

An Interview with
WALTER LEONARD ANDERSON

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Conducted by Arthur L. Norberg

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Abstract

Anderson, who had been trained by the Navy at MIT and at the University of Minnesota, began working for ERA in 1946. He was hired by Joseph Walsh and worked with Howard Daniels, Frank Mullaney, and George Hardenbergh on engineering design problems. Anderson talks about a number of early design projects at the Naval Ordnance Laboratory, including design of a relay control circuit and a time delay scramble. He discusses the influence that Arnold Cohen had on him. In 1949 Anderson moved to the Arlington office where he worked until 1955. He worked with the sales office as a technical liaison under Herbert H. Goodman. He worked on development of UNIVAC and the 1100 series, and he discusses some of the applications that he was involved in assessing and the technological constraints of the time. Anderson was also a liaison between the Minnesota group and the Pennsylvania group, and he discusses projects in which he worked with John Coombs, William Norris, William Overn, William Butler, Robert Torrey, Ted Bonn, and Herb Rickards. He discusses the change in operations when ERA was changed from a subsidiary to a division of Remington Rand. Anderson also discusses the establishment of General Kinetics, Inc. with Robert Gutterman, Al Roberts, Frank McKutcheon, and William Goggins. The interview concludes with remarks on the nature of their early consulting work in mathematics, engineering, and the film industry.

WALTER LEONARD ANDERSON INTERVIEW

DATE: September 11, 1986

INTERVIEWER: Arthur L. Norberg

LOCATION: Charles Babbage Institute (Minneapolis, MN)

NORBERG: Wally, would you give me a little bit of background on your early life, where you were born, what your parents did, and where you went to school?

ANDERSON: Well, I guess we start at the beginning then. I was born in St. Paul, Minnesota. My father was a railroad man. He ran the Great Northern dining car commissary until he retired at 65. I went to the public schools, Johnson High School to be specific, and the University of Minnesota. I got my bachelor's degree in EE. I always wanted to be an engineer.

NORBERG: Why?

ANDERSON: I built radios. I liked to see things work, make things work; and electrical things seemed interesting to me. It was a toss-up during freshman year whether I would go into chemistry; but I soon decided it was electrical engineering. I think it was because of radio experience. I had to hustle through college because World War II was on. I worked a couple summers; but I think my junior - senior summer I went to school and graduated in March of '44. Got a direct commission in the Navy. They put me in airborne radar. They sent me to indoctrination school at Fort Schuyler and Bowdoin College for pre-radar. I took an entry test and because I didn't get the word in advance, blew the top off the test. I became one of three people to stay only one month. They needed us out in the fleet. I didn't realize that if you did too well on the test, you'd get a short course. Then I was trained for four months at MIT in radar.

NORBERG: What sort of training did you get?

ANDERSON: Well, this was electronics, more practical than the university electronics. You learned resistor color

codes and how things are wired, nomenclature and things. I knew all that anyway from building equipment; yet the entry test at Bowdoin consisted of complex algebra. they didn't give a test in practicalities; they gave you a test in complex algebra. Of course, I had had a good university course in that. But anyway, at MIT we got radar theory and a lot of work on actual equipment. And then into aviation training. I got a quick dose of aviation. Of course I had a commission so they weren't going to make a pilot out of me, but they did make a fine flying radar officer out of me. And I joined a torpedo plane squadron and went out to the Atlantic in time to help kill one submarine. My squadron got one of the last ones in the war.

NORBERG: So, can I get some of the chronology straight here? What year did you get your degree?

ANDERSON: March '44.

NORBERG: March '44, okay. And...

ANDERSON: I was operating in the Atlantic fleet in early '45.

NORBERG: By early '45.

ANDERSON: Yes. We came back to Norfolk after VE day and, incidentally, I don't want to get too personal, but this does kind of apply to the electronics background that was very beneficial when I got to ERA. Because I was well trained in military radar and things like that. All the shipboard radar Loran and things like that, as well as the airborne IFF circuits. So VE day came and although I hadn't been out very long, we were kind of heroes and had steak every night and could go to the beach all day. I was pretty bored because I had to report for orders everyday and had gotten pretty interested in the person who is now my wife. She was at Wellesley College and I couldn't visit her very often because of having to report for orders every day. So I got pretty bored and volunteered for the Pacific. I got out there just in time for the war to end. They didn't know what to do with us aviation types in the flight operations staff, so I was made V-2 division officer, which was the hanger deck division that included all shipboard electronics,

and later, acting air officer on an aircraft carrier called the Shamrock Bay, mainly used then as an equipment and troop carrier. But I didn't have enough points to get out so I had to stay until about August '46. The ship actually worked its way around through the Panama Canal and I got back to Boston in time to see my fiancée graduate and then proceeded back to my home town to look for a job. I was going to get a master's degree. I thought about Columbia, but ended up at the University of Minnesota. I answered an ad and pretty soon I'm talking to Joe Walsh and then Bill Norris and they hired me. I worked on my master's degree while working at ERA.

NORBERG: When did you first see the advertising? Do you remember?

ANDERSON: Probably August '46.

NORBERG: August of '46.

ANDERSON: I joined ERA in September '46.

NORBERG: What sort of an interview process was it, do you recall that?

ANDERSON: Yes, I recall the interview. I was interviewing Joe Walsh. I guess the academic credentials, bachelor's degree, and MIT training were most of it. The interview was somewhat on the personal motivations thing. It was a good business interview. I don't think I handled myself too well in demanding the pay I was expected to get. ERA was trying to operate on a budget and save. It was fair, but not excessive. I think that was the case with almost everybody who came in there. And we were issued, incidentally, Northwestern Aeronautical badges with the little wings on them temporarily until we got ERA credentials.

NORBERG: Now did the same sort of questions arise in discussions with Bill Norris?

ANDERSON: No. He was at an appropriate level. I think the idea was Joe Walsh would probably say yes or no in

advance and Bill Norris was going to agree or disagree, something like that. At some stage, Bill Norris offered to let me buy stock. I'm not sure it was the same interview; I don't think it was. I remember that interview more clearly. He gave me a good build-up on how risky stock is and his faith in the company and that I was an insider and going to be allowed to buy stock. And I was wondering how many thousands of dollars were involved. Well, it turned out to be a hundred shares at 10 cents a share, which later helped when we started our own company with Bob Gutterman, Bill Goggins and Al Roberts. That became \$1600 and that just came in handy.

NORBERG: Did you discuss with either or both of them, the sort of tasks that you would be asked to perform?

ANDERSON: No. They were not able to reveal the exact nature of the work, but they seemed to assure me it was right in the line of stuff that I was interested in doing.

NORBERG: Were they interested in your former training in airborne radar and other forms of electronics?

ANDERSON: I believe so, yes.

NORBERG: But you don't remember that distinctly.

ANDERSON: I felt pretty confident during the interview about that training. As a matter of fact, the Navy training was superb. When I hit the fleet, I knew what I was supposed to do. It took a couple of weeks to find your way around the ship and all that, but as to the job, I thought the training was just right on the mark. I knew what to do. And it's so seldom when you enter a new occupation with that feeling. In fact, as a side incident, the Sonobuoys were in trouble -- those little floats that would be dropped from a plane and had a microphone under water and would broadcast an FM signal so you could listen for submarines or to submarines. They weren't working when I got there and it turned out that the receivers in the planes had been tuned up as AM receivers instead of FM. Well, as soon as I realized that, I just had them all retuned, about thirty of them, and everything was fine. In any event, I felt quite confident in electronics.

NORBERG: If they didn't discuss during the interview what sort of work you would be doing, what were the first tasks that... I shouldn't keep using the word tasks because that's...

ANDERSON: Well, a task was a formal project.

NORBERG: That's right. What sort of project were you first asked to do?

ANDERSON: Okay, the first thing after you walked in, was minimum paper work -- sign up, indoctrination, and then march down the hall to meet Herb Nillis. Herb Nillis was running Alcatraz, Task #1. And I wasn't told exactly what I was supposed to do, but I was told to design the printer control circuit. Now this was not electronics; this was a relay control circuit, but it was logic and it was terribly complex. It had all these ifs, ands, and buts. So I quickly had to learn Boolean algebra, which you kind of know because you've done design before, but never by that name. It was a very intensive control circuit. Other engineers were doing counters and we soon realized that we were making frequency counts of characters and you soon know what the business is, but there was a big security atmosphere. And people, especially who had been in the military, were well aware of the secrecy.

NORBERG: Who did you work with very closely at that time? This would be late '46 now.

ANDERSON: Well, Herb Nillis was the boss. He was supervised by Larry Steinhart. I don't think I worked closely with anybody else in my same area. There was a man named Jacobson, and Paul Gibson, a mechanical engineer, Carl Hipp... A bunch of names I don't remember because I didn't stay on that very long. I wasn't there when it was delivered. It seemed to me it was kind of an endless project... Well, I can tell you a Bill Norris story here. They had a counter, I guess it was designed by Larry Steinhart and it was supposed to count characters. Lots of these counters made up a big display board. It was a kind of a contraption type mechanical counter. It worked from a relay clapper with a complicated mechanical linkage, and it was causing no end of trouble. So Bill Norris walked in one day with a counter from a Stock Quotation System in New York, which advanced the counter wheel half on closing the relay and

half on opening, or maybe two-thirds, one-third. He said, "Do it like that." People scrapped the other design and began to do it like that. Dick McGee picked up the mechanical design at that point.

NORBERG: Do you have any idea how Norris came across this?

ANDERSON: Somebody, somewhere, I don't know. It could have been through the Navy circles or it could have been some Wall Street contact. I felt that that was the bottleneck of the project. Things like that can take a long time to do them when you do them the first time. Even this printer control logic was complex and the printer itself was brand new, a line printer. And that was a contraption, too. Oh yes, Cliff Olafson and Erwin Schultz were the mechanical engineers on a printer. Anyway, somehow or another, Howard Daniels needed somebody and I got picked for that. I went over and helped Howard Daniels on an airborne voice privacy scheme, and there was a real genius, Howard Daniels.

NORBERG: Tell me a little about Howard Daniels.

ANDERSON: Well, he was kind of a glum character, but brilliant. And I had happened to run across his name when I went through the university and I was in the Honor Societies, Tau Beta Pi and Eta Kappa Nu. And I think it was Eta Kappa Nu where I was secretary and I had all the records from previous years and he had been indoctrinated some years before. He was a straight-A guy and I just sort of remembered the name. There weren't too many. Now I was working for this guy. I don't think he had a graduate degree, but he had been a university instructor in freshman engineering somehow for a while. He was not draftable; he had heart problems.

NORBERG: Here? At the University of Minnesota?

ANDERSON: Yes. At the University. This was his story: he said he wasn't too successful at teaching. He didn't make it because he was too tough on people. He began to flunk a large number of students out of freshmen math and the University didn't like that. But he wouldn't pass them. So that was an impasse and that's the way he told the

story. Now the reason he wouldn't pass them is that they just weren't up to his standards. I don't mean straight A at all, not at all. He was a very tolerant guy of lesser minds; but the statistics were, and we were told when we entered the University of Minnesota, that only one quarter of us would still be there at graduation. And he saw no reason to let people wait until junior year to finally decide not to be an engineer. So he was going to screen them out. I don't know whether he screened out three-fourths or what, maybe as much as a half, I don't know. But that was the way he did it. So he went to the Naval Ordnance Lab and that's how ERA picked him up with Gutterman and Rubens and perhaps some others. But anyway he was a very innovative guy.

The task was to do airborne secrecy, and I had had some acquaintance with that in the Navy with a time delay scramble device put out by Bell Labs using a metal tape. Incidentally, the Navy sound recorders were wire recorders. We didn't have magnetic paper tape or plastic tape in those days; the Germans had that. The U.S. got it from them. This time delay scrambler design of Howard's was familiar to me because I knew what the Bell one was like, although we never used it in operations. Howard wanted to build it small so it could go in an airplane and use low power. He had a most amazing set of inventions in it. He had a commutator consisting of a little bitty commutator, but it had to be very precise. So he built three commutators, an odd and an even commutator and then he had a smaller commutator that just had two parts to it, which would switch between the two crude ones. So the fine switching was with this little commutator with two positions enabling the other ones to have time to get into position. That was just one example of the kind of way he did things. He couldn't build a small enough precise commutator, so he just divided it into three commutators geared together. It was great.

NORBERG: What was your interaction with Howard Daniels?

ANDERSON: Well, I was supposed to be a circuit designer. We had technicians to do building, but I also did some bench work. But Howard had already decided on a way of doing it in general and so it was just a matter of implementing his ideas and that was okay. This was supposed to be a time delay scramble. And one of the contributions I made is an existing patent in his name and mine. We took a cross-wired wheel and divided it into three parts to make it variable. The cross-wired wheel was small, it was about as big as your finger, the shortest

finger, but it had three parts so you could take it apart and vary it. So instead of having a different wheel to change wiring, you could get different combinations from one wheel. This is in addition to the position of the wheel itself as it rotated, so we invented a variable cross-wired wheel. The patent calls it a switching device. Even at the outset was a public patent. But one of the little features I came up with was that you could turn the center section around. So that would give you another element of variability.

NORBERG: Now when you say turn it around are you talking about rotating around the axis?

ANDERSON: No. There were three cross-wired wheels in one. It was an assembly consisting of three cross-wired wheels which when connected, would give you a path through. But you could take the center part out and insert it in several positions. This was in addition to rotation to change the relative positions of the three parts.

NORBERG: But you'd have to do that manually?

ANDERSON: That would be manual. You could issue this to an airplane and say, okay, today's daily set up is 952. We could switch that thing around instead of having a large stock of different wheels.

NORBERG: So this is not unlike the rotor technique used in Enigma and machines during the war.

ANDERSON: Yes, once in use they did rotate in the same way but each wheel assembly was variable in itself.

NORBERG: Yes.

ANDERSON: While it was similar, we saw no need to put three to five wheels in sequence there. For the kind of privacy you needed, airborne missions don't last long and your value is to be able to talk to each other. Going back to my Navy experience, our squadron when we were flying in formation, the squadron doctrine was to have the wings a foot apart. Don't worry about that. They were a foot apart, but your wing was back even with the other guy's tail,

and then you were displaced three feet up so that you would have a V formation. You'd like to talk to each other because you can't see each other, you have to watch the head guy. Then in missions, you need to warn each other or make a change in plans. Anyway, whatever the airborne requirement was, it was not a long security delay, you didn't have to keep security for days or years. Howard knew exactly how to do this job. He had miniature wiring, miniature light-weight conduits, and it was nifty little system. We delivered it, but it never got put into production. The time-delay scramble really isn't very secure. People can produce a voice spectrogram and piece it together and possibly design equipment that could catch it on the fly and break the code. But I thought it was secure enough for the purpose. I really don't know why it wasn't produced.

But another thing that Howard invented, I think needs to be in the historical records. He called it the unit increment system for speech privacy. This was a way of taking an analog wave form and doing very simple coding, digital coding, by deciding whether the signal had increased or decreased in a small time interval. And that's the unit increment. After many microseconds, you sample to see if it has gone up one notch. If so, you post a one; if it has gone up two notches, you still just post it for one. So you have to choose your intervals so that it doesn't go up ten notches and you can't register the income. But this system later became known as delta modulation when it was invented by others. But he was ahead of that. And we built a demonstration circuit. His way of deciding whether the voltage went up or down was to charge a capacitor to the incoming voltage. Then take the voltage coming in at the next interval and compare it to what was on the capacitor. And if it increased, post a one; if a decrease, subtract a one.

NORBERG: How would you know whether it went up or down?

ANDERSON: You'd have to compare it each time. If it went down, a one was subtracted so it was really a ternary system. The circuit itself was so complicated that few could understand it, but the principle was simple and elegant. The whole thing was about twelve tubes.

NORBERG: What would this have been used for if it had been used?

ANDERSON: Well, this would have made a digital voice privacy system, which is capable of more security. Once it's digital, you can do all sorts of things. And essentially it is one method used today. He invented it way back when and we built the demonstration unit. Whatever the mission of the job was, we fulfilled it, got paid, and then I got assigned to Task 13, the Atlas project (now called ERA 1101). But that previous story continues because one day a guy from the Navy came in -- I think his name was Al Cohen -- to see Howard and he wanted a demonstration of this device. So Howard called me up and I came over and it was still sitting on a shelf. I blew the dust off it, connected it up and it worked. Anyway, Al Cohen never did forget that. I don't think they bought any, but it was a good demonstration.

NORBERG: Where was Cohen from? From the Navy you said?

ANDERSON: The Navy, the Bureau of Ships. One reason I remember his name is that when I moved to Washington I found he was starting up his own company, which was a fabrication company.

NORBERG: Back to the other half of my question which got lost in the discussion of the inventions and developments. How did you work with Daniels? What was his style at the office? Would he get up at the blackboard and start describing something to you or would it be done some other way?

ANDERSON: Yes. Well, on blackboards and papers. Sometimes it was pretty intense. He would pretend he couldn't remember how to do differential equations. Then he'd go through a long derivation of something, a general solution and the special solution, pretended to be stumbling along, and maybe he was. But, he'd always come out right. A brilliant, brilliant guy.

NORBERG: Who else was working in this group?

ANDERSON: Ed Korhone was one of the main ones. And then later John Hogan and Frank Kline and Curtis Fritze.

This was back in the front end of the Minnehaha plant, which was a little administrative office, and then in back they had Daniels' lab and in back of that Bob Gutterman's office and lab. Bob Gutterman had a lathe in his reception room. There were a series of railroad type rooms; you could walk from one to another. But Daniels style was glum, glum a lot of the time. Kind of a sour guy, but he had a real sense of humor. May I tell a couple of stories?

NORBERG: Please do. I'd like to hear them.

ANDERSON: I've forgotten most of the pet names he had for things and people, but a couple of them stand and sing loud in my mind. One was germanium diodes: he'd always call them petunias. "Hand me a petunia," and then he'd short something out with the diode and decide whether that's good or bad. And then people, usually behind people's backs -- and I was telling this to some of the folks yesterday -- there was a secretary named Shirley. His name for her was "Certainly".

NORBERG: Certainly?

ANDERSON: Certainly. Well, this is a good example of the kind of humor that he had. But the one I thought was most clever...

NORBERG: Well, why did he call her that? Did she always say certainly when you asked her something?

ANDERSON: Oh no. He was just making fun of the name Shirley [Surely].

NORBERG: I see.

ANDERSON: It wasn't that he was in crypto work; but he just liked that. Let me give you the next one which is more vivid and maybe you can appreciate more. Knight Pryor, I guess you know that name, he's on the list but wasn't able to come yesterday. He called Knight Pryor, "Eve", E-V-E, for night prior. See?

NORBERG: I see. I'm beginning to get the picture.

ANDERSON: I don't think he was the only person to do this, but he was more clever than the others who were doing it. He had names for everybody. Some of them weren't directly a play on the name, but there was some relationship. The style got perverted a little bit. But anyway, I thought he was hilarious. As dry as could be. He would come up with these things. I was working on a master's degree but ERA wouldn't let me work part time.

NORBERG: They would not?

ANDERSON: They would not initially let me work part time. So I was doing about two hours a day at the university here, coming in about 10:30 and then working eight hours. And that worked out all right, because Howard knew what I'd be doing in the evenings. I was diligent; didn't do any studying on the job. I was married, went home, had dinner and then was supposed to study. That was very hard to do, that studying part. My energy level was reasonable, but it began to show. So I think I said, "No, I've got to work half time." We finally did strike a deal where they let me work half time, and that worked out fine.

NORBERG: When did the half time begin?

ANDERSON: After about a year. I didn't start my master's work the first thing. I went back and took some liberal arts courses just because, being an engineer, I was a little dry on liberal arts. I took all my electives in the business school during my undergraduate work with the possible idea of completing a five-year business and engineering program, but with the war on that was not possible. But I did use all my electives for business courses. So I needed liberal arts and I took some sociology and psychology. Another course was an interior decorating course. I started probably in early '47, somewhere around there and got my master's in December of '48, working most of the time. I had to do a thesis.

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ANDERSON: The thesis did relate in some ways. I did a study of the multi-vibrator, using charge and discharge equations rather than trying to analyze it through linear oscillator techniques. People have tried to do that to predict multivibrator things and there had been a little literature on the subject. I did some innovation in getting more stability in a multivibrator, and also in the measurement of the jitter in a multivibrator. As a reference, I used a quartz precision oscillator that had been previously used in the EE department for their radio station.

NORBERG: Who were the predecessors who had done some work on multivibrators by that time?

ANDERSON: Well, there were, Kiebert and Englis, I think, were two of them. They were, I think, commercial guys. The Eccles-Jordan circuit and the flip-flop were in the books, but not much on the multivibrator. And yet multivibrators had been used a lot in the radar systems so they were well known in those circles. I think I studied this mostly for my own benefit, but I think I also did something reasonable there for ERA.

NORBERG: Who were the faculty members you were working with?

ANDERSON: Let's see. Dr. S.C. Larson I guess was my direct advisor, initially. It was Dr. W.G. Shepherd who was my advisor later and he was tough. And I had these very long equations for him to cope with. But they weren't highly mathematical; I just ended up with complicated RC circuit equations.

NORBERG: During this same period, then, from late '46 through to the middle of '48, who else did you have associations with at ERA? You mentioned Daniels, you mentioned working on Alcatraz and on the privacy device...

ANDERSON: Herb Nillis, Larry Steinhart, and John Hogan (I'd been at the University and in the Navy with him and he came to ERA somewhat after I did). Howard Daniels, Bob Gutterman, Sid Rubens, Jack Hill, Frank Mullaney, I'm getting into the 1101 now. George Hardenbergh, Bill Keye...

NORBERG: Was there any direct association with Norris?

ANDERSON: Not much, no. When I worked on the 1101 the first thing I was supposed to do is do was design a negative power supply for the workbenches. It was a good thing for a junior engineer.

NORBERG: This is Task 13 you're talking about rather than the 1101 as a machine.

ANDERSON: Yes, that's right; the 1101 label came later. But from the beginning we were building a machine. The power supply was pretty easy because they had a positive power supply and George Hardenbergh had made some sketches of a negative supply and Bill Keye, who got me started, had the most fantastic tidy engineering notebook you ever saw in your life. He had calculated power dissipation and the ranges of operation for every part he used in his designs. He had had good training where he had come from -- Airborne Instrument Company. But anyway he was a good guy to follow because nobody could be quite as tidy as he was. So the power supply was quickly done. But I shortly got into taking the memory drum writing circuit and trying to make it more reliable. It was having trouble with false firing and so on. And I think I almost solved that. I think when the thyratron was in good condition it was a fine thing, but with a little degradation it would either be hard to fire or there would be this extra little blip which usually didn't do any damage.

NORBERG: When you say you almost solved it, what does that mean?

ANDERSON: When the thyratron degraded, like a tube, a gas switching tube, they'd have a tendency to, let's say, misfire weakly after the main pulse. By the way, I rearranged the circuit, I got that to a minimum. But you couldn't prevent misfiring with a very bad tube.

NORBERG: How did you rearrange the circuit?

ANDERSON: Well, mainly the driving circuit. I lowered the impedance of the driving circuit.

NORBERG: Was this a circuit that someone else in the ERA had designed?

ANDERSON: Someone had started it and had been using it experimentally, so I didn't invent it. It was a clever circuit. A resonant LC circuit and all you did was charge up the capacitor and the thyratron would dump it through the coil which was the head. It would write a half sine pulse and then it would recharge. During the recharge, it would misfire. I had to do a lot of analysis. I think I got known around the lab for the work. Arnie Cohen was around and certainly was a mentor, although I didn't work directly with him.

NORBERG: If you didn't work directly for him then why would you say he was a sort of mentor?

ANDERSON: He taught classes in computer logic. You've probably heard of the machine called COGNAC?

NORBERG: Yes. Tell me about it.

ANDERSON: A fictitious cogitating machine that he had laid out as an example, a blackboard notes type example. And this, of course, was very helpful to get a quick dose of what our Task 13 was going to do, how things were going to fit together. So anyway, I did a few other things on that job, but that was the key thing. Frank Mullaney was doing the read circuits and everybody had a job. George Hardenbergh, of course, was the fellow who carried the logic along.

NORBERG: Tell me a little about Hardenbergh. He seems to be difficult to draw out.

ANDERSON: Well, he was, I think, a physics graduate, possibly from Harvard, and very mathematical. And his job was doing some of the intricate logic like processing algorithms. The machine language can only go so far, I mean you have to interpret a machine command and do a lot of switching to move this register into the accumulator and so

on and so forth. So he was working on a dividing algorithm and so on. He had a technician working for him who was competent and neat, but George himself would put circuits together with no chassis. He just wired resistors and capacitors and things together and tried them out. One day I think he went to the stock room carrying one of these chassis -- less circuits, and the guy at the stock room said, "What do you want?", and he said, "I want some more of these," and pointed to them. Now George knew what it was, but he figured the stock room guy wouldn't possibly know it by its right name, so he just pointed at it. But these circuits worked, you know, but you didn't dare touch them because you'd short out things.

NORBERG: I see, yes. Did you work closely with Hardenbergh at all?

ANDERSON: No, we were in the same big work room.

NORBERG: While you were working on these projects for Task 13 how long a period are we talking about? As I recall, Task 13 was given in about the middle of 1947 and therefore, when did you get onto that task and how long did you stay on it?

ANDERSON: It would be hard to... My three jobs in St. Paul, it's very hard for me to remember the sequence. It was Alcatraz, then with Daniels and then this... No, I may have it inverted. It was Task 13 before I went to work for Daniels.

NORBERG: Well, if that were the case then...

ANDERSON: I wasn't on it too long -- less than a year. I worked for Daniels, I think, most of the time.

NORBERG: How did you come then to leave St. Paul?

ANDERSON: Well, I got my master's degree and I wasn't getting a lot more pay and it seemed like a good time to

change. I'd gotten promoted so I wasn't in any distress. But, principally, my wife was from Brooklyn and we were going to have a baby and it seemed like a good time to make a switch for all the reasons. We lived in a furnished apartment and she wanted to be closer to home. Her father was a pediatrician and there was concern about the baby. I heard about the Arlington office and Bob Gutterman had already left and gone there. I approached Dr. Engstrom because he was there in Arlington much of the time. I can't quite remember the sequence. But the approach was to Dr. Engstrom and he said "possibly" and he'd think about it.

NORBERG: To do what?

ANDERSON: To arrange for me to move.

NORBERG: Yes, but to do what when you got to Washington, what did you expect to do?

ANDERSON: Well, I had some electronic talents and they were trying to do a broader thing at the Arlington office. They were relating to the customer and I had some knowledge of the systems we were building. Anyway, I didn't hear from him, so the next time he was in town, I found him and he had never gotten around to it, which was kind of typical. So I pressed him a little bit and somehow or another it finally was arranged. I was going to join the C. B. Tompkins math group. What did we call my job? We didn't have a name for such things then, but I guess you'd call it a systems analyst job today. So Engstrom arranged it and I got transferred.

One administrative thing was interesting. Before I left St. Paul, I wanted to know how much accumulated leave and sick leave I had, so I went up to the upstairs office and eventually found out. But they were very suspicious of why I wanted to know. So I said, I'm moving and I want to make sure of what my record is before I move. Of course the records were kept in St. Paul and there would be no mix-up, but they were very, very suspicious of people who wanted to know just where they stood with sick leave and other leaves. There may have been people who used all their sick leave as it accrued.

NORBERG: Was the baby born before you left or after?

ANDERSON: After.

NORBERG: When was the baby born?

ANDERSON: May '49.

NORBERG: May of '49.

ANDERSON: Yes. My first night in the Washington area was the night of March 31. On the morning of April 1st I got up and there was snow, in Washington.

NORBERG: That's how you remember! April 1, 1949.

ANDERSON: Don't trust me on all these dates, but some like that are ingrained. I found that the Arlington office was a headquarters. And I am a little confused about that, although Norris hired me, I think he had moved back to Washington. There were corporate shuffles, Meader had taken over. And Norris was vice president of marketing.

NORBERG: And he was in Washington when you arrived?

ANDERSON: Yes. During a long period of time he was there, and then he became in charge in St. Paul. But Engstrom was there, Norris was there, and Parker was there. And I think I mentioned yesterday that Parker had his Lincoln, his fancy Lincoln coupe sitting in the parking lot at Minnehaha Avenue. I got to Washington and there it was sitting in the parking lot in Washington with a different license plate. There were two cars; he just had duplicate cars. I guess the secretary brought it in or took care of it when he wasn't in town. Of course, he used an airplane a lot to go back and forth. But anyway, the Arlington office had this very powerful math group.

NORBERG: Who was in it besides Tompkins?

ANDERSON: Oh, jeez, I don't remember everybody. Al Roberts was certainly in that and Jim McLynn was one name that I remember.

NORBERG: McLynn? M-C-L-Y-N-N?

ANDERSON: Yes. I think he later worked at the George Washington University Logistics Research Project. I've got a few math stories. To repeat what I said yesterday, Tompkins was a supervisor and he had me drafting certain things. And he said I was succinct, which I didn't take too much as a compliment. Even though my writing was very clear, I think he wanted more academic volume in the narratives. One job I was supposed to look into... This is to illustrate the attempt of ERA to diversify.

NORBERG: Yes.

ANDERSON: It was to digitize the output of a wind tunnel employing Toledo scale devices as measuring devices. This was at the David Taylor Model Basin. So I went out there and looked at the thing and decided to use a shaft convertor, which was just being worked on in St. Paul, to convert the rotary motion of the Toledo scale. So we laid this out on paper. We never did get a contract from them. This was essentially proposal work. And then there were some short special assignments with Erwin Tomash in mostly the things that were either mildly classified or unclassified. The customer had a way of changing the name of things and compartmenting off a piece of something, and one of those things I did contribute to was called counting distributor. While you could see how it worked, it wasn't obvious what the applications might be. But some of them were very sophisticated.

One thing I remember very clearly is that John Coombs was visiting from St. Paul and they needed some sorting techniques. And John Coombs contended that you could sort a stream of information by just comparing two

numbers and putting one ahead of the other. And I couldn't see that it was possible. But I worked on it. And finally, I believe, I reinvented the binary sort. Of course, I had the big advantage that I knew somebody had done it before, somebody like Von Neumann or somebody else. I didn't know who, and John Coombs didn't know how to do it, but by taking two numbers at a time and comparing them -- putting one in front of the other, and then doing it in groups of two, then four, and then eight -- you could sort. If you did it right, you could do it with a deck of cards. I laid this out in a patent disclosure and everybody was happy with that. Tompkins may have left by that time.

Another thing that happened -- which kind of gets me close to where I live today -- was one of the sorting applications was the U.S. Treasury check reconciliation. By this time we'd been bought by Remington-Rand and we were doing some cooperative work with their Washington office and helping sell computers, UNIVACs and the 1100 series. For this particular application that sorting method seemed applicable, because all you had to do with check reconciliation, it seemed, was to sort them out. You could take an incoming check that just came in from a bank and see if it had been reconciled before. So you could get rid of all the reconciled ones, then you had to go through the numerical order until you found the one you wanted. That seemed like a good batch thing. So I proposed a sorting machine. By this time, of course, I had been working in the lab on projects, too. And I proposed a sorting machine using only magnetic tape, a comparator, and a Geneva movement to move each block of data. So you could put the data into a block buffer, and compare the two numbers to see which one's larger and decide which one you're going to let go ahead and wait for the next.

NORBERG: What is a Geneva movement?

ANDERSON: Well, that's a motion picture mechanism, a motion picture camera and projector film advance mechanism. A motion picture has to stand still when you're looking at it, then it moves. The shutter goes down and so you don't see it moving. But it's on for a while, then it moves and then it's on again. So this seemed like a very appropriate way of dealing with sorting when you don't know which tape you're going to move ahead. They both stop. They both stop while you're comparing the contents of the buffers, then you decide which one moves ahead. So I proposed this and we figured we could build these for about \$75,000. Four of them would do the Treasury check

reconciliation, maybe six. And so I proposed this and we prepared a patent disclosure. Bill Butler had real struggles with the patent disclosure, you know, all these numbers and examples. John Webster was our patent administrator and, of course, I dealt a lot with him. But UNIVAC would not propose this idea to the Treasury Department. They proposed UNIVAC I, UNIVAC I because that's what they were selling. This thing was too cheap. And, of course, IBM got the job. But that IBM equipment lasted for years and years and it was in the GAO building where I now work. That's how we get back to home. This is about 1956 -- and IBM finally delivered their product, maybe a little before then. My predecessor at GAO, Ed Mahoney, worked very intensively on that job, George Stickney and Howard Gannon, two other government guys. Ed Mahoney got a cash award for the GAO part in pursuing this. Of course, IBM got the equipment work and UNIVAC didn't get any. So it was kind of an interesting thing. But I think it illustrates some of the work that was done in Arlington in attempting to broaden the business and it didn't always work.

We spent a little time on and off, maybe as much as once a week, supporting the sales office in Washington. And that was a colorful scene. Herb Goodman, Herbert H. Goodman, ran that sales force on the second floor of 1615 L Street. Except for his office and maybe one conference room and maybe another office, there was a sea of three quarter sized desks. And Herb was about 300 pounds, usually wore a Stetson hat, a Eugene Paulette(?) -looking-kind-of-guy from the movies. He'd come out of his office a little after 9 in the morning, look around, and if he saw a salesman, he told him to get them out and sell. Maybe I'd have to be there working with a salesman on something, but there was a nervousness about not being out. And at 5:00 the salesmen were supposed to come back in, report their sales for the day, tidy up the desk work, and get prepared for the next day. So he ran a tight office, and he was trying and his people were trying to sell UNIVACs and the 1100 series a little bit.

NORBERG: Well, let me get something straight, Wally. When exactly did you leave Remington-Rand?

ANDERSON: June '55.

NORBERG: '55, okay.

ANDERSON: See, I had a longer version of what I said yesterday, but the way the thing was I did a flexible one with...

NORBERG: When you said UNIVAC and 1100 series I just wanted to make sure...

ANDERSON: '49. Yes, I was in Arlington April '49 and left in June '55. They closed the lab on the heels of our leaving.

NORBERG: Yes. Now getting back to the sales possibilities when you were working with the sales office. What sort of sales calls did you go on?

ANDERSON: Quite a number. I was usually called when it got technical. They were trying to sell a computer to their clientele for typewriters, Kardex, library systems, all these things. So they had accounts. And then they had cold calls, there were a few fraternity brothers, and the usual mix of sales opportunities. A couple of examples: National Geographic. They had found out, through some kind of personal connection, that there were about 4 million dollars available to buy a computer. So they had all these applications where they wanted a UNIVAC I, map-making, accounting, subscription or membership fulfillment, and so on. So I got called in there. I didn't spend a lot of time there, because I decided it wasn't worth it. What I found out was that it was a vivid case of misapplication. Map-making: they weren't going to use the computer for that; it was way too early. Other applications: they weren't going to implement any of them. Accounting: this wasn't big enough to justify the computer. They had 2,000,000 subscribers and about 18 product deliveries a year per subscriber, 12 magazines and 6 other billings and promotional things. And that's a lot of printing to address the mail. And they had addressograph plates, which did the printing, and they had little tabs on the addressograph plates that would say when the subscription was up and when to send the next notice and if the member was interested in some other side program. So everything was on an addressograph plate. The problem was that 23 girls handling addressograph plates in little boxes got dirty hands from the ink. It was a personnel problem. UNIVAC had no high-speed printer, it had a Uni-typer. The subscription

list was easily done on the UNIVAC but the printing, no. I couldn't see how in the world they were going to get the printing done. Too many subscribers every month for sure. And then six other things during the year. So that's two and a half million address printings per month. It was kind of a peak load. It wasn't just printing the address. You know, you have to do something with it, stick the label on or something.

NORBERG: Do you know what system they accepted at that time or did they not...

ANDERSON: They didn't do anything for a number of years. But I wrote all this up and somewhere in the files is my handicraft saying this is not a valid application. And that, of course, was not what they wanted to hear.

NORBERG: Not what who wanted to hear?

ANDERSON: Well, the people who wanted to sell. I was supported by ERA management, of course, because they didn't have that vested interest in UNIVAC, although I think there was cooperation. And it was not an application for the 1101 either, the 1102, or the 1103, whatever was current. But another application was the Perpetual Building Association, which was a savings and loan organization. So, I went down there and it was an impressive place. They had actually four paper forms that the tellers used for deposits, withdrawals, this and that. And they used compressed air systems. They would do a little ticket at the counter and send it in an air tube upstairs where they had a Kardex file. I couldn't figure anyway to beat this with a computer. We didn't have terminals! What are they going to do with a computer? They were, at some future time, going to open a branch office. Then I could see it. Then I could see there was going to have to be a way to get information back and forth which you could do over a teletype line and I could then see it. Again, my recommendation was no application. That's how it went. But there were other jobs.

I think the most constructive stuff was in the later days of the lab when we were supporting 1100-type stuff and my part of it... By that time, let's see. I was still doing lab work on the Air Force radar intelligence work, which lasted about four years. And we eventually got that into a digital form. Well, we designed equipment and moved

production into the St. Paul manufacturing plant. This was more sophisticated experimental equipment instead of the breadboard type stuff and projectors and things that we were using in Arlington to analyze and digitize radar patterns. We and the Air Force had devised some better ways to do it, and so we made drawings and they were built in the St. Paul, delivered one of a kind devices for further experimental testing. What it accomplished is the ability to take other intelligence data like aerial photographs and digitize them in such a way that you could predict the radar patterns. Now to jump ahead, when we started General Kinetics, although this was classified work, it was in our heads. We had no materials. We proposed that we continue this work for the Air Force. And they said something has changed. "We're not going to do that that way." And we thought it was kind of puzzling. But the work essentially dried up both for us in General Kinetics and for ERA St. Paul. And a little bit later, we found out that the ability to use the U2s had changed the need. But we had some patent disclosures and one issued patent on a device, one of the back up devices, a radar cross-section analyzer. I, Gordon Welchman, and Clair Miller were coinventors.

TAPE 2/SIDE 1

ANDERSON: To continue, what I'm trying to get at is that the Arlington work was paid for, I think, by the projects that we were doing and we did not get that much sales support, although sales support at the time was very colorful and interesting and somewhat productive in the end. But the radar intelligence work did allow us to be able to give the Air Force a way of predicting a radar pattern in advance using intelligence information. We were able to predict with optical means the effect of buildings, trees, and fields, and so on, more or less what you could expect for radar patterns. And that in fact we did some prediction of U.S. cities such as Kansas City and Cincinnati, which the Air Force was close to and some other cities artificially saying here's the way it's going to be and produce a digital maplike radar pattern. You see this now at carnivals when you buy a picture of yourself printed using white, black, and gray squares. I had Flexowriter build for us a few typewriters that would print gray scales. We used the numbers zero through nine and instead of the numbers we had various kinds of gray scales. While we couldn't get all black, we could get lots of dots for dark regions and then very few dots for light areas. We started with printing the letters, the Ws and Ms for dark things and worked our way from an ordinary typewriter. The Flexowriter people didn't know why they were doing this, but they did make a printer that would print a pattern. And we had the line feed and the

letter spacing equal, a tenth of an inch each, so we got squares of different shades of gray and we printed these. One of the interesting things we did to dramatize it, and the Air Force agreed, was an experiment in which we sent a night letter consisting of a radar picture of Kansas City. We synthesized Kansas City from other material, sent a night letter to the Air Force with that digital pattern which they reproduced at their end. So we proved that we could send it over the wire without distortion. And this was the second longest night letter up to that time. The longest one was a congratulatory telegram to Eisenhower at his election. Some people had gotten together and sent a letter to him. In our experiment Western Union didn't know what they were sending, of course. We had to bring the tape down to them, because we didn't want to read off this gibberish over the phone. So we played off the tape at our end and the Air Force picked up the tape at their end and reproduced the pattern. But it went through four other cities as relay repeaters and it came out well. What I'm saying is that we were doing some good solid scientific work and getting paid for it and then doing, on the side, this sales support work for ERA and UNIVAC computers. But to get at the more productive sales support, and this was more towards the end of the ERA, '53-'55, I was doing a number of things. I was a liaison to the Eckert-Mauchly Corporation for some of our relationships and also their attempt to spread magnetic switching and magnetic amplifier logic throughout the corporation. At one point we were learning that technology and I was commuting to Philadelphia a couple of days of the week and Bill Butler and Bill Overn and some others from St. Paul were there. This was kind of a more learning type of experience than liaison. I attended quite a few J. Presper Eckert lectures and did some work with Bob Torrey, Ted Bonn, and some of the magnetic wizards. That was good duty. And then my beat was the Air Force, because of my radar background and got involved in laying out future machines. Hank Forrest, I think, was 100% in sales work and we had a lawyer, an on-board lawyer named Herb Rickards.

NORBERG: Rickards?

ANDERSON: Yes, Herb Rickards. He had been a Navy contracting officer and so he knew contracts and had gotten a law degree on the side. So he was the in-house lawyer. And Engstrom was relating to people. I never saw General MacArthur, but it was an example of a team approach when we were dealing with the Air Force Cambridge office, which moved to Hanscomb field, and then also the Rome Air Development Center. Herb Rickards and I as a pair

would go in there and usually meet people from St. Paul. You want to hear a good story?

NORBERG: Go ahead.

ANDERSON: But let me set the stage for this story. This work was on technical air control systems (TACS) and later base air defense ground environment (BADGE). A popular nomenclature before the L series began. We were relating to the folks there and buying them an occasional lunch and dinner, no big politicking. But the team approach. I'm exaggerating, but there was a concept of this where General MacArthur would be talking to the commanding general of, say, Rome Air Development Center, so I heard. I travelled with Engstrom and Rickards at times where Engstrom would be talking to the technical director, Herb Rickards would be talking to the contracting officer, and I would be talking to a technical person, perhaps with some St. Paul people, perhaps not, laying the groundwork for these big systems. And it was a team approach. And just for fun, when we had the company Cadillac, I would joke to myself that the chauffeur would be talking to the guard at the gate. So it was a team approach, it really was. Okay, now one of the most...

NORBERG: Can we hold it there for just a moment? In this team approach, to what extent were you reacting to RFPs or to other sorts of stimulants? You might know what was going on at Rome or was this a continuing relationship or whatever.

ANDERSON: Usually it was prior to RFPs, helping draft the requirements and specifications. This was good insight on Herb Rickards and other parts and St. Paul people, too. This was in anticipation, and we tried to control RFPs most of the time. And this worked. I've forgotten which was which, but one of the Air Force groups was doing an analog approach and another one was doing a digital approach. We were working with both and that was great.

NORBERG: Trying to do what with both, get some sort of cross-over or develop different systems in each case?

ANDERSON: There were different approaches, and we wanted to be a contractor for that approach and this

approach. And they had somewhat different missions, but the approaches were different. But it didn't matter to us. We weren't that fixed in digital, because we had other things going. Nothing big came while I was there, but \$5 million dollar level things did come out of that later and I felt that the groundwork we laid was good, even though it was all fits and starts.

On one particular occasion there was going to be an example of real cooperation among the different parts of the company. A big meeting was going to be at Rome Air Development Center. And I was on my way there. I was going to be in Philadelphia. I wasn't feeling well. I was in the hotel and feeling just real lousy, but I went to the Eckert Mauchly plant that day and it was a miserable day, too. Somehow I did what I was supposed to do for Philadelphia Remington-Rand, and then I went out and got a train to New York and then the night train up and around to Utica. And there were no compartments available, so I got a lower berth. But I'd been doing a lot of train traveling and the weather was stormy, so great; no flying that night. The next morning I got up and found that Bill Norris was in one of the upper berths. By that time, Bill had gotten into his train mode. He'd done a lot of flying with Parker, and he was fed up with that. And Herb Rickards, I guess, was there already, so he had rented a car and met us (that part I'm fuzzy on). So we're up there bright and early in the morning. The Philadelphia boys were going to fly in: J. Presper Eckert, Jim Weiner and Fraser Welch, who was a private pilot and had an old Bellanca plane. It was real old, maybe 1930s but it worked. We found out later he had only a visual flight license, he didn't have an instrument license. But the weather had cleared up and they were going to fly in. And we were waiting for them wherever you wait for people to fly in. They had permission to land at Griffis Air Force Base in Rome, NY. And we got news from somebody that they had been detained because the head of operations was after them for an illegal landing. Well, the circumstances were that they had landed while the field was closed. There was a red light on the tower at that time instead of the green-white light that flashes so many times a second and tells you what airport it is. You can see that from long distances, so you'd come in and you know it's the right airport. And they had permission to land. But what had happened is there had been an emergency and some plane like a B29 was making an emergency landing and they wanted to clear the field for that, so they closed the field down. The red light tells you you can't land. But they came on in. And they had heard on the radio a phrase that led them to believe they could land. It turned out that they got released from custody because the thing that they had heard was something like "follow that fire truck in,"

or words to that effect. It turned out that these words were instructions to the plane having the emergency. And so they landed in the middle of all that. The whole point was they were private aviators and they were going to make a big impression on the Air Force by coming in in their own plane. And of course that backfired completely. Well, we had our meeting and there were the opposing positions between Norris and Eckert in serving the Air Force. Yet everyone was trying to be cooperative and everyone was trying to show what a unified company we had; it worked out, I guess, reasonably well despite the dramatic arrival.

NORBERG: What sort of opposing positions? What were they arguing about?

ANDERSON: Well, there was an undercurrent of professional jealousy and corporate politics. I'd never see it go up to the surface, but the ERA people thought that those Philadelphia geniuses couldn't build reliable equipment while the Philadelphia people felt that the bunch of pedestrians at ERA were doing that kind of stuff while we're at the forefront and so forth. But anyway, that's the background as I viewed it.

The story continues. There's another episode. I found out later -- I think it was through Herb Rickards -- that the Philadelphia aviators didn't have enough gas to get back to Philadelphia, so they wanted to buy some gas. Of course you can't buy airplane gas at a military base. So they flew to the Oneida County airport, which is the Utica airport and in the course of gassing the plane, they bumped the fuel pump and damaged the plane. I'm not sure the latter is true. It was a comedy of errors. But anyway, they did get back. Good fun. Those intensive working sessions with the Air Force at all these levels did eventually produce contracts. The company had things they could do, and there were very few companies like that. Of course the magnetic drum storage was a main thing.

One more story. One time, this was towards the very end of my experience there, I was working on a cooperative proposal with the Norwalk people, the Norwalk Lab. Jack Ferry was the liaison, the administrative guy who was calling shots on this. And what the job was to build a converter that would take radar information, put it into digital form so it could go to the computer for processing. Whether it was air traffic control or whatever. So I was doing the technical proposal. And the magnetic drum lent itself to this pretty well. You could take the radar rotation and stick

the incoming pulses in positions on the drum and then peel them off in rectangular coordinate form if you wanted to. You could get the radar polar coordinates from it, range and bearing and you could mathematically convert it or you could do it with a drum geometry. So this seemed like a workable thing. We proposed a modest special purpose machine for this, and priced it out. I'd learned to price machines in an interesting way. That's one of the things we were doing in Arlington. I'd do a flow diagram and try to count parts, tubes, etc. Using as much of an actual circuit as I could do, I would estimate so many dollars a flip-flop. But these estimates were never very good; usually they were on the low side. So what I would do is take the three jobs that I knew about that were closest to the one being estimated, like the Logistics Computer, the second 1101, etc. I had access to numbers like that. And you could kind of figure just by averaging these that the machines would cost about that much. That kind of estimate includes the learning curve and all the waste motion and trash and stuff like that. So I would do that kind of an estimate for my own benefit. And then I would come up with a parts count modifying my detail work with the reality of the other. So I'm not sure I was a good estimator, but I was a better estimator by using that technique. Management wanted some hard figures. So anyway, that was the kind of approach. And I had a reasonable estimate on this particular instance. Okay. The thing was about ready to go, and it had been up to Rem-Rand corporate for approval. And I'm not sure who said these wonderful words, but Jack Ferry was on the phone one day and he came back with bad news. I said, "What's the problem?" He said, "We've got instructions to do 'a non-laughable proposal that's unlikely to succeed.'" So we fumed and stormed and calmed and fretted and did all those things. There wasn't much I could do technically to be non-laughable and unlikely to succeed.

NORBERG: But what was the point?

ANDERSON: They didn't want the job.

NORBERG: Then don't bid.

ANDERSON: No, they wanted to be cooperative for this particular one even though this wasn't part of those TACS and BADGE systems. This was another thing. But that particular office had had a lot of trouble with the government

and they had some claims against the government. But they didn't want the job. I didn't want to change my technical proposal. The way that management dealt with this was that they priced me up a \$100,000 above whatever we had for profit and everything else. We didn't get the job. Some years later, I became pretty good friends with Jim Burrows, who's now head of the Institute for Computer Science and Technology at NBS. He said he was on the Mitre end advising the Air Force on that and he knew that our management had screwed up the proposal. So I told him my story. And he said, "Well, let me tell you the rest of the story." He said Burroughs Corporation got the contract, and they got to build 800 units. Burroughs just had a feast on that. And Jim said, "You guys should have had that contract."

NORBERG: Did you visit the Eckert-Mauchly division during this period?

ANDERSON: Yes.

NORBERG: What was it like there in comparison to St. Paul?

ANDERSON: Well, it was more basic research. As I say, I was commuting to Philadelphia to relate on these magnetics circuits, the possibility of using magnetic amplifiers. Of course, Seymour Cray later did his own thing in that area in St. Paul. And I don't think he was part of the liaison or educational team. But what was it like? The vacuum tube circuits I saw in UNIVAC I thought were very sound. They were textbook circuits. The ERA circuits that I did and we did were not really textbook circuits. I think they were more innovative and more pragmatic in that you'd settle your circuits by actually bench-testing. I suppose they did that, but I looked at their circuits in many cases in connection with magnetic work, and I thought they were rather classical circuits. And there was no problem there. Physically, the place looked different because it was more researchy. And what I remember is that the NIH factor was very large. Now they were doing drums, but they weren't going to use the ERA techniques and go from there. Eckert was building high-speed drums using air bearings. I mean high-speed. He had small drums, high-speed motors, I think they were plated instead of sprayed oxide, and they were so fast -- and I've forgotten exactly the RPM, 100,000 RPMs or something like that -- they went through Mach II. When they would rev up, they sounded like they

were running and then restarting. That was because they were going through the speed of sound, and then it would sound like it's starting up again. So they were very impressive with their air bearings. The idea was to get the drum to run faster so your access time would be shorter.

NORBERG: That's right.

ANDERSON: They were doing that kind of stuff at that time. A lot of magnetic work.

NORBERG: Did they sell a lot of those drums?

ANDERSON: I don't think so. I'm not sure where they were used. That was an attempt to get higher speed access. And they had these magnetic amplifier circuits and a lot of solid state work. They also had a lot of troubles. The Solid State 80, I think, was on an Air Force contract and it was a nightmare of diodes and unreliability initially. And they were late. I was involved in that kind of thing. That was a hush-hush development. It was commercial, well, it was unclassified Air Force but they weren't going to let anyone see that kind of stuff. And they were pretty good conceptually. But reliability was poor at that stage.

In my view, Jack Hill was one of the main persons responsible for the ERA reliability. And others who went through a couple of the early projects, which were kind of ordinary electronic radio, radar kind of thing. But the big machines had to be much more reliable. And Jack Hill came in and said, "No tube over a third of its rating, no resistor over half its rating. None of these Jones plugs, they're going to be military connectors." A whole host of these simple policy things that ended up with ERA having a much more reliable kind of machinery.

NORBERG: Were they Jack's or were they the customers?

ANDERSON: I believe they were Jack's, based on his experience on previous projects. Or maybe he didn't, maybe he inherited them. The other thing, and this is my concept of the ERA air conditioning, I never heard anybody express it

this way, but what seemed to me was we were building machines inside an air conditioning duct and other people were building the machine and trying to air condition it. Now they all looked like computer cabinets. But Jack Hill came in with 3M plant experience and designed an air conditioning plenum with the machine parked inside it. He called it a cabinet and it was designed and looked like a cabinet, but it was a different mental attitude. And take the UNIVAC computer; very, very hard to air condition. And if you didn't uniformly cool it, some places that were hotter than others would have troubles earlier than other places. IBM in some of their machines used room ventilation, drawing air into the machine after trying to cool the entire room. But it was a different mental set. It might have been unconscious on Jack Hill's part, but he did a real fine service in that regard: circuit reliability and the soundness of the ability to keep the thing cool. Now you know, he didn't spare the horses. Also, electrically, we generally used a motor generator set to give us our high DC voltage and then the power supplies which regulated came off that. This gave us the advantage in that line transients wouldn't cause any trouble. The inertia of the motor and generator combination carried through simple line transients. In contrast, the UNIVAC people didn't do that same kind of thing.

I had some acquaintance with the Air Force UNIVAC in the Pentagon when I moved to Arlington. It had been in there one year, almost one year and the guy in charge was named Bob Kopp -- I don't mean to tell anti-Eckert-Mauchly stories, because I think generally their circuitry was real sound -- but instead of a delta transformer system to take the three-phased power and use it in the machine, they used what's called a Scott connection, which was two transformers on the three phase circuit. The other just was like a virtual transformer. But just two. And these were adjacent to the computer in a cement ditch in the floor and they got too hot. So temporary fans were placed there to blow this hot air away and cool the transformers. And Bob Kopp, being a good government administrator, said, "You know, this really isn't right. Everything's working fine, but that's really not right. Our guarantee was for one year." So shortly before the year was up, he had the fans taken out. They were just floor fans. The transformers overheated. Apparently they tried to save money with this particular transformer connection, but under those circumstances in that ditch, there just wasn't adequate cooling. I don't know what air movement they figured on, I don't know. Out in the open air or something.

NORBERG: So what happened next? Was the installation taken out or did they use something else?

ANDERSON: No, they had to put in adequate transformers, which was costly; -- e.g. not time consuming in the sense that they had to rewire the whole thing. You know, I've got a Howard Daniels story, too. I told this one to some folks this week. One time Howard came to Washington. He was consulting on something. He was asked to go look at the UNIVAC card-to-tape machine, at the Census Bureau. For some reason, I'd been there and knew how to get there so I drove him out there. And it was nice to see Howard again and we went to the Census Bureau in the early morning. Being his usual self, he didn't want to do this extra task. He confronted this machine -- I guess it was operating -- and the symptoms were that it would overheat about 10:00 in the morning. It was turned on about 8 a.m. and overheated by 10. So they had fans blowing on it and the cover open to let the heat out. And he was supposed to diagnosis this. So he looked and he said, "Maybe you wouldn't have the problem if you lowered the Venetian blind." The sun was streaming through the blind. That was it but maybe it really wasn't it. I didn't want to stay there all day and see if it overheated, if we lowered the blinds. But apparently Howard had been instructed either by Meader or somebody else to take a close look, so he kind of looks at the back of the machine and here's the back cover with a grill and a fan. It proved not to be adequate, because they had to use a floor fan. He opened the door and we saw the fan mounted on the grill, right in the middle of the grill. He said, "Maybe if you put a baffle there where the fan isn't blowing it would be all right." See, the fan was just churning air and not pulling it through the machine.

TAPE 2/SIDE 2

NORBERG: Could this be done simply and quickly with no problem?

ANDERSON: Yes. The idea Howard had recognized that this grill with the fan in the middle was not drawing air all the way through the machine and a good part of the air was just being circulated in the location of the fan. Covered with a baffle material, simple sheet metal with a fan sized hole in the middle would be a great help. Well, that was it.

NORBERG: Quite remarkable.

ANDERSON: But they had a customer engineer on site and designers and it was just a little design oversight, but Howard just walked in and made the diagnosis.

NORBERG: You made mention of a couple of competitors a few moments ago: Burroughs and IBM. What was the view from Washington of your competitors in the business at this time? We're talking about the middle '50s period.

ANDERSON: Well, okay. IBM knew what we were doing, in the sense that we'd delivered a security machine -- one that hasn't been discussed and which I'm not going to discuss. IBM had built the input-output punch card feeder for that. This card reader would take cards, read it, go to the machine which would do a lot of special purpose electronic operations, come back out and punch cards. Commercial Control Corporation was a part of IBM which was doing that kind of stuff. They may also have been the maker of the electric typewriters and the punched tape units we used, a predecessor of the Flexowriters. So IBM was well aware of the security stuff, at least some parts of IBM. But I don't know exactly the view of the IBM people, except the ones that I worked with knew what we were doing, that was high-speed computers and we had big drums, you know. Some of the Task 9 machines had 30-some inch drums with quite a lot of data and there were other machines and so on. And IBM had enough entry to those things with their punched card machine used in the security business that they knew what we were doing. Whether it was for these official input-output devices or just their punch card devices, some parts of IBM knew. I think the view we had might be instructive, though, because other parts of IBM didn't know. For example, one day in the Arlington thing we were invited to witness a demonstration of the Card Programmed Calculator, which was a desk sized machine, and we attended this. I don't know what date it was, but whenever they were introducing and promoting the Card Programmed Calculator. It had sixteen words of storage in a box, which was the size of a third of the desk. And this one had two boxes or 32 words of storage. Now these were big words, maybe card size -- 80 characters. But 32 words of storage! And here's this big calculator that they were very proud of. Now we'd been delivering bushels of Ks of memory, I'd like to say millions of words of memory, but certainly millions of bits. We had delivered maybe 20 machines -- a few like Task 13 that were general purpose but others that were special purpose with huge amounts of

memory. And they were trying to sell us that. It just didn't make sense. Of course, once IBM got into the business, they went fast.

And as I mentioned to somebody, we had a lot of difficulty trying to sell machines out of the computing center because IBM would have a spec. sheet which would always be better than any live machine we had to demonstrate. I always said that an eight and a half by eleven inch piece of paper could always outsell us, because they put faster and more memory on this piece of paper, no matter what you had. The Japanese did that in the initial stages of their computer business. They would take the 360 and 370 and just up the specs and build to that and say "we outperform IBM" -- simple! For the period when they don't have the machine, the brochure or specification sheet is supposed to make people wait. IBM could make people wait but the Japanese couldn't hold as many people waiting. It was that kind of a thing. So IBM was a tough competitor.

NORBERG: How about some of the others? You mentioned Burroughs specifically.

ANDERSON: We didn't see too much of Burroughs. It's hard to tell who the competitors were. I don't really remember too many. There was competition. I think Telemeter Magnetics, if I remember correctly, a Philadelphia outfit, was a competitor. I think the biggest trouble was not competitors but trying to convince people that they needed a computer and that it could do something for them. And I don't mean just those instances which were not valid applications, because there were plenty of valid applications, but it was kind of early in the game. One of the private dreams I had, which I never adequately documented, was take UNIVAC I in New York, the demonstrator, and connect it by a phone line to the 1101 in the computing center and do some useful business problem that had a lot of multiplication. It would come off the wire and go on back. We never did it, but I believe I wrote a memo on it somewhere around the time which would have been a nice example of computer cooperation and time-sharing.

NORBERG: How effective was the computer center in helping to promote sales?

ANDERSON: Well, I guess it was some, but I think it was way too ahead of its time, and possibly we didn't know

how to do it, really.

NORBERG: What sorts of things were you doing with it, then?

ANDERSON: Well, we were trying to run sample problems. Trying to run sample problems. We learned a lot from it. Norm Yarosch was telling me yesterday about one trouble he had. He was using it as a demonstrator to compute any date, you know, doing a day of a week that any date would be on. He had written this program, and say what day was Lincoln's birthday in 1926, it would do that. And he was going to demonstrate this. And he said he had problems because somebody put in a non-valid date. It was a date which was not in his range, which went from maybe the 1600s to maybe 2000 something. And he had no edit in the front end to screen out invalid dates. So the machine just didn't know what to do and went wild. So he learned a lesson from that demonstration that for the range you're operating in, it might be a perfect program, but you have to do something to tell the computer to ignore all else. He was telling that story last night. But anyway what we tried to do was work people's problems and I wasn't in that very much.

Dr. Clair Miller hasn't been mentioned. I've mentioned him in the last couple of days. He was a genius, a Ph.D. from the University of Chicago. We had him and we had Herb Kanner, another Ph.D. I think from Chicago. And we had Bill Fiden, who had all but the doctoral dissertation from Columbia, a physicist. And these three were working on our radar project. We had a classy outfit, really. Clair Miller got invited to do something on the 1101 and he recognized that there were some spare bits in the instruction and he found something for it to do that was useful and they built it into the machine. I've forgotten just what it was, but he had that kind of insight after minimum time with the machine. Sometimes we'd have lunch by the machine (and this was a regret that I have), and we played Nim with the machine. It was nice thing to do. It was the kind of thing where it depends on who starts, but it was kind of fun. And what I regret is that we didn't do some mathematical computations and just publish tables, which if you've done something like $y = (1 - e^x)$ or something like that, something that didn't have a published table, you could have published it and received some notoriety. What I'm saying is that it would have been a productive thing, instead of wasting the time. We didn't yet think in terms of using every spare minute on a computer.

NORBERG: What gave rise to the decision to leave Remington-Rand?

ANDERSON: Oh, they were going to reorganize and eliminate the Arlington Laboratory. They had acquired ERA in '52 and John Parker was still president because we were a subsidiary. According to this one story, one day James Rand sat on his yacht in Florida and said, "What would happen ERA was a division instead of a subsidiary?" The accountants worked on it and said it's going to save money, so many thousands of dollars a year, and he said, "Do it tomorrow." But we in Arlington had contracts and novation agreements were needed. We couldn't do it "tomorrow", but they made ERA a division. So Parker then became a vice president of Remington Rand because a division didn't need a president. And things began to drift away, Engstrom was vice president and Norris was back in St. Paul, a vice president. Roy Bryant was running the Arlington Lab and then he left and Bill Goggins who was a retired admiral took charge. I learned a lot of integrity from that guy. We started our business together, a great guy. He was a fine administrator but technical to some extent. But the lab was shrinking because of a number of reasons and there was this desire to pull it on in, economy of scale, whatever reasons. So many of us were invited to move back to Minnesota.

In fact, I was asked, "How many of you folks are going to move if we wanted to move you all?" It wasn't an idea of dumping us, it was the idea of dumping the lab, I think. And I think they wanted to control the contracts and everything else from the headquarters in St. Paul; that is, Norris wanted to do that. And we understood that. So in spite of being a home town guy, I said, maybe 5-10% would move. I had enough sense to say that the number of people that moved would be a function of the distance, an inverse function of the distance. I didn't want to move back, we were happy where we were and had frequent visits with the Minnesota family. I'd been going back on business often enough to see my folks and they were in shape to travel, too; so no problem there. I guess a few people moved. There was the corporate shuffle and they closed the lab. We saw it coming and we incorporated General Kinetics ahead of time, I think in November '54, watching this process and trying hard not to do any GKI planning on the job. We met at Bill Goggins' house and incorporated. Then in April, we said we've got to tell the Remington Rand folks we're going to leave. We have got jobs we're doing and responsibility and we didn't want to

leave them high and dry. Bob Gutterman was the root of General Kinetics. He's the one who wanted to start a business. And I was second in saying yes, let's do that. He needed encouragement. So then Al Roberts, Frank McKutcheon and Bill Goggins wanted to come in. Because of the corporate politics -- and I don't think this story has ever been told -- we decided we had to tell the Remington-Rand, the UNIVAC people, and the ERA people all at one time. So on a particular day, I guess it was in April, 1955, Bob Gutterman was in St. Paul on some business and we were in Arlington. I guess Engstrom was in Arlington at that time. Somebody else was in New York, and we staged it to tell them all at the same time. We said that we want to leave and start a company and we don't want to set a date because we have these project responsibilities and we want to come to an agreement. And that kind of a thing seemed to work. It served our purpose that nothing abrupt happened and it was a thoughtful end to a relationship.

As I recall hearing second hand, Bill Norris' comment was, good luck, but we won't do any business with you. You're good friends and fine, but we aren't going to do any business with you. But the time dragged on and we could never set a date to leave. About that time, I was working with Jack Ferry in Norwalk and he was assigned to wine and dine me and take me to the country club and show me how great it is in Norwalk and try to get me to stay with the company. That was fine, except that wasn't what I wanted to do. We finally set the date at the end of May. On June 1st we were going to start our business. And that's the way it was. And pretty soon, with people leaving and not being replaced, we had a pretty small crew. Maybe Hank Forrest was left. Quite shortly after that they closed the building and we started General Kinetics in the computing center location in Arlington, 555 23rd Street South.

NORBERG: What did you people design General Kinetics to do?

ANDERSON: General scientific work. We put out a little brochure in two days, just listing our backgrounds. Goggins, who had been in Navy communications, had really an illustrious Navy career. He'd been a communications specialist, but also a battleship captain. He got his Admiral rank when he retired. But Goggins, Gutterman, Roberts and I started GKI. McKutcheon bowed out by the time we left ERA. So we put out this little leaflet and our goal was general scientific work in mathematics, electronics, and so on. Bob Gutterman had a little consulting practice that came on rather quickly and in a while we did do some work for UNIVAC in Philadelphia. We started jobs labelling

them K1, K2, etc. -- K for Kinetics, and I set up an administrative system somewhat simpler than ERA.

NORBERG: Can you describe a few of the early tasks that you people did?

ANDERSON: I don't remember what number one or number two was. And Bob brought in some construction work having to do with his consulting. He was always getting involved in something new mechanically. Number three I remember was for Reed Research. Stanley Reed had seen our brochure, liked what he saw, and had a contract with the Canadian Post Office for automatic mail sorting. And he envisioned doing something for the U.S. Post Office, maybe in computing, something like a letter picker or something. The Canadian Post office had a bunch of conveyor belts and needed an optical reader. He was fussing with somebody else and he also wanted us to do some work on it. I guess I did most of it. It was a very short project, not very costly for them, but gave us credentials. And he got patent disclosures out of it. What I tried to do is find a simple way to read an address. We went to the post office and saw how they sorted. So what I came up with was a -- oh, what did I call it, "top hats and tails" is what I called it. But somehow, it was never implemented, just a paper design and a little bit of lab work. Using a scanner photocell reader, you try to find the bottom of a typewritten address and recognize the bottom as the city. Then let's say in Washington, you would notice that a lot of people would type Washington D.C. So what you'd do is do a letter count, just count the letters, the number of letters, try to ignore the punctuation if you could, and then D.C. where you'd try to use or ignore the punctuation. But what was supposed to make it work was in Washington, you had a "g" which hung down below the line and then you had letters that would stand up, "t" and "h" and "i" and capitals would stand up higher than all of them. So I called tall letters "top hats" and hanging letters "tails". I would count the letters and in the course of the scan with three photocells you'd see which ones were tall, which ones hung below the line, and make a judgment on that that it was indeed Washington or that it might be Bloomington. So with some cities you might not know the difference, but the differences between Washington and Bloomington are an "l" and an "h" and the letter count. You would presort it so only typewritten letters were scanned. You would take those that were not Washington and D.C., not Washington but had the two initials and you'd decide they were North Dakota, South Dakota, North Carolina, South Carolina, etc. -- so all you could do was a presort. Or, in any city, you might be able to do something like recognize only that city. Anyway it was a start and I thought realizable with the

current technology at that time. So I took it over to Stanley Reed and presently he paid us and later his patent attorney came over and we told him what we were doing. They filed for the patents. And then they reinvented having found some other sophistication. But I wouldn't claim to be the inventor of record. But I contributed to the patentability. I did an awful lot of things. I have forgotten most of the jobs. I have a list somewhere of what the jobs were. One of them was a film cleaner for Byron Laboratories. Bob Gutterman had a friend who was chief engineer at Byron. And they needed something to automatically clean motion picture film, negatives, because negatives are all dirty from editing and handling and splicing. So we toured the processing plant and Bob came up with the idea of using ultrasonics, which had been tried before. But when he looked at the literature for prior art. It was not common for inventors I knew to look at the prior art.

NORBERG: They usually find that out when they go the patent office.

ANDERSON: Yes. He found essentially that predecessors were doing ultrasonics in a teacup and he realized, being a good physicist that more power was needed. So we got some big ultrasonic transducers on loan, ran an experiment and did indeed clean grease pencil markings off. And then somewhere along the line, we were able to prove it dramatically, first by having a tank where you just have the film sitting in there, in the chlorinated solvent, and had grease pencil marks on it and you'd turn on the ultrasonics and the grease would just pop out. But we did it with an example and Byron was convinced, so he decided to help us build a machine. He'd pay for the construction; we'd pay for development. And then we'd give him the right of first refusal on sales.

Now it turned out, as initial projects always go, that the contract we had with Byron didn't in any way pay for all the construction costs. His part was fixed price. So we swallowed it and proceeded to a prototype and called it "CF-1, Cleaner, Film #1." And it worked. It was in a sealed cabinet and Bob invented a non redepositing dryer which peeled off the moisture. Drying cabinets were normally as big as this room, the film being gently dried so you didn't disturb the color with temperature changes. But he had figured out that if you used heated air (now this wasn't a precise calculation, but seat-of-the-pants intuition and some calculation), that if you approach the film with a jet of air and peeled off the moisture and the air was warm, what little evaporation took place was balanced by the heating by the

warm blast of air. The process would not temperature shock the delicate color and it indeed peeled off the moisture and the debris. So stripping was better than drying. So we got the patent, and it was a strong patent.

Byron decided not to do the sales, leaving us the ability to demonstrate Byron's machine for lots of people. A representative of Allied Artists, which was a film producing and distributing company, Milton Lipsner, liked it and formed a company called Lipsner-Smith, with a dentist named Eli Smith and his brother Jerry Lipsner. We took a minority interest and developed a second model called CF2. Then they did a clever PR stunt. They took the big C and F -- cleaner and film -- and put the 2 as a subscript, so it looked like a chemical process, CF₂. We used Chloroethane, (1-1-1 trichloroethane) as the safest solvent. And we produced four of those machines in our lab and they went to the Department of Agriculture and three commercial film labs. On the basis of those four, it was a real go. The reason we needed sponsorship was that the whole film industry needed this but was afraid of television tape and wouldn't invest 10 or 12 thousand dollars into buying a machine. They would only lease. Lipsner Smith had the money and did indeed offer leases. And they wanted to do the old IBM stunt: they wanted to lease only, sell if they had to, (price high if they sold) and sell the solvent. We'd package the solvent under the name CF₂ and that worked out. We turned over the production after the first four and the rest were produced in Chicago. That worked out and after about maybe 20-25 machines, these machines made a major change in the motion picture industry. And I mean a major change in processing and so on. So Bob got the gold technical Oscar from the Motion Picture Academy about 1960. It was the year Bob Hope got the honorary Oscar and Hope said to Gutterman, "I see you copped one, too." The Technical Oscar is a little different in shape from the Oscars for performers.

What it did in the motion picture industry was amazing, because when we initially visited Pathe Labs in New York, they had a bunch of women and the film negatives were either on a worktable or on a big drum and they used the household solvent, carbon tetrachloride, to clean the film. That's the poorest for safety. Ladies using carbon tet with Q-tips were taking off these editor's marks and dirt. Editor's marks on the film itself, splicing faults, spots of grease, or dust make white spots on the print, so it was very important to clean the negative. So they were doing this and they had the rooms with either a big drum, or the film was coming along the table with a lot of air blowing to get this carbon tet out. Six or eight women were doing this all day, eight hours a day and three shifts, that's not safe work.

So we were doing a safety improvement as well as better cleaning. What Bob got the Oscar for is essentially the elimination of white spots in the movies. The machines are still in use, but as discipline in using the machines goes hay-wire, and they make continuous production runs and don't maintain the machine in between, some white spots will reappear. But there was a while when you had almost no white spots when you went to see a movie and that was why he got the Oscar. We used the ultrasonic technique in cleaning UNIVAC metal tape, so we did get back with UNIVAC. We also built metal tape testers for UNIVAC. We could do many things through Bob's ideas -- I usually did the electronics, that was kind of routine. But the things that Bob as an inventor could do! For example, UNIVAC would punch holes to indicate bad areas or splices in the metal tape, because you had the convenience in UNIVAC metal tape of locking out the ability to write on bad areas. A bad area was identified with holes, two holes punches two and a half inches apart. Bob devised a way to punch on the fly so you could test and punch in one pass. It was kind of rinky dinky to watch but we built one machine for the Army Map Service, which they allowed us to demonstrate to everybody, called a U1. Then later, bigger machines which UNIVAC bought for their Utica plant. It was a very uneasy relationship because the Philadelphia people were building their own testers where they had to stop, push a button to punch and then move on. And we made some inroads but they kept wanting to do their own. But the ultrasonic cleaner technique was used in a series of metal tape cleaners for UNIVAC until they went out of the metal tape business. This led to using the ultrasonics for cleaning the stock that tape is made from, the Mylar before it's coated. So we built some factory equipment for the tape industry.

NORBERG: How long did you stay with General Kinetics?

ANDERSON: Nineteen years.

NORBERG: And then went to GAO?

ANDERSON: Yes.

NORBERG: Well, I want to thank you very much. This has been very good.

END OF INTERVIEW