EDWARD GINZTON

An Interview Conducted by

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PLACE:

McMahon: This is an interview with Dr. Edward L. Ginzton, recently retired Chairman of the Board of Varian Associates. After earning bachelor's and master's of science degrees from the University of California Berkeley in 1936 and 1937, he attended Stanford University, from which he won a doctorate in 1940. After spending the war years at the Sperry Company's laboratory in New York, with the Klystron group from Stanford, he returned to join William Hansen at Stanford's Microwave Laboratory in 1946. Between 1947 and 1968, Ginzton advanced to a professorship of electrical engineering. At the same time, he became a founding director of Varian in 1948, and in 1959 Chairman of the Board. His technical fields have included the microwave tube and measurements, linear accelerators, and circuits.

McMahon: If you're comfortable with it, I'd like to begin with your years in the 1930s, getting educated. You were at Berkeley?

Ginzton: Yes, I did my undergraduate work, as well as the first year of graduate work, at Berkeley. I started in January of 1933 as a student and graduated in three and a half years. I tried to find a job--any job--and I found it next to impossible, so I went to graduate school simply because there wasn't anything else to do. I graduated Berkeley, tried to get a job and couldn't, and so I continued on at Berkeley for a year. That gave me an opportunity to do some research along certain lines called negative feedback, that became important later on. I did my thesis under a professor of electrical engineering, Abe Thulis. He didn't know

much about electronics but there wasn't anyone else to supervise me, so I was asked to consult Professor Edwin McMillan in the physics department with my thesis, and he obviously wasn't very knowledgeable about electronics; even though he got a Nobel Prize in physics he was not prepared to continue with this relationship. I once again looked for a job after my first year of graduate work and came across some kind of announcement that at Stanford there was a fellowship available in electronics under Professor Carroll, who was head of the High Voltage Laboratory at Stanford. I went to see him, and described who I was, and what my ambitions were. He said, "Well, you don't belong with me, but there's a young professor here at Stanford by the name of Frederick Terman who is trying to build up a department; why don't you go see him?" So I did, and he became interested in the research work on negative feedback which I had done on my own without supervision. He became interested that I could do as much as I did without help or advice. So he invited me to become a research assistant in his new department and to continue with graduate studies towards the Ph.D.

McMahon: You completed that by about 1940, didn't you?

Ginzton: Yes. The work was completed in 1939, and I didn't get the degree until 1940 or early 1941.

McMahon: I'm surprised that California had nobody in electronics, or at least interested in developing that field. Terman had begun that in 1925 or 1926.

Ginzton: His principle was very small, but he was interested in electronics. He was the only person in his department who understood a semblance of the electronics field.

McMahon: Do you remember the department developing during that time? Karl Spangenberg comes in around that time I think--

Ginzton: Spangenberg came to Stanford in 1937, the same year I did. I came as a student, he came as an instructor, and we became good friends. At that time, as I recall, the EE department had sixteen graduate students altogether, very small, and Terman was very busy teaching his courses, being head of the department, trying to develop the department, so he didn't do much teaching, in fact. He provided a sense of interest and direction to all sixteen of us--Bill Hewlett, myself, and a number of others.

McMahon: That was quite a group of graduate students!! Did you work around the lab a lot? The Radio Laboratory was really well developed, as I understand.

Ginzton: It was just an attic laboratory and there wasn't anything in it except a few pieces of material.

McMahon: Dr. Villard was telling me the other day about Hewlett developing the trick of climbing in the second-floor window when he didn't have his keys to the building, so I had a sense of a lot of late-night activity going on there--

Ginzton: It was an exciting field, and it was new. We were all interested in electronics and became interested in negative feedback. Bill Hewlett was slightly ahead of us because he had taken a course at MIT under Professor Guilleman, which was very important. We all worked together a lot of the time, but Hewlett and I worked together very closely during the development of the negative feedback principles.

McMahon: Were you able to use that with the klystron work? You got pulled into that around this time, or did you not?

Ginzton: In mid-1938, the klystron was already invented by the Varian brothers, and they needed help in the laboratory. Somebody in the physics department, I think it was Professor Webster who was under him, asked Terman if he had anybody to send

over to help. Terman asked me to go to find out what they wanted, and I was recommended by Terman to Webster, and so that part of the story began.

McMahon: Was there pay associated with that? Terman was always seeking paying jobs for his students, I understand, when you went over to work on the klystron.

Ginzton: Yes, I think it paid something like $90 a month.

McMahon: Villard said that you were one of the few students that didn't have a money problem when he got there. I know Terman was part of the negotiations for the Sperry money that came in and made the klystron work possible. Were you aware of that at all?

Ginzton: I think I know the story fairly well: I came in in 1938 after the agreement was reached, but it didn't take very much time from the beginning of the invention to agreements with Sperry and Stanford. They all would have gotten attorneys, and it all unfolded in about a year's time.

McMahon: You mentioned a while ago that Terman didn't teach very much. I'm surprised at that.

Ginzton: He was busy doing administrative work, developing the program, hiring people. He taught one course--I took one series of courses, one single continuum from him--then the rest was learned by working together. Beyond that we organized ourselves into an advanced seminar, in which each student was assigned two chapters of a book. We would lecture for a couple of weeks at a time, and then become students in the same seminar part of the time.

McMahon: Then were there other ways that you would learn from Terman? Was he around the lab?

Ginzton: He provided guidance, supervision, and advice to all his students. He came into the laboratory, usually at late afternoon, and he would talk to the students until a very later hour, until around dinnertime. He was very much a moving spirit in the laboratory.

McMahon: Was it ideas he would have, would you talk to him about your experimental work?

Ginzton: He would explore ideas, possibilities, he would make suggestions. Most of the time he would critique, supervise and encourage.

McMahon: Tell me a little about the student seminars.

Ginzton: It was an informal course.

McMahon: And you said Spangenberg would be around sometimes but basically the graduate students were running it.

Ginzton: Spangenberg was one of the members of the group.

McMahon: He must've been about the same age as some of the other people.

Ginzton: Slightly older.

McMahon: Tell me about Spangenberg some--I'm just getting to know him through correspondence and talking to people. What kind of a person was he?

Ginzton: He was almost the same age as we were. He came out from Ohio State where he had studied under Professor Elrood, another pioneer in electronics. He already knew quite a bit about something called electronic ballistics, the behavior of electronics, under the right circumstances, and that course was his specialty. He knew a lot about that field, and he taught us a lot from a regular course, but in fields such as communications theory he didn't know much more than we did. He participated in the development of this material for student lectures.

McMahon: Was Terman also learning in these lectures?

Ginzton: I don't remember him participating-- maybe he did sometimes.

McMahon: What kind of books did you have for say the communications theory? Would Everitt's book be part of that, or Terman's?

Ginzton: Terman's book was the most important single document.

McMahon: Which book is that?

Ginzton: Radio Engineering.

McMahon: The one that first came out in 1932 and then was revised later.

Ginzton: I think we used the earlier version of it.

McMahon: In reading that book and having the work you did at Berkeley in electronics and the work going on here, was there the sense of fresh stuff being in it? Would you run into fresh material?

Ginzton: All the time. There was a series of very important papers in the Bell System Technical Journal by Harold Friis, H.S. Clark, and Anthony Nyquist-- these people were contributing a great deal to the literature. There was a great deal of stimulation at Bell Laboratories and they were miles ahead of us. They published a lot, but not everything they knew. We were repeating some of the work they had done, branching off in new directions now and then such as with the oscillator.

McMahon: By "repeating" you mean duplicating some of what they had done?

Ginzton: Just to learn from the experiments.

McMahon: That must have been exciting, to be studying under a man like Terman and a young professor like Spangenberg and then to be studying with students also.

Ginzton: It was a very exciting time, because we were learning a lot in a very short period. Many of the students were very very good.

McMahon: Tell me something about the years when all of that was changing--maybe 1940-1942.

Ginzton: My knowledge of Terman's activities between late 1939 and 1942 is meager because one branch of the activity at Stanford was focused on the klystron per se, and many of us, including a fair number of EE students, were pursuing the invention, its implications and ramifications of the klystron. Something else was going on in radio engineering under Terman. The first branch, the klystron group, almost entirely went over to Sperry in Garden City, New York to work.

McMahon: When did you go to New York?

Ginzton: Late 1940.

McMahon: Almost everybody in that group went to New York?

Ginzton: Almost everybody. It was quite a group, maybe twenty people.

McMahon: Did you have a sense of yourself going just temporarily and then returning? You were there until 1946.

Ginzton: When we went to work in Sperry Laboratories, the war was obviously imminent. Any thinking person would recognize that the war was coming. With the wartime activities and its implications, one didn't know what to expect from the future. If you ask me personally, I'd say I hoped to be able to come back to a university, when it became possible, and I would be willing to accept any salary just to be able to work with people like I did at Stanford. My greatest ambition was to come back to Stanford to work with Bill Hansen.

McMahon: Was Hansen involved deeply in forming Varian Associates, then?

Ginzton: Yes, he was one of the co-founders.

McMahon: So Hansen, you, the Varian brothers, and a number of other people beginning in that late 1930s period were together in some sense?

Ginzton: We worked together, dreamed of the future together, had lots of ideas that we wanted to pursue, and this occupied some of the free time that we had during the war. We wanted to come back to California, we being the entire former Stanford group. You've seen this picture? This is the group that formed Varian, and more than half the group are Stanford people.

McMahon: When was that taken? 1948? 1949?

Ginzton: It was probably taken in 1948, soon after formation of the company.

McMahon: What was it like in New York? You consider that war work. It was at Sperry, but it was war work. Did you have contact with other people at the Radio Research Laboratory?

Ginzton: We did have contact with the Radio Research Laboratory at Harvard, under Terman, from beginning to end, but our principal contact in the Boston area was the Radiation Laboratory at MIT where Hansen was asked to lecture on the theory and practical aspects of microwaves. He ran a program at MIT two days a week, developing the understanding of MIT people in the so-called field of microwaves.

McMahon: Hansen was in New York with you, but two days a week he lectured at MIT at the Radiation Laboratory. What year was that?

Ginzton: 1941, 1942, 1943-- in the formative years of the Radiation Laboratory.

McMahon: The cavity magnetron became fairly important in that work at the Radiation Lab. What was the klystron's part in that?

Ginzton: They are complementary. The magnetron came to be known as the best source of microwave power for transmitting purposes; someone could not have done without the magnetron. One had to have receivers, without which nothing would work either. The klystron was essential to make the receiver work.

McMahon: What was your work at that time? How did you complement this activity?

Ginzton: It evolved with time. One part of the work which I was responsible for was development of microwave measurement techniques--techniques of identifying those parameters which had to be measured in order to describe the performance or characteristics of power transmitter sensitivity, receiver and plumbing alike. Lab #13 at Sperry was responsible in the field of microwave measurements, and there was another lab responsible for developing the klystron, and another lab still separate from that, for the development called Doppler radar. Before I left I was responsible for these three laboratories.

McMahon: When did you find out you were going to return to Stanford? How did that position develop?

Ginzton: Hansen was without question the intellectual leader of this group. He was on leave from Stanford for the war effort, and he planned to return to Stanford at the very first opportunity. He became seriously ill a couple of times, which made it necessary for him to return earlier than the rest of the group. In California he was developing ideas for the formation of a microwave laboratory to pursue practical and theoretical applications of microwaves. He knew that his ideas required the support of many other individuals--he couldn't do everything that had to be done by himself--so he began to work out a proposal with the president of Stanford for the creation of a laboratory that would employ at least one additional person

besides himself. He got approval, so he started to look for some specific individual whom he would want to invite; for reasons of his own he chose me to become the second member of his laboratory. That was maybe in late 1944.

McMahon: So at that time as a younger colleague, you wouldn't have been involved in the negotiations for the structure of the laboratory?

Ginzton: I did not participate in the creation of the laboratory. I was an engineer at Sperry and Hansen was a senior professor in this field at Stanford. He made arrangements at Stanford to expand the laboratory.

McMahon: What was the relation of the Microwave Lab to the old Radio Laboratory?

Ginzton: There was no connection.

McMahon: Did something else evolve from the Radio Laboratory? Did it become perhaps the germ of the Electronics Laboratories?

Ginzton: The EE department under Terman before the war had the Radio Research Laboratory at Stanford. That evolved into a bigger activity and became known as the Electronics Research Laboratory. In that laboratory, the ERL, as well as in the Microwave Laboratory, there was a substantial degree of postwar interest in research that was appealing to the Office of Naval Research. They sponsored much of the activity in both of the laboratories. In 1949 the Korean War was imminent; the government asked the two laboratories to start some classified work. It was difficult to conduct classified work under the auspices of the Microwave Laboratory and the ERL laboratory, because of the working conditions. There were many students from other countries, and it wasn't possible to create classified work in the laboratories themselves. The Applied Electronics

Laboratory was formed in 1949, with Mike Villard, Bill Randall, Don Peterson, and a few others.

McMahon: So that was something happening over on the side for you, and you're at the Microwave Lab during all that time?

Ginzton: The microwave laboratory had a world completely of its own. It was a part of the physics department.

McMahon: Does that mean that the EE department was not involved in it at all?

Ginzton: It was Hansen's baby. He was interested in [the] application of microwaves.

McMahon: Se he was application-oriented?

Ginzton: No, he was interested in application of microwaves to construction of a new type of physics accelerator, which wasn't applied work at all--it just turned out to be the essential ingredient for building the new accelerator which he had been committed to. For a long time he tried to develop a source of high-energy particles for research in physics.

McMahon: Is there a direct connection then between the Microwave Laboratory and later work on building an accelerator?

Ginzton: It was a stepping-stone. The Microwave Laboratory, as a facility, was created before I returned in 1945. It evolved its objectives and accomplishments, but it is the laboratory where the main accelerator work was born, under Hansen, because of Hansen's interests.

McMahon: You also come back to Stanford as an assistant professor, didn't you?

Ginzton: I came back in March of 1946 as an assistant professor of physics, and I had a joint appointment which Terman arranged as an assistant professor of Electrical Engineering.

McMahon: And your degree had been out of the EE department? How did the other physicists think about that? Were any of them bothered by having an applied man or an EE man coming in?

Ginzton: Not me personally--the work itself was nothing but the direct line of work of the department in X-rays--there was some concern on the part of some physics faculty about the fact that our ideas might become gigantic and dwarf the physics department's activities and sense of direction. After it became obvious that we were going to be successful in accelerator work, arrangements were made to spin us off and make us an independent unit in the university.

McMahon: So the Microwave Lab became independent of physics at some point?

Ginzton: That was around 1930 or 1931.

McMahon: What did Terman have in mind in having you in the EE department?

Ginzton: Many of the ideas which were of interest to Hansen from the EE department had applications in accelerator work. Microwaves were new, and many students came to Stanford to try to study more about microwave theory and techniques, a variety of applications that accrued tremendously in magnitude and importance after the war. Many of the students who graduated from good colleges such as Reed University wanted to come to Stanford to study microwaves, but they couldn't get admitted to the physics department. Actually they didn't want to close the physics department itself, because the riches were in applied work such as was being done in the Microwave Laboratory, so Terman saw the need to have some type of association with physics which would enable students to continue their interests either because they were EE people who were interested in researching microwaves, or physics people who wanted to do applied work in physics. He

thought that a fair number of EE students could receive direction and do their research by being members of the Microwave Laboratory, so he was very friendly and very supportive of Microwave Laboratory activities. He knew that our accelerator activity could become very important, and he provided a degree of support within the university superstructure which paved the way for further contracts and projects in the Microwave Laboratory.

McMahon: Let me ask you about your work. What type of position did you have with the Microwave Lab when you came back?

Ginzton: At first, I was an assistant professor in then two departments. That is the way it was until Hansen died suddenly in 1949, and somebody had to carry on the work. I was the senior person in the laboratory, and everybody took it for granted that I would become the director. I acted as one, even though nobody appointed me. It was a standing joke between me and the university that I was never appointed Director of the Microwave Laboratory, nor was I ever released from that appointment. So as far as I am concerned, I'm still Director. The Microlab split into two elements--one is now the Department of Applied Physics, and the facility in which its works is called the Ginzton Laboratory. Together they are a continuation of the Microwave Laboratory of old.

I had interests of my own, independently of Hansen, which had to do with further development of the klystron, trying to determine how much power you could possibly generate by means of the klystron. There was interest in further advances in microwave measurements. I finally wrote a book on the subject, so I was a professor doing a number of things, and when Hansen died one of these was the supervising the construction of the linear accelerators; there evolved a

family of these. First we had the completion of the small one, testing the idea of powering the linear accelerator with a very large klystron. We had the construction of the first billion volt accelerator, which became known as Mark III and in turn became the prototype for the two-mile machine, which we also began in my laboratory.

McMahon: Let me then ask you to return to those same post-war years, those years between your returning and then taking over direction of the Microwave Laboratory. What was happening at the university at the larger level? Terman was central to beginning the university-industry relationship. I suspect part of that structure was to be in close contact with governmental activities in terms of contracts. How were you involved in that, or what did you know about that?

Ginzton: It's a very complicated story and I am not sure I can get it right; I would have to be much more careful than I'm being now. In the first place, Terman supported the formation of Hewlett-Packard. That led by the end of the war to a small company employing 200 people. That little company promised to be very successful. They were working on the laboratory and the factory and paving the road, not on university property. In 1948, finally, the idea of appropriating and starting Varian was supported by Fred Terman. In fact, we founders asked Terman to be a member of the founding group, and he agreed. He did a lot to encourage the development of Varian brothers and our laboratory.

"It's a good idea, let's do it, we can find money," he would say to us. "You people are obviously bright, very successful, you're responsible for the invention of the klystron, which played a very important role during the war, and now you surely have other ideas you want to continue with." It was general supportive

conversations. Because we wanted to have the most prominent people we could find, we asked Hansen to become a member of the Board of Directors. One reason was that he was an interesting person to have as a member of the group; a second was that his name was helpful; the third was that he was interested in providing specific advice of what to do and what not to do. Varian, like Hewlett-Packard, began to develop very rapidly after the war, and it became obvious that very soon we would outgrow the tiny facility at San Carlos where the company was formed, that we ought to identify some piece of land where we could build our own laboratory. One of the cornerstone ideas of the Varian Associates enterprise was that we wanted to be very close to the research being done at American universities, but Stanford University in particular. It was obvious to this group, both at the Board level and to consultants, that we would push our ideas even further with access to Stanford faculty, Stanford students, and some of the Stanford facilities. I believe it was my idea to try to convince the university to lease us some land, and this was debated at Varian because even though it might have been a good idea, people said that Stanford land was more expensive, and might have other limitations. But we decided to try it, or made the proposal for leasing a piece of land to the business office of Stanford. Terman was asked for his advice and obviously supported the idea. For one thing, he was on Varian's Board of Directors and for another he saw the beginning of further development of industry in the proximity of Stanford. We started with six acres and increased holdings to seventy-five acres over time. Hewlett-Packard made a decision to follow our example, and Terman helped provide Hewlett-Packard a site of leased land. After that Terman's ideas began to generalize. You could see this growing

into a very large enterprise, and he became the spokesman at Stanford developing the idea that this could grow into something that might be called a park.

McMahon: Tell me some more about Hansen. Was he at Stanford through most of those years in the 1930s?

Ginzton: Hansen got his Ph.D. at Stanford, and he did go down to MIT for a couple of years as a fellow on a research fellowship, but he returned to Stanford after the MIT period and remained here until he died.

McMahon: He wasn't an older man when he died, was he? Did he die young? I'm wondering if he was a peer of Terman's.

Ginzton: They were roughly the same age. Hansen died at the age of thirty-nine. He was born in 1910.

McMahon: So he was ten years younger than Terman-- Terman was born in 1900, both Terman and Everett. So Hansen sounds almost like a prodigy in some sense. He was moving very quickly.

Ginzton: He was dynamic, brilliant, he contributed a lot more to electronics than most people know about.

McMahon: Would it be fair to say he was always applications-oriented in some sense? Being at MIT must've influenced him.

Ginzton: Yes, he was applications-oriented. He was motivated, in a long-term way, by his interest in physics. He was after important kinds of results in physics, but to make it possible to conduct his research in physics he had to develop basic components which were not yet available.

McMahon: So it was the need for hardware, which is generally developed by engineers, to do his physics that brought him to both...Let me ask you about the years of the

industrial park. When was that? Was there a Varian group before it formed and became incorporated in 1948?

Ginzton: The Varian brothers came out to California after the war. Russell Varian came in 1946, Sigurd Varian came maybe in 1947, followed by many other people after the incorporation.

McMahon: You were the first to get land. Kodak and Hewlett-Packard came later. So really you're talking about Varian beginning the industrial park

Ginzton: It wasn't part of a plan, a thought-out program. Things occurred sporadically. Each time something occurred, Terman was there on the spot, supporting the idea, and businesses quickly learned that Terman was a good person to consult about questions such as the utility of Stanford lands for the future.

McMahon: Do you remember a figure, a person in the business department?

Ginzton: Alf Brandon. I've forgotten what he was called then--Vice-President for Business Development or Management or something like that. Alf Brandon was a key figure in the business office, and he consulted with Fred Terman. Terman in turn was instrumental in introducing industrial concerns to Stanford University to try to convince people such as General Electric, among others, to establish laboratories on the Pacific coast, at Stanford. One of the most important developments which led to the Silicon Valley was the establishment of Shockley Laboratories.

McMahon: Yes, that was 1953 or 1954? How was that a key development?

Ginzton: William Shockley came here already with the development of the transistor under his belt. It was a rudimentary device, but he had other ideas that began to develop properly after he came here. The principal thing that occurred with Shockley

coming here was that he gathered a bunch of extremely able people who wanted to help him develop the transistor for a variety of applications, and to study transistor principles in more detail. Shockley proved to be a very difficult person, so he was an excellent magnet to attract good people but these same good people couldn't work with him very long, so they split off in various directions and formed establishments on their own, and that started a chain of events which led to the Silicon Valley.

McMahon: Do you remember what was happening within the Microwave Lab and Varian Associates when the information about the transistor and solid state devices started?

Ginzton: Nothing, nothing happened with that one way or the other--the ideas we were pursuing then were independent of solid state devices, and they were important but not related in any specific way. We were after the development of the klystron and high-power devices, very high power which the transistor could not help to furnish. The linear accelerator idea was not really into anything else, it was just an idea all its own.

McMahon: During the postwar years when you were working with Hansen in the Microwave Lab, and when you took over, where were you getting support?

Ginzton: There was some money given us as a fund arrangement between Sperry and Stanford to continue the work in the laboratories. We promised to provide Sperry further ideas and they in turn provided money for some research. In 1946 we pursued development of the linear accelerator, for which Bill Hansen had received support from the Office of Naval Research as a specific project. Hansen pursued that with the aid of several graduate students. At almost the same time I

and Professor Chodorow were able to obtain another grant from the Office of Naval Research to test the ability of the klystron to generate very large amounts of power. That was a purpose which the Navy wanted to explore for military application purposes. But also, independently of that, the high-powered klystron would be an essential ingredient for Hansen's accelerator.

McMahon: The Navy must've known that too.

Ginzton: There was no question that everybody understood the dual purpose of it, of the high-powered Klystron Project.

McMahon: What were some other uses the Navy wanted from it?

Ginzton: Principally radar.

McMahon: In the correspondence that Terman carried on at the Radio Research Laboratory with people in the department, the question of money and projects was very important because of his plans for the university itself. He wanted to get professors who were research-oriented, and wanted graduate students and facilities for them. There was no way for the university to do that without support.

Ginzton: The support could come from the government or from industry, but generally industry was not in a position or didn't have the position of supporting it.

McMahon: So the Sperry money was a small part then, after the war--

Ginzton: But it was an important part, because it had no strings attached whatever.

McMahon: Did they just stop working on the klystron at the New York lab?

Ginzton: No, they continued. Sperry Laboratories after the war continued to be about as important relatively speaking, as they were doing the war. They had several hundred people involved in the microwave/klystron area at Sperry, and only about

twenty to thirty people at Sperry came out to join Varian. Twenty or thirty was a small number to Sperry, but it was a huge number to Stanford.

McMahon: Let's take that story on into the 1950s and after you become Director of the Microwave Lab. Is there continuing funding for these same projects? Are you still working to develop the high-powered klystron?

Ginzton: The linear accelerator idea became more concrete; Hansen was able to demonstrate the principle of the accelerator before he died, and also to sell the idea to the government for a much-larger accelerator, something that eventually became known as the Mark III accelerator. It became of interest to the nuclear physics branch of the Office of Naval Research, so the money was coming in from the ONR from two directions. One was electronics, so we could do the klystron, and secondly from the nuclear physics branch for accelerators, and that work continued to expand until today.

McMahon: The Navy is still involved in that. The Ginzton Laboratory is still functioning and still working on these same projects today?

Ginzton: Not the same projects: it shifted over to solid state, to lasers, to work on some other things besides.

McMahon: Were you teaching very much during those years?

Ginzton: I was teaching full-time between 1946 and 1953, about seven years; then I began to teach part-time because the accelerator project became very large and required constant supervision. I continued on as a part-time professor until 1968.

McMahon: All that time you were busy with the Microwave Lab, but your responsibilities with Varian were growing, my sense is.

Ginzton: The responsibilities with Varian were done with consulting the board members, general advice, until 1959, when Russell Varian died and I was asked to become the Chief Executive Officer, Chairman of the Board, of Varian in 1959.

McMahon: You were able to continue teaching, but on a half-time basis. So did you see yourself as a CEO or as a professor? How did you handle it? In 1962 the IRE was fifty years old, and Terman asked a group of people to look to the year 2012. He was describing the professor and he called him an electronic scientist. He said he would be part businessman, part scientist, and part engineer. I guess that was the logic always--not just what Terman wanted, as I'm perceiving from talking to you and reading the record, but from what was happening at Stanford, that all of this was coming. You're certainly an important part of that, because you're saying that Hansen stayed really interested in pure physics in a sense, and the other was really only an aid to that. You were really straddling two worlds.

Ginzton: We were responsible for many events which got the university tangled up in industry. For example, one series of events had to do with the application of the klystron for radar purposes, as a transmitter. The possibility of using a klystron as a transmitter didn't occur to people until we started this work in 1946. The development of the klystron went along very rapidly and very satisfactorily. By 1949 we had a lot of techniques under our belt, and the military became interested in testing our developments in industry, so we were asked to develop on a classified basis a klystron for S-band. We were encouraged to license Varian Associates to develop the S-band klystron for radar applications. Litton Industries was encouraged to work with us to develop the L-band klystron, also for radar. General Electric was to join us in a separate project to develop X-band klystrons.

We would be developing some of these same klystrons I just mentioned to demonstrate their utility in what needed to be known in more detail to make such devices possible. Industry people assigned their engineers to follow our footsteps and to develop practical tubes for various applications. What happened at Stanford was extremely important, because it got industry involved in a big way--this is the Korean War period.

McMahon: Of course the government funding that had been very large during World War II had dropped some and then grew again to very large sums during the Korean War.

Ginzton: Industrial support from the military dropped abruptly by a very large factor after the war--but the ideas you and I are discussing right now came along independently of the war. Ideas, students who continued to work, a real space--money was important for buildings and fellowships. Terman was behind every important event I can think of, in a sense of encouraging people, whenever the university came into a relationship with industry. The university could be skittish about entering such arrangements. It didn't understand them, as university people had never done this kind of thing. So Terman was always there to support industrial ideas and to explain to the university why it was a good idea for the university to participate rather than being aloof or even opposed. It's in contrast to places such as Berkeley, where similar opportunities existed but they did not bridge the gap between academic concerns and the industrial world.

McMahon: There must have been some protests about this on the campus was it mostly in the form of grumbling humanities people? or were they even aware of it in the 1950s?

Ginzton: I wasn't really aware of any sustained or organized opposition. There were many individuals who were concerned about the government using the campus for military applications. Conducting classified work was a problem, but there wasn't any organized opposition in the early days.

McMahon: I guess it was the late 1960s before you got that. Is SRI involved in any of this? Do you have a sense of SRI being a part of all that when it happened?

Ginzton: Not in those days. SRI became important when people such as Villard found opposition to their work because of its classified nature. SRI was then willing to accept projects and people. Villard and a number of his associates did have some relationship with SRI, but it was peripheral-- it didn't involve many people.

McMahon: To follow that up, were there any relations between the Microwave Lab and the electronics labs?

Ginzton: ERL provides basic facilities and space, basically, for a portion of the Microwave Laboratory activity. The Microwave Laboratory, per se, did not do any classified work.

McMahon: So the work on the linear accelerator and the klystron for the Navy was not classified?

Ginzton: No. The Klystron was a nuisance--a major nuisance--to any project, both ideological and practical. You couldn't employ foreign students or clerical people. Security was required but very difficult to manage. It was nice to be able to do this kind of work in a building dedicated to the purpose for the administration.

McMahon: Did some portion of the ERL become kind of a catch-all place for classified work, either theirs, or yours? That sounds like a creative step in itself.

Ginzton: To us it looked like a creative step, and to Terman too. It later became a focus of student-faculty opposition.

McMahon: Again, you're talking about much later. In the late 60s was that work still over in a corner in a sense?

Ginzton: You'd have to ask someone else-- that's beyond my tenure at Stanford.

McMahon: Who would be some good people to talk with about the Electronic Research Laboratory?

Ginzton: Villard is one. Professor Marvin Chodorow was more closely involved than I was, and he has a good memory, a good library, many facts available to him. He was a member of the Microwave Laboratory staff as a professor of physics and electrical engineering, but has been around ERL activities. There are other people from the earlier period who are not easily available anymore--people such as Lester Field, who went to Hughes Aircraft after a number of years at Stanford.

McMahon: But they're all available to me in the archives, in a sense, even if I can't interview them.

Ginzton: Dean Watkins, the head of the Watkins Johnson Corporation, was very important in ERL activities.

McMahon: Is there some area you'd like to discuss that we haven't covered?

Ginzton: Nothing much, just a couple of thoughts. People are referred to always in this area as Silicon Valley people. But that is certainly the wrong idea. The development of electronics occurred on the San Francisco peninsula for a very long time; its roots go way back to the formation of the Federal Telegraph Company.

McMahon: Yes, I know something of that early history. There was that fine article by Arthur Norberg that kind of brought all that together. He ran the Bancroft project, which was an oral history project, but he also wrote a piece that was in the IEEE Spectrum in 1976, that tried to tell the story of the beginnings.

Ginzton: There was one wave of developments predating the Federal Telegraph Company. Dean Starjurden, the president of Stanford, wanted to create a superb university, and he believed that the ability to do that rested heavily on his ability to bring in excellent faculty. He reported from Cornell University, and the faculty, once they were here, wanted to have good students, and they established a program of bringing students from all over the world.

McMahon: Did he bring in Rhine? You mentioned Cornell--

Ginzton: Yes. A Stanford student whose name I don't remember was responsible for working on the ideas that originated in Denmark. He developed the pulse mark for radio transmission; he established a company to provide world-wide communications. This man was employed for Lee De Forest, and tested his earlier invention, and these ideas led to formation of the Federal Telegraph Company. Terman recognized the importance of radio and electronics. He realized that much of the work that had been done prior to his time was empirical and embryonic, so he had the idea of developing Stanford University into a research center, to bring together many many people for various skills. Anyway, that's all a part of the first important developments that had nothing to do with Silicon Valley.

The second development was the idea of microwaves and the invention of the klystron--that development was from people coming from Stanford, and that

expanded into more undulating devices, being an essential ingredient in developing microwave electronics. The construction of the two mile accelerator and the technology which came out of it is just present in dozens and dozens of ideas and applications. Such things as cancer therapy are now much different than they would have been without it. And the third way was the structure at Stanford, starting with Shockley's arrival to the now Silicon Valley.

McMahon: You see Shockley's arrival as the beginning what is called the Silicon Valley? Why is that as opposed to Varian and Hewlett-Packard?

Ginzton: Varian had never been involved in any major work, not even now, in silicon technology or the tremendous progress that field has made. We're just not involved. And Hewlett-Packard wasn't involved except in a small way initially.

McMahon: Taking the term literally, that is, silicon, that term wasn't a factor in Silicon Valley?

Ginzton: People like Bennett began to study the utility of the transistor for various kinds of electronic applications, and he can tell you more about how that branch unfolded.

McMahon: I use that Silicon Valley term because my studies began long before that was a term, but that's kind of a journalistic term.

Ginzton: Stanford and Varian and Hewlett-Packard were all involved in Silicon Valley activities at various parts but it wasn't a big deal-- it was just a device that was useful.

McMahon: You mean the transistor wasn't a big deal-- it was another device.

Ginzton: It wasn't a big deal to us.

McMahon: My sense of the transistor in solid state was that it was just simply another stage of the tube. It was more durable, perhaps, in some uses.

Ginzton: It was a development that made some applications practical that couldn't have been practical without it--for example, the computer. So some applications are not possible with the transistors of solid state devices to this day, because of the inability to utilize large power.

McMahon: But the miniaturization was critical, I guess, in terms of not just computers but of missiles and space vehicles. They couldn't have packed all of that in there.

Ginzton: The point is that Varian, Litton, Hewlett-Packard and dozens of other companies became successful without being dependent or involved in a major way with the development of the transistor or its further evolution. What Shockley caused to occur--and Robert Noyce, Fairchild and Intel, Gordon Moore and others became very important, and started this chain of events [by] which the transistor became the integrated circuit. That's the big deal now in applications to the computer and a host of other important applications, and now you can call it the Silicon Valley.

McMahon: Was Noyce at California? Did he come out because of Shockley?

Ginzton: Essentially. Noyce and Gordon Moore and many others came to join Shockley. They worked with Shockley for a year or two.

McMahon: Perhaps the difficulty in getting along with Shockley was responsible for spinning off these people.

Ginzton: There was a major revolt, and people just quit.

McMahon: In tracing the Silicon Valley to Shockley, all those events had nothing to do with Hewlett-Packard or Varian or Litton or many other people. I doubt they know to this day. Do you have a sense of why Shockley came to this area?

Ginzton: He wanted to start a business concern and exploit opportunities which he thought were immense--and he was right.