or drain or from the source into the channel area. And he said, “As you shorten [the source-drain dimension] they start flowing very quickly and the actual device [channel] turns on in one or two nanoseconds, [which] was extraordinarily fast at that time.

So I’m sitting there and I said, “That’s really interesting. These devices are fast.” The [circuit delay] problem was the device didn’t produce much current. It had to drive a capacitive [load]. The capacitance had to be charged up to get [a voltage swing of] five volts, or in fact at that time eight and a half or maybe even 12 volts. so it took a long time to develop that large voltage swing. So I said, “We’ll have an array of MOS devices but we’ll go into bipolar devices off chip so we can [sense the current instead of requiring a large voltage swing], We’ll drive them [the memory array] from bipolar devices. That'll drive the word line fast. And when the [sense] current starts to flow [in the memory cell] it doesn’t have to go up five volts to trigger a circuit because [it can go] off chip into the base of a bipolar transistor [where] we were getting a beta [current gain] of ten or 20 or [even] 50. It [the bipolar device] amplifies it [current]. It gets up to logical [levels] very quickly and we would able to read the array really fast. [The key was decoding and driving off-chip from bipolar devices, and sensing off chip, also with bipolar devices.

We wrote a paper for the Electron Device Transactions. They had a special issue on memory and we wrote a paper which was [on the potential of] MOS memory. We did some simulations, using this approach, and reported that you’d be able to get access well under 100 nanoseconds. I think we reported— I can’t remember the exact number - it was [well under] 50 or 60 nanoseconds ,which [at that time was] really, really fast. I remember the introduction of the issue said that Terman and Pleshko, or, Pleshko and Terman, reported access times considerably faster than expected from MOS at the time.

That approach then came into the IBM mainstream. We were designing a chip and it met two other [technology] streams within IBM, very important. One was that IBM up to that point was using core memory, and they had a 750 nanosecond access core system. They knew they could design a 500 nanosecond core system. So, yes, if they pushed it, maybe they could get down to 250 nanoseconds. They could see no way to go beyond that, just simply impossible because to make a core go faster you [had to make] it smaller. If you made it smaller, you had to run the XY select and the sensing wire through the core. IBM had marvelous machinery to automate that wiring, but when it got down to 500 nanoseconds they said, “It’s really tough. Two-fifty maybe we can do it, but 125 you’ve got to be kidding folks.” And so they still continued to design the 250 ns system but they took a flyer and said, “We’ve got to see what we can do with MOS devices.” So it was this approach of doing the off chip sensing and driving that was important. The fact that they needed something to replace cores [met with] the innate perversity of Research. Research had people working very hard on MOS devices but it wasn’t the [conventional] P channel device which the industry was using. The industry was using P channel devices because if you had a large number of surface states, it raised the threshold [voltage]. That means you had to raise the supply voltage but the device would still work as a digital device.
Hodges: You could turn it off?

Terman: [Right]. You could turn it off. But if you had an N channel device and you had a lot of surface states the device could become depletion mode or very low threshold [if it had a high surface state density]. You couldn’t turn it off and it was useless [as a digital device]. Around ’67 or so the Research device and process groups were beginning to get the N-channel device under control, getting technology that actually worked. The N channel device, of course, was important because the N channel device had three times the mobility of the P channel device, and that’s much more current that you can get out of the device than with the P channel device so that was very important. It gave us speed. It also enabled us to lower the voltages which was important and cut down the power.

Hodges: Now along the same timeframe there was the first use by IBM in computers of semiconductor memory which was the cache memory right?

Terman: Yes.

Hodges: So this was another approach to speed?

Terman: Yes. That was the so-called Farber-Schlig cell, which had a 16 bit [chip] and then a 64-bit [version], and they were bipolar. They were fast, no question about that but they were never going to get the kind of densities that you needed for the mainframe [memory]. The MOS device was much better for density [and thus cost]. Yes. [If] you lost power an MOS device [memory you] lost the [stored]information [i.e. it was volatile]. But it was pointed out that if acore [memory] lost power they never trusted what was in the core. They always re-booted the information. So the dynamic-ness of a memory, [like] the semiconductor memory, wasn’t a problem. And [with] the Farber-Schlig cell, [though] they got to 64 bits, they were really pushing the technology at the time and [could not get] the density [needed].

Hodges: So it was never thought to be more than a cache?

Terman: Yes that’s right.

Hodges: So where did the push for the MOS main memory come from? Did it come from the [product] divisions or did it come from Research?

Terman: I think [from] the Divisions. Research was working on it, but the Divisions knew they had to find something that’s extendable below 500-nanosecond access. With the problems that the core was going to face they were very interested. And so we started, “we” meaning IBM as a whole. The Research people and the people in the development divisions started this [joint] project to take the N channel technology, making sure that we could actually make N channel devices with reasonable yields and the performance, to the right [system] specs that we had and pull this altogether. So it was a group between Burlington, Fishkill, and Yorktown, working together to bring this about. There were a lot of meetings going on back and forth on this and we ended up making a thousand word, I think I’ve got this right, a thousand word, probably 64 bit, maybe 32 bit, memory [prototype].

Hodges: Did this have bipolar peripherals?
Terman: It had the bipolar peripherals. Off-chip sensing...

Hodges: Did that go into production?

Terman: Oh, yes. The prototype was the basis for the system that went into production. It went into the Model 370-145. It’s what really, at least we like to think, established semiconductor memory and said to the industry, “you can do semiconductor memory, and you can make it with enough yield and it will be reliable”. The chips they were making were speced out at 100 nanoseconds but it was actually running at about 65 to 70 nanosecond access. The other [industry] approach was Intel, the 1103. The 1101 is a three device cell, I think, and the 1103 was the one device cell, if I remember them right. They were straight DRAMs. Dennard had already invented the DRAM cell.

Hodges: The 1T.

Terman: The one transistor cell, but there was the three transistor [DRAM] cell which Intel came out in one kilobit in around, what ’70 or ’71? But that was kind of small [in bits per chip]. But then very quickly the one transistor cell came in as the main store. But remember the one transistor cell required on-chip sensing, on-chip decoding, on-chip the whole bit. To get under a microsecond access you had to work pretty hard. The [IBM] mainframe main store needed something around 100 nanosecond access [which was much faster than the 1T cell could deliver], so the MOS six transistor cell with off-chip driving and sensing was what IBM used, though it was clearly limited in density in the long run, because the decoding and sensing was off chip, the number of chip connections would get very large as the number of bits/chip increased.

Hodges: And expensive.

Terman: Oh, expensive. But the system worked. Remember it’s not the cost of the chip. It’s the cost of the system and the performance of the system and that’s what sold it.

Hodges: So the memory system, the main memory system what do you suppose that cost?

Terman: Oh, I have no idea.

Hodges: Even in those dollars?

Terman: In Research we didn’t pay any attention [to cost] and probably the numbers were artificially high or artificially low because they were internally generated. The systems people would pay internally some money for it and that wasn’t real money [because it stayed within IBM]. It wasn’t a competitive bid. If you had gone outside maybe but we were the only people making N-channel at that time.

Hodges: So how was all the interconnect done for that mixed technology?

Terman: We had flip chip ball grid arrays so there was enough chip contacts, although it turned out [the contacts] were on the chip periphery. We didn’t have that many going off-chip as it turned out.
Hodges: So on one of those ball grid modules you’d have both MOS and bipolar chips?

Terman: Yes, right. If you put them [on separate modules], if you put the bipolar off the substrate then the delay would have been a lot longer so, yes.

Hodges: Right. And those were just those little half inch square modules right?

Terman: I guess it was SLT by that time. Well it was SLT or definitive SLT but it was a number of chips on the same, multi-chip modules, yes. That chip, the 1,000 [bit chip], it was called the Reisling chip because the final was designed in Germany and the chip went to two kilobits and it was used in IBM systems for almost a decade.

Hodges: Production.

Terman: They finally took it out in the late ’70s, yes late ’70s because it simply didn’t have a high enough density. You had to go to the one transistor cell because the densities at that point were getting up to 64 kilobits.

Hodges: And so for a decade you made these mixed technology modules?

Terman: Yes, yes.

Hodges: Wow, I didn’t know that.

Terman: Yes.

Hodges: I remember a visit to Boeblingen in the later ’70s and saw the production line.

Terman: That was…Sindelfingen was the actual place.

Hodges: Sindelfingen yes.

Terman: They [the IBM Sindelfingen and Boeblingen facilities] were close together.

Hodges: Right. At that time was it policy that you tried to keep the MOS process as simple as possible and it like five or six masks?

Terman: Yes.

Hodges: And then there was the factory in Burlington which had more automation that produced that kind of process, right?
Terman: Yes. Remember people went to MOS because MOS had four masks, instead of [the] six masks which bipolars used and so they said, "Well this is going to be cheaper [and give better] yields" and all that kind of stuff. And, of course, now that looks like it was nothing. Now, what do we have - 24, 30 mask processes now? So, yes, [at that time] you tried to keep the number of masks down, but as time went by, well you needed a second level of metal. One level of metal wasn't enough because if you had one level of metal you had to run an awful lot of things on diffused interconnections which were really slow and high resistance, and capacitance, by the way, and so you kept adding levels of metal. And there was a brief time it said four to five levels of metal was all you needed but it just kept on growing from then. Also, a number of masks have been added to improve the device characteristics.

Hodges: I think IBM stuck with metal gate longer….It was metal gate in Sindelfingen right?

Terman: Yes.

Hodges: Original aluminum gate.

Terman: Yes the aluminum gate... We did do metal gate logic, but what drove us through the '70s in the memory business was the Self-Aligned MOS (SAMOS) process which was an aluminum gate process. .

Hodges: Which meant the metal gate was the mask against the implant for the source and drain?

Terman: Yes, right. [actually, not true…]

Hodges: Self aligned.

Terman: It was self aligned, yes, but it was self aligned in another sense - they had a really neat {DRAM} cell and they laid it out and it was small by the standards of the day but it involved cutting a hole in the oxide layer [over the channel] and then re-growing the gate oxide. Bad choice. Then you put metal over it. This gave you a very dense memory cell. Wow! It also gave you a dreadfully slow MOS device, but the cell was self aligned in the sense to the effect that the area of the cell was small. So I remember in about '73 or '74 they said, “You guys look at the silicon gate. The silicon gate is a much better process.” And we looked at it and they said, “Yes, but the cell is going to be 20 percent larger and we’re going to have to delay our process. We have deadlines to get this out and that would take us six months [longer].” [However,] one of the drawbacks of the SAMOS process was they “discovered” hot electrons because it had an oxide nitrite interface [in the gate insulator region]. Oh-oh big problem, folks, because that interface is where you trapped hot electrons.

Hodges: Back to the Terman method…

Terman: And so that became important, yes, this whole business of surface states and the whole hot electron problem made some people’s careers and [increased] understanding what was actually happening there. So it was one of those really interesting effects that happened that was good in the long run because we understood the device physics of MOS devices [better] but it also slowed up that program by a few years. The IBM 1T DRAM eventually [was a product]. IBM actually was the first people to make a 64-kilobit chip for production. However…
Hodges: Still SAMOS I think.

Terman: It was still SAMOS and I remember one review that [said that] IBM was coming out with the highest density, slowest, highest power and biggest chip in history. But at least they were first, somewhat of a snipe, [but IBM was first].

Hodges: So the outside speculation…that’s about the time that Intel was on the ropes and IBM made an investment in Intel.

Terman: Yes.

Hodges: And, of course, Intel had taken the silicon gate right from the start and I'm wondering….many years have passed. It should be all right to tell the story. Did IBM learn a lot from Intel in terms of moving over to the silicon gate?

Terman: I think there were people up in Burlington who were saying “silicon gate, silicon gate, silicon gate”. I don’t know whether they interacted directly with Intel or not and whether there was a transfer. I'll be honest with you…IBM was not really open to taking, bodily lifting things from the outside industry. We thought we were the best and by God we were going to do it our way. And so I’m not sure what we did, how much we did. It was not down at the Research [Division] level. If it was done, it was done at the division level.

I do remember going to Intel, with Dale Critchlow somewhere, probably late ’70s or early ’80. We visited Intel [with some] research people and some people from the divisions and we came back with the conclusion that Intel didn’t know anything that much more than we did, so nothing happened. I think we just decided not to do a joint effort.

Yes, we were late in getting into silicon gate. We should have known better because the silicon gate logic was the key and within a decade became really, really important. To IBM it wasn’t so important because they were making high speed systems. But then all of a sudden, we decided we were going to start making smaller computers, [PCs] and workstations and that sort of thing and we really were slow in getting into silicon gate.

Hodges: Then a potential diversion came along with CCD? At the beginning, people thought this was going to be the greatest memory device.

Terman: Yes.

Hodges: And you could do multi-level memory in one cell.

Terman: Well what happened was, of course, somewhere around 1970, the bubble [memories] came out and they [Bell Labs] were saying, “This is going to be one of the great memory devices of all times.” These little magnetic domains were shifted around. And then the CCDs were a response within Bell Laboratories to the bubble [memory].
Hodges: Exactly.

Terman: Both were shift registers and you looked at the CCD and you said, “Gee that cell is actually smaller than the one device memory cell” because it required no contact within the cell. You could shift things around. And so they were both serial but between the DRAM which was the first level of the main memory and the magnetic storage disc storage there was about four orders of magnitude of access time. And so if you couldn’t fit it into what you had in the main memory you had to wait forever to get it back from the disc. The idea is you put something in between. This was going to be that extra memory level. And IBM was seriously doing this.

I remember in 1976, if you looked at the growth of number of bits per chip on DRAMs, it should have been a 64-kilobit chip but it didn’t come out that year. It turned out to be the 64-kilobit chip was a CCD. And you said, “Oh, CCDs really are good. It’s a new technology. They’re going to go ahead faster than DRAMs.” And I wrote an article, one of those ill-fated articles that turns out to be totally 100 percent wrong, saying CCD memory will continue. It’s got about a factor of three density advantage at the cell level [over DRAM], and the chip utilization (the portion of the chip area that’s in the cells) is bigger with CCDs, therefore it’s going to be more dense, [and thus] cheaper. It’s going to be much better than DRAMs. It won’t replace the DRAM but it’s going to have its own niche.” IBM actually went so far as to [do a design and get some exploratory contracts with [outside companies], but they could not yield the CCD memory so IBM canceled the contract. I remember getting a call about five months later, this plaintive call from a guy I knew down at one of the companies saying, “We can yield them now, no problem. Can you get them to reverse the cancellation?” I said, “I’m sorry, the train has left.”

Hodges: So IBM actually didn’t try to manufacture these things themselves?

Terman: That’s correct. Now, the last gasp [for CCD memory] was, as you say, the recognition that CCDs are analog devices. In bubbles it [the storage] was [digital] - you had a little domain or you didn’t have a little domain, one or zero. With CCDs you [could have] one electron, two electrons, ten to ninth electrons, ten to the nineteenth electrons [so you could store 4 or eight or more levels, representing 2 or 3 or more bits – multilevel storage]. So we looked at this and did a complete analysis of how you could do multi-level storage. There was no multi-level storage anywhere in the digital world at that time. We came to the conclusion there was one really important trick, which we got from a guy in [IBM] Poughkeepsie. You launch and sense [the charge packet] in the same [physical] cell, so any geometric difference in the place where you were launching (creating) the charge packet, and [were] sensing the charge packet after it had shifted through the CCD array, was cancelled out [eliminated]. That was critical because you were dealing with a minimum sized [cell] area and the statistics on that change could wipe out the sense signal difference between levels .

Hodges: But you couldn’t launch with the same driving circuit used for sensing.

Terman: No, but you used the same geometric area. To create the charge packet with a voltage, and then sense the voltage that the charge packet created after shifting through the CCD array.

Hodges: The storage cell.
Terman: It had different circuits [for launching and sensing]. And we did some other things. Normally, CCDs it would go like this (moving finger back and forth). They would just go back and forth and you’d come out and you’d actually go through and come up the other side and come back to the same input place. That was called a boulestendrofic path I think was the word. Somebody dug up this word that I’ve never heard before or since and I’m [probably] spelling it wrong because it’s not listed in a dictionary and I can’t find it online, but it’s the path that a bull would follow in plowing a field. I thought that was kind of interesting. It meant if you shifted through a four kilobit shift register, you shifted through every cell and [there could be a lack of complete shift [of charge at each shift]. You could have shifting losses. And if you tried to do multi-level storage, you’d lose [sense signal between the adjacent levels]. Your charge packets could get merged together somewhat. [A much better CCD layout was series, parallel series organization]. You would shift in [a line of cells], then take this whole line and shift it down in parallel [in the parallel section, and then shift it out. So if you had 64 by 64, (4K) bits, instead of 4K shifts you actually had 64 in, 64 down, actually 62 down and 64 out, reducing the number of shifts dramatically [to approximately 190]. So we analyzed this, came up with the conclusion that “Yes, you could do it with decent efficiency of the charge shifting”. If you have two levels, you put your sensing levels in between [the levels, and the sense signal is ½ the difference between the nominal ONE and ZERO levels]. If you have four levels for two bits you have three gaps that you have to [distinguish between] [so you lose more than half the nominal signal difference for each additional bit you want to store]. You’ve lost over half your signal per bit so you lose the sense signal pretty quickly. We gave a paper at ISSCC saying, “Yes, it is possible, but it’s tough.” And by that time, DRAM had moved on and was getting better and better and there was a thing called the number of squares in a memory cell, if you remember that.

Hodges: Yes, sure.

Terman: You take a line width and a gap and you average them. That is what you call a line and then you take and make a square of that dimension and that is the square. How many squares using the particular technology that you’ve got does the DRAM or the CCD cell use? Well, the DRAMs originally were running 25, 30 squares, [versus] the minimum according to a very simple model of eight squares for a folded bit line cell. The CCD was around six squares I think so it was going to be much better [than the contemporary DRAM cells]. But the people laying out DRAMs got smarter and smarter and smarter and the technology got more complex but better in aligning things so you didn’t have to leave [alignment] space [in the call layout]. Clearly the train had left and DRAM was going to win. CCD wasn’t going to win. So CCDs went out and of course were never heard of again. No, that’s not true (smiles). They got a Nobel Prize last year. Good things happened to CCD just because they were analog.

Hodges: Of course because they are excellent imager,s not as memory devices.

Terman: That’s right.

Hodges: I’m not sure we’re going to cover everything here in my outline but a possible interrupt came along when the German guys invented this new bipolar competitor for MOS, MTL, merged transistor. Some people called it I-squared L.

Terman: Yes. They [IBM] called it MTL.

Hodges: So that was an interesting competition mostly within IBM.
**Terman:** Yes. It was invented as a logic that would rival MOS devices but would be a little bit faster.

**Hodges:** In density.

**Terman:** Yes, in logic. It was first done by [Horst] Berger and [Siegfried] Wiedmann from the [IBM] German Laboratory. They had been working with it for a couple of years, and they finally were allowed to publish it at ISSCC. [Then] lo and behold, the Phillips people came up with exactly the same circuit at the same conference. Both the Philips group and Berger and Wiedmann were in Europe, a totally different [independent] effort, but exactly the same circuit, which [Philips] called I-squared L. [As a result] there was a big flurry [of effort in the industry]. “Well, this is going to replace MOS devices.” No, it didn’t because the MOS devices were still easier to make. The MTL/I-squared-L also had a lateral bipolar device, which was a bit tricky to make. But, yes it was a competitor [MOS logic and static RAM], but nothing was going to get to the density of the one transistor cell. [However], it had a static cell and actually there was a time, with some work that we did later on, where it looked like it might be a competitor to high speed MOS static RAM.

**Hodges:** So let’s take a look at your first tour of duty on the research staff; that is, you were taken out of the lab and come in and advised the higher levels in the corporation about how things were going to go.

**Terman:** This was an eye opening experience because you’re down in your department working with other divisions and so forth, so you saw what happened within IBM at that level. And going outside of Research was pretty eye opening and dealing with the management out there. But being in Research and being in, if not daily contact with Ralph Gomery, certainly direct contact with those people, seeing how Research worked, what they did, what their attitude was, was really interesting. Gomery, for 16 years, was the Director of Research and was a “demigod” of his time. Now, he did push Josephson [Junction technology]. He had a strong effort in Josephson junctions, cryogenic logic technology which could make memory, but not very well. People have asked me why was Research successful? Well one of the reasons was that failure was an option. The Josephson project, they got really good people together. It was really exciting. They went in. They designed these things. They were going to be sub-nanosecond cycle time, which at that time was really big [advance over current machine cycles times].

**Hodges:** Juri Matisoo?

**Terman:** Juri Matisoo, Dennis Herrell, also Carl Anderson, and others. Really good people, by the way, really outstanding technical people. They kept making progress, carrying things forward but a problem was that if they didn’t meet all the milestones at a given point on the schedule, they would say, “Oh this landmark didn’t quite work out, so we’ll correct it as we move on to the next stage.” And so they were beginning to have problems. They didn’t quite have things corrected when they moved to the next phase.

**Hodges:** What you’re talking about is milestones along the development path?

**Terman:** Yes, milestones, what you’re supposed to do, what you’re supposed to demonstrate. Finally, a guy at the same time I was there, a guy named Robert Gunther-Moore, came in at Gomory’s request and he was one of these forces of nature. He came into look at the Josephson project. He looked at it and he said a number of things. One of the things was that they were doing 2-D planar geometric Josephson active [tunneling] areas. He said that just the tolerances from the lithography made the Josephson
circuits marginal.. The peak to valley current of Josephson was not large, and they did not have enough
gain in the cell. He said, “You’ve got to get away from that.”

Hodges: Even if you went differential?

Terman: The differential was better but now you’ve lost density. So they went to the edge defined
device, which had a {layer} thickness definition in one dimension, [which was a big improvement] The
project went on for a couple of years after that but it was interesting to watch him work.

Hodges: “Him” was Gomery? You said it was interesting to watch him work.

Terman: [Robert] Gunther-Moore. It was also interesting to watch Gomery. All the Research Division
[Departments] came in and reported to him. Once a year there was a big status report [meeting] and
there were quarterly meetings that went on for various projects, and you saw what people were doing and
how they were doing it, how people ran their departments, how they pulled things together. In some
cases, like the Physics Department, it was pretty independent work, but they also came in and told you
how good they were and what kind of impact they had. And I thought, “Boy, this Research Division is
really something. There’s this incredible breadth of capabilities here, going all the way from device
physics to very theoretical math, [and] stuff in technology and computer architecture and implementing
computer things and into the semiconductor devices, the device physics, and so forth.” It was really an
eye opening experience to be there and see how smoothly things seem to run and how they worked
together and how we were interacting with the divisions. The interaction was not as important then as it
was the second time I was on the staff because Research was a bit more independent [at the first time]
but it still had many things it had invented which had worked their way into the mainstream of IBM.

Hodges: Ralph Gomery himself was a mathematician right through?

Terman: Right.

Hodges: Was there any problem for him in terms of being the leader for the so heavily physics-oriented
and engineering-oriented work of the sort you were involved in?

Terman: He seemed to understand and be very receptive to new ideas. As far as making technical
decisions, the program development and the like, that was done at a lower levels I think. The Josephson
Junction] he was pushing, but I think what he did was to look at it and say, “Here’s a chance to do
something that’s a real breakthrough project. It’s cryogenic. It’s a [new and] different technology but it
would be able to go down to sub-nanosecond cycle times which in itself is going to be a dramatic
breakthrough.” So I didn’t have any feeling of people saying, “This guy’s a mathematician, [he] doesn’t
know what’s going on.” Ralph stayed there for 16 years because he could understand what people were
doing well enough to be a leader and get them going in the right direction and give them enough slack
that they could do things. But, yes, he would cut things off and I do remember a few times when he
would say, “This isn’t working out. You need to find a different direction or something better to work on.”

Hodges: Then you went back to the lab...?

Terman: Yes.
Hodges: I guess you were by then leading a small group?

Terman: Yes.

Hodges: And you were looking at the high speed DRAM and the micro 370 microprocessor in that period?

Terman: Yes. We had a number of things going on in that period. There were about 20 people [in my area], four groups. One question that came up was “Why can’t you have a fast DRAM?. You’ve got a very fast static RAM. Why can’t you have a dynamic RAM that’s as fast as a static RAM?” And the obvious answer was because the cells are different. Okay, given that the cells are different, take that out, set that aside, and say there’s going to take X number of nanoseconds to do the cell. What about the rest of the chip?

And so we started working on how fast could we make a DRAM go. And as we started on this, we acquired a Stanford PhD graduate, Nicky Lu, who was one of the students I recruited who had been doing device physics but then got very interested in this and turned out to be an extraordinarily good circuit designer, [learning from Hu Chao, also an excellent designer in the group]. And we designed a DRAM that was about three times faster than a conventional DRAM.

Now, we did this and a year or two later Hitachi did the same thing and came up with a high performance DRAM [product]. But it turned out there was no market for it. [The system] didn’t need the extra speed. But what was interesting was you had all these [circuit] tricks that we had done to to get the speed and as time was moving by, the logic part of processors was getting faster and faster. The DRAMs were not getting faster at anywhere near the same rate. So we found the circuit tricks that we had done were beginning to be used in some of the successive generations of DRAMs, so the work wasn’t wasted. It was just too far ahead of its time. There was no need for a product that was three times faster but as the technology got faster and faster, to have DRAMs keep up with the logic became pretty [important].

We worked on a one-micron CMOS process which came out [in IBM products] just about the time the rest of the industry did [one micron]. I was part of a joint effort involving my group and the processing group. It was again an interesting thing to go through and say, “Okay what do you have to do to move from run of the mill CMOS to the next generation?” That was good.

[For] the high speed [MOS]static RAM effort, Stan Shuster started this work and worked with Barbara Chappell, who was the wife of a person who was in the [Research] physics department. [Bipolar cache chips just didn’t have the necessary density; the idea was to replace the low density bipolar cache with much higher density MOS SRAM that was compatible in speed with the bipolar processor logic. We’re going to need memories close to the bipolar logic, [compatible with the bipolar performance. DRAMs had the density, but weren’t fast enough.

Hodges: All the mainframe processors were still high speed bipolar?

Terman: All high speed bipolar, yes, so they were going very, very fast. The DRAM memories were pretty slow. The static bipolar memories just didn’t have the density. You couldn’t put enough [data] in them. You had to go to the backing store [too often, which reduced the processor throughput].

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Hodges: Weren’t the architectures still with the two level memory with the cache and then a memory?

Terman: Yes but by the time you got to the back level of the mainframe [DRAM] it was too slow, so we needed to do a static MOS RAM that would be really fast. Dick Pashley at Intel had done a high speed, what was sort of high speed at that time, 50 nanosecond access static RAM, which was pretty good for that time, around 1980 I think [but that was much too slow for the mainframe performance we needed].

Hodges: Four kilobits.

Terman: It was small, yes. Masuhara of Hitachi some years later, about three years later I think, came up with a fast CMOS static RAM.

Hodges: That would go right into the same socket...

Terman: Yes.

Hodges: And one-third the power.

Terman: One-third the power and that was what was interesting about CMOS. What he showed was that CMOS could be every bit as fast as N-channel [MOS] and at much, much less power. And I also remember talking to processor people, taking the data from one ISSCC meeting and presenting a big chart of the performance of the processors in N channel and CMOS, and the [corresponding] power and it was clear. The CMOS processors were a factor of three to five lower power [at similar speeds]. We were beginning to get at that power barrier and that was it. It was very clear. You had to do CMOS.

A person at [IBM] Burlington, Ron Troutman was a big CMOS supporter, advocate, and he won that argument very much [and finally IBM moved into CMOS]. We decided we needed to design fast MOS memories. Stan Schuster and Barbara Chappell really were the drivers designing a series of very high speed SRAMs and they gave a series of papers at ISSCC which were really very, very fast.

Then we got serious about it because we realized that we were fast but we weren’t really fast enough for the looming generations of bipolar mainframes. There was this question of going to the backing store you had to spit a page of information out rapidly, and we said, ”What can we do to reduce the cycle time so we can get a page of information in fast in CMOS technology?” By that time, Barbara’s husband, Terry Chappell, had seen the light, left the physics department and came over to work in our department and the three of them turned out to be this astonishing group of high speed circuit designers. They designed the static RAM which got a best paper award in what I think 1991 at ISSCC and also, by the way, went into an IBM product and they got a substantial IBM Corporate Award.

Hodges: And that was a main memory target? That was for the main memory?

Terman: That was for the [cache] memories. The bipolar mainframe logic got a high speed MOS static RAM for the next level of memory and that was a major step because of the dramatically increased density CMOS provided. It happened because people had the flexibility to go out and do this. They didn’t design it overnight. There was a series of about three static RAMs that Stan and Barbara did and then,
with Terry, they got down to really looking at what they could do with this final version, which really was astonishing, [having the unique characteristic of] a cycle time faster than the access time. That means you pipelined information and had much faster page transfer. Bipolar could have been used for this memory, but the density was

**Hodges:** Sure. So this kind of progress, but you also were running up against power problems in the processor itself, right?

**Terman:** Yes.

**Hodges:** And this indicated that maybe a look should be taken at CMOS for the processor.

**Terman:** There was a lot going on because…. well, there were two things. One was these processors were draining a tremendous amount of power using bipolar ECL type logic and [while] they could make the technology go faster and faster…. but good heavens these processors were draining an awful lot of power.

**Hodges:** Water cooled?

**Terman:** Water cooled, yes. There was another problem and that is making this technology that would go faster and faster was costing an arm and a leg. A new [generation] of [bipolar] technology would cost at that time maybe $200 million and at that time IBM didn't sell the technology outside so they would make tm at $200 million or whatever it cost. I don’t know the number. And they would make multiple thousands of processors so…. I don’t know how many chips that would be. The cost per chip just to develop the technology would be incredible. They got to the point where they said, “We cannot afford to make the next level bipolar technology.” They commissioned a study, “What could you do with CMOS?” By the way, they had tried to do a CMOS version to replace the bipolar processors in the early ’70s and it failed.

**Hodges:** That was the micro 370?

**Terman:** No, no that was a different one. We'll get to the micro 370. Thank you for reminding me. [At that time] they said, “CMOS is fast with low power. We'll make short channel devices and we can make the processor circuits really fast. We have the lithography capability, we'll get low power and high performance.” Well they forgot about latch up. They re-discovered latch up. They [also] got all sorts of problems in the devices, short channel effects and so forth, and they didn’t understand how to design the MOS devices. That really was one of the things that led to the Dennard scaling paper. Dennard, [worked] under Dale Critchlow, Dennard and some people working with him [Dennard], did a paper about the laws for scaling MOS device [to]smaller dimensions. The scaling laws work up to a point. They recognized that there are some physical limits which we’ve come up against more recently and are sort of overcoming now, but that [paper]told the industry how to make MOS devices smaller and smaller, and that [paper] really led the industry’s directions, but it took a while.

The early 70’s CMOS project problems really killed off CMOS within IBM for some time. It took maybe ten years to recover from that before we realized what CMOS could do. So it came up again, and people
looked at CMOS and realized that we should do it for the mainframes; the other people said “No, we can still do the bipolars.”

And fundamentally what won out was the economics. We could get a CMOS mainframe much, much cheaper with a mainstream [CMOS] technology we were using for memories and smaller system processors; yes, it was going to be a little bit slower, but again at the processor level….if you look at the box, remember the integration level was much higher so the packaging density was much higher. So you were able to put chips closer together. You didn’t lose anywhere near as much performance as you thought you would. They did it developed CMOS ,mainframe processors, and I think IBM made the transition without any real hitch.

They lost the really high-end market to, I think it was Hitachi, or maybe NEC, but we could never figure out how they [Hitachi or NEC] managed to afford it. We figured it was a loss leader for them so they could say they had the fastest processors, but the cost penalty to them compared to what we were doing in our MOS processors was probably very significant. And then, of course, as time went, as scaling progressed, the CMOS processors went as fast as the bipolars possibly could have done because of the packaging delays would have held up the bipolar processors, [and also a much better power/cooling environment]. It was not that long. There were a few who said, “Hey, we’ve gone one generation too early” but they really weren’t thinking of the finances of it all.

Hodges: I remember sitting on an NRC study with John Armstrong somewhere in that period where he was absolutely unwilling to countenance the demise of bipolar. He said, “Bipolar will be one of the key technologies for the decades ahead.”

Terman: Well it is (smiles).

Hodges: He was head of Research at that time wasn’t he?

Terman: Yes. Yes, he was head when that change was made. I give the people who made that change credit. I assume Nick Donofrio was one of the people. I’m not sure who the other ones were, but they looked at the future and said, “The future…. we’ve got to look beyond the next generation and do it and do it right.” Now you mentioned the micro 370. This goes back to another one of the projects we were working on. Micro 370 was a microprocessor which would execute the—

Hodges: Full instruction set?

Terman: Well, the 370 instruction set, [but] not the full instruction set [in hardware]. It was a microprocessor. It was a RISC architecture and so half or so of the instructions would be done in software. But the idea was you would actually have a real bona fide microprocessor, much like the Intel microprocessors, that would do the 370 instruction set. Well where do you find people who can design this? Only in Research, because only in Research did you find people who had any experience designing custom circuits, most of it being in memory. So a group of people in my area and [another group] in the [Research] computer science department got together and implemented micro 370 using custom design to do it because the only way you were going to fit the necessary amount of hardware [circuitry] on a chip was to use custom design.
The processor worked, but it did not become a product. We probably could have guessed that. But when we came to going over to the CMOS design for the mainframe processors what did you do? The place to go was Research because they had people who had designed a custom microprocessor [chip], and so Research was the place that knew how to do the design.

Did we have the right number of designers, a lot of designers, enough to do a full IBM mainframe processor? No, but fortuitously or otherwise, this was during the time when the downsizing had hit IBM. IBM was going through very bad times and it was downsizing. And so we [Research] closed up much of the physics department, we closed up our processing area in Yorktown. [At that time] there were processing areas in Yorktown, Fishkill, and Burlington. Gerstner indicated that three is too many, and that Research should go, so it vanished. We had a group of people [for whom] basically it was "leave the company or find another job [in IBM]". These were bright people and what we said was, "How would you like to stay with IBM? How would you like to be a circuit designer? We will give you a course in circuit design and at the end of, (I think it was five months), you will be a circuit designer and you will work on what's called the Alliance processor, [the] CMOS [mainframe] processor.” And it was surprising the number of people that were interested in doing this as opposed to leaving the company. We had physicists. We had the processing people.

**Hodges:** And were you involved in organizing and teaching that?

**Terman:** I was involved in the second round of it. I was on the staff during the first round. [In the course] they [started with basics:] “This is an MOS device. This is how it works. Here's how CAD tools work” and so on. They went through the whole [design] business.

**Hodges:** Smart people can learn new things.

**Terman:** That’s right. I think I had just come off…..I'm trying to remember.

**Hodges:** You’d been on staff to [Jim] McGroddy?

**Terman:** I was on staff with McGroddy and I hadn’t come off when that [the course] was first started. They signed up about 40 people through the first round and I think they had about 30 on the second round. I was involved in the second round. [For the second round] we cut the course back because we realized we didn’t need to go through all the details.

It worked quite well, actually. Of those 70 people, my take is almost everyone of them turned out to be a really good circuit designer. A good number of them became really outstanding circuit designers. One guy left and went to Cadence because he liked [doing CAD] tools. Some people would design their part of a chip and never do that again because they couldn’t take the detailed work. They’d go off and do CAD tools or work on the package or some of the architecture stuff. Other people would get in and do that [chip] design and say, "That was fun." They really loved it and they would sit with people trading off power and delays between them so the overall system would meet its specifications.

It was extraordinarily successful. There was one guy who went back into what was left in the physics department and just didn’t like it, but overall it was extraordinarily successful. Again, smart people can learn things and it worked very, very well. And then that group did work with other groups in the company...
so that knowledge of how to do things spread through the company. It started in Research, started back with the micro 370, and even before that, started back with designing of the high performance memories where we knew that performance was a goal.

Hodges: Then the last few years before you retired you went on and did some other things. You were president of the IBM Academy.

Terman: Yes.

Hodges: What did that involve?

Terman: That was eye opening. As you go up these levels [in a company] your eyes get opened. The Academy was formed in 1989 by Lou Branscomb as an IBM internal version of the National Academy of Engineering and the National Academy of Science. The idea was to limit it to 300 people of the top technical people in IBM - not the top technical people, because you [would] never get agreement as to who are the top 300 technical people [in IBM], but [very] top technical people. And these people would get together once a year at an annual meeting. The first 50 or 60 people were appointed. The Fellows made it automatically and maybe it was the first 80-90 or so [that were members at the beginning] - about 50 or so Fellows and perhaps another 30-40.

Hodges: IBM Fellows?

Terman: They were IBM Fellows, correct, and then from then on [after the first year] the members of the Academy elected the new members, generally about 35 a year, until we started reaching this 300 cap. The Academy members got together once a year [at an annual meeting] and discussed things, got inputs from people up the management chain, and also from outside, and also talked among themselves as to what they felt that IBM should be doing. They developed study projects and looked at what’s happening and worked to advise high level managers and so forth. So it was a real opportunity to meet people you wouldn’t meet otherwise. It was an opportunity for the people to actually have a chance to influence things [in IBM].

Fellows had a pretty good access to almost anybody in the higher level management, and being an Academy member also helped people who weren’t Fellows. That [the Fellows] was a very restricted group of, as I said, 50 people or so. Other people [Academy members] through these studies had the chance to interact. You [had the chance] to come up with an idea, work it out with the people in the Academy or a group in the Academy, a subgroup, and then go to the receiving executive, get their buy in. It wasn’t that we did something and said [to the Receiving Executive], “Okay, you should listen to us.” It started out with presenting the idea to the Receiving Executive - “This is what we’re going to do.” The Receiving Exec would have an opportunity for suggestions, agreement would be reached, and the Receiving Exec would have buy-in. So when you came to him or her at the end [of the study or project] they had some skin in the game. We were reasonably successful, not as successful as I would like it to have been, but it was [reasonably] successful, and this ability to interact with all sorts of people from around the company was great. Members came from all the geographic divisions. We had a very good representation of women members, for instance, so we were very successful I think in getting really good people together.
**Hodges:** So Research, what fraction of that academy group came from Research?

**Terman:** I think at the peak it was probably between 15 and 20 percent. It was not dominated by Research at all and, in fact, towards the last few years I was there, Research people were having a harder time getting in because I think the members felt there were [important new areas to bring into the Academy] — I remember at that time we had acquired Lotus Notes. IBM was branching into Services Applications and Solutions, (SAS) and Research wasn’t involved much in the SAS effort and we felt the need to bring people from that discipline in to the Academy.

I was elected to the Academy Technology Council, which was the senate or the ruling body of the Academy and there at that time. Then they asked me to run for— I actually ran for President once, [but] didn’t make it. They had just made the president two one-year terms and a full time job; having a part time president just was not effective. So I don’t know if you knew Bob Guernsey, but he was a computer science guy. He was elected. And then a couple of years later I ran, got elected, so I’m the only President of the Academy who was defeated once and got elected and spent two years interacting.

Among other things, the Academy president sat in on Nick Donofrio’s meetings down in Armonk for IBM Senior Management, and Sam Palmisano was there about half the time, where the were discussing the technical directions of IBM. People come in to present what was happening in their department.

**Hodges:** And were you asked for your input?

**Terman:** No. I came in and I could have asked questions. I did once or twice but fundamentally it was for the high level management, Senior Executives, but I was there. I think that was a terrific thing. Sam Palmisano is a very nice man, but I have seen him ream somebody out once or twice. Nick was also a very nice and capable person and handled things quite well. It was just interesting to see the people, the different personalities that were involved.

And when I stepped down as president, I went through my last two years. I still had two years on the Technology Council and as Academy Past President, retired at the end of the two years. For the last two years, I went back to IBM to Research in a staff position, in the Systems Department, and then I retired to run for IEEE President-Elect.

When I ran for IEEE President, I decided that “I can’t do both [IEEE and IBM],” so I retired. Being on the staff for the [Research Systems] department was also one of those things, again broadened [my perspective] because the department had many projects. It also had financial goals [and limits] it had to meet. I learned there about how the department was run, how the financials were handled and so forth. When I became president of IEEE, the knowledge that I got from IBM at that level was really very valuable, and [the contrast with] how IEEE was organized was interesting. It was not what I would have expected.

[BREAK]
Hodges: Do you want to go back? They’re not going to edit it so we could do it now but it’ll be a big jump back.

Terman: It was interesting that I had no idea, I think. The “Terman Method” only became known to me because, in fact, the name was explained to me by some people who I just happened to meet in Japan. Then I have run into the name a few times since then.

Hodges: So it’s on Google under the term, you can Google it under the Terman Method?

Terman: I haven’t done that. I haven’t done that. I should do that. (LMT NOTE: I did successfully Google it after the interview)

Hodges: I’ll tell you the thing I want to ask you and I’ll give you a veto on it if you don’t want to do it, but I want to ask you to speculate on the role, the evolving role of research, in a major corporation like IBM and in the nation.

Terman: Yes…

Hodges: It’s not going the way it did during our careers.

Terman: I know.

Hodges: It’s going some other way and a lot of people are worried about it.

Terman: Yes. I think it’s a serious question unless somehow— the goal I think from the company standpoint has been we don’t spend money on research. The academics will do it. Now, are the companies supporting the academics enough? I don’t think so.

Hodges: And engaging enough.

Terman: Well now, okay, here’s the Frederick Emmons Terman model: which is industry, academics, and government [working together]; government supplies money by supporting research. Industry and academics work together – the academics do the research, the students get trained and experience doing the academic research, and take the knowledge they have gained into industry - and everything is fine. Where that model works, it’s pretty good, Stanford, Berkeley I think it’s pretty good. Maybe in the last twenty or ten years things have been changing. [Academics and industry are overlapping] - Mark Horowitz started, I don’t know how many different companies.

Hodges: Right.

Terman: You guys started companies….

Hodges: It’s still the same thing.
Terman: Yes, so that seems to be okay, but the question is: who’s going to do the basic research and who’s going to fund it? Because if the government doesn’t fund it, then where does that money come from?

Hodges: So we better get this on tape.

Terman: Yes.

Hodges: Do you want me to go back to the Terman Method?

Terman: We can mention it at the end maybe or something, whatever, just an insert.

Hodges: And your term with McGroddy during the downturn.

Terman: The term with McGroddy, yes.

Hodges: Let’s do the McGroddy thing..

Hodges: So you had a second tour on the Director of Research staff when Jim McGroddy was Director?

Terman: Yes. I was a little reluctant to do that because I was involved with some really neat things going on in my Research group. It [also] turned out I was also going to be Director, VP of TAB (IEEE Technical Activities Board) and so I wasn’t sure that I could do both. Frank Mayadas called me said, “Oh, no [problem], come over. It’ll be fun. It’ll be great.” So I “came over” around in the late ’80s, about 1990 I guess. [LMT Note: it was in the middle of 1991]

Well, that was an interesting time at IBM because IBM started going down really, really badly. I heard IBM lost $16 billion in one year or something like that and it was looking very bad. We were continually getting input from headquarters to cut back. What John Akers did was to say, “Cut back across the board” which of course is not what you do in bad times. In bad times you cut out the bad projects and you keep the good projects but he was cutting everybody across the board. So we would get the numbers [funding] reduced by five percent. Then you get it three months later reduced by ten percent. Then it’s reduced by another seven or eight percent [a bit later].

Hodges: Then the board cut Akers.

Terman: Yes. It was getting down pretty bad, so most of the time at that point we were cutting things back and then Akers was gone and [Louis] Gerstner came in. Who knows what was going to happen with Research? Research was 3,000 people or so. It had not grown as fast as the other divisions in the good times, so [the] hope was it wouldn’t be cut too badly in bad times. But there was also a substantial chance that Research would vanish [altogether]. There were a lot of people there who were doing things that were not directly applicable to getting revenue into the company, and their concern was that that sort of effort would go. Maybe people could change what they were doing to work on revenue-oriented things, fine, but [Research] was a fairly expensive group.
Gerstner came in and McGroddy, who was the Director of Research and, fortuitously, McGroddy was outstanding. One of the things he had done - I’m diverging here, but this was important - he had started laboratories that were joint [efforts] between Research and the divisions. We had always had problems getting ideas [from Research] into the divisions. Now, a few years ago, [he] started these labs. These were laboratories at either in Research or in the divisions and people had virtual red hats or virtual blue hats depending where they came from Research or the divisions, but they worked together. They talked together. Everything was done together [in the same physical laboratory]. There was no difference to them while they were there so that when the effort got carried through, the ideas got transferred because either the Research people had been involved in carrying through into production or the division people had been involved in getting the research in a position to go to the divisions for production, and it worked very, very well, particularly well in the semiconductor business, but also in the computer science side of things.

He [McGroddy] was an outstanding physicist and I was told by the physics people that when they came to talk to him and be reviewed by him he asked questions which showed that he was really up on his physics. He knew what was going on. So he was a guy that knew the importance of interacting with the divisions properly, transferring information, and also was a very good technical person at this time. So he and Gerstner talked for a few days. The Research Division stayed. We were very relieved.

Hodges: No more cuts?

Terman: Well there may have been cuts. For instance, we lost the processing facility. Yes, okay and the physics department went from 250 people and being world famous down to about 100 [people] I think is what it is now. And there were some other similar things. We went from about 3,100 down to about 2,400 [employees], so there was a substantial number of people who moved out but Research stayed and it stayed as a Research Division that was doing research and which was funded [for such].

There was some [IBM corporate] funding that came, like the [old] Bell Laboratories funding, money that just had no strings attached to it, and [there was other money from the divisions to do the things that divisions felt they couldn’t do, because they were getting the next level product out. It wasn’t a bad funding model. So Research survived, and went on to do really great things, particularly in the Alliance program which started about that time or towards the end of it, Research is still designing processors and still doing architecture for advanced systems and so forth. Now, I’m told was that Gerstner told McGroddy later that [when he visited Research for the first time] he’d made up his mind to terminate the research division and that McGroddy talked him out of it.

Hodges: So now reflecting back over your 45-year career at IBM and your other experiences, we had what we thought was, you and I would have shared this view, that there was a pretty good model of major corporations supporting research divisions and laboratories and doing a lot of very excellent work but this model seems to have shrunk and is in great danger.

Terman: Yes.

Hodges: Where are we going to be? Where do you think major corporations ought to be and the society internationally should be doing?
Terman: I think this is a major problem because there used to be research divisions. Bell Laboratories impact on the world, the electronics world, for decades was incredibly [important]. Then things happened there. Unfortunately it broke apart. IBM Research had real impact within IBM and later on around the computing industry and the computer industry software, hardware, and so forth. It’s still doing that but it’s not the same and really IBM [has] become the top research area in the world or organization in the world because everybody else vanished. There’s a real question of where are the real breakthroughs going to come in the future? Presumably the argument is it’s going to come out of academia. The research will be done by professors [and their students].

Now professors, in the model that’s at work at Stanford, the industry and academia work together. The professors are interacting, but the question is: where is the funding going to come for this? The model that my father very successfully implemented and which has been used around the world to some extent, sort of the model they tried to implement has been that government funds research. Research gets done in the academic world, making professors better, educating students, and those students go into industry. Industry works with the professors. The information gets transferred that way and everybody gets lifted up by this process.

But now it seems to be broken. We’re not sure whether the government funding is coming or going. It seems to depend on particular needs and where that money goes. Companies don’t seem to be interested in doing research themselves because they’re very focused on the bottom line in the next quarter and research people [and their research] may take five or ten years to pay off. So what’s going to happen in the long run? We have to make sure that research and academia is done and the question is where does that funding come from?

I would like to see companies more interested in funding that research. I’m not sure it’s going to happen because of the [financial] pressures. I’d like to see governments more interested in funding research that is going to help them, that can start [companies and] industries. I mean the whole electronics industry started from a relatively small amount of funding, so that’s I think what needs to be done but the current world condition doesn’t seem to be for it. The direction seems to be on having something that looks good the next quarter so the stock goes up.

I don’t have a good answer for that other than I hope somewhere along the line people will realize that there’s a lot of things that need to be done. We’ve come a long distance. Take a look at where we were at the end of the Second World War compared to where we are now and the technology level we have and what that’s done for society and say, “We’ve got to take that long term view.” I don’t see politicians really interested in doing that and yet that’s something that’s absolutely essential. When we had the Cold War there was a long term view. “We’ve got to win the Cold War. We’ve got to keep ahead of the enemy”. That’s fine. Now it’s more, “Well if we’re going to do something we’ll get a better way to detect terrorists getting on airplanes.” That’s not necessarily a long term view. So I worry about that also. It’s just a concern as to will we see as much advance in fundamental technology advances in the next 50 years that we’ve seen in the last 50?

Hodges: Well you’ve spent almost as long with IEEE as you spent with IBM or, no, somewhat more. How did that start out?
Terman: Oh, it started out, my father said, “You should join IRE” and I was a graduate student. I said, “That sounds good to me.” It would cost a couple bucks, I think, and so I joined and not knowing there were no student branches, no nothing. You just joined and for that I got this Proceedings to the IEEE.

Hodges: IRE.

Terman: Proceeding to the IRE, yes, it came in and nothing else. So I went through [mt PhD] and went back to IBM, still not much going on from IRE….. and [then] they changed. They had this meeting and they changed to IEEE, which really annoyed me because both that and the change from cycles per second to hertz was occurring at the same time and it seemed like the fundamentals of my life were changing. You couldn’t trust them. (smiles) And also quickly the change from measuring line widths in mills or tenths of a mill to microns came in and it took my mind for about six months to adapt to that change.

Finally, I reached peace with all of these changes that were happening.

When I got back [to IBM] there was no [IEEE] group going on but it turned out there were in fact technical meetings going on in the area. I drove into New Jersey upon occasion to attend technical meetings. Most of the time it was in New Jersey, sometimes up around Poughkeepsie but [I] started getting into meeting people at local meetings, again meeting people I wouldn’t have met otherwise. I met IBM-ers who were in Poughkeepsie or in Fishkill that I had not met before who would go down or drive down an hour into New Jersey and have a meeting on semiconductors, one or more aspects of semiconductors.

Hodges: And one of those was the so-called 4-10 committee which was a committee of what was then called the circuits. They didn’t even have societies right?

Terman: The Solid State Circuits Council at that time.

Hodges: No, no it was the Circuits Group.

Terman: Group, okay, maybe at that time. [LMT note: The Solid-State Circuits Council was formed in 1966]

Hodges: So that sometimes met also in IEEE headquarters in Manhattan?

Terman: Yes. The 4-10 committee, I was invited to their annual meeting by Don Peterson from Berkeley. He said, “You should come to this. We’re having this meeting in Sanibel Island on the Gulf in Florida in July.” Okay, I was young and naïve and I said, “Florida in July that sounds interesting,” went down there and it was an interesting meeting. I mean there’s all sorts of things going on in areas that I wasn’t particularly familiar with but it was interesting to meet the people and see what people were talking about and what they were doing. I was sort of beginning to get into the circuits business out of the computer business and the magnetic memory business. I got a chance to talk with them and get involved in this committee and then the committee did have meetings, one day meetings, in New York City at the United Engineering Center which was near the U.N., which is a good location for me. I went in by train. It took me an hour or so to get there, and that was an era when people would literally fly across the country for a one day meeting. You asked them to give a talk on microprocessors or whatever. They would
come to that meeting because they would meet five or six other people [speakers] who were working in the same field and perhaps 10-20 other attendees; that was one of the best ways of doing it in an informal environment. That was occurring at of the mid or the end of '70s, so I actually was in the semiconductor memory business at that time.

Hodges: You had several other activities. One of them was the awards board you got started with.

Terman: Yes. It turned out that the IEEE Awards staff was located in the same United Engineering Center [in New York City] and at that time, IEEE did not have money for people to travel. So they were looking for people to get involved who were local, who could come in easily…. and there I was. And this phone call comes in from Maureen Quinn, [head of the Awards Board Staff] who was a legend in her own right, I think, among volunteers who were involved with the Awards Board at that time, who said “Would you like to be involved?” And I said “Well, this sounds interesting,” I went down there and was involved with the awards board for decades after that. It was very interesting. You, again, met a lot of people. You saw things you wouldn’t have seen otherwise and got involved in the activity, understanding what people were doing and the process that IEEE developed for giving awards was quite interesting.

Hodges: You had quite a long span with the Journal of Solid State Circuits. I think you became one of my associate editors.

Terman: Yes, you said “Would you like to be an associate editor?” and I said okay, again, being young and naïve. ’74 was when I started so maybe it was probably-- You were three years or five years as chair?

Hodges: I was three years as editor.

Terman: So it must’ve been ’71. And I’d been involved with the 4-10 and it sounded like something that was interesting and that was very good. This is something I would advise people to get involved in because you see papers, you see them early before other people see them. It’s not ethical to use the information you see, but you get a much broader view, again, of who’s doing what. And you make contributions. You give back to the industry and to the technical community, which is really important. I found that to be a very interesting effort to do, and of course, being editor of the journal was, again, another step forward, or step upward.

Hodges: I think you organized the first special issue based upon the European Solid States Circuits Conference (ESSCIRC).

Terman: Yes. I was involved starting the European Solid States Circuits Conference. That was started in Europe, obviously, but I was on the program committee because I’d known a number of people that were involved with it. And when they decided to start it, I contacted Karl Stein. He was on the program committee also. I said “Karl, would you be willing to be editor of a special issue?” and he agreed to do it. Karl was the guy who gave the paper on the first sensible sensing [circuit and approach] of the one transistor memory cell. It was a legendary paper and also [he] was a recipient of the Solid State Circuit Award at one time.

Hodges: Had you known him earlier when both of you were working on magnetic memories?
Terman: No. It was semiconductors all the way, and I forget exactly how I knew him. I guess he came to ISSCC, and when they started the ESSCIRC conference, I said “This is a guy I know. He’s a very good person.” There was some pushback from the conference because they kind of wanted to have the papers in a European journal. But I think it worked out very well because that conference, good as it was, would’ve been invisible to the US if it had not been for the Journal of Solid State Circuits [special issues]. Because of the Journal, and presenting some really good papers out of ESSCIRC, it had visibility everywhere and became a much more important conference that if it had just stayed in Europe with some sort of publication maybe in a local journal in each country as it moved around from country to country [year after year].

Hodges: You were also active in the Electron Device Society and eventually became president in 1990.

Terman: I used to go to the [International] Electronic Devices Meeting (IEDM) and would go down the night before, and this is literally the truth.

Hodges: In Washington?

Terman: In Washington. That’s where it was being held. And I would go down there, taking the shuttle down, and having nothing to do [in the evening], I would just go in and sit in the [Electron Devices Society meeting]. And so they got to know me. Juri Matisoo [for whom I worked for at IBM at the time], became the secretary of Electronic Device Society, -- Juri was a very good technical person but didn’t want to do the minutes. So he would pass the minutes information over to me and I would write the minutes for him. And so they got to know me through the fact that I was involved. I was present and I was involved there. And then, lo and behold, Craig Casey asked me “would you like to be on the ADCOM,” and they voted me on the ADCOM. That was done by the [EDS AdCom]. It wasn’t by the members. It was done by the ADCOM, the people on the ADCOM.

Hodges: Voted its own members.

Terman: Own members. It has both advantages and disadvantages; works out okay for EDS. And then after being on the ADCOM for a while, he asked me-- I’m not sure if Craig Casey asked me to run [for president]….. I think it was him. But then he asked me to run for president and that was again voted by the ADCOM. So I had a year as president-elect and then followed Craig Casey as president of Electronic Devices Society for two years.

And one of the first things I did and maybe the best thing I did for Electronic Devices and certainly the longest lasting thing, was to hire Bill Vandevort, who was the Executive Director until he retired earlier this year from Electronic Devices Society -- [he was] an outstanding success. He got several IEEE staff awards for his outstanding work as executive director for EDS, and [was] a guy who came in, hit the ground running, hired Laura Riello, who’s still with him there. And they moved EDS forward. EDS was a very smooth running operation and Bill just made it better and better as time went by.

Hodges: They have a staff of four or five people now.

Terman: Now it’s in that range, maybe up to six or so. They run a lot of conferences, something like 150 conferences. That was one of the strong things EDS did extremely well.
Hodges: You yourself were involved in quite a few of these, especially internationally.

Terman: I was lucky because IBM supported that kind of effort, even from the people in the divisions. But the Research people, it was kind of expected that you’d become well known. And so I had a chance to get involved first with ESSCIRC, knowing people from Europe who were doing circuits, and then the VLSI Technology Symposium, which Walter Kosonocky from RCA, was asked by some Japanese people to [be involved in] starting up. He invited some people from the US, of which I was one. And that was a resounding success. It alternates between the US and Japan and has very good attendance. It had extremely good papers -- IEDM is the top-ranked flagship conference, and it is in the second rank, just underneath the top rank.

That was so successful that about four or five years later, they decided to have a VLSI Circuit Symposium to go with the Technology Symposium; at the same location and with a one-day overlap, so people could attend both of them. And I was asked to be on the first program committee. The chair was from Japan but I was on the first program committee and became the first US chair of it. That [conference] is another resounding success which is still going on now. Both conferences are quite well attended and are still going on, and the Circuits Symposium has an annual special issue in the Journal of Solid State Circuits.

Hodges: There’s a Taiwan meeting that you’ve also been instrumental in.

Terman: Yes. Hwa Yu, who worked at [IBM] Research, had very strong ties in Taiwan, and they decided, because Taiwan’s technology was coming up so well, that they wanted to have their own conference, which would be an international conference, bringing papers in from around the world, but which would be in Taiwan and would be an opportunity for Taiwan people to attend the conference, and see what was going on around the world [and also present their papers to an international audience]. They had a number of very strong invited papers. They did very excellent on this, not just the plenary session at the beginning that had four papers, but [in] each technical session, they would invite a paper from somebody who was really well known to come in and talk to discuss the directions in the technology. And that has been going on for a long time quite well. It started in the mid-'80s I guess and now it’s split into two conferences, one on technology and one on circuits and systems, and it’s still going on quite well. So that’s been another success.

Another new conference was the Low Power Symposium. When low power started being important, a group of us got together and decided we should start a conference on low power. There weren’t any at that time. I think it was held in the Biltmore in Arizona in August, which kind of matches, from a weather standpoint except for the humidity, of being in Florida in July. But it was, again, a couple hundred people attended it. That conference is still going and it was kind of the first time where a group got together and focused on low power technology which became very important at that time.

Hodges: You’re known around the world for helping other folks organize locally and be successful.

Terman: It’s great fun, let me tell you. And I would advise anybody who has the chance, to do it.

Hodges: You got appointed to the Society Review Committee of IEEE.

Terman: Yes. The Society Review Committee was what year?
Hodges: ’90 to ’93.

Terman: Yes, so I would’ve been on TAB as being president of EDS. And Larry Anderson, of Bell Labs, was the guy who was running the TAB Society Review Committee (SRC). What it did was to review every society [and Technical Council] in IEEE and at that time, there were probably 35 or 36, 37, every five years. So it was a lot of work. People [the S/C reps] had to put their ducks in order, come in and tell the Society Review Committee who didn’t know anything about [their activities], what they were doing. But SRC had looked at what other Societies [and Councils] were doing. So we brought in a perspective to what you were doing, which you didn’t have. And that was important. [While] I was on the SRC we finished the first round [of S/C reviews].

Larry had been doing it for five years, and I became the chair of the committee and we were starting the second round of reviews when an unexpected change happened. I was involved with one of the committees on TAB and Bruce Eisenstein was TAB VP at the time. And I remember going to a meeting, a TAB meeting and sitting there and thinking “The treasurer is a really good guy. He knows what he’s doing, but he doesn’t make a good presentation. There’d been one meeting and then a retreat. At the end of the retreat, he [Bruce] came up to me and said “The treasurer has lost the confidence of TAB. I want you to be treasurer.”

So I stopped being the Society Review Committee chair after about a year and took over as treasurer, and worked with the staff people. Heidi Zazza-Roth -- she was married at that time; she’s now Heidi Zazza -- was the staff person for the Technical Activities Finances Department. She was excellent. She was just a great help [and continually propped me up], and we formed a very good team, well enough that I was appointed for a second term, which is not usual. Normally it’s just a two-year term. It would’ve been about a year and a half to go [from the first appointment]. But I went on to a second term.

And this was through a time, although we have downturns recently, this was a time when there was a downturn and there were some real questions about how to do things. And in fact, while I was there, we did [created] the TAB support line, which funds the TAB Department and the [TAB] expenses that are not assignable to individual activities there. We did have one year, in fact, where the TAB support line was negative….. meaning, in fact, we gave S/Cs money back because the reserves went up so much, but that’s not happening now. But it was a very interesting time and I got a chance, again, to meet a lot of people and be involved with TAB for a long time. That was interesting and worked [out] very well.

Hodges: Then you were elected president of the Solid State Circuit Society in 1998.

Terman: What happened on that was, that Harry Mussman was president of the Council and he came to me and asked “Shouldn’t we be a society?” And I said “Yes. You really ought to be a society because you have members and the members will have a voice in IEEE.” The people on the council [governance] are the representatives of their respective societies so they [the solid-state circuits community] don’t have direct members [on the council] of their own. So the people in that whole solid state circuits community didn’t have representatives on IEEE and on TAB. But if we [were a society and] had members, they would have representation.

But the council had been very successful, so why should we rock the boat? We discussed it and worked on it. Bob Schwartz was the first president of the society but the changeover occurred-- I guess Schwartz became president [of the Society] in his second year. It was on Schwartz’s second year and I had been
elected when Schwartz [was president for the last year of the Council]-- I would be the first elected president of the Solid State Circuits Society. Previous to that, he had been elected by the council members themselves. And Mussman talked to me and he said “You have a lot of experience in TAB. You know what it’s like,” and he wanted to make use of that experience. I don’t know if I had that much experience because both Harry and Bob were really good people. They could’ve done it without me.

**Hodges:** But you’d been President of EDS, which was very well established.

**Terman:** Yes. There was some momentum there but also, they were extraordinarily good people.

**Hodges:** You were elected vice president of TAB in 2001.

**Terman:** Yes, after I finished with the Solid States Circuits Society. And by the way, one of the first things I did with Solid State Circuits Society was to hold a search for an executive director. And Ann O’Neil, we selected her, and she’s still with us. So I feel it’s two for two. I’m not going try for a third.

And then the next thing [I did] was to run for TAB VP, which I did. It’s selected by the entire membership and [as] luck would have it, I got in. And that was one year on the board. There’s one year as director-elect, VP-elect, and then you’re on the board. And again, you’re up a new level because you’re now seeing how the board operates, not how 40 societies and councils plus 20 miscellaneous hangers-o [in TAB] operate. It’s the actual [IEEE] board and where the actual authority resides to make the IEEE work and to make the directions for IEEE. That turned out to be 2001, and when things were going downhill. Remember, that was a downturn? We were coming in with all sorts of things we were going to do and basically, we started finding out that our reserves were dropping.

Years ago, when I was [TAB] treasurer, I talked about [the concept of] phantom reserves. [In the IEEE bookkeeping] IEEE [as a whole] has the reserves. Each society and council and each other individual [financial] IEEE operating unit had reserves, If all the individual reserves were positive, and you added all the individual reserves up, they added up to the total IEEE reserves. [However], if one of those entities, any one of those entities, went negative, then their negative reserves had to be spread over the other reserve buckets, by some algorithm. So now, you got what I called phantom reserves. You had less reserves in your bucket than you thought you had because someone else’s bucket went negative.

Well, the year 2001, the phantom reserves hit. We had a negative budget for IEEE, we overspent it, and then the reserves dropped down dramatically because the stock market went down. And so we had much less money than the sum of what individual people thought they had, because the Corporate Department reserves went negative and the individual societies and councils and others had to help cover the deficit. So that was a very exciting year as we worked our way through that.

**Hodges:** It’s quite remarkable that you did. Nevertheless, you were elected division director and president-elect after that.

**Terman:** Yes, Division-Director for Division One, which was the Solid State Circuits Society, the Electronic Devices Society, Circuits and Systems Society, and a few others, at that time. It’s changed a little bit now. And for two years, again, back on the board 2004-2005, a chance to have some impact. But now the bottom of the [downturn] was occurring when I was on the board. And, in fact, [the reserves]
started moving up and things were getting better. The reserves were going up $30 million a year and, of
course, we thought that was going to go on forever. And it didn’t.

I got elected President, or President-Elect [for 2007], and things were good. And then, I became
president [in 2008] and the bottom fell out. We hit a downturn again. I came in with three [major] things
that I wanted to focus on [in my term]. One was the finances. [In the recent years] our reserves were
growing. We’d gone up $150 million in five years [to about one-quarter billion dollars], so the question
was, what are we going do? Are we going to limit our reserves or not? How far are we going to let our
reserves increase? That problem took care of itself. Our reserves dropped down by about a third. So we
went from $250M down to $180M, something like that, down $70 million. So it was a big drop off.

The second effort, I wanted to have a focus in India. Two years ago, we’d started an office in China and
we started to have an official presence there. But China and India are the two biggest growing technical
areas in the world at that time and still are – great growth. China had its own political organization and
was moving up rather dramatically. India was also changing rather dramatically but we weren’t paying as
much attention to it. It did not have growth coming down - being driven - from the top. It was more
haphazard and by individual groups growing, and I felt that was something we really needed to get
involved with. Also, India was very strong in software. IEEE was not a particularly software-oriented
organization and software was not particularly well handled or covered by professional societies. So I felt
that was a pretty good opportunity. So we started out on that. It’s been a tougher nut to crack than we
thought, because it’s an example of capitalism with individual groups working together, rather than some
force coming down from the top. It also has a lot of infrastructure problems. But it’s paid off pretty well
and India has the largest number of members in any country outside of the United States, about 27,000
members. So it’s a very fertile ground for the future.

The third effort was something which I give my wife [Bobbie] credit for having really pushed, humanitarian
efforts. As I was President-Elect, we started noticing people were doing things around the world of
applying technology to societal problems. And that was something we felt was really important, that IEEE
is a large, international technical organization with almost half its members residing outside [the US]—[it]
is an organization that can do things around the world. Furthermore, we had this on-the-ground structure
which involved chapters, sections, student branches and so forth around the world, where people who
were interested could actually do things at a local level.

The person who preceded me as president was Leah Jamison. She was the founder of the EPICS
program, Engineering Projects in Community Service. It started at Purdue in the mid-1990s, 1996 I think
it was, and [has] spread around the world. There’s at least one university in New Zealand that’s doing it
and there’s about 16 or 17 in the US, and there’s a bunch of high schools now that are doing it. And
hearing what she was doing and the effect they were having, seeing what people were doing around the
world, finding out that there was work being done at UC-Berkeley, there was work being done at MIT and
other US universities. IEEE is an organization that should be focusing on this because it turned out,
among the young students and the academics, there was a lot of interest.

Also, this was a very good way of attracting female people to the technical fields. So we said this is
something that IEEE needs to do and really be involved with. We’ve continued that. We are now in the
process with IEEE, I hope, of coming up with a real proposal that’ll be a major effort in IEEE, which will
work to apply technology to solve local technical problems around the world, and we’ll see what happens
with it. But it’s one of these things that I feel is extremely important.
Hodges: You were diverted from these programmatic initiatives when the Executive Director left IEEE and you basically had to put on another hat as well as being President.

Terman: An additional hat on the same head. If you have two heads, it’s a lot easier. Yes, it turned out that he’d been hired a couple years before and he decided that he was going to leave. So in the second quarter basically, he was in the process of leaving. And so there were a couple of alternatives we could’ve done. We wanted to look for a new Executive Director. In the meantime we could’ve brought in a hired gun. Some people do this. They bring in an executive director who comes in for just the purpose of running the staff [in the interim]. But here [at IEEE] that wouldn’t have worked very well. The staff and the volunteers were so intertwined that by the time that person got [up to speed], he or she would be going out because you would’ve hired somebody. It was too complex for somebody.....

Hodges: You mean somebody temporary?

Terman: A temporary person, that’s right. The second alternative would be to have a current staff person be acting ED. But there was not consensus as to whom that person should be. And then the third alternative was the President and CEO, myself, be the acting Executive Director, and that is what I did. And I think that was another extremely valuable experience because as the acting executive director, I got a look at how IEEE and the staff people were doing. I attended the meetings of the Staff Management Council, which is the equivalent for the staff of the board for the IEEE – where the highest level of management people got together and discuss what they were doing. So basically, things went through without a hitch and then early in the next year, we hired [Jim Prendergast] early the next year.

Hodges: You’re a lucky guy.

Terman: I would say it was because we had very good people who knew their job and also knew that this was not the time to create problems. Remember, this was [during] the downturn and there was one focus, to keep IEEE moving as well as possible in the environment of the downturn. And so they were pretty well focused also and they were good people.

Hodges: It did work better than we thought was possible.

Terman: Yes. We hit bottom. Our reserves were down by about 30% and they’ve come back up pretty dramatically since then.

Hodges: The organizational leadership as well. You deserve a lot of credit for that.

Terman: Thank you.

Hodges: This was a three-year cycle -- president-elect, president, and past-president. Then you drop off the end and you’ve still got an important role.

Terman: Yes, of course, I’m now Chair of the Ad Hoc committee, the Humanitarian Activities Ad Hoc Committee, which is moving towards this major effort which the Board hopefully will vote on in June. They [the IEEE Board] voted the money to prepare a real proposal in June on a telecom in March. So
we’re now moving toward it. I hope this works out. This is being recorded for posterity. I hope posterity will look back and say this has worked. We’re talking with ASME, American Society of American Engineers, about joint effort. Between the two organizations, we have 550,000, over half a million members. We’re more around the world than they are, but there are technical efforts which are complementary, both electrical and mechanical. I think that’s very powerful and also a model for the rest of the world for this kind of operation in the future.

So we really have the hope of getting together and getting humanitarian efforts, getting a real focus on it, and five years from now being able to say “Well, here’s a list of things that we’ve done. Here’s the impact we’ve had. And not only have we gone in and helped this village solve their problems about water or energy or whatever, but what these people learned, we helped them to keep it going.” That’s one thing because a lot of these projects just vanish when the [outside] people leave. Keep it going, expand it to other villages and in fact, keep it going and make it sustainable and expand it to where it goes worldwide. The problems in India and problems in Africa in many cases are similar problems and a solution done in one place with reasonable tweaking can be done someplace else. So we have real hope that we’re going to make real advances on that.

Hodges: We shouldn’t overlook the job you also hold as chair of the Nominations and Appointments Committee.

Terman: Yes, that’s an ex-officio position for past past presidents, or P-squared as we sometimes call it. And it’s an interesting effort because N&A has a bunch of positions you’re supposed to recommend people to the Board to fill, starting from president-elect and working down. There is a strong process for getting recommendations from volunteers. It’s important to get the right people. Having this experience of going through the board and having a lot of an opportunity to see many people and being on the board for a number of years, you get to see who’s good and who’s good maybe for one position but maybe not so much for another position. And [N&A is] a very important position because the people that you’re deciding on now are going to be the leaders in the next year or two, and that’s very important. Some things like the President-Elect will be elected by the membership, but you want to get the right people there in the first place so they have the right people to vote among. So yes, it’s--

Hodges: Keep the pipeline filled.

Terman: Well, the pipeline filled and filled with the right people.

Hodges: There’s been a proposal last year for a radical restructuring of the IEEE board. Do you have any comments on this?

Terman: That proposal has been brought forward by Pedro Ray, the current president when I went off the board. So I only had snips of it before it happened and I haven’t been involved in the discussions there. I think that getting the board down-sized makes a lot of sense because 31 people, there’s too many different opinions coming up and everybody thinks they have to say something. Could you restructure the present board and improve things? You probably have to change people’s attitude and what they think they should be doing. When the people come on the board, they think they’ve really got to be involved in the discussions. So if you change the board around but keep roughly the same size and the same organization, things aren’t going to change that much. So getting it down to a smaller number of people is good.
Originally, the structure, which goes back to the initial IRE structure with a little tweaking, when IRE became IEEE, with a little tweaking because of the additional number of representative, that structure creaks pretty badly. No sensible company would have a board that was 31 people. Neither would any sensible company have a board which rotated where half the people would go off every year. That’s a really major problem that we have. So on the other hand, we need the input from people who are in the trenches doing things. So I basically support what [Pedro Ray is] proposing. The details of it-- I will be at the next couple of board meetings so I’ll begin to see how things work out. My take is that the idea is pretty good. The direction is pretty good. There are some things about bringing extra people in onto the board who are not from the volunteer group-- people who are well-known in the industry, top people. And IEEE probably needs that input. Do they need to be on the board? Good question. You’re going to have to come down and give me the details of how they’re going to interact, as opposed to the way we have now with the volunteers coming in. The volunteers come in-- When you walk into the boardroom, you’re told repeatedly, you walk into the boardroom and you shed everything, your whole past. You are working for the best of IEEE. But that [probably really] doesn’t happen. It’s impossible to ask people [to do that] because people come from different backgrounds. They may think they’re working for the best of IEEE but it’s being affected by where they come from, whether it’s TAB or RAB/MGA or EAB or whatever else. So I think having people from the outside coming in is good because it is people who are going to be not just coming in from a specific IEEE background. But how you work that out, I still have got to think that through, but I think we’re overdue for a change in how the Board is structured. Now, will doing what Pedro wants to do make us more effective? Ah, that’s a very good question. It’s worth the try I think. It’ll be very hard to go back but we can tweak things. I think we need some change because we are dreadfully slow in doing things. Other organizations seem to operate a lot faster. ACM seems to move quite efficiently, quite effectively and quite a bit faster.

**Hodges:** Our conversation is part of a series here at the Computer History Museum. Why do you think this program is important?

**Terman:** Well, there’s the old saying that the people who don’t know history are doomed to repeat it. I think there’s also a bit of a history-- I’m a bit of a history buff going back and looking at how things actually happened. It’s very interesting to see how people interacted and things changed and I think it’s something that people will want to go back and understand. A name is one thing. A name with an interview, when you see them actually talking more than just one or two lines, or maybe a Wikipedia thing, where you can actually get what happened, I think that’s important to understand and to use that as a basis for going to the future. Because the past has a lot of successes. It’s also had a lot of problems that were overcome, failures, things that didn’t work. You should know about those things because maybe the specific problem that we solved in IEEE in 1999 isn’t what happens in 2039. But some of the things that you learned out of that success, it’s worth having that.

So it’s important from an educational standpoint and just seeing where things were, what was overcome, and maybe looking at the problems you’ve got 20 years in the future and saying “Wait a minute. These guys overcame problems that were just as bad. They had other problems, different kinds of problems but they solved them this way and maybe this is a way of going [now].” So I think there’s a lot to be learned from doing this and getting these information, and just meeting the people. They’re not photographs.

I’ve given some talks that involved Charles H. Duell, who was the head of the Patent and Trademarks Office in 1899, and there’s just this picture of this rather severe looking person. I would have loved to have had a recording of him, what he said, what he thought. He was reported to have said the patent office should be closed, which was false. But it would’ve been very nice to be able to have that and to
talk to people. We don’t have Edwin Armstrong around. I would’ve loved to have heard what he had to say. The people that started this industry, how valuable that would be just to go back, and fascinating to hear them in their own voices talking and saying the problems, what they said and what their view of the future was and whether it worked out.

Hodges: Thank you for this time today.

Terman: And Dave, I’ve really enjoyed talking with you. I think it’s been a great job and it’s a great team we’ve made here, working together and I really thank you for all your interest and the questions that you’ve asked. It’s been very interesting. Thank you.

Hodges: Thank you.

END OF INTERVIEW