Oral History of Robert (Bob) H. Norman

Interviewed by:
David Laws

Recorded: May 7, 2009
Mountain View, California

CHM Reference number: X5445.2009

David Laws: It's Thursday, May 7, 2009. We're here at the Computer History Museum in Mountain View, California, and for the next couple of hours we're going to be interviewing Bob Norman on his career in the microelectronics industry. Can you give us a little bit of background on where you were born, where did you grow up, and a little bit about your family?

Robert (Bob) Norman: I was born in New York and grew up a lot in New York. I joined the Navy at 17 and so I had to come back to finish high school. When I first came back I tried to go back to the old high school but I couldn't fit in the desk. So I worked as a draftsman and ended up going to night school and finishing up. I applied at Harvard and got a letter saying you're number one on the list of those who didn't make it. Please apply next year. I didn't want to do that and I saw something that said that Oklahoma A&M had several hundred openings for freshman engineering students, and so I did that. This was July. They accepted me and I became one of the many returning vets that took advantage of that wonderful GI Bill of Rights. Well, I got recalled in '50 for the Korean War and then I went on to graduate in '54. I was interested in electrical engineering from the day I started. I used to fool around with old radios as a kid and. of course , I passed the Eddy (Eddy Aptitude Test which determined your acceptance for Radio Technician training) test in high school and started in the technician school in the Navy. I got bored with that and went to sea. I was afraid the war would end without me. I think the high school kids that came to college in that era resented us because we were more serious about school. After four years of high school they were in a more playful mood. Those of us who went to college were not that much in a playful mood and the instructors respected that. I remember my chemistry teacher asking who in the class had been out of school the longest, and this one guy raised his hand, and he was a Colonel, flew B-17s, and so that chemistry instructor says, "Well, then I will ask you if you understand what I just said and if you do I assume the rest of the class did." And that's kind of the attitude in college at that time. I did very well in math and in engineering. I used to tutor in math and at the time I had long legs so I would sit in the front row in my double E classes, and unfortunately I would stick my legs way out and fall asleep. And I woke up once and everybody was laughing and the professor said, "I will repeat what I just said for Mr. Norman's benefit." He just said, "If you do as well as he does in this subject, you can fall asleep too." So anyhow, I really enjoyed the engineering course.

Laws: What stimulated your interest in engineering? Was it something from the family or a friend or a mentor?

Norman: Yes. My father. When I was about fourth grade or something I was asked to write a composition about what I want to be when I grow up, and I said, "I want to be an engineer." It was about a paragraph. And I showed it to my father and he was an engineer. He flew in World War I for the Canadians and was shot down and wore a basket on his leg, but he went to Carnegie Tech and he worked in the steel mills at night to pay for going to school during the day. And he said, "They let me sleep sometimes" but he worked his way through school. Anyhow, he took a look at my paragraph - and remember I'm thinking locomotive engineer. He says, "You never are an engineer. You study engineering," and he, shall we say, he rewrote my paper except now it was a full page long, and of course when I brought it in my teacher knew exactly what had happened and she made me stand up in front of the class and read it. He invented the oil burner among other things and was heavily involved in aviation after the war. He used to take me to watch fly-ins with these guys in these old crates. There was a big vacant field and they would all land there and yap at each other and take off. So anyhow, I was kind of doomed to become an engineer. I really liked it as I said. When I was recalled I went to a refresher course and became a technician in a PBM seaplane squadron flight crew and - I'd look at the status bar and there was a whole bunch of red disks up there and those red disks meant that the equipment wasn't working and therefore the aircraft was not up. And so part of my job was to get rid of those red disks. Part of my job was training the technicians in the squadron to help that effort. I'm very proud of the fact that we went from a typical 37-type percent aircraft availability to a 72% aircraft availability. We doubled the fleet-wide average. We had to fly 19-hour patrols plus any Search and Rescue call-outs, and that was every other day. If anything didn't work when we got back, we had to fix it before we went to bed so I have a picture somewhere of me in the middle of the night in a nose hangar leaning over a radar. Actually, the radars were pretty reliable. They were Philco radars and I developed a pretty good opinion about Philco from working on that stuff.

Laws: This would have been when?

Norman: Nineteen fifty through fifty-two.

Laws: This would be the time of the Korean War.

Norman: Right. But some equipment was awful. The bombing equipment-- we had an- APQ-5 bombing equipment and I was radar, bombardier and countermeasures. And with the APQ-5 you could almost fry eggs on the case. One of the things that really surprised me is it used a lot of the precision wire-wound resistors. I thought of those as being rock solid and accurate no matter what and they were the worst offenders under temperature and they would change value dramatically. And of course all the components in those days were on these strips and you had to really disassemble things to get at a component to change it. So it was a fairly big deal but nevertheless I used to say that's why we got the big bucks.

Laws: Very early on you learned the importance of reliability and maintainability.

Norman: I became devoted to reliability and maintainability. In fact, that's what drove me and it drove me throughout my career.

Laws: You went to Oklahoma A&M. When did graduated from there? Do you remember the year?

Norman: In the summer of 1954.

Laws: After graduation you went to Sperry Gyroscope. Is that correct?

Norman: Yes. I went to Sperry the preceding summer. I was one of the handful that they hired as summer employees, undergraduates. They normally were hiring masters and then they tried this experiment, and there was some handful of us they hired as undergraduates. So I worked and that was terrific. When they were interviewing me I asked, "Well, what are you doing with transistors?" And they said, "Well, you're it."

Laws: Had you studied transistors at all at college?

Norman: No. What happened was I had won a paper competition, the AIEE paper competition, on the use of matrix algebra in circuit analysis, and there was a co-winner, Billy Law interestingly enough., This was after the November '52 Transistor issue of the IRE Proceedings and his paper was on the transistor, and that was my expertise, reading his paper.

Laws: Now you were the most knowledgeable person at Sperry. Sperry was in Long Island. Is that correct?

Norman: Yes, Great Neck. They had a big operation there, I think 15 to 17,000 engineers and support personnel. They had a lot of analog instrument servo work and things like that. They built this Sky Sweeper radar guided anti-aircraft gun and a whole lot of things.

Laws: What did you do during that summer job?

Norman: I started off. I got some point contact transistors in tar-filled cartridges from Western Electric and I had little jigs and fixtures built. I started fooling around with the circuits. I did a lot of measurements on it and found among other things that there was a big slot in the alpha, - the current gain, of the transistors. Those were transistors selected out by Western Electric for their own use. So that kind of cooled me on using them as a source.

Laws: It was just nonfunctional in that region?

Norman: There were no transistors in that region. They selected them out for their own use and then sold the rest to people like us. I was involved in the digital stuff. They hired another guy later who was an analog transistor guy like me and he worked with the analog people. We were in the Advanced Weapons System Development Department and I was in the Digital Section. Sperry was then building a digital computer, SPEEDAC, a vacuum tube computer, and I was part of that project and doing the transistor work and then doing some communication studies that they had asked me to do. So I was A) busy and B) happy doing this stuff. I only saw one junction transistor in a plastic bead. I never saw any other that summer so all my work was with the point contact.

Laws: You graduated with a BS in electrical engineering?

Norman: That's correct. Yeah, and a minor in Math.

Laws: Then Sperry hired you full time after you graduated. What was your appointment at Sperry? What was your assignment?

Norman: Well, I went right back to the same section and there were a couple other transistor guys, and my job was to kind of lead the transistorization. We had what we called T&D, transistorization and digitalization, and that is applying digital computers to weapon systems and transistorizing them. , So my job was to come up with ways to do that and to propagate that information in the company. And at the same time again I was working on SPEEDAC so

I was learning digital. There was the October ’53, Digital Computer issue of the Proceedings . It was a very useful issue. The bone I pick with the IRE, IEEE now, is that they don't have the Proceedings the way they used to. It's such a valuable resource but that's another era. So I start working on SPEEDAC, a vacuum tube computer and the way that it was done: They had plug-in modules, which were say vacuum tube flip-flop and vacuum tube gates. So it'd be some half a dozen tubes or so with delay lines to make sure that there were no timing- race problems, and I'm trying to emulate that with transistors. One of the problems in that era was that in the vacuum tube version of course there was no such thing as fan-in and fan-out issues. I'd say, "Well, how much load-driving capability do you want these to have?" "Well, infinite." "And how about the fan-in?" "Infinite. We do it with vacuum tubes." And I said, "Well, it's not going to happen." Fortunately, about that time Sperry was building a Marine Corps digital computer [the MSG-5] using the VT fuse pencil tubes that implemented the SEAC/DYSEAC logic of the National Bureau Standards - dynamic logic where a one was a circulating pulse and a zero was none.

Somebody dropped off some hermetically-sealed pulse transformers with some hope that we could do that. But I looked at that computer and I said, "This is a perfect opportunity to see what actual loads we have to contend with." And so we studied the loading on those modules and it turned out that for the most part a fan-in and fan-out of two was sufficient with a standard deviation of two in each case. So that if we designed for fan-in, fan-out of four, then we covered all cases except the clock lines. And so we came up with a number 16 based on the word lengths we were using then. And that made a wonderful change in the way we could go about designing the transistor logic. I had the advantage because we were doing real time military computing and I was able to talk about “good enough” to get the job done. There was a whole other branch of computing which was [characterized by] never good enough. That's the kinds of things that Whirlwind and IBM and Univac were doing where they had to go faster and faster and I didn't have to. A typical clock speed for our computers was 260 kilohertz. When I was given the job of the first anti-submarine warfare signal processing computer, we had a clock speed of 400 kilohertz. Where we did the correlations of the incoming audio, the correlations ran at 10 megahertz but the smoothing, the post integration I think ran about 400 kilohertz.

Laws: What year was this when you were working on this anti-submarine computer?

Norman: Fifty-five--

Laws: That was in your first year of work—

Norman: No. I think '56.

Laws: About how complex was that? How many gates? How many transistors? Do you remember?

Norman: There were three hydrophones and we would correlate the pairs, one common and a pair on each side, and it was of the order of two thousand gates on each side. . No. Two thousand gates total, maybe a thousand gates on either side, and then we had a drum memory which became a learning experience in its own. In fact early on we made our own drum memories. I'll never forget the first one. The machine shop made the drum and sent it to us and they had cut the shaft too small and it was ridiculous. You coudn't make it bigger. You had to go cut another drum. They would fabricate the drum and then plate it and that's how we got the memory. This was in the days when there was a British paper that wrapped wire around a drum for storage.

Laws: What other notable machines did you work on while you were at Sperry?

Norman: Well, the NAVDAC Polaris computer which was used on the first Polaris launch as the shipboard computer, and at the time I think that was the first [Navy] one. Then the Sergeant coordinate conversion computer which went on the launch system for the Sergeant missile, which was the first Army computer.

The <u>first</u> computer I built [alone] was a machine tool control computer using DCTL using the Philco surface barrier transistors. It ran at very low voltage so I figured well, I'll solve that problem. I'll just use small storage batteries, to supply power. Then I don't have to worry. At that time the power supplies we were using were 500 volt power supplies so if there's a little noise on the control pot you'd blow out all the transistors. I kind of did that from time to time, but the first time I did it was on this machine tool control computer. I was reaching in with the alligator clip to connect the power and I hit the AC and it blew out the whole thing and we had to do it over, and it wasn't that good a solution, but what do I know? So on that anti-submarine warfare computer the good engineering decision [was] that it's idiotic for me to worry about designing the power supplies. We needed three of them as I recall, one for the logic and two for the drum memory. I figured I should be able to buy power supplies. Well, that was in the days of the hype being much better than the fact. So I wound up having to use the magnetic components of the power supplies [that] we bought and put in our own circuit and devices, such as the rectifiers and regulators and one of my early patents was for the constant current source to use in the regulators. Everything I tested [by running] voltages up and down until something quit. It's the standard-type thing that I would do then. As I'm running the voltage up on its current source the wire literally changed color and I decided I ought to put a resistor in there, which I then did, but at any rate the power supplies worked fine. We were an early customer for Photo Circuits [Inc] when they first came out with printed circuit boards, and that was better than sliced bread. The PC connectors were useless, those finger-type things. Transistors were expensive so we used Molectro connectors and each transistor was on a connector so that if something went wrong with a board we still had that transistor. , And when I built a shift register or something and it wouldn't work I started slapping the boards so that the finger connectors would make contact. So we decided that the approach to take, and this was a marked difference from the commercial guys, we went to the six by twelve [inch] boards, two of them in a module with soldered wire connections at one end so that it's really a six by twenty-four board folded. And that was a module and the connector on that, because there were so few of them, was a very good pin-type connector. We essentially didn't have connector problems after that. Remember of course we always had temperature and vibration and all that stuff, on everything we did. In the early days of testing the submarine thing I had the two channels cross connected in the rack, and interestingly enough I had [built] one of these plug-in things that just brought wires through so you could pull the module out and test it, and I had the wires crossed in that plug-in thing so when everything was plugged in, and I had one of the post integrators out on this adapter, the thing worked like a hose. And I would take the module off the adapter, pull out the adapter, plug the module in, and nothing, and I used- I think I kind of created things for myself like that. We had targets out to 26 miles that we could actually get position on passively.

Laws: So you were just listening for signals.

Norman: Just listening, and correlating what was coming in.

Laws: That's impressive. I believe you had some ideas on using semiconductors for storage at this time. Did you actually put that into practice?

Norman: Not at Sperry. Well, yeah, I did. The National Bureau of Standards had come out with this diode storage scheme, a pumped diode storage thing. Sperry then had Sperry Semiconductor. In fact, Bob Youden who's around here, used to work at Sperry Semiconductor. We had a fundamental problem with TI transistors. We were trying to do silicon stuff, but the high collector voltage and the high storage time meant that we had to use them in unsaturated logic. So one of my inventions is called Zener Diode Coupled Logic. Actually, when I was first experimenting with that, I used batteries then used low-voltage Zener diodes as couplers. We dropped the voltage into the next stage and so forth, and Sperry Semiconductor was making those. Actually, that was our first exposure to tunneling, because at Sperry, when we were testing them, we were getting these strange blips on the V-I characteristic which was the same thing that others had observed.

Laws: So these were silicon diffused transistors at this point, or were these still grown-junction?

Norman: No, our silicon stuff was the TI, grown-junction transistors. Toward the end of the time I was at Sperry, we got a handful of the Philco silicon-- I forget what they call it. But they were the silicon equivalent of the surface barrier-- oh, they called them surface alloy transistors. I think we got 25, or some number, and standards blew the whole thing, and those were the only ones in existence. Philco sent us 25 more. You had asked a question and I wandered off.

Laws: About storage. Using semiconductors as storage devices.

Norman: So I was very interested in that, but my boss-- Pete Isaacs at the time-- pointed out, he said, "You're proposing to use a quality that the semiconductor companies are trying to get rid of." And that made a lot of sense. So I dropped that for a while. At GM-e, later, we actually fabbed some registers using that, and they worked over a narrow frequency range. This was just before the MOS stuff.

Laws: So this was a dynamic sort of memory then.

Norman: Yes. See, remember that in the signal processing stuff, in the antisubmarine stuff, we were using delay lines. In fact, the incoming audio from the hydrophones was compressed in these 50 microsecond-long crystal delay lines, by sampling it and circulating it so that we'd have a 50 microsecond word that represented so many seconds worth of audio. That's what we correlated. The fundamental thing underlying that whole system was an MIT paper that showed that the statistical properties of a hard-limited signal were the same as of the signal. So were able to do all that using hard limited signals, which were a lot easier to deal with than the original audio. But anyhow, one of the problems in that system, of course, is that with temperature, the delay would change. So I was always looking for ways and always part of my underlying interest, as I moved from technology to technology, was how to clock a delay line so that we would no longer have to worry about temperature changing the delay.

Laws: While you were at Sperry, you filed quite a number of patents, I believe. Is there one of those that became useful later on?

Norman: One became general use. In the early days, we had ripple-through counters, and you had serial fast carry counters, and you had parallel fast carry counters. The parallel fast carry counter had the advantage that all stages switched at the same time, because they used the same clock. But the problem with that was that by the time you got to four stages, the gate pileups were huge. So I came up with this scheme where I developed in the first stage a carry gate, which I then propagated serially through the counter, but the clock went to all stages. So up to the time that that serial thing ran out of the clock, I could just have as many stages as I wanted, and they would all change stage at the same time. So that was one of them. The most important one I guess was the Resistor Coupled Transistor Logic or Transistor-Resistor Logic. At the time, the names escape me, there was an outfit in Newport Beach that had developed a transistor magnetic logic, where they used shaped cores and it was two cores connected. So one could saturate the the common path, without interfering with the other -- which was a great idea. It was the summed inputs at a transistor, which then saturated that core, and then you interrogated the core with the input to the next stage. That was kind of interesting. And of course my job was to evaluate all these things. And I'm looking at that thing one day and I said, "You know, why don't we take the core out of there?" Which I did, and then that became Transistor-Resistor Logic. Remember of course the most expensive single component in the computer was the transistor, so were able, in effect, to cut the number of transistors in half. Again, we weren't going very fast, so it didn't make any difference.

Laws: Do you remember what you were paying for transistors in those days?

Norman: I think about the time I left Sperry; we were paying about twenty dollars apiece. And resistors were a few cents. But actually, I had done work a couple years before that said it didn't pay to use the composition resistors. We were using the film resistors. The problem was that the composition resistors had such a wide diversity of thermal coefficients, and of course everything we did had to work over a temperature range.

Laws: A full military temperature range.

Norman: Right. So we always used the film resistors. Incidentally, one of the things that I have found that I have is the film from one of the boards in that antisubmarine warfare computer, which I plan to give you.

Laws: The photographic film that generated the pattern on the pc board.

Norman: It's the film that they used to make the board.

Laws: Great. So at Sperry, you must have talked to a lot of different vendors of semiconductors, and during this time you came across some Fairchild product, I understand.

Norman: Yes. Actually, Raytheon advertised a microwave transistor which was nonexistent, which was common. There were very few real transistor manufacturers in those days, and in small signal stuff the number was even smaller. The 2N43A was a real transistor that you could buy, and actually that's the transistor that Bell Labs used in the TRADIC But they were trying to do DCTL with that, and that was not a good transistor [for that]. In working with the Philco surface barrier, that transistor had an appreciable input resistance that limited the current going into the base of the transistor in a DCTL configuration. That was all important on two counts. It prevented the problem of current hogging, which was in certain configurations, if a gate over here was on, it would pull the base voltage down so low, you couldn't turn another gate over here on. You couldn't hold it on. That resistor was the big secret.

Laws: Was that a deliberate design of the transistor, or what that an inherent part of the manufacturing?

Norman: Because of the structure, it was a base tab that was etched in on both sides and replated. In fact, Bob Noyce worked on that transistor. In fact, they really submarined me later, because we were building the Weapons Direction Equipment for the Navy on a contract, and Philco substituted a better transistor.

Laws: You were using an unspecified parameter there I guess, Bob.

Norman: Right. I'm happy, Philco's happy, and then all of the sudden they're going to make life better, and they got rid of that resistor. This was a micro-alloy transistor, which had a very low base resistance, and suddenly the shift registers weren't working. And we're paying five thousand dollars a day to the Navy because we were late. So I had to go in and redesign that shift register.

Laws: Put a resistor in front of every base?

Norman: In effect. Well, I don't remember just what it was, but something like that. It was DCTL we used, because transistors were expensive, we didn't use all the transistors that Philco did. They had the master/slave stuff and all that. Because this was low-voltage, low-impedance stuff, I said, "Well, instead of using RC networks, I would use RL networks," and Sperry made all these cores wrapped around a resistor. That's what we used to get delay, to prevent race conditions and things. So I had to redesign that, but fortunately the only change was that component, and we were able to fix it. But I was mad as hell.

Laws: We were going to talk a bit about your introduction to Fairchild and their devices.

Norman: Yes. Don Farina was working for me at the time, and one of the jobs we had was for cable drivers for the airborne countermeasure system. I think I said before we had a problem with the high-current transistors for the magnetic drum and core memories. We were using General Instrument germanium transistors, which was touch and go because it was inside the case of the drum memory. They got hot in there. Here comes Fairchild with these silicon transistors, and we thought we'd died and gone to heaven. And they delivered. It was a most remarkable outcome. We started using them anytime we had severe requirements. One of them was this cable driver for the airborne countermeasures system, and Don worked on that using their transistors, and it worked. That was another wonderful outcome.

Laws: These would have been mesa transistors?

Norman: Yes.

Laws: And they were 2N696, 697?

Norman: Right, right, right. Yeah, I guess I'm not giving enough detail. Anyhow, thank you for asking. Underlying all this, the Fairchild salesman at the time was Howard Bobb. Now, Howard Bobb had been selling to us-- as I say, we started out using vacuum tube diodes in the logic, and then came Hughes with the gold-bonded diode. We tried some other diodes. Mostly diodes in that era were the crystals that you used for RF detection in radar systems. Here come Hughes with the gold-bonded diodes, and those were incredible, and they were tiny. And Howard was the sales rep. They delivered! I mean, this is getting good. Then Howard shows up, he'd gone on to work for Fairchild. Howard always carried around this little microscope on a tripod, and he'd always bring it in and spread the legs. He put one of these transistors under it for us to look at. We were properly awed. But the other thing, Fairchild was also making a PNPN transistor. And at the time I had been given the job-- I kind of got these odd jobs as we went along-- of lowering the power dissipation on the PPS-4 Marine Corps [Pulse Doppler] Man-Pack radars. It's like a drum thing, about that big [15"] diameter, about that [12"] deep, but it took a whole bunch of storage batteries to run it. So I was given the job of reducing the power. Well, we went to four-layer diode type pulse modulators with the transmitter, and that worked like a hose. You just had to know to make sure the voltage stayed-- you could use a series string. I'm talking about old hat stuff, but it's just one of things that you had to be careful of—when using a series string to make sure the voltage was balanced among these devices. But the other thing where we did break a whole new ground is that we used one of those PNPN transistors to switch the cathode on the klystron, and that saved a bunch of power, and it worked. The thing that we didn't know when we switched was whether we would get incidental FM, which we didn't. So that was a nice solution. That was another Fairchild device, and lo and behold, unlike most of the rest of the industry, when you ordered something, it showed up on the loading dock.

Laws: So you had a positive impression of Fairchild then from your introduction.

Norman: Absolutely. And of course, the reliability of the stuff they sent us. Everything worked, which was also novel at the time. Meanwhile, Howard grabs Don Farina and sends him to Fairchild, which upset me somewhat. But Howard and I just got along. We were both old Navy types. But then John Ready calls me. John Ready had been a Philco salesman, as had Howard at one time in his career.

John Ready had been a recent Philco salesman and was now at Fairchild, and he called me about coming out for an interview.

Laws: So this would have been what year, Bob?

Norman: This was in '59. I would say about the March timeframe. I hate to keep doing this, but going back, I don't know, some period, I had gone through the cost-- we were building these things-- and I told the company what these computers were costing-- well, as I said, we worried a lot about the cost of the transistors. I pointed out to the company that these computers were costing us three hundred dollars per transistor, not including the cost of the transistors, and a lot of that cost was in the printed circuit boards. We were using two-sided printed circuit boards, sometimes eyeleted and sometimes plated through holes. I think the problem of plated through holes is sometimes those pulled out, and I don't remember what we would end up doing. But the point is that the area taken, in effect per gate, on the printed circuit board was an important cost factor, in fact the predominant cost factor in the computers we were building. There were efforts by the Navy and the Army to do something about that size thing-- it wasn't the size of the computers so much as the cost of building that size.

Laws: Cost per square inch of the PC board?

Norman: Right. So of course I was pretty heavily involved in that stuff and followed it closely, but I didn't see anything that made any sense to me. We were just substituting an unknown process for a known process. I knew what these resistors were like and the boards were like and all this stuff, and people are working with ceramic substrates and all this stuff, but nobody seemed to be doing the homework on how all that stuff would work from a reliability standpoint in the system. So I just kind of watch it and more or less stay abreast, but nothing else. Then the famous call to go to Fairchild. Now, I was happy at Sperry. I was never bored. Anything that was out of the ordinary, they would throw at me, like that radar. That was the only radar I worked on at Sperry. I loved it. People all over the company were building digital computers and I would get these screwball jobs and do them, and I was going up. I made the chief engineer's annual report three times. Once for having a bunch of technicians borrow engineers' badges so they could come in on a weekend on work on that ASW computer. When you're supposed to put a computer on a ship, the ship sails with or without the computer, so these guys came in and, needless to say, a whole bunch of us would have been fired. So the engineers weren't there. Technicians did the work, wearing engineers' badges. Monday, the lab rep from the union...

Laws: So the union would not permit the technicians to work the weekend.

Norman: No. Not when they weren't on overtime. They regulate all that stuff very closely. So the union boss for the lab-- the guys that hadn't been involved in doing this would come in and they would find the thing they were working on Friday was finished. This is going on all over the lab. So this guy goes around and says, "Look at that," and they have a complaint. So they call me over, and I said, "I'm sorry, I did that." Then they'd take me to the next thing. I said, "I did that." So all these jobs that had been finished up I said I did. Then only one person's getting in trouble. So I made the chief engineer's report.

Laws: So you were notorious in Sperry by the sound of it, Bob.

Norman: Yes.

Laws: So in 1959, you got this call from John Ready to go back and talk to Fairchild. You were very comfortable with the job at Sperry, but you still went and talked to them anyway.

Norman: Yes, because they were so different from anybody I'd dealt with before in doing the things they said they would do, and doing them well.

Laws: So you were impressed with the opportunity.

Norman: Yes.

Laws: What was the job you went to interview for?

Norman: It was not an opportunity in my mind when I went, I was happy to visit.

Laws: Who did you meet with when you came out to interview?

Norman: Vic and Bob Noyce.

Laws: Vic Grinich and Bob Noyce.

Norman: Yes. It was Bob Noyce that told me about the planar process and the ability to put interconnects on it. We talked about what kind of logic I would use on something like that, and I said DCTL. Because in my mind, for a company like mine, DCTL is hard because you got to buy all these transistors. But for a transistor company, DCTL is easy. I talked about some of the important qualities needed in the DCTL transistors to make it work, which kind of fascinates me because the stuff that he did, did not reflect that. The last thing he wanted to do in the DCTL transistor was that circular base, because there you're trying for speed. That's the IBM motive. We're trying to add some series resistance in there, not drop the series resistance. But of course I didn't know about that stuff [What Bob was doing at that time.]. But I was fascinated by the planar process and the kinds of things we talked about.

Laws: So this was about March '59?

Norman: Yes.

Laws: This is while they were writing the patent disclosures?

Norman: I suppose, yeah [I didn't know about the patent disclosures]. I don't know exactly, but I do know that Ed Baldwin was still General Manager, and we went to Whisman Road. It was being built. Just when that would be, I don't know.

Laws: About March. Baldwin I believe left the week before Hoerni demonstrated the planar transistor, and they all chuckled that he didn't get the opportunity to take those secrets with him. So then they offered you the job in Vic Grinich's department?

Norman: Yes. Vic offered me either Device Evaluation or Applications Engineering, and I said, "Which one would I work on integrated circuits?" He said, "Either." He said, "Whichever one you choose." And I said, "Good, because that's what I want to work on." Then I said, "Which would you prefer that I take?" and he said, "I prefer you take Device Evaluation." So I did. That is kind of fascinating. When you look back on it, I guess we were kind of sold on each other, on our ability to do what was needed. It was funny, when I was over in Mountain View, Bob called over Jay Farley, and he said to me, "Jay has achieved a 10,000-hour MTBF on the transistors." I said, "That's wonderful. That means a computer with 10,000 transistors has an MTBF of one hour." <laughs> And Jay was standing right there. That was not a nice thing to say.

Laws: So you moved out to California fairly soon after the interview?

Norman: It took a while. I joined Fairchild in August. I had a lot of stuff going on at Sperry that I had to complete, and I wasn't going to leave that stuff hanging.

Laws: You were married at this time, Bob?

Norman: Yes.

Laws: Have any kids?

Norman: Yes, two. Very young, and my wife was pregnant with the third.

Laws: So this was a pretty traumatic move for her, I imagine, at that time.

Norman: Yes. But she was very happy. When I first said we were going to Palo Alto, she was happy as a clam. I don't know why, but she was. So that was good. This was a wonderful environment here at that time. There really were not a lot of people. There were very few. Gordon [Moore] was one of the only native Californians I knew. Now there are all kinds of them. So anyhow, we came out. I lived in a motel for a couple of weeks on El Camino, and we found a house on Heatherstone Way in Sunnyvale for a month, and then we had to move from there. Bob found us a house on Lundy Lane, about a quarter of a block from his.

Laws: In Los Altos.

Norman: Right. So we moved there. It had this big olive tree in the backyard, which created all the diversion I needed, because every time I looked, there were olives on the patio for me to clean up. Meanwhile, when we started out, we were in the 844 Charleston [Road building].

Laws: Palo Alto.

Norman: Yeah, right. Then very quickly, they rented the one behind it, which was, what, Fabian?

Laws: Fabian Way?

Norman: Fabian Way, yes. And that became our building. Well, "our building"-- we weren't the only ones in there. Isy Haas joined me initially. Don Farina of course joined me. Isy went back over to Jay's section or group, shortly afterward, and Helmut Wolf was part of it. I think the first person hired into that was Orville Baker, and Dick Anderson and Howard Bogert, Dick Crippen. The lab was run by a former Navy chief, I think, -- Al Wesolowski. We had all these girls, testers. Don ran the girls. I hardly knew them. Don knew every one of them intimately. He was a great lover. But Al, one of the things that we did early on, we started-- it was the kind of thing that I had done at Sperry. If you walked into a lab at Sperry, everything's whirring. It's all these things running until something quits. Then they find out why and fix it, and so forth. When we started getting devices, I started testing them-- margin testing them, life testing them and temperature testing and all that.

Laws: These would be the first planar devices, I presume, at this point?

Norman: Yes. The first device we got were the chemically separated ones.

Laws: Those were integrated circuits?

Norman: Yes.

Laws: That was later though.

Norman: That was later. Yeah, we started testing the transistors first, and these devices were incredible. I had never experienced anything like that. We had to go buy a Rhode & Schwartz meter to measure the collector current, because Farina measured the characteristics over several decades. Any other devices we'd ever dealt with before, there'd be some finite range that they'd act as they're supposed to, and quickly the surface effects would change everything. So, as I've said before, you learn to use these transistors as statistical devices rather than definitive. All the early design I'd done at Sperry was all on a statistical design. In fact, I used to have three girls cranking those rotary calculators, taking the data and reducing it to the statistical properties. And here we had a transistor that actually fit the

Ebers-Moll equations. Which was fascinating because if you read the Ebers-Moll paper, they say that those equations only apply to alloy junction transistors because they work with some reasonable relationship between the forward and inverse alpha. It turned out that they fit the planar transistors all the way, and the only difference was that the inverse alpha, instead of being similar to the forward alpha, was down around 0.4, or some number like that. But otherwise they were great, and that made it possible to actually design the circuit, rather than just look at the transistor as a statistical beast.

Laws: So Don generated this data that then allowed you to design the DCTL circuits for the first integrated circuit.

Norman: Right.

Laws: When would you have started that design work? Late in '59 sometime?

Norman: I would guess, yeah. As I said, we had to go out and get that meter, because we didn't have one, well we didn't need it in the company before that.

Laws: What other kinds of tools did you have available? Were there decent scopes available? No computers, I presume.

Norman: No. A few years later at Fairchild, I looked to hire a programmer. As I said, I had programmed the computer at Sperry. I figured we should have a programmer to help us do this stuff, and some came in, and they didn't-- I'm talking about machine language programming, and they didn't know from machine language programming, and they weren't interested in learning. So that kind of went away.

Laws: Let's carry on with the story of the development of Micrologic at Fairchild. So you took Don Farina's data and developed the DCTL design.

Norman: One thing that bothers me is that all of that stuff, we put in lab notebooks. We were very good about those. It's a shame-- I have seen some pieces of stuff, the mention of the static RAM. I've seen the copies of that from my lab notebook, but the design and how we did it and all that is all in there and it's a shame that that those notebooks weren't saved. I don't know.

Laws: It's quite possible they're all in those archives at National where they have a huge amount of Fairchild material that they're very protective of.

Norman: I know. I hate to tell you, but the axe I grind at the drop of a hat is I that am so upset at the way they (the IEEE) changed what they published, in the Proceedings, because so much of what I accomplished and so much what I think of what the industry has accomplished over the years was stimulated by some guy's paper in the Proceedings. Here comes a paper in the fall of '59 from IBM on test to failure. Vic Grinich looks at that and he says, "I want you to do that on Micrologic." I said, "Fine." Obviously I was already a believer. So we proceeded, and I tested it. We went from liquid CO_2

temperatures up to 125 [degrees centigrade]. I think we went to 150. We had the welded case on the TO5. I had gone over 125 with the TI transistors when I was at Sperry, and I opened the oven door and all I had were a bunch of bare transistors because the cases were soldered on and they just popped off. But at any rate, we did the mechanical-- at the time Fairchild was shooting transistors from a pneumatic gun for a mechanical shock-- test. We did the centrifuge test in all the axes, and we ran them up to something like 150,000 G. If I remember correctly, we got something that was very unusual in the experiments. We got a very tight distribution of failure, some number like 135,000 G or something. We had random failures at lower accelerations. We had chips come off the header and we had leads come off. Every single device that failed, we would open the can and report on just what had failed. So we were able to safely-- and again, I may be off on the numbers-- but we safely put in then a centrifuge test and I think it was 125,000 G, which we then used for a long time. We did thermal shock; we did mechanical shock. Most of the stuff didn't seem to bother anything, except the plastic-- the chemically isolated [Micrologic] devices-- because those failed right away. We made the initial screen the thermal shock. We'd heat them up to 125. We'd put them in this icebox we had. We had a big icebox, looked almost like a coffin, and that was at minus 55, I think. [That icebox became responsible for a near-miss accident, which we used to start a formal Safety Engineering procedure. Al Weswolowski became Fairchild's first Safety Engineer.] We wouldn't put anything on life test that hadn't passed that. Well, I dropped all the chemically isolated devices out, because there was no point to me to put something on that I knew was going to fail. I'm trying to find out what the reliability of these things are, not the unreliability.

Laws: So the reliability data you were publishing was on the junction isolated devices?

Norman: Right. Nothing else got onto life test. After the very beginning when they just failed, boom, right off.

Laws: So you were writing a number of papers and presenting a number of papers, Bob. You said you presented your first in, I think you said, February of Nineteen...

Norman: Sixty.

Laws: You made a comment to me earlier about meeting Jim Early after this, and he was very impressed with the work.

Norman: Yes. I'm a young engineer, and here's a-- well, that whole session, that was the first-- I had spoken at the Solid-State Circuits Conference a couple times in the evening sessions. In fact, I introduced Transistor-Resistor Logic at an evening session a couple of years before. I asked my boss-- we were in the systems business, the computer business, and they were still talking about the core transistor logic-- and I asked him, I said, "Can I go up and talk about what we're doing?" He said, "Sure, go ahead." Because Sperry didn't look on itself as being a circuit design company, but rather a computer design company. Well anyhow, so that was my announcement of TRL, was at the Solid-State Circuits Conference. I'm sorry I go back and forth like this. So anyhow, but I had never given a paper. I'd given papers when we met with customers or on systems stuff and things like that, but my first real formal paper, was this one on Micrologic at the Solid-State Circuits Conference. And Doug Engelbart gave the first paper in that session, which was an incredible thing on scaling.

The platform was not very high, and I stepped off the platform [after I gave my paper] and just a few feet beyond that was where all the speakers are sitting, right there in the front row. But I'll never forget Jim's comment. I was floating on air.

Laws: And what was it that Jim said to you, Bob?

Norman: He said, "Now that's a real engineering paper."

Laws: That's great. Jim was an icon of the [semiconductor] business in those days.

Norman: Yes. So I was happy as a clam. But anyhow, then as usual we talked to customers at that thing, then I had to get back to work. Again, this test to failure program was now ongoing. The problems producing the parts were really discouraging. I would go out to the production line and they'd show me a cup of shards. The silicon had broken up into these thin strips, and that's what they were dealing with. The pilot line under Murray Siegel was somehow able to make parts-- I saw them-- but manufacturing couldn't.

Laws: And the pilot line was at Charleston Road?

Norman: Yes.

Laws: And the production line was at Whisman Road.

Norman: That's right.

Laws: Was Phil Ferguson involved with the pilot line?

Norman: That was later. My wife knew Phil Ferguson's wife. So Fairchild went on a recruiting tour of TI in Dallas and took me along, and Gordon Moore, I don't remember who else. I think Harry Sello maybe. Anyhow, I'll never forget. They put us in this hotel. This huge round bed was in Gordon Moore's room. I had never seen anything like it. So my job was to basically talk to Phil Ferguson, and he came to work, and they put him, I think, they were reorganizing somewhat, and they put him in charge of the pilot line, in R&D. He was doing pretty well. But Motorola had come out fairly early on with this epitaxial process, and I used to talk to Jean-- Jean was still there.

Laws: This is Jean Hoerni?

Norman: Jean Hoerni, yes-- about using epitaxial isolation. Now Jean had tried using epitaxial material. Of course, we were device evaluation, so we would always see these things, and his epitaxy looked like orange peel. Of course his focus, remember, was not integrated circuits. He was trying to come up with a good PNP power transistor. In a sense, that was his job. Of course, Tom Sah, worked for him, and we evaluated all the stuff that Tom came up with trying to work that problem. But anyhow, I suggested, “If we use an epitaxial isolation, then there’ll be a lot less furnace time, and stuff we’re doing is all low-voltage so I don’t think the orange peel is going to be a big deal.” But he would have none of it, and it was later that Phil, on the pilot line, took the masks and set them up so it would work with epitaxial isolation. And that worked like a hose. It was when the pilot line began out-producing manufacturing that manufacturing finally got religion.

Laws: I guess Charlie Sporck was involved at this point?

Norman: I’m not sure how much he was involved in this particular [process]. Charlie had a lot on his hands because he, in effect, made Fairchild into a manufacturing company. What he accomplished is incredible.

Laws: So he probably had to be sold on the value of epitaxy before he’d allow it in his building, knowing Charlie.

Norman: Yeah, yeah.

Laws: So it epitaxy came in when? Do you remember what year that started to be produced?

Norman: The year that Motorola started doing it was in maybe ’60. It was a while. But one of the things, when we left to start GM-e -- I wasn’t about to start using epi at GM-e without it running at Fairchild. To me, it wouldn’t be fair for Phil to leave Fairchild without epi being in use there. So Phil got the epi going and the yields going, and that was doing pretty well when we spun off.

Laws: What was the incentive for founding GM-e, Bob? Was it something that Fairchild wasn’t doing that you could see the opportunity to accomplish?

Norman: In the first place, we were completely unaware. We’d gone back and we’d got MIT Instrumentation Lab started using Micrologic for the Apollo computer, but we didn’t know it was actually happening. An AC Spark Plug division in El Segundo was buying Micrologic, but we didn’t know it was for Apollo. Then here was a company in Denver, Martin Denver.

Laws: Martin, They built a computer called Martac.

Norman: See, we didn’t know that. We would go back for the IRE conventions in, I think it was December. Howard and I were talking. It was taking a while for people to start using this stuff. One of the poorest decisions I made at that time-- before the production started on the Micrologic, Vic talked to me about, “Well, if we can’t get Micrologic going, how about using what we’ve done to make a DCTL transistor.” And I was against it. I said I didn’t want to dilute what we’re doing, and that was a very bad choice. We should have done that.

Laws: That would be a transistor with some resistance in the base, essentially?

Norman: Right. We had the perfect device. There were a lot of sockets out there for silicon DCTL transistors, but I was so worried about diluting what we're doing that I said I didn't want to do it, and I shouldn't have done that. [One thing Howard Bogert did do was cut the connections on a Micrologic Gate to see hjow well matched the transistors were. The matching was exceptional over temperature, so he could design analog stuff that capitalized on the matching rather than absolute values.] Anyhow, so Howard Bobb and I started talking about combining both systems and device expertise under the same roof. That kind of translated at Fairchild. Bob Noyce worked on setting up this Fairchild Space and Defense Systems on Porter Drive, and Howard and I went over there. Our job essentially was to use Fairchild parts in military systems. We got a contract very quickly to build a single-axis triple redundant adaptive flight control for the Air Force. Gene Franklin at Stanford Control Labs had done a lot of work on adaptive systems. At very high speeds, and this was a problem on the X5, The control parameters would change, and planes would get in trouble because as the speed changed, as it went through supersonic speed, the flight parameters would change, and Standard Control Systems couldn't handle that. So we got a contract, just an experimental thing. [It worked.]

Before that, while at Fairchild, John Orleman of NSA came to see me-- another customer-- and he's asking me about Micrologic. And we're talking, and he's got a pad that's about that (1") wide and about that (1-1/4") long, and he's sitting there as I'm talking to him, and I'm writing on the board and stuff, and he's making little marks in this pad. I don't know what to think about that. A few weeks later-- we're still at Fabian Way-- Gordon calls me up to the office. He says, "We have a contract from DOD to develop a low-power Micrologic." I learned later John had been a stenographer in the Signal Corps before he went to work for NSA. Everything was DOD then. NSA-- you couldn't say that you knew what that was. But anyhow, so we got a big contract from them to develop a family of low-power circuits optimized toward the cryptography stuff that they were involved in. That's when we moved over to Mountain View and they put up a secure room with ultrasound detection and all that. [We had a big discussion in Bob Noyce' office about making a full Master/Slave Register on a chip, 2-"S" Elements. Then Dick Anderson came up with the "R" Element which only used 7 transistors.]

Laws: Are those the ones they call the R13 circuits?

Norman: That's the R13 circuits. And of course we did all the shake, rattle and roll on that and wrote up all the reliability specs. That's when we actually got all that stuff written up, which then became the standard for the shake, rattle and roll, and life tests and all that. It really bugged me that people would later try to get out of doing it by embellishing the standards. We had already established that if we would run this stuff for 24 hours at 125 C that was sufficient, and people just kept adding on to that. And of course the more you added on to it, the more inventory there was tied up in these life tests and the more pressure to not do them. That's another book.

Laws: So you were working at this Fairchild Space Defense Systems. Is this where you first had the idea of semiconductor static RAM?

Norman: What happened, IBM came to Don Farina, and they were looking for a static RAM. Well, I guess they came to Fairchild and Fairchild brought Don Farina in. Don called me. We lived near each other at the time. He called me, and the next day I went over to his house. I know it was Tuesday because we used to kid about invention on Tuesday. Anyhow, so I was at his house, and I sat in his breakfast room, used the table. In a matter of hours I designed the static RAM. The next morning I go into Space and Defense Systems, which Bob [Noyce] had set up, and there is Roger Borovoy sitting in my office. Now, Roger Borovoy knows that engineers hate with a passion to write up patent disclosures. So he said, “I’m not leaving here until you write this up.” So I sat down and wrote it up. Thank God. What happened was Don had told IBM that we had a solution and had showed them the solution. Not that day. It was after. It had to be, because what happened is that the guy from IBM went back to Los Gatos and wrote it up as his own invention. I never knew that, except IBM was gung ho about this thing called a Harper cell. The guy’s name was Harper, and it was later, when IBM claimed an interference against the Fairchild patent, I got back into it, and here of course, everything went into my notebook, like I said, and they had the copies of my notebook entries. IBM wanted Fairchild to give up the patent, and they wanted me to sign off. Because I was the inventor, they wanted me to sign that. I said, “I’m not going to sign it. It’s ours.”

Laws: So you were awarded a patent for this RAM cell?

Norman: Yes.

Laws: I’d like to know the number of that sometime. [Editor: It was US 3562721 Solid State Switching and Memory Apparatus. Filed March 5, 1963, Issued February 9, 1971]

Norman: I may even have it. Any patents I had, I was awarded. I got an email from this guy in a transistor museum, has found several of my Sperry patents.

Laws: Jack Ward.

Norman: Yes.

Laws: So you talked to Howard Bobb about founding a company?

Norman: As I said, the idea was to have a quality semiconductor company and a quality systems company under the same roof, as a way to make the best use of emerging technology, and that that would be a good business. We went around looking for backers. We talked to a lot of people. And then what happened is this company—Pyle-National Company that had been building electric generators for steam locomotives-- decided to move into the present by going into the military connector business, and decided to make a further move. They went to Art Lowell, who was at that time the head of the Navy’s Bureau of Weapons. Art Lowell was the guy that, during the Korean War, he was a Marine colonel, and he had the pilots in his wing take faulty electronic equipment off his planes and put it on the ramp with the statement, “If I ain’t going to work, I’m not going to carry it.” They said, “Well, what about gunfire control?” He said, “We’ll use Kentucky windage,” and things like that. Anyhow, Art was running the Bureau of Weapons.

Norman: Yes.

Laws: To help and raise the money from Pyle?

Norman: Right.

Laws: And Phil Ferguson joined you?

Norman: Yes.

Laws: Right at the beginning?

Norman: Yes. And our first employee was Jim McMullen who was a whiz at epitaxy. And our second employee was Don Farina, who we almost killed because we arranged it—- Don worked for Phil by then. And I'm there at Don's house talking to him with the papers for the GM-e laid out on the table. And in walks Phil Ferguson. And Don just about had a heart attack—- we almost lost him before we started.

Laws: Because he was working for Phil at Fairchild?

Norman: He was working for Phil <laugh>. [At GM-e] Don designed all the Milliwatt Logic parts that we had developed for R-13 with one little problem, he made them better, made them just a little faster so they wouldn't qualify. The agency didn't want to mix the parts. So it was a great favor he did us. And anyhow, at the same time the Navy was trying to get suppliers for the TFX TTL (Transistor-Transistor-Logic) parts. And they wanted production suppliers, so we were spending money like it's going out of style demonstrating to Litton I think it was, that we had production capability in semiconductor parts. We had the people, we had the equipment, but no contract, there wasn't anything going through. Meanwhile, on the systems side, we got a contract from Nortronics division of Northrop on the IHAS, Integrated Helicopter Avionics System, which we proposed doing that job with DDA computers, which were distributed around the choppers, so if it got hit the computers were arranged so that if something failed, another could take it over. Anyhow, we did all that. And these were 20-bit DDA registers. And we didn't get that one, but we did get a contract from Wright Field [Air Force Base] Bernie Widrow at Stanford Control Labs] had [published] papers on the Adeline and Madeline self-programming computers. And we got the contract from Wright Field to do a digitalized [version]. His thing used resistors, and then it was a reward, punishment scheme for changing the values of the resistors in this network to teach them to do something. I kind of mapped his scheme into a time domain scheme, the probability of a pulse coming out of node was a function of the weights. And that turned out to be pretty powerful many years later.. But anyhow, and I got a patent on that and it was called a Statistical Decision Switch.

Laws: Did you implement this?

Norman: We implemented that with the Milliwatt Logic, and shipped it to Wright Field. And they were very pleased with that.

Laws: So when did you start to work on the first MOS products?

Norman: Well, while I was still at Fairchild, when I was in Space and Defense Systems, I was over at R&D I used to go over there from time to time, and I was having lunch with Gordon. In Device Evaluation, we were evaluating these things that Tom Sah was making. And we found out about the drift and things like that, first with the Surface Control Transistor and then he went to a Surface Control Diode. So we were part of that. And later on, the MOS Field Effect Device was a great switch. Like any Field Effect it had no offset. Gordon asked me, he said—- he wanted to know what I thought of that device. And I said "Well, the problem it has is that it is a high-impedance device, which means that you gain all this being able to use a very small switch, it's a very good switch. But the resistors will take a lot of room." And that's where I left it because that's where it was at that time. But that problem bugged me. I kept trying to think of some way to do that, to take advantage of that device, it was so much easier to handle than a bipolar device. And actually I did a 20-bit stored charge thing, which just worked over a narrow [frequency] range. But just about that time, I remembered that the idea of using vacuum tubes as current sources, which were used on the RCA bombing equipment that we had so much trouble with. Then it occurred to me that I'd used transistors as current sources in regulators, so the idea of using an MOS device as a current source occurred to me. And then I said to myself "Well, not only that, I can clock it." And so I went to Jim Imai and had him figure out what transconductance devices we should use. And then I had Gene [H. E.] Stephenson build this thing and start testing it. I was in the Systems Division at the time. And he said "There's a problem", he said "the edge goes through, but the other edge doesn't go through." So we looked at it, and he said "You need another transistor." So it went from a four transistor per bit to a six transistor per bit system. So that's why Gene is on the patent. So anyhow, that thing worked like a hose, and we of course were making MOS devices, actually some of our devices flew on a Pioneer F, as switches for choppers. And so, you know, my interest in registers, so we laid out this 20-bit MOS register. And Frank Wanlass made the first run, and I was then visiting Bruce Erickson at NSA R&D. And Frank called me there to say they had working registers on the first run.

Laws: And this was June, 1964?

Norman: Here is the picture of the first [device], it was June 9th I think. And the rest they say is history. But anyhow, our first contract on that was from Picatinny (Arsenal) to do a timer fuse, and this was kind of an ongoing thing with them. They would take emerging technologies and would implement the artillery shell timer fuse in a new technology and evaluate it.

Laws: This was Pchannel MOS?

Norman: P-channel MOS, yeah. Remember that RCA had done work sometime before with Complimentary MOS, except nobody could make the N-channel. So the whole idea that the current source obviated was the need for the N-channel device. We had, in effect, a load resistor. And so that's why the P-channel was so important at that time.

Laws: When did you start getting involved with the Victor Calculator Project?

Norman: Well, Howard Bogert built a calculator using the Milliwatt Logic, an eight digit calculator, you know, one of these things about this wide, [6”] and this long [9”] with blue, decimal indicator tubes. And two things happened. Howard Bogert was then Chief Engineer at General Microelectronics. And it worked like a hose. There was a little squib about it in Electronic News in about March of '64. And it raised quite a bit of interest. And then here comes Victor, and Victor was a fascinating company. Of course they were number one in the US and number two in Europe in these slope-front calculators that they sold and serviced. And what they did that fascinated me, [was that] they had built a mock-up of an electronic calculator with vacuum tubes that was a room full of stuff. And what they had done with that is to say, “Someday we will build an electronic calculator with new technology and we want to know what the logic will be of that.”

END OF INTERVIEW – TO BE CONTINUED AT A FUTURE DATE.

