Oral History of Thomas (Tom) Stanley

Interviewed by:
David C. Brock and Gardner Hendrie

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David Brock: Well Tom, to discuss the early history of microelectronics at RCA, I thought we could begin by asking you to discuss your general involvement with semiconductor electronics at RCA in the early 1950s.

Thomas Stanley: Yes. I first joined RCA in 1951 and they had a lovely tradition at the laboratories of a research-training program. And that allows the new arrival to work in many different areas at the labs and then get to meet people that he may encounter later on. I was fortunate enough to meet Paul Weimer then, just after he had invented the Vidicon [tube], which of course is the camera tube that made possible journalism in television on a much broader scale because it was a small and compact device. And in that research training program, my first exposure to semiconductors was with a man named Loy Barton and we built an old transistor radio. It was sort of a joke. We couldn't really judge its performance in the building for live reception because nothing much came through. So we'd take it up on the roof and it performed with increasing vigor as it got colder until it broke into wild oscillations. But later I got involved in transistors more broadly.

My first permanent assignment was with what was called the Industry Service Laboratory [ISL] under Al Barco and our special responsibility there was making sure that the RCA licensees, from whom RCA derived a significant income, were abreast of good things that we were doing and so would be enthusiastic about continuing to take licenses. Early on we put on quite a dog and pony show of all the wonderful things that you could do using transistors. This was just when alloy-junction transistors were first coming into being. I got to know Charlie Mueller then who was a, you know, a real contributor or the main source of our good devices then. The star of that dog and pony show was an old transistor television set which Jerry Herzog [Gerald B. Herzog] and Bob Lohman who then converted into microelectronics, made under the supervision of George Sziklai. Of course George Sziklai was the man who first came forward with the concept of complementary symmetry. So interesting the kind of long tales that many of the important people have in this area.

After these sort of toys that we showed at this transistor symposium, we went to work on more nearly professional devices. I think the thing that gave us a lot of pleasure was a very compact and highly professional portable transistor radio that Dave Holmes, Larry Freedman and I put together that met all the specifications that a very high-class radio might.

Brock: At the same time that you're doing all this work for the licensee community, how was RCA itself using transistors and semiconductor devices in its products?

Stanley: Well, at this stage of the game there was very little commercial in the way of transistors. I do believe we did sell a few point-contact transistors for a while, which was sort of the primitive antecedent to the junction transistor. And the early alloy-junction transistors were sold. But at the time we built this transistor radio, and even then later a quite impressive transistor television receiver, there were no commercial sales of transistors by RCA.

Brock: In this same period, could you describe the rise of printed circuit board techniques as a contributing technology to microcircuitry?

Stanley: Well, I think printed circuit boards had a very important influence on what came later. They were not necessarily microcircuits but I can remember in my first days at the Industry Service Lab, taking a tour to our factories in Indianapolis and seeing acres of workers with heavy soldering irons taking components out of a bin and inserting them into their chassis, fastening them to lugs, soldering them on. When the concept came of making a circuit board by photolithographic techniques, it really changed the whole nature of manufacturing and in a sense, what came later followed that only in miniature. So the printed circuit boards themselves were not necessarily miniature. But I think they were the beginning of a change in electronic technology.

I know even with the little radio that we made, we didn't actually use a printed circuit board but we faked it. We used a piece of thin bakelite with little holes in it and all the wiring was on one side and the components were on the other. And of course we were so fortunate because the industry had begun to develop all the little, tiny resistors and tantalum capacitors and the transistors themselves were very small. So our entire circuit in that radio was about the size of a pack of gum, although the heavy components were the loudspeaker, which the bigger it was the better it sounded. And the batteries, the bigger they were the longer the radio played. You had a direct reason for the size of those. But we kept the active components very small and later I wished we had used printed circuits because we ended up -- the radio was a sufficient success that they asked us to make ten of them for distribution, for passing around to important licensees and executives in the company. One of them was Akio Morita of Sony and many years later, I was very pleased to hear him give credit to the little RCA transistor radio that we had put in his hands as what really got Sony started. I take a point of pride in that.

Brock: Could you describe the more general shift at RCA from television to computing research in the later 1950s?

Stanley: In the middle 1950s, color television had finally begun to take off. I, in the early days at ISL, was still working on color television, which had not yet come into being. But by 1955 there was enough momentum in color television that the management of the company felt that we needed to step into another major arena and that was to be computers. Of course there was a corresponding move into anything related to computer technology in the laboratories, to the great delight of the researchers because there were all sorts of sandboxes you could play in for computers that were different from what we'd been doing up to that point.

Brock: Could you describe your own involvement in RCA computer research in this period?

Stanley: Yes, I observed it at a distance at first. But in 1957 I was very fortunate in that Doug Ewing, our executive vice president of research and engineering I think, called me into his office and they informed me that I had been chosen to receive a sabbatical, a fellowship to go on sabbatical for a year, preferably abroad. They wanted me to do that because they felt that the new world of research was going to be computer-related work and not so much television. I didn't really entirely agree with them that home instruments could be abandoned. I felt that there was an awful lot to do there still. But I wasn't going to turn down an interesting opportunity like that.

And so I was assigned to Jan Rajchman's computer group to get a running start on whatever I would study abroad. I was fortunately accepted at Queen's College Cambridge to study under a lovely guy named Maurice Wilkes who was a pioneer in computing and who knew Jan Rajchman. So I had a real stint in Rajchman's group and briefly worked in superconductivity there before heading over to Cambridge for a year, with my family which was a wonderful treat.

Brock: Was the work that you were doing, the device research you were doing relating to superconductivity, was that connected to Project Lightening?

Stanley: Project Lightning was the major contract -- as a matter of fact. that may have been part of the formation of Jan Rajchman 's group, to undertake Project Lightning. The initial Project Lightning was in a superconducting memory and IBM had a very large program in that area too. Then I think, if I recall, it was after I went to Cambridge that Esaki, Leo Esaki, had described a diode that came to be called the tunnel diode. Right away I think RCA Labs were ripe and recognized that this might be a preferred device to use in such a computer. I don't recall that there were any other solid state devices considered as candidates for Lightning, although we did -- also in that I think I worked on, upon returning from Cambridge we did work on phase-locked oscillators which used the non-linear capacitor, the properties of reverse biased diodes in oscillators that were pumped by a carrier and then the sub-carrier carried the information. This involved three-phase logic, and they're two very sort of strange forms of logic. We used the majority logic with tunnel diodes, and I think in the phase-locked oscillators too where the search of the logic was a question of a two out of three volt rather than binary logic, so.

Brock: What is the connection between, in particular, that superconducting device work and microcircuitry?

Stanley: I think there is a tenuous connection, only in that you couldn't make superconducting devices without making them small because they had to go down into a helium bath. And because Dudley Buck had invented something called the cryotron and its dimensions, you know, was if you made it smaller it was more sensitive. I think there was a microcircuitry aspect of that. But Lightning per se, microcircuitry was not other than as a consequence of having to make it fast. Lightning was aimed at computing speeds 100 times those of existing computers. And so with that, of necessity, what you made ended up being small.

Brock: Outside of Project Lightning and the computer research laboratory activities, there were several other areas of activity in semiconductor electronics and microcircuitry taking place in RCA broadly. One of those was the government-supported work of Torkel Wallmark in the integrated electronics group in RCA Research. Could you tell me a little bit about him and his work?

Stanley: Yes. Torkel, I think, came to RCA from Sweden, possibly as early as 1953. But I think the integrated circuit work he was involved with, didn't begin until 1957. And at that time, he was I think, then in the electronics research lab under Ed Herold and they had an integrated circuits activity much earlier. Harwick Johnson had invented or shown a circuit that involved integrated circuits, showing that you could in fact on the same wafer, and that was germanium at that time, make resistors and capacitors and so you could make a little circuit on a wafer. In the meantime, of course, integrated circuits had come into being or at least they had by the late 1950s. And Torkel of course had the insight that while integrated circuits permitted you to make very little capacitors and resistors, for logic purposes you'd be better off if you got rid of the little capacitors and resistors and made your logic entirely out of active devices. He pursued several approaches to this, ultimately settling on field effect transistors. But I learned of this later because, of course, at the time that he was doing that I was still happily working on things like transistorized television sets for the Industry Service Laboratory.

Brock: Could you describe the large “Micro-Module” program that RCA was involved with?

Stanley: My exposure to the Micro-Module program was after it had become quite mature. What was most impressive about that, was the very excellent work they did on reliability and on how to measure failure rates and how to assess the performance of the Micro-Module circuits. One sometimes used their little wafers as a showpiece for what you were doing. But I did not have a direct knowledge of what they were doing and who the customers were. To some degree I think that may have been because I suspect that often that work was classified and therefore it was not, you know, widely talked about.

Brock: Would you say that the Micro-Module program represented an approach to the microcircuitry issue through packaging, where Wallmark's was through semiconductor devices?

Stanley: I think quite so. The Micro-Module approach came early enough in RCA that it incorporated, at the beginning, things like sub-miniature vacuum tubes. So basically you were taking the existing components, making them as small as possible, addressing the issue of how you could interconnect them in a very compact and reliable way, and how you could manufacture them. It was about making big things little. Torkel's approach, while it was energized by this idea that we were boasting about – how small we were making things – was rather asking how can we make a functional device that has properties that inherently will end up with the equipments being very, very much smaller.

Brock: What would you say accounts for this great interest in microcircuitry, miniaturization and reliability in the superconductor world at large in this period?

Stanley: Well, I only can guess that an awful lot of electronics at that time in the Cold War was going into things like nosecones and satellites and this called for an advance not only in making things compact, but also making them reliable and knowing how reliable they were likely to be. And so a certain amount of that glamour spread and the idea of making Dick Tracy radios to wear on your wrist became popular. But I'm sure the motive force was government applications then.

Brock: Just as a quick follow up to that question: Did these government applications necessitate a different level of reliability than electronic components had had in the past?

Stanley: They certainly did. I think they necessitated not only a higher level but also a better knowledge of what the level of reliability was.

Brock: How did you and your colleagues at RCA react to the announcements of Solid Circuits by Texas Instruments and Micrologic by Fairchild Semiconductor in this 1958-1959 time frame?

Stanley: —1958-59 is just when I was happily in Cambridge and so I don't have a first hand answer for that, but it's clear that it had its impact when I came back. Our semiconductor division was quick in taking up the challenge in terms of making integrated circuits for application to a home instrument sort of circuits, in addition to pursuing the computer-related circuits which I think were the primary target for integrated circuits at the time. But as I said, I didn't have the first hand exposure to that.

Brock: Again, just one quick follow up question in here on the expanded activity of RCA in integrated electronics following the spur of Texas Instruments and Fairchild: How did that new activity differ from the sort of approach that Wallmark was taking in his program? Did that involve a shift away from Wallmark's approach toward a more Texas Instruments or Fairchild approach?

Stanley: I infer from your question that you're asking about the period after my return from Cambridge.

Brock: Yes.

Stanley: Perhaps I ought to tell you a little bit about how I at first entered on the integrated electronics scene, and then how that expansion occurred. I had come back from Cambridge, expecting that I'd be working in Jan Rajchman's computer lab. There had been some changes. I think an integrated electronics group had been formed under Ed Herold with Dete Jennings as the head of that group, probably in about 1958 or 1959 while I was in Cambridge. But Bill Webster [William Webster] replaced Ed Herold at that time and as director of the Electronics Research Lab, Webster having headed up the applied research activity in Sommerville in the Semiconductor Division. Dete Jennings who'd headed the group wanted to move on, and so Webster approached me and asked would I move into his lab as head of Integrated Electronics Research. I think the rationale there, even though I had not a background as a device researcher, I had very much gotten my finger into the device pie in connection with the kind of circuit work that I'd been doing in the Industry Service Lab. I was happy to take on that job.

One of the important activities in that area was Torkel's work. It was after I had been there about six months I guess, that Bill [William Webster] attended a meeting in which Atalla [Martin M. 'John' Atalla] described his work with a metal-oxide-semiconductor diode and what it was telling him about surface states and about the conduction processes there. Bill came back very enthusiastic, as I think many others had who heard of this work, that thereby it might be possible for a micro-resistant device to be made rather than just Atalla's diode. He enlisted the help of Charlie Mueller who was such a talented guy. And Charlie by the way, for all his genius, the fact that he had a very talented technician named Ethel Moonan was an important part of the resources brought to bear there. I hope at this meeting that you're having in May that the contributions of people like Ethel Moonan will be recognized because she did a lot of important things for us. But anyhow, Charlie came to me with the results and there may have been a little while until this all happened. But my memory was that fairly soon after Bill came back, Charlie came and showed me that he'd only achieved current control. He hadn't had achieved total cut-off over a limited range. But he definitely had shown that you could have an MOS device that could control current. And so at that point, we thought this was something that we really ought to pursue.

We slightly increased staff. We'd managed to hire Steve Hofstein, a very bright, recently graduated young man and put him on the job of making MOS devices. I think one of the important things that Steve did was to get the research labs to catch up with the semiconductor division, because most of our research up to those days was sort of done with toothpicks and nail files. The individual researchers loved to be able to boast that they'd made things with their fingers. Steve went off and discovered, well, how you make things with a planar process and photolithography and diffusion furnaces and that sort of thing. We suddenly had a very large capital equipment bill. But fortunately Bill Webster was a very enlightened laboratory director who realized, “Well, that's the only way we're going to get anywhere.” And so we began to make not just individual, wonderful little devices, but arrays of MOS devices. Steve achieved, I think, quite some success. He was rewarded for this by being offered a chance to go and get his PhD at Princeton. Fortunately we then hired Fred Heimann and he and Steve worked together. They were a team even as each of them was away during part of their work together, getting their PhDs in succession. I recognized that this was a better device than what Torkel was working with. What's the word?

Brock: Unipolar?

Stanley: Unipolar transistors, thank you. Torkel’s unipolar transistors. Even though you could make logic – you could use, basically, relay logic with those devices – there were awkward aspects including the fabrication technology. The MOS devices gave promise of being much easier to use. In addition, there was some of the work that Paul Weimer had done in his TFT [thin-film transistor] work that suggested the idea of complementary symmetry. And so I think fairly early on we recognized that MOS held a greater potential. Ultimately Torkel's work was wound down. They were in parallel for a while. At that time, this is 1961-1962, most people in industry regarded MOS as an interesting niche device that might be used in some high fidelity circuits and it would have digital application perhaps in pocket calculators and so forth. But it was so slow that they would never be a threat to bipolar transistors. Perhaps because it was our baby, but we were more sanguine. Let me just read a little bit from *To the Digital Age* [Research Labs, Start-up Companies, and the Rise of MOS Technology by] Ross Bassett because he unearthed something I would have lost, which was the paragraph that I'd written in a 1961 research report.

Stanley: The 1961 research report stated: “The day will come when hundreds or even thousands of elements will be formed and interconnected in a single processing run. When this occurs, there will be a decrease in cost of two or three orders of magnitude for a given information-handling capacity.” So we were making bold statements like that. But it was based a little bit on careful analysis. Again, one of my small contributions to the program was a piece written to provide a report when Steve and Freddy were awfully busy. I said, “Well I can do something for a change,” about scaling. And again in Ross' book he says, "Stanley's aim, was to determine the effect of shrinking the device's physical dimensions and other design properties by a fixed factor. This work showed that while a bipolar transistor did not scale, that is one could not improve its performance, by shrinking its dimensions by a constant factor, the MOS transistor would yield decreased delays as its dimension shrank. This analysis showed that at least in abstract sense that a clear development path existed for the MOS transistor, allowing for continued improvement." Well, this was 1962. That's three years before the very able paper and it's more sort of wishing than showing positive results. But it shows that RCA had confidence in the ultimate importance that the MOS transistor would have. There too I had some props. We did in early 1963 feel that the time had come to have a press release on this because even though Heimann and Hoffstein had published their work in the technical journals in late 1962, not much attention had been had in the press. The company felt that if this work was that significant, we perhaps ought to tell the world a little more. So there was a lovely article in *Electronics Magazine* here and that was the cover of *Electronics* and that was our baby and we even made *Newsweek*. And if I found the article and read from that, it would sound like what a reporter would make of the comments I read a little earlier. But I think it demonstrates that RCA was very much in the vanguard at that point. And our work also in vanguard but that came later was in complementary symmetries.

Brock: Would you say then, that in describing the course of MOS developments at RCA in the early 1960s, that the frame for those developments was integrated electronics?

Stanley: Yes. We did have government contract support work for this. We might have been wise to have worked more closely with the product division toward their customer needs. But they seemed pretty capable and independent on that score. And so RCA's presence in sales of MOS devices as they evolved was not as great as it might have been if we had really had our eye on that ball.

Brock: Following that thread of the product divisions, at the same time that the labs was pursuing MOS and complementary MOS within the framework of integrated electronics, could you describe work being done elsewhere at RCA in bipolar silicon integrated circuits?

Stanley: The product division did have some very nice circuits for television and I'm sure they had some customers in other lines too. They, I think, were given credit for some of the ingenious circuit work. I don't know of other bipolar devices that were pursued. But RCA definitely was a little behind Bell and TI in the silicon work. It had come late to the silicon area.

Brock: And how did the work being done on compound semiconductors, gallium arsenide, fit into this mix?

Stanley: The government was behind the move of RCA into gallium arsenide. RCA, of course, had a lot of experience with electrically active solids going back to its work on phosphors for television tubes and camera devices and so was well fitted to carry that sort of work on. The problem was that gallium arsenide didn't make very good transistors and for years and years a large effort was maintained there without any commercial benefit to RCA. One of the problems with gallium arsenide transistors, was that they would lose energy by emitting photons. It took a while, I think, 'till people realized, "Well maybe that's a good thing and we could do something with that." And then RCA did make some, you know, important contributions. I think particularly in some of the lasers that operated at infrared frequencies and were important for communications, those came out of that gallium arsenide program. But that may have been after that effort for gallium arsenide transistors came to an end. I have my own negative views about government contract support because to me it's not surprising that the government contractors might happily support your going on a unicorn hunt. They would be less enthusiastic about having you go out and trap muskrats. But of course trapping muskrats put meat on the table. RCA laboratories perhaps was a little neglectful about putting meat on the table because they were busy off hunting unicorns.

END OF TAPE 1 / BEGINNING OF TAPE 2

Brock: In reflection Tom, how would you characterize the overall experience of RCA with the rise of microcircuitry in the late 1950s and early 1960s?

Stanley: I think RCA, in microcircuitry, made very significant contributions, many of which I had only limited exposure to: in things like some miniature vacuum tubes and things like the Micro-Module program. I've talked at length about the contribution I think we made in the MOS area, and one mustn't forget that we almost glossed over the equally important area of charge-coupled devices for use in cameras, which again is an RCA and Paul Weimer contribution and a very important part of what was being done. They were both in Paul Weimer's original work and some work in my group, in TFTs, may have had some impact. I don't know to what extent that work which was in the 2-6 compounds -- cadmium sulfate, cadmium selenide -- got picked up and whether that's in use at all. I don't know whether that's important in flat panel displays today or what the technology is there.

So RCA made a significant contribution in the early years. By no means do I want to minimize the extraordinary contribution of the people we're honoring in this meeting. I feel a special gratitude to them because it's nice, if you've had the kind of wild dreams of which I've spoken, to have them verified in later years, even if it hasn't been by your own alma mater that it happened. Those extravagant claims we made for the future seem very modest in terms of what has actually come to pass. In later years, RCA had many distractions, and again, I turn and recommend to any reader who might want to follow it Ross Bassett's book, in terms of the interaction of all the players in the MOS field and why some of them succeeded better than others.

Gardner Hendrie: One of the things I think it would be very interesting to hear, is maybe you could tell us a little bit about your family background, your parents, where you grew up, your siblings. Just give us a little flavor of what the environment was like when you were a youngster. Maybe to start with, where were you born?

Stanley: I was born in East Orange, New Jersey; our home was in South Orange. My father had immigrated to this country as a teenager, with just a few loose coins in his pocket, from England. But ultimately he found a job with a company called Bowne and Company, and in not too many years was the president and owner. And so we had a comfortable life. In addition, my mother's family, who were German immigrants-- my grandfather on my mother's side was a successful businessman and ultimately, because the company, Roessler Hastalacher that he controlled had assets in Germany. He realized his company was subject to confiscation when World War II was imminent. And he sold his company for shares in Dupont, which was not the worst investment to have made. So our family was comfortable. My father was quite a bit older than my mother, a very charming and witty man.

Hendrie: What was the company that he went to work for?

Stanley: He was hired by a man named Stanley M. Dewey, and the thought was that perhaps there was a willingness to hire a young man named Stanley because of his boss's surname. Bowne and Company was the name of the company and they were printers and stationers. They had been founded in 1775, a year before the Declaration of Independence. And Robert Bowne, the founder, was an important New York citizen involved with, among other things, with the Erie Canal. The company prospered, mostly when the Securities and Exchange Commission came into being in the 1930s. Suddenly, instead of printing menus and flyers for the steamship companies or something, these printing companies were called upon to do compliance documentation, things like first public offerings and securities offerings. That made for a great advance in the company's activity. Ultimately, after my father's death, my brother became president of the company. After his retirement I served as a chairman of the executive committee for three years, four years I guess.

Hendrie: This company was located in Manhattan?

Stanley: In Manhattan, yes, although it had subsidiaries nationwide and abroad as well.

Hendrie: Could you describe how many brothers and sisters you had?

Stanley: I had one older brother. He was tall and slender. I was short and round. You perhaps remember the 1939 World Fair where the symbol was the Trylon and the Perisphere. We were called the Trylon and the Perisphere. And my brother called me Crisco, which meant fat in the can. But nevertheless, I think I, you know, I was a fairly active young man and a swimmer particularly. My dad, of course, taught me how to body surf, which was a joy for me all through my life.

Hendrie: Where did you get your first education?

Stanley: I went to the public schools in South Orange. The South Mountain Green and Gold was our grammar school up through 6th grade and then through a fairly dismal and dark junior high school. There was, you know, concern that I might end up in a reformatory or something because I was an independent sort. But I was lucky in junior high. I had a wonderful teacher named Tom Giddings and I used to write poetry then, and he encouraged me, so that was a wonderful contact. And then I was lucky enough in that they had already decided to send my brother Ted to the Lawrenceville School, a secondary school in New Jersey. So with some misgivings that I might disgrace his good record there they sent me there, for the last three years of my secondary education.

Hendrie: What's your earliest recollection as a child as to what you wanted to do when you grew up?

Stanley: There's no question: I wanted to be a painter.

Hendrie: Did you do that?

Stanley: Yes, I was at Lawrenceville. Tom Buechner and I, and he's quite an accomplished portrait painter, formed the Easel Club and painted there. Because of the fact that we were anticipating entering the armed service one way or another, some of us accelerated our program. I really was at Lawrenceville just two and a half years, graduated in February. I was trying to outdo my brother in every way I could, and so I was the president of four of the different clubs from —the Periwig Club, which put on productions, and the Easel Club, for painting, and I've forgotten what all. We were a fairly small graduating group in that February group and I walked away with every academic prize, I think, at gradation.

Hendrie: What was your experience with science and math?

Stanley: Well I enjoyed those too. I got the physics prize.

Hendrie: But you enjoyed lots of other things?

Stanley: My problem was that I, you know, I liked to do a lot of things, and I had decided I wanted to go to Yale because of their wonderful art school. I also was, you know, fairly convinced that I didn't want to paint for a living. That, to me, was a terribly, you know, would constrain and make…you ought to do something else to earn your bread and butter. I thought about possibly earning my bread and butter in some technical field. I've forgotten when it was. I know my brother was at Princeton. My brother, of course, being three years older than I was, was drafted immediately upon graduating, served in the Battle of the Bulge, was wounded at the Colmar Pocket.

I ended up going in the Navy. I managed one term at Yale and then went into the Navy. But by this time, even though I had volunteered before VE day. Both VE day and VJ day happened before they finally called me up, which I thought was sort of a waste of time. Why didn't they just forget the whole thing? But at some point in that interval there I was giving Ted a ride to Princeton, maybe when he was thinking about applying or something, and I noticed a sign that said RCA Research Laboratories. And I thought gee, I never noticed that before. We spent our summers down in Spring Lake, on the New Jersey coast, and so I went in there and here was this lovely building out with green grass all around it and trees and, you know, my vision was, gee, when I go to work I'll have to be in a city, but here was this lovely place out in the country. And it was research laboratories. That kind of sounded like a fun thing to do. And it was only an hour away from body surfing. It was just an hour's drive to the Jersey shore. I said, "Oh okay, when I finish college I'll go to work for RCA Laboratories so I better study electrical engineering."

Hendrie: Now that's a really unusual way for someone to pick what they're going to study. That's very cool. How long were you in the Navy?

Stanley: Not even a full year, not even a full year. They finally called us up in the late summer, early fall, and they discharged me in August.

Hendrie: Were you still convinced you wanted to go to Yale at this point?

Stanley: Well I'd already been at Yale a term, you see, and had found it charming so…

Hendrie: Did you think of any other schools?

Stanley: No, no. One of the things that I had in those first few months I had at Yale, I had an English teacher named Mr. Wimsat, who was seven feet tall. And he taught us Chaucer and I just couldn't have imagined how much fun Chaucer was, you know. I have much of Chaucer in my head but, you know, I just couldn't wait to get back with that kind of teaching at my fingertips.

Hendrie: Yale is not known for electrical engineering. Talk to me about how you prepared yourself.

Stanley: Well Yale's lack of a reputation in electrical engineering is well earned. When I went back to Yale was September of 1946, and I had what ended up being my sophomore year, which started out being my freshman year but I kind of hurried up and got through. That's when I got my education as well as by, you know, the people I roomed with. Then I discovered that oh, if you're going to be an electrical engineer you're going to go into the engineering department and learn about steam tables and how to do mechanical drawing with India ink and a pen with an adjustable nib. There were, you know, some enlightening periods at Yale.

But what did happen to me, of course, I was in a hurry to get through and get out, and of all the stupid things I could've said…I have this extra sort of adjustable half year, I will get my degree in four and a half years and get an education while I'm at it. Fortunately Yale now does give its engineers an education but it didn't in those years. Somehow nobody had explained this to me when I had told them, "I'll come to Yale but I'll be an electrical engineer." But luckily I can remember T.S. Eliot came to lecture at Yale one time and they didn't want too much of a mob so they were handing out free tickets. But of course they handed out free tickets to this lecture far away from the part of the campus where I was doing my electrical engineering and so I ended up being too late to get a free ticket to go hear Eliot. But I discovered that the tickets that they'd given out was the same color as my laundry stub so I merely forged a ticket and went in and found there were a lot of empty chairs anyhow.

It was hard to get a decent education. I was getting good grades in my electrical engineering work and I remember convincing my somewhat reluctant advisor instead of metallurgy, to let me go take a course in poetry with Cleanth Brooks, who of course was very able. I managed to struggle to get an education while I was at Yale. I was anxious to get out of Yale and the professors who were supposed to write letters of recommendation for me were very kind of slow to do so and I was worried about that. I had already gone down and talked to the people at RCA and then it turned out they said, "We don't want you to go to work for RCA. We're warning you-- we didn't want to tell you but I guess we better. We're awarding you the Westinghouse Educational Fellowship for your graduate work." I forgot what the sum was, but it was sufficient so that I was able to pay all my college expenses, including my graduate year, either through my earnings of having been a lifeguard at the seashore -- well I didn't earn any body surfing -- and that educational fellowship. So I stayed on at Yale and got my masters there at Yale.

Hendrie: Why did you specialize in electrical engineering?

Stanley: Electrical engineering. As an undergraduate, as I said, you did things like study the steam tables. Of course enthalpy and entropy and all were all very interesting, I guess, but I'm not sure it was the best use. I was lucky in I had some very gifted teachers when I did graduate work. Pete Schultheiss and John Bowers taught servo mechanisms and taught some very complicated mathematics. I almost don't remember the names of them anymore, of dealing with sophisticated feedback circuits and that kind of thing. All this was directly applicable to my work when I got to RCA, although nobody would've guessed it, but the circuits I designed for this little radio depended on the concepts I learned in servo mechanisms so…

Hendrie: Did you take any physics or chemistry courses?

Stanley: Well, you took physics and chemistry sort of in the course of things. Since I'd only learned that I was going to be there as a graduate student late in the game I said, "Well, now do I stay at the hall of graduate studies?" "Oh no, you're an engineer. You go find yourself a place to stay somewhere in town." That's the way it worked in those days. I thought, "That sounds kind of dreary." So I had four great friends, well, two particularly close friends, but the four were moving together as seniors in Timothy Dwight [Timothy Dwight College, Yale University], and I said, "How would you like to have an extra roommate and we'll all split the room rent and we'll all spend 80% of what you otherwise would be. We just won't tell anybody. And they said, "Oh sure." So my graduate year was spent living in a senior dorm, which was much more friendly and also it kept me kind of immersed in the academic side of things. So that was how I did that.

Hendrie: Did you have to write a thesis or anything?

Stanley: Well we did. Bob Case and I put together something that was highly regarded, a gyroscopic velocimeter. We used a gyroscope out of the back end of an airplane that was war surplus and made a little box that you could sit on the table. It could sense rotations. It was sensitive enough that it could sense the rotation of the earth. You turned it a quarter turn and it would show the difference in the rotational speeds that it would experience, and yet it had to be quick enough to sense various…it made quite a hit. My contribution there, one of the things you had to do in order to make it stable was…it worked in two axes, and the power supply to the two axes was 400 cycles, but 90 degrees out of phase so it basically was two independent systems but they interacted with each other by virtue of precession and the inertia connecting them. You had to have a feedback circuit from one to the other that sensed the rate of change. The usual way of doing this in circuits that involved a 400 cycle drive was you rectified it, turned it into a base band or digital integrations and differentiations, all remodulated, and went back. I pointed out that if you just look at the signal that's driving these motors, if you subtract each cycle from the next, the subsequent cycle, you, in fact, will generate something that is the rate of change of that carrier, and it will only be a ¼ cycle delayed from the driving signal. We used that to do the cross coupling. I guess if I'd been invention minded that might've been an invention I could have had at that time. But that was regarded as an interesting thing.

Hendrie: Are we now up to where you join RCA?

Stanley: Well actually I graduated from Yale in the summer of 1950, was accepted to go to work at RCA, was there two weeks, had an accident that resulted in my broken neck and paralysis. But RCA was very good. They stuck with me and kept in touch, and so after six months I could hobble around well enough to go back to work. I went back to work at RCA in January of 1951. I was still hobbling around. As the years went by I got so I could walk pretty well but I still had a great collar and walked with a crutch and all. And met my wife on April 1 and went back to-- it was a Sunday in 1951, went back to my coworkers at the labs who were working on transistors at that time and said, "Well, I just met the girl I'm gonna marry," which we did in not very many months.

Brock: Tom, we reviewed in a systematic fashion your experiences at RCA and your experiences with semiconductors at RCA from the early 1950s to the early 1960s. I thought we could revisit some of these periods in this experience and ask you to reflect on or share any particular telling or memorable experiences from those times. Was there anything from your work in developing the transistor radio or the transistor television set, or in that work you were doing with the RCA licensee community? Any stories from that part of your work experience that stand out?

Stanley: Let me see whether there are a few things that would have been a diversion in our earlier conversation. Much of what one learns in a career can be individual instances of something which were very striking. I spoke of how I admired my first real boss, who was Al Barco. Part of his appeal was that he was, you know, totally honest, totally tactless, and very, very sharp. I remember being a little in awe of him. He had made, in the days before the Tektronix scope, he had made an oscilloscope that had the performance characteristics of a Tektronix because he felt he really needed something like that. And we hardly dared touch it but I remember we were working with it and unable to get a clear signal, and Al came in to look at it and then we said, "Well geez, we're having trouble." And he pulled back and gave it a good whack on the side. It came back to life beautifully. I said "Well it's fine to do that if you know where to do it and it's your baby." I always loved his directness in solving problems. In the early days we got hold of one of Charlie Mueller's first transistors. Charlie had —said you can only use it up to 75 milliwatts. Well, that was immediately a challenge to Al, so he put it in a bench vice, filed off the Araldite [epoxy] from the collector side of it, soldered a big fin on it, ran it at half a watt, and showed the community there's nothing that they can't do. You can make a power transistor if you just get the heat out of it. So he had a wonderful way of solving that kind of problem. The scientists hated him because he was so honest and direct and pointed out the folly of much of what they were doing, so that they were able to persuade Jim Hillier to sort of move him to one side. But he was a great guy.

Another experience, kind of interesting, shows you the hazards that you have. We had, in addition to making a little pocket radio, two experiences. We built a very respectable automobile radio. It had to operate at higher powers and so forth. And we showed it to Delco, the electronics arm of General Motors, as licensees, and they were invited in. We were pretty pleased with this thing and putting it through its paces. We would show them the performance characteristic of our radio and the performance characteristic of the Cadillac, top of the line, Delco radio. We'd made all the same measurements on the two devices. I remember one of the characteristics had to do with the automatic volume control performance of the radio for a wide range of input signals. One of the engineers from Delco said "Well, you know, see, our performance is much finer, much superior. You have to be able to match that in that radio of yours." But he had gotten the two sides mixed up! The one that he was pointing to as inferior was in fact the Cadillac Delco radio performance. It was an interesting experience to have had anyhow.

The other thing of course that the car radio program did: With my pal on that, Larry Freedman, we had to test it to show that it continued to operate, in order to please them, all the way from 40 degrees below zero, which of course is the same in Centigrade or Fahrenheit, to 80 degrees Centigrade, which is about 176 or 185 or something like that Fahrenheit. I said "Well, you know, the steering will be soft at that temperature." They said "Oh no, it'll be under the hood and it'll have the heat on it." Well anyway, you could measure all of that range of temperatures. Down in the basement was an old structure called a Tenney chamber where I think Cellosolve acetate was the operating fluid, and you could heat it up and measure performance at different temperatures. You started out by grinding up dry ice and throwing it in there to get it down to 40 below, and then you sat and cooked it and measured the performance as it went up. We were doing this with our automobile radio, and occasionally it would blink out. Larry and I, we'd have to quick go find out what didn't like this temperature. We'd open the Tenney chamber up with all these fumes and the Cellosolve acetate and try to find it and fix it and then close it up again and go ahead. Well, driving home from work not long after that, Larry went blind, and somebody else had to take the wheel. Fortunately he was in a car pool and someone took the wheel and he went home. He recovered his sight but when we went back we talked to Al about this we thought we better look it up in the toxicology books. We found out that, yes, the fumes of Cellosolve acetate are very toxic and they can make you go blind. But we learned that the hard way.

Sort of rambling back on interesting things you learn, Cambridge was a wonderful experience. I studied with Maurice Wilkes. I had no real idea I'd be getting into integrated circuits yet. That was in the days when all the computers had names. Ours was the EDSAC. Forget exactly what EDSAC stood for [Electronic Delay Storage Automatic Calculator]. The top surface of it had a succession of peaked roofs, all kind of in a row, and I remember asking Maurice, "You know Wilkes, what's with the roofs. I can't imagine that that really improves the ventilation." It was a perforated sheet. He says "Oh Stanley, that's anti-teacup devices." He said, "The largest single element of downtime that we had on the computer in the early years was people would set their teacup down on top of it and then they'd knock it over and the tea would go down into the computer. And of course it wouldn't like that and we'd have to get all the tea out of there before we go again. So we made this change in roof so people couldn't put their teacup down on the top of the computer." So a very, very practical solution to the problem. But to me one of the most educational experiences I had at Cambridge was I had a lovely office mate named David Wheeler, and in those days the way you talked to the computers was with punched paper tape. Our big competition was the Ferranti effort on a computer. I don't remember the name of their computer up in Liverpool. I guess it was--

Hendrie: Manchester.

Stanley: Manchester, okay. It was in the north of England, yes. So we were getting to more and more lengthy programs, and one of the desires was to develop a fast paper tape reader. The Ferranti people had done an absolutely beautiful job and we went down -- I don't know if I was presenting again, but we went to a conference in London, and Wheeler was presenting his fast paper tape reader down there and Ferranti was presenting theirs. Ferranti had done an extraordinary job of tailoring a servo-mechanism so that the take-up wheel of the fast reader would take up the paper tape without tearing it, because that was the big problem. When you'd just try to run an ordinary paper tape reader, once it got to a certain speed the paper would tear. So they had a very elaborate sort of mechanism because of course the radius changes on both the feed and the take up wheel in the course of a run. And he presented his paper and showed what speeds they were able to achieve and all. Wheeler got up and showed his paper – on a fast paper tape reader. And it was smaller than the Ferranti one and it was faster. He described it and, you know, it had all these wonderful specs, but then he finished and the question and answer came. And the audience said "But Dr. Wheeler, you haven't described the pick up reel, the take up reel." He said "Oh, we catch the tape in a waste bin." And everybody burst into great laughter and he said, "Could I ask the earlier speaker when he's finished his run what does he do with the take up reel?" They said "Oh, well we put it on these separate sort of hubs and we rewind it on to the feed reel." And Wheeler said "We rewind the tape onto a feed reel too. It comes up out of the waste bin very easily." This taught me that you should never do anything you don't really have to.

END OF TAPE 2 / BEGINNING OF TAPE 3

Brock: We'll return to one topic that you had brought up, about the conferences and the technical support that the RCA labs offered its licensee communities, especially with transistor technology. It reminded me of what I have heard about, and read about, similar activity by Bell Labs, right at this very same moment. The Bell Labs and Western Electric transistor licensees were having conferences and publications for them. Was that something that was a widespread practice or was this something particular to Bell, to RCA, and to transistors?

Stanley: I think we're talking in a sense about two things. RCA was very much under the eye of the anti-trust division of the government because of its powerful position in color television. RCA really could have totally dominated television had it not been restrained, because everything -- the color tube, the cameras, the system, after the happy demise of the CBS whirling disc system -- these were all RCA developments. And RCA had one of the most popular if not the most popular television broadcast division, the NBC division. RCA made the tubes, it made the receivers. And so the government required RCA to offer their patents to any comer if they paid license fee. And it was in RCA's own interest to teach them. They weren't really required by law as far as I know, to teach the licensees but did so to keep them happy with the fat fees they were paying. They established what I spoke of as the Industry Service Laboratory that I was part of. And we wrote, I think, rather fine licensee bulletins, describing each of the things we did and we went into quite some detail. These would also then be recorded as Princeton Technical Reports. Now the same kind of licensing activity, but not nearly so much in the hands of the Industry Service Laboratory, characterized the work in semiconductors. But I think more important, almost than the license -- the licensing work sort of made it legal, made it official -- was a wonderful community of people who knew each other. Bill Webster was on a close friends basis, particularly between, I think there was a real affinity between the RCA Labs researchers and the Bell researchers. But this was true of many others too and of course there was a good deal of people having graduated from Bell, less so out of RCA I think. So there was a real community and these people knew each other, trusted each other, and they earned their trust by not abusing what they learned. But that kind of community was very important for people like Steve [Hofstein] and Freddy [Heiman]. In a sense, even though I'd had the experience with RCA's licensees in the television area, I felt my presence might even be a little intrusive, so I tended not myself personally to attend so many of these conferences and didn't have the good fortune of getting to know some of these people as well as Bill did. But I knew them through Bill. Does that answer?

Brock: Yes it does. Yes, thanks. Another question area, very different, something that we've talked about in the past is that at RCA, in the later 1950s, there seemed to be a wide-ranging exploration of different electronic device types, different materials, all looking at the extension of electronic components in there. And I was wondering if you could just talk about some of the diversity of the activity that was going on?

Stanley: You know, I remember that it existed. I've perhaps blocked because it was so unsuccessful. I think we did an awful lot of silly things and they had silly names like bionics and molelectronics and anything to give a government agency or the military agency something to go to their congressman with and say, "Here's the next new wonderful thing and we ought fund this and that'll put food on the table for all of the people who do those things." But I think it was distracting and the reason that the smaller companies were more successful, in many instances, is that they didn't have a lot of this nonsense going on. Now that's a jaded, present perception that I have. I certainly was not innocent of pushing some of these things myself. But they were, for the most part, unsuccessful. Something that I should have mentioned in our earlier conversation, and maybe you could sandwich it in, is that a good deal of our work after the initial MOS successes, was on trying to develop connection means. It seemed that gold bonding, was an awfully primitive technique where an individual technician had to make each connection to a wafer, must be something that would be supplanted by something else. And we and I think IBM spent a lot of time trying to find out sophisticated ways of making interconnections, none of which in my knowledge, ever really worked out.

Brock: That would be attaching the--

Stanley: Attaching the wafer--

Brock: Oh.

Stanley: Attaching the silicon wafer to the package or to adjacent wafers. We did do a certain amount of the circuitry that was on the wafer. But the most important thing was how do you get from the wafer? It was such a ridiculous thing that you had these very, very tiny devices on this wafer and then you had the big gumshoe connections to the outside world. Of course the sensible way to solve it was to not go to the outside world to the very end, which is what Intel ultimately was smart enough to do.

Brock: In the same way that we've talked about the contributions of RCA to the broader microelectronics effort and story in this period, is there a parallel to contributions that RCA made in the area of computing more broadly through its computer research?

Stanley: I think if you said what was RCA's contribution to computing,I'm not really aware of those things in the computer divisions that may have been become more widespread. I think Jan Rajchman made very important contributions in magnetic devices early on. For example he invented the transfluxor which was like a magnetic ring but with two holes in it so that a small amount of flux could control a large amount of flux. It didn't ever really fly I don't think and of course there were a lot of works done on making many little holes in a sheet of magnetic material instead of individual cores wired together. And I don't think that really got off the ground either. It may have, but I'm not aware that it did. But I don't really know of any other areas in computers that RCA could say, "Ah, we contributed to that."

Hendrie: Were you involved at all or had knowledge of at the time when Jan got into the patent dispute on coincident current, magnetic core memory with MIT? He is alleged to have done some very early work. But it was just a little bit later, like weeks or months later, than Forrester did at MIT.

Stanley: I remember this being a painful distraction for Jan, but I did not get involved in that. Jan was one of the most charming people. Because English was not his first language, he used it beautifully, subtly, I remember his describing when we were working on optical memory. He was working on optical memories. And he said "Well, we try to find any phenomena at all that will annoy the light."

<Laughter>

Stanley: You know, it was so wonderful way of describing how to modulate things. Oh there was something else. Jan knew Turing. He worked with Turing in Princeton I think, when Turing was at the

Institute for Advanced Study and so he was really an honored-- but not, you know, practical product oriented. There was an appalling degree of strong language used sometimes in our gatherings but never Jan. Of course he was made responsible for Project Lightning, which was this National Security Agency project and therefore classified. Jan's family was still behind the Iron Curtain and so he was not able to get a security clearance. So he could write the final reports and so forth of Project Lightning, but after he had lifted his pen from the paper and finished writing them, he was not permitted to read them.

Brock: <Laughs> We did talk about, briefly about, Harwick Johnson and in his early demonstration of putting different circuit elements in a single piece of semiconductor material. But it is my impression from your remarks that in some sense his work was a one-off, that there wasn't a continuous line that his work sort of originated or was part of.

Stanley: No, I think it was correct to say that it was not a continuous line. He, I think, became a group head under -- not that. The choice of that as a management position didn't really come into being until about 1958 or 1959 or something. So, he I think, more or less supervised the group of which Torkel [Wallmark] was a part. And like many people without responsibility, he sort of no longer made a lot of contributions himself. I first remember Harwick and Larry Giacametto, I think that was his name, were rooming together and were always teased about the fact that they always seemed to wear identical black shoes and wondered whether if you looked carefully you would sometimes discover one of them had on two left shoes and the other two right shoes. But they did a lot of work on equivalent circuits. So you know, a lot of RBB prime emerged from their efforts. But I think more and more Harwick played the role of elder statesman at the labs and not so much of an active contributor of wise ideas.

Brock: Was he active in the sense of promoting the research line on integrated electronics?

Stanley: I don't think so. I don't think so. I think he just was a wise, you know, semiconductor consultant.

Brock: Okay. Let me just refer to my question list here to see if there's any other areas. Well, we could perhaps turn now to the later trajectory and talk about from the middle 1960s, you know, the rest of your involvement--

Stanley: Sure. Sure.

Brock: --With the MOS and MOS integrated circuits and you went into next.

Stanley: I remained the head of the Integrated Electronics Group. As I say, I think our work was, the hard aftermath of following what appeared to be initial success with really nailing down the stability problems. Jerry Hertzog and Joe Scott picked up on complementary symmetry [CMOS] and had quite an impressive facility in which to pursue that, and got themselves a contract to support it. And as I say, we messed around but not terribly successfully with other ways to interconnect things. I think we certainly did not have the momentum in my last few years, but there was not that many of them as head of the Integrated Electronics activity. We re-organized and I had a tour of duty for about three years as manager of research, one of two managers of research and business evaluation, which did not measure up to expectations. Perhaps it was doomed from the start. Then I was called back into the mainstream with the title of Director of Systems Research and I had responsibility at that point for about half of all of RCAs research efforts, which included communications, microwave, computer and I think we still called it systems research, but it was basically the home instruments side of things.

Brock: And home instruments--

Stanley: Like the television and all of those really.

Brock: Oh, okay.

Stanley: I guess I'm trying to remember what, Consumer Electronics? I guess consumer, all consumer electronics, that's what we called it. I've forgotten the titles. But it was basically a responsibility and it came with the problems that when the board got around to it, that I would be then staff Vice President, the Director title was when the then director of the Consumer Electronics Lab learned that he was not to move up, he decided to go back to Camden or to Cherry Hill. And so I acted both as the, you know, staff VP and as the director of the Consumer Electronics Lab.

One of the disciplines that I learned from Al Barco was try always to give the people who report to you full credit for what they do and don't compete with them. And yet I couldn't resist sort of hiding in a corner and doing a little inventing now and again. What happened on a very small scale, I felt that especially with the MOS as a pickup device, that one would be able to sense very small changes in capacitance with a good noise figure and I proposed a system called video disc and ultimately got the patent on that system. And after I tried to infect somebody else with the thought that maybe they'd thought that up and would do something with it. I had an interesting responsibility as I not only reported to Bill Webster as head of the Integrated Electronics Group for him, and I reported to Al Barco who was director of the Systems Research Lab as head of his group of fellows doing integrated electronics research. And so in our staff meetings I talked up the idea of a video disc system and it ultimately took hold. And as you know, Margaret Graham points out in her book that, you know, it more and more gained traction. We had a big show to the general public about it. But unfortunately as when economic circumstances seemed to be a constraint, it would be put on the back shelf for a year or two. RCA then made a major investment, hundreds of millions of dollars in video disc, and we developed absolutely beautiful technology -- by this time my role in it had been largely forgotten, although they very kindly honored me finally at the end of it. But when we finally came to market with it, it was just when video tape recorders were very much in vogue and the dealers particularly didn't want to muddy the waters with a new system. And so it failed. It was a great disappointment to me because I thought, "Well, this would be a nice feather in my cap. I like it."

Brock: Could you talk to the principle of the video disc and how it is similar to or different from things like compact discs and DVDs?

Stanley: Well, basically the consumer function to be served was a read only, inexpensive thing which would give you an hour or whatever of television material from whatever source. And you'd have a player and just like you played a record. It was the fourth quadrant. You had radio, you had television, you had you know, records, but you didn't have the television correspondent to that. And we had a system. We were well aware that our approach to it was one that might have a limit. It used a stylus and a conducting record surface but the capacitance changed that this stylus sensed. This gave you a signal in which the signal and noise and the bandwidth and everything exceeded anything that you could do in magnetic tape and in a medium and with a player that could be much less costly, but it was a play only medium. And so as such, it did exactly for the purchaser what DVDs do today. And we, I think, nobody recognized, "Well the day will come where it would be better to do this thing with optical means." We were aware that the replication process goes right down to molecular levels. You can do replication, although it's been surprising that it's never been applied to microelectronics because you can replicate right down to atomic scale. So we recognized this and it made the grade. I had responsibility for it only sort of at a distance because it was being pursued and there again I tried to kind of keep hands off. But it was pursued by Don McCoy and one of my lab directors in a group for which I had responsibility. But as I say, it used a disc about the same size as an LP in a protective carrying case and but as I say, the performance and cost and all were there but the market was not at the time.

Hendrie: How far did you take it at the labs before you handed it over to a product division?

Stanley: It was-

Hendrie: Where was it handed over?

Stanley: This was a lab in Indianapolis, both the record and the receiver or electronics part. It was a totally collective operation, variously under the -- I think Dick Sonnenfeld probably was responsible for the management more than anyone else. Don McCoy moved with the program -- a large part of the labs moved over to Indianapolis to pursue it. There was never any schism between the two groups.

Hendrie: It wasn't one of these, we'll take it this far and then hand it off?

Stanley: Not at all. Not at all. And that's why it succeeded in everything except the key question whether anybody would pay for it.

Brock: The bridge between the MOS and the video disc was that--

Stanley: Because I was working on the MOS I recognized that if the first stage, with the noise properties, the sense capacitors changed, I recognized the MOS was the ideal device. And so that if it was what the first thing that the needle saw, it would give you this performance. I remembered my first calculations I thought, "Well I'm a little on the thin edge of feasibility here" but that was conservative calculations…that of course, it did much better. But of course the whole key to the DVD is not so much the technology of recording and replicating, it's the information compression that our digital techniques have made possible. So that's what allows you to do all the wonderful things you're doing now.

Brock: It's the software rather than-

Stanley: Yeah, yeah the fact that you don't record just the raw signals.

Brock: Interesting. And well maybe we could just talk a little bit then about when you left RCA and--

Stanley: Well I should first--

Brock: --And thereafter.

Stanley: I should, to be complete, what happened of course is RCA in about 1973 was it, RCA went out of the computer business. And it was evident that we needed to contract the level of effort at the labs because of one chunk of RCA was gone. And it was apparent too that all the various lab directors, I'd try to keep a low profile and been successful enough at that that it was evident to everybody, even myself included, that the labs could function-- my chief kind of counterpart in the management structure at that point was one of those who was departing. And we realized well, you know, “Why don't the lab directors just report directly to Bill” and “Gee, Tom we probably could find something for you to do.” And so I went from this line position of responsibility for half the labs to more of a staff role, but inevitably ended up with some line responsibilities. I had responsibility for manufacturing research and very pleasantly for me. But anyhow, you know, to my good fortune, I was kept on with a title of Staff Vice President but now what was called research programs which was more of a staff role, an advisory role, a tolerated role. But I did have responsibility for our laboratories in Zurich and Tokyo. And so that and the responsibility for manufacturing research meant that I spent quite a lot of time visiting associates or competitors or so forth overseas. And also of course, the-- I'm trying to remember when the video disc program wound up, but certainly yes, I think, in the course of that I remember twice getting a call saying, could you possibly next week get over to Berlin because the Teldec people were also showing there. I'm not quite sure what the timing was on that, where I was in my management role. But I did have, the last eight years or so of my labs experience was in the second role. I must admit I think anyone would prefer a line job to a staff job. But it was nice that I could continue at that basis. Then as I'd mentioned to you in an earlier conversation concerns, family concerns made me retire fairly early.

Hendrie: So what year did you retire?

Stanley: 1982. Actually what happened was -- I think it was 1982. One part of our corporate belt tightenings required us to close down the Tokyo laboratory. I was sort of slated to retire just about that time and I said “Well, no it's my responsibility. I've got to go over there and face the music, because they were not very happy about that at all.” And so I stayed on until I could perform that difficult task.

Brock: I was just going to ask, in the time since you've been retired, have you maintained a connection to science and technology at all or have you returned to painting? Have you finally become a painter?

Stanley: Well I, one way or another in the course of time, done a little painting, but not as much -- somehow it's difficult, I always found, it's difficult to paint if you have time to do it. You're more able to do something if you're pressed from all sides. <laughs> I have designed a succession of ways to get me in and out of a boat and down to the water. One thing I had designed which was quite successful and it's mechanical engineering so I had no business doing that, but it's called the Wunderhoist. And it's a machine that sits in about a six inch portion of the back seat of my convertible, which reaches out, picks my wheelchair up off the adjacent roadway and puts it in the back seat of the convertible and vice versa, so that I can get around and drive and so forth and have my wheelchair right there when I need it. So I've played that way but it had no connection at all. I've never served on any committees. I have served on committees but not techno committees. I did, I guess it was semi technical, I did serve with the folks who run Voice of America and that sort of thing. Also, fortunately, my long time friend Bill Webster and I have stayed close and we get to see each other once or twice a year most of the time.

Brock: Well great. I think that's all of the questions I have. Gardner, do you have any?

Hendrie: I'd like to just ask one sort of open-ended question. Do you have any advice that you might give to young people who, you know, are contemplating a career in science or engineering?

Stanley: Yes.

Hendrie: --A technical career.

Stanley: Very happy to unburden myself. If you are contemplating a career in technology of some sort, that will keep you very busy. You will learn everything you need to know once you get to work, if you have at least a few of the fundamental building blocks, because everything you do will be new to what you have spent your time and education. So go to poetry classes, go to art classes, go to history classes, go to philosophy classes, go to psychology classes, what's going to be important in your life and it's going to make you a much better engineer or scientist than those of your fellows who have not had that broad education.

Hendrie: Well thank you very much. That was very heartfelt, very clearly, long thought through. Well thank you very much--

Stanley: Yes, yes and now we get the furniture movers in and we're in good shape.

Hendrie: --For doing this for the Computer History Museum and the Chemical Heritage [Foundation].

END OF INTERVIEW

