

An Interview with

HUGH DUNCAN

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Conducted by Arthur L. Norberg

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Abstract

Duncan describes his engineering education and entrance into the Navy in World War II. He discusses his work at Communications Supplementary Activities-Washington and the events leading to the formation of Engineering Research Associates (ERA). Topics include: the introduction of John E. Parker to Howard Engstrom, Ralph Meader, and Norris by the financial brokerage firm of Auchincloss, Redpath, and Parker, the work of John Howard, the level of Navy involvement with ERA, research activities, especially regarding memory devices, and the management of the firm. Duncan reviews the management of the ERA and Eckert-Mauchly acquisitions by Remington Rand. He concludes with a comparison of the management techniques of Remington Rand and International Business Machines.

NOTE: Interference generated by the tape recorder caused two gaps of several minutes duration.

HUGH DUNCAN INTERVIEW

DATE: 14 August 1986

INTERVIEWER: Arthur L. Norberg

LOCATION: Del Mar, CA

NORBERG: One of the things that we had done [at Berkeley] is we gathered sociological information on people like yourself who have been involved in these events and so out of these 75 or so interviews that I mentioned, there's quite a lot of information on the background of people that is not available elsewhere. So can we start there? Can you tell me something about your background?

DUNCAN: Yes. I graduated from Stanford in 1940 and got an Engineer's Degree in 1942, which is too complicated probably to go into. Let me say a master's degree because it's simpler...

NORBERG: Yes, I know about the five-year degree.

DUNCAN: Well, no, that's just the problem. At Stanford, at the time I went, they gave no master's degree in engineering. You could get an engineer's degree, a degree of engineer they called it; E.E., M.E. or whatever, which required two years of graduate residence and a thesis. Since then they have gone to master's degree, but... So I go around life telling everybody I have a master's degree because it's easier than getting into an explanation.

NORBERG: Had you been living in California before you went to Stanford?

DUNCAN: No, I grew up in Washington D.C. Went to high school there.

NORBERG: When were you born?

DUNCAN: 1919.

NORBERG: What year were you coming out of Stanford with the Engineer's Degree?

DUNCAN: '42.

NORBERG: '42. Had you worked with Fred Terman or with someone else on the faculty?

DUNCAN: Actually, Carl Spangenberg, who's now dead, was my faculty advisor.

NORBERG: What sort of work had you been doing under Spangenberg?

DUNCAN: Well, in those days, you know, the klystron had just come into being and there were a whole host of useful things at about the level a graduate student of that kind could do. Mine was on looking for means of providing termination of coaxial lines. And a particular technique that I looked at was cathode sputtering, if you're familiar with that, which originally was used, as you probably know, for coating mirrors, high-quality mirrors. So that's what I did.

NORBERG: Who else was around at the time?

DUNCAN: A guy by the name of Bob Muchmore who later became, stayed with... Well, let's see, go back to the beginning. When I was an undergraduate, the graduate students who served as lab assistants included Joe Pettit, who is now at Georgia Tech, if he hasn't retired, and Ginzton, Ed Ginzton, who, I guess, has retired.

NORBERG: Was John Woodward still around?

DUNCAN: I didn't know of him if he was. One of my roommates was a guy by the name of Bill Kayes who just retired as Dean of Engineering. He was a roommate for a year.

NORBERG: As you were coming out of Stanford in 1942, what did you plan to do?

DUNCAN: What I did was go to work for Sylvania, got a job with Sylvania. Then after a year I decided I wanted to go into the Navy, and was able to do that in spite of rather poor eyes, because I had a contact in the Navy who had asked for me. And that's how I got to the outfit on Nebraska Avenue.

NORBERG: Who was the friend?

DUNCAN: A guy by the name of Real Admiral Earl E. Stone, who, at that time, was head of the Navy communications annex. As a lieutenant commander. in the early middle '30s, he had lived next door to us.

[Recording Interference - 6 Minutes]

DUNCAN: That group was a small department under a somewhat larger department headed by a man by the name of John Howard, are you familiar with him? Do you know Vannevar Bush of MIT who developed high-speed photographic techniques which were used before the war and during the war by this organization. And his contact in the Navy before the war had been primarily a Lieutenant Joe Wenger, who intermittently went to sea so his career wouldn't be destroyed and came back to work in this area. Another man who was called into the Navy (I suspect at Wenger's behest) was Howard Engstrom who was associate professor, I guess, at Yale and who had written a paper on cryptanalysis techniques, the mathematical basis for techniques. He, in my opinion, was responsible the quality of the staff that got assembled primarily in the mathematical areas. He had a number of engineering type people the majority of whom came from places like IBM, which is exactly where you'd expect such people to come from. I didn't know anything about IBM when I arrived. There were a few younger types like me and like George Smith -- I don't know if you had heard of that name?

NORBERG: No.

DUNCAN: Well, George Smith was a graduate of CalTech and he was assigned to this organization. He worked there and at ERA for a year or two after the war and then left to go back to CalTech, got his Ph.D. there. The last I heard was one of the vice presidents of research at Hughes. I'm not giving this in a very orderly fashion, just as the thoughts...

NORBERG: That's all right. Go along your way, that's fine.

DUNCAN: At any rate, to get back to my own rather modest contributions or at least my experiences, let me put it that way. I worked for this research group and I would say it's really a development group, because what we did primarily was to liaise between Washington and the work going on at National Cash Register on new types of machines. My particular responsibility was a variation on the standard machines which involved statistics, rather than simple coincidence. And so I went to Dayton to National Cash for a period of several weeks to work on the final tests of that machine, brought it back, wrote the instruction books for it, and trained people to run it and followed it. Shortly after that the war was over and then it all became academic. I stayed after the war was over because Jean (my wife) was expecting a baby and I thought that I'd better stick around until the baby got a few months old before I went off to ERA and I'll talk about that in a moment.

NORBERG: Fine.

DUNCAN: John Howard, as he left, asked if I would take over and organize his files, the files that were left in his office. And that was what I did in the rather quiet days in early 1946. I also wrote some job descriptions. A guy from Civil Service came in and wanted job descriptions for the new civilian organization. I found interesting correspondence in the files like the letter from Vannevar Bush (to CNO maybe) complaining about the fact that Lieutenant Wenger got yanked off this job which was just getting interesting and sent off to sea. The answer was it will kill his career unless he does that.

NORBERG: Who else was in the research group besides you and Engstrom?

DUNCAN: Well, you're putting me in exalted company, but... Engstrom headed both mathematical and, what shall I say, technical research, I guess, or engineering research. Mathematical research group that he built, and I started talking about him and how he built it up, included a whole host of young, relatively young (they were probably in their 30s most of them), assistant professors of mathematics from a variety of schools. George Kramer from Tulane, a more renowned person was a guy by the name of Donald Menzel, who was an astronomer for Harvard and later went back to Harvard Observatory as director. His job was primarily in the area of -- and I don't think this is classified -- of determining optimum -- are you familiar with MUF, and LUF techniques? You know, those were the days when most communications were high-frequency and part of the problem of high frequencies was the problem of the skip distance. MUF is the maximum useful frequency; LUF is the lowest useful frequency for a given distance of transmission. That was the area in which he worked, establishing MVFs, LUFs, etc. based on ionospheric observations. The others, I was going to name some of these people; George Kramer, who later went to work for ERA and then to IBM and retired. In fact, he worked for me a little while at the tail end of his career at IBM. A guy by the name of Clifford, who was at some university in the South. A guy by the name of Marshall Hall who later became a professor of some prominence at CalTech. Charlie Tompkins, whom you're familiar with, who had been at Maryland. He was very active and I think very instrumental in ERA, but in terms of mathematical talent, as far as I could tell, he was probably not in the class of some of these other people. And I say that with a great deal of humility because I'm not sure that I can judge that level of mathematical competence. Another one was Gleason, what the heck was his first name? In fact we worked together on the statistical machine. Andy Gleason, who was probably the most brilliant of the group.

NORBERG: Andy Gleason. Right.

DUNCAN: Lou Tordella was one of the group, and later became deputy director of the agency. So Engstrom put all these people together and he, I think, was able to attract them because of his own stature.

NORBERG: You didn't mention Norris in that group?

DUNCAN: Norris was in a third arm which had to do with setting up the intercept sites. He was the manager of a smallish group which did that. Among other people who you probably have interviewed or will, Joe Walsh, is one of them and he spent much of his career out in such great places as Adak putting up intercept sites. But Bill's job was on that end, receivers, antennas, recorders. In terms of rank, he was among the junior of the management people. I believe when I met him he was a lieutenant and Engstrom was a commander and soon became a captain. Another member of all this was, of course, Meader, Ralph Meader, who was purely and simply a promoter. We had been put in charge of the Navy group in residence at National Cash Register. Norris had a number of young people there in his group, several of whom joined ERA. The guy who later headed up Ampex and before that was at NSA was...

NORBERG: Arthur Hausman.

DUNCAN: ... Art Hausman, who worked for Norris and was just out of college. There were one or two others whose names totally escape me now. E.E. Richardson, I think is one of them. Well, the original ERA structure, as you doubtless know, consisted of a triumvirate of Engstrom, Meader, and Norris, who had the key vice presidential positions in ERA, and John E. Parker. Now I know something about all that because I was something of a spear carrier in the early days of ERA's formation in the sense that I took notes when the formative planning group had meetings. And I suppose I should back-up now and talk about why ERA was formed. The Navy could see that the rapidly burgeoning electronics business with its various high speed techniques were necessary to their equipment following the war. I'm sure the Navy was eminently proper and circumspect but I strongly suspect that there was at least tacit encouragement that someone start a company. It would be hard for me to tell exactly who the driver was. Certainly the most prominent person was Engstrom. He was the name around which it evolved. How that triumvirate of Meader, Engstrom, and Norris, got in touch with John E. Parker to my knowledge is through a financial brokerage firm which was somebody Redpath, Parker.

NORBERG: Auchincloss.

DUNCAN: Auchincloss, Redpath, and Parker. Different Parker, merely coincidence. They apparently knew John E. Parker and I can remember going to the first get together of John E. Parker and these other people. There were probably half a dozen of us or more. I was there as certainly the most junior member. I'm sure Tompkins was there, Larry Steinhart about whom undoubtedly you've heard, was there. John Howard may or may not have been there; it was likely that he was there. And then Meader, Engstrom, and Norris.

NORBERG: Do you recall when that meeting took place?

DUNCAN: How close do you want it?

NORBERG: Well, as close as you can get it, because I don't know exactly when Parker met with these people.

DUNCAN: I'd say February, March, February maybe of 1946. Maybe as late as March. I was still trying to decide what to do myself then. My former manager at Sylvania came down to offer me a job. I ended up trying to talk my boss into going to ERA at that time. That must have been about April. So I would have made up my mind at that time, and there was clearly an ERA at that time so it was probably in February.

NORBERG: Can we drop back just a couple of minutes. You mentioned the meeting with Parker and the various Navy personnel. How many earlier meetings had occurred that you participated in?

DUNCAN: The earlier meetings were all internal which were talking about getting people, talking about setting up a rough salary structure and so forth.

NORBERG: Do you remember when those occurred?

DUNCAN: Those occurred probably within a few weeks of that time, because...

NORBERG: I see.

DUNCAN: I'm sure many earlier discussions took place. By the time it got formal enough to have somebody who would write the minutes, which is what I did, it was pretty well along. So I'd say all this took place in the February, March time period. And earlier whether somebody at Auchincloss, Redpath, and Parker had said we think we can find somebody who's interested and that was enough to set it off or whether the name of Parker was already there, I couldn't say.

NORBERG: So you had no earlier knowledge about what this group was trying to do in terms of raising financial support?

DUNCAN: No. I have the suspicion that Engstrom felt that he should remain somewhat aloof from all of that activity and so probably the people who provided the thrust and energy at that time were Tompkins, Steinhart, and perhaps Norris, although I'm not sure. Maybe even Meader.

NORBERG: Do you have any idea why they involved you at this time when John Parker was being approached?

DUNCAN: No, I don't. Other than that I was apparently someone who they thought would be a good candidate to work at ERA. I had by this time taken over the remains of John Howard's and since I could write reasonably well, seemed to be able to organize the files reasonably well. Those may have been possible reasons, or just maybe because they were lonesome, who knows.

NORBERG: I wouldn't quite go so far as to agree with that.

TAPE 1/SIDE 2

NORBERG: Now you were going to go back to a discussion of why ERA was formed.

DUNCAN: Very simply, I think that the Navy would have been happy to continue its relationship with National Cash Register. National Cash Register decided there was no future in that business and wanted to get back to making cash registers. So they made it quite clear that they were no longer interested in that business. And that certainly led the Navy to cast about. What was said, of course, I wasn't privy to. It certainly would not be said at general meetings. Wenger may have talked to Engstrom. These are pure conjectures. The Navy undoubtedly made it known that they would be very receptive to any proposal of a responsible set of people.

NORBERG: Well, it turns out that the Navy produced a document in what I estimate to be August of 1946 from the internal statements. It can't be earlier than that, it could have been later, but it seems it was not. At Wenger's behest, this document was written so as to illustrate the Navy's involvement, or lack thereof, in the formation of ERA. And both of those things are parallel, of course. The Navy was doing a certain number of things to encourage this development, but at the same time, they were keeping hands off certain aspects of it so as not to be accused later on of any sort of wrong doing. Now in that document, it is reported and, since there is no author on this I don't know who wrote it, all I can say is that the people who are mentioned must have approved what was said, it is reported that it is, it was Engstrom and Meader who were the two people who were asked to consider this problem of what the Navy should do after the war.

DUNCAN: That sounds exactly like the way they would have put it.

NORBERG: Now that is February of '45, though. Not as late as February of '46.

DUNCAN: Yes.

NORBERG: And in fact, there seems to have been some coupling between that event, the Navy decided what they were going to do about continued development of this type of machinery that they felt was necessary and Wakelin's

task, in CNO, to evaluate the various projects for the Applied Mathematics Panel of NDRC. And that he called to the attention of these people, he claims, that's not clear as to whether that's true or not, but anyway -- who knew what is always hard to determine after the fact -- but anyway, he called to their attention, he believes, the projects at Harvard on the Mark I and ENIAC at Pennsylvania and so on. And that those things all came together in a bit of a constellation around February of 1945 to stimulate the Navy to think that more needed to be done than they could do as a unit within the intelligence service. And so that lends credibility to what...

DUNCAN: That sounds quite reasonable, but it seems a bit early.

NORBERG: Now, let me give you one more piece of information. I don't normally give out this much in an interview because I'm trying to get as much as I can from the person across the table from me. In talking to Norris recently, Bill commented that Meader was brought into this situation so as to keep things clear with the Bureau of Ships so that they later on couldn't object. They would have a piece of ownership well in advance of any formal approval being necessary by the Navy. Now that doesn't jive with this historical document. Because the historical document suggests that Meader and Engstrom were in this from the beginning, rather than Meader being brought in later on. Now, that's not necessarily inconsistent. He could have been brought in at the very beginning because of the Bureau of Ships interest, I don't know. Now at the same time that these things were being developed in February of '45, that is, some sort of position statement being developed within CSAW, there was a proposal developed, which I have, I have a copy of the proposal, and as best we can determine it was done in February of '45, a proposal which not only said what this new company would do, but what sort of products it might develop over the course of a few years. Though everybody seemed to understand that a lot of research would be necessary. That document was peddled around to a variety of companies, American Airlines, Socony-Vacuum, and so on -- to try and get some financial interest. You don't remember any of that? You had no part in any of that?

DUNCAN: In February of '45, I'm sure my position was too lowly to have been.

NORBERG: What do you know about Meader's activity before the war? Do you know anything about Meader at all?

DUNCAN: Other than that he was a member of the Bureau of Ships, nothing.

NORBERG: His son told Arnold Cohen, not me, that Meader had some patents on counting equipment that he had developed before the war.

DUNCAN: That Meader had patents I can easily believe. That he created them, I doubt.

NORBERG: That's consistent with what other people have told me. All right, so getting back, then, to early '46 and your decision to go with ERA. Do you recall why you decided to go with ERA?

DUNCAN: Money.

NORBERG: Why didn't you go back to Sylvania?

DUNCAN: Well, because ERA offered me more money than Sylvania did and I was interested in the kinds of things ERA was going to do. I didn't want to go back to hands-on microwave stuff. That was fine, but everybody was doing that. This looked like something new. I remember seeing Terman at the IRE convention in January of 1946. He asked me what I was going to do and if I was going back to Sylvania. He kept up with everybody. I said, "Well, there are some other interesting things." And he said yes, there are a lot of interesting things around now.

NORBERG: You were still in the Navy, though, at that meeting.

DUNCAN: Oh yes. I didn't leave the Navy until May, June, I guess, of '46.

NORBERG: Did you move to St. Paul right away?

DUNCAN: Yes.

NORBERG: And what did you find when you arrived in St. Paul?

DUNCAN: Well, let me get back to Parker, okay?

NORBERG: Certainly, yes.

DUNCAN: The first meeting of Parker and those who were interested in forming ERA was arranged through the auspices of Auchincloss, Redpath, and Parker, because the other Parker was at the meeting also. We had lunch at the Metropolitan Club, the one diagonally across from the Transportation Building on 17th street. As far as money goes, what Parker had to offer, (perhaps Norris can shed more light on that) and has... My impression was that what he was offering was a plant which he rented from the Army Air Force -- the plant where he was making the gliders, and very little else. I don't think he put much money into it. Now, clearly, in the very early days -- and Joe Walsh would be a much better source of this information than I since he was the controller in those days -- the company must have gotten something in the way of progress payments from the Navy, and they borrowed money from the First National Bank of St. Paul, from Lilly.

NORBERG: That's correct. Richard Lilly.

DUNCAN: I remember Joe exultant when we got to the point where we didn't have to borrow money every month but only every few months.

When I got to ERA there were a set of maybe eight or ten tasks, which the Navy had formulated, and the Navy in this case probably was primarily Joe Eachus. And I went to work on one of the researchy catch-all ones to look at various ways of developing more modern techniques than gas tubes for digital counting and storage. Looked at counting tubes, special designing and configuring special counting tubes. I even looked at magnetic cores, can you

believe? This was before anyone had used magnetic cores for this purpose. I saw an article in the *Bell Labs Technical Journal* about magnetic cores which had very rectangular hysteresis characteristics, and which Bell had used as modulators for tank transmitters, or tank receivers. That looked like a natural thing to me for storage, so I wrote BTL to see if any cores were available and they sent me a few. They were wrapped on ceramic cylinders and were very thin -- perhaps one or two mils per wrap. I assigned Al Fenaughty to develop circuits to try these cores out. And we rejected cores because while the transfer of energy from during transition was longer lasting than the spike of energy created by "transformer effect" when driving cores further into the saturated state both provided a large signal output. We didn't follow it to the next obvious stage which is to gate out the spike using a transition gate. So the glory properly belongs to MIT and the Whirlwind people.

NORBERG: I'd like to drop back, though, one more minute, because the actual incorporation of the company is January 8, 1946.

DUNCAN: Then the meetings must have taken place in late '45. I must be off. Because I believe that the meetings I described -- at least some of them -- took place before the incorporation.

NORBERG: Yes, I think that's right.

DUNCAN: That wouldn't surprise me. My memory isn't that good.

NORBERG: When was your child born?

DUNCAN: In April of '46.

NORBERG: Did you take any trips around Christmas time that you recall? Did you go back to visit your parents or relatives?

DUNCAN: My parents lived in Washington.

NORBERG: Your parents lived in Washington, so there's no necessity for that. Okay. I'm looking for things that you can pin events onto and therefore probably recall when these things occurred.

DUNCAN: Well, they could easily have occurred anytime after August or September, because that was about the time that the war-related work I'd been doing was no longer relevant. The only thing I remember exciting during that period was setting up a demonstration for Admiral Nimitz. We set up a machine demonstration for him when he came visiting NCA. This machine was one where the results were typed out. I won't elaborate any more on that. We'd set it up so it would run for perhaps 45 seconds or a minute before it would stop and type out. And Nimitz came stalking in, you know, a little guy in all those stripes. Absolutely a passive stone face, piercing blue eyes, and he stomped to a stop in front of the machine where he was supposed to stand and all the twittering captains (whom I had looked at as being the final authorities), behind him, wringing their hands. So I pushed the button and the thing ground for, well, 45 seconds, which seemed an eternity. Engstrom came over and he said, "Are you sure it will work?" All I could say is, "I hope so, sir." And it did. It stopped and it printed out in English the Donitz capitulation, which showed it was a fake, but I don't know that Nimitz cared. Nimitz was appreciative of what this sort of thing had done during the war. You have Layton's book, I assume?

NORBERG: But if you say he would know then that this message was a fake, this had to have been after the end of the war.

DUNCAN: No, I said I knew it was a fake because it was in English. He may have known so also. This was well after the end of the war, perhaps two months.

NORBERG: Well, it would be easy to date when Nimitz returned to Washington, that's not going to be difficult, at least as a front end date before which he could not have been there.

DUNCAN: All I can remember is it seemed to me the weather was reasonably mild when we went to the Metropolitan

Club and that could easily have been October or November. In fact, in Washington, it could easily be any time.

NORBERG: That's true. But you also mentioned along the way that there had been discussions in which you were present in terms of salary structure and divisions of responsibility perhaps within the company and so on.

DUNCAN: All right. I'd like to tell you when that took place, but I can't.

NORBERG: All right. I know that some documents were prepared as early as September 1945 in which there was a description of what this engineering group was going to be, that is, the technical founding group was going to be, what their responsibilities would be, and what they were going to contribute to the company.

DUNCAN: Well, then it was probably fairly shortly after that, because the next logical step would be to say who can we get among people that are here, who can we get to stay and how much should we pay them.

NORBERG: One last question on that, again, just to see whether we can elicit any response that you haven't thought of yourself. Do you recall any discussions outside of CSAW with other Navy officers? And I mean offices CES, not ERS. For example, the Secretary of the Navy's office, any of those other places. You don't. Okay, then let's just leave that. All right. Then you arrived in...

DUNCAN: But there may have been such a document, you know. You titillate my memory, but... And I have this dim recollection of Engstrom mentioning this, the effort of ERA -- since I was probably subjected to some sales talk --was supported as far up as the Secretary of the Navy. But this is not well enough remembered to give you any feeling of comfort.

NORBERG: When you arrived in St. Paul, who did you find there at that point?

DUNCAN: Well, let me give you the structure. I believe Engstrom was in charge of engineering. Norris later, I

guess, took over engineering and Engstrom later took over research and development or something of that sort. So let me say for the purpose of this that it was Engstrom, who was not living there and never lived there. He and Parker would come out and stay at the Commodore Hotel.

NORBERG: So Parker was living at the Commodore.

DUNCAN: Not living. They'd come out for a week. ERA was run on a day-to-day basis by people like Walsh on the financial side, John Howard who was director of development, and then miscellaneous people who were hangovers from Parker's old days, such as Bertil Lindquist and Chuck Allen.

NORBERG: I've heard the tales, yes.

DUNCAN: Basically the development people, Navy support people, were under John Howard and these other things were support activities or ventures independent of the Navy. Gutterman and Daniels who were largely trying to promote things outside of the standard Navy program.

NORBERG: Was Rubens there when you arrived?

DUNCAN: He came about the same time. He and Ammerman started working on some kind of a magnetic recording scheme. They had a rotating piece of metal with magnetic tape pasted on it. Don Ammerman worked for Rubino. I was asked to do research things until a project whose number, I believe, was eight came up and I headed that as well.

NORBERG: This device that Rubens was working on, had you ever seen that before during the time you were at CSAW?

DUNCAN: No.

NORBERG: In fact, it came from NCML. Ralph Palmer from IBM had been working on it before.

DUNCAN: I knew him well. That wouldn't surprise me.

NORBERG: Now Rubens arrived in July of '46.

DUNCAN: So I must have arrived at about the same time.

NORBERG: And that's very well dated. That's not from his recollection, we can date that.

DUNCAN: Well, and I must have arrived about the same time. Who else was there? I believe Cliff Olofson had come by that time and George Roenning.

NORBERG: Who was that again?

DUNCAN: George Roenning.

NORBERG: Roenning, yes. Now what were these people doing, do you recall? We know what Rubens was doing, you just said that, but how about the others?

DUNCAN: Almost all of the people that I'm mentioning, except where I specifically said otherwise, were working on various Navy tasks. Roenning was working with a couple of other people, Moe might have been one of them, although I'm not sure of that, on some kind of a relay device. Bear in mind that the IBM electric typewriter was in being at that time, which I'm sure you know. It was then called Electromatic. It was widely used at the Navy place and these may have been attachments to the typewriters for various special purposes.

NORBERG: To do what with it?

DUNCAN: Gosh I don't remember anymore, but you know it could have a rack of these relays for some specific cryptanalytic test because everything was specific then. It was computer specific. The first general purpose computer of any significance was Goldberg as far as ERA is concerned. In fact, the equipment that I managed and delivered, which was indeed the first machine of any significance that ERA had done was computer specific. I think the timing was good because people were getting very nervous. By that time (it was February '49) nothing had come out of all the money the Navy had been pumping in except little routine jobs. I can't talk about what it addressed, but it was electronic. It used counter circuits, which were essentially the ENIAC circuits.

TAPE 2/SIDE 1

DUNCAN: The next machine I did multiplied and summed information.

NORBERG: How did we jump to the second machine and as late as 1949? We were talking about the middle of 1946 with a group of people. You mentioned earlier that you had done the short project at least on the magnetic core materials, magnetic materials, which had led into what seemed to be, at that time at least, a dead end at ERA. What was the specific task at that point that you were trying to solve?

DUNCAN: These were primarily research tasks, look at ways to create memory. They were tasks in which we could define what we wanted to do and if the Navy approved them as being useful, something that they thought would be a useful or potentially useful product, they would approve it, fairly informally I suspect, through the office there in St. Paul.

NORBERG: Was it a coordinated research program, do you think?

DUNCAN: Coordinated in what sense?

NORBERG: In the sense that there were a half a dozen different kinds of memory ideas being investigated at the same time.

DUNCAN: Well, the Navy was certainly aware of the different things that were going on. Coordinated? Probably not at that stage in the sense of being directed to an end product or products.

NORBERG: My sense of coordination here is that many of these early tasks, at least, follow directly from things that had been going on at CSAW before.

DUNCAN: It is possible. The magnetic tape, from my point of view, I hadn't seen anywhere. I make no claim that it necessarily was the first thought in that regard. Later, people like Stifler and others got into the magnetic core business, but that was much later. By that time Whirlwind had shown the feasibility. I remember doing some work in conceiving the configuration of a tube which might count, and did a rubber diaphragm study. I don't know if you're familiar with all this kind of stuff.

NORBERG: No, I'm not so please go ahead.

DUNCAN: Well, in those days, the typical way to study characteristics of a vacuum tube was to make a mechanical analog. How would you do this? You would make the analog by using a heavy rubber diaphragm perhaps the size of this table stretched taught...

NORBERG: Okay, four feet wide.

DUNCAN: ...and use sticks of various kinds which you punched up and it deformed, and the deformation was not a bad synthesis of the potential gradients in such a thing. And you could drop ball bearings, which were the electrons, to see where they rolled. The idea, and this one was for a switchable counting tube which counted to ten. My

sophistication was not at the binary level in those very early days, that was fairly obvious fairly quickly.

NORBERG: What happened to that project on the tube?

DUNCAN: Nothing.

NORBERG: Nothing.

DUNCAN: It looked like something you could do, but nobody was really wild about building a brand new tube either, even me. And it seemed to me there ought to be better ways. And the magnetic drum probably was getting along well enough, although that must have been a year after, nine months to a year perhaps, after the early work of Sid Rubens had any substance. The drum itself evolved and techniques were developed such as coating the drum using cathode coating techniques.

NORBERG: Yes. Okay. Did those projects that you worked on, the magnetic materials, the tube, etc., happen serially or were they going on at the same time?

DUNCAN: In parallel. And my researchy kind of thing tapered off because a new project came in, probably early in '47, which was one I mentioned: the machinery delivered in early '49 to do a specific job.

NORBERG: Who did you work closely with while you were doing these projects on both ends -- the people below you and the people above you?

DUNCAN: Well, John Howard was the guy I worked for. He was director of development and my title was project supervisor. He was the one I worked most closely with. As we got into projects like the one I've mentioned starting in '47, actually I worked most closely with Joe Eachus, because he was the one who conceived of what he needed.

NORBERG: What project was that? Was that Goldberg actually, or was it something else?

DUNCAN: No, it's number was eight.

NORBERG: Eight doesn't ring a bell with me because there were several. Is that 1108 or N-1108?

DUNCAN: No, it's task eight.

NORBERG: Oh, all right. I can go back over them.

DUNCAN: Goldberg was nine.

NORBERG: Nine. That's right.

DUNCAN: I will give you the title: Hecate.

NORBERG: Oh, right. I don't know much about the specifics of Hecate, but I recognize the name.

DUNCAN: I won't tell you any.

NORBERG: That's fine, I can find out from the Navy. What sort of person was Howard to work for? How did he interact with you is the point?

DUNCAN: It was somewhat laissez-faire management, both under Howard and then later under John Coombs, whether because I was doing what I was supposed to or whatever reason, there was very little direct supervision. I didn't feel that I needed technical advice from these guys, so I pretty much worked on my own. The biggest decision I remember having to make was in the case of this project Hecate.

NORBERG: Go ahead, what?

DUNCAN: A great many diodes were required. And in those days, germanium diodes were, to say the least, unreliable. And there were no silicon diodes yet. Leftover from special tubes that were built during the war for NCR was a great reserve stock of 4-element diodes, that's four diode elements in a single tube. And since they weren't being made any more even though were plenty around, what you do? Do you say, well we're going to go for it and use these? Certainly in terms of space and simplicity of building that's the most desirable thing. So I went to Norris and asked him whether he felt I should use these or not. And he said, "What do you recommend?" I said, well, I think we ought to take a chance and use them. I think they were called GANG's or something. He is supposed to have said after I walked out of this office, "What's a GANG?" That's all right, you know. Norris would have backed me; he was a man with guts. Roles could easily have been reversed. If I would have been the superior, I probably would have asked a few more questions.

NORBERG: Now do you recall ever coming into Howard's office and saying I'm having a problem with this and I'm thinking about this and that and so on, and what's your reaction to this?

DUNCAN: The development job I thought was quite straightforward. Flip-flops for counters had been developed and they were operating quite successfully in the ENIAC machine. I knew what I needed to do. I used IBM plug-boards, which were perfectly okay; the Navy had a source of them. IBM itself was dubious of the quality of the insulator in the plug-boards. I'm just tossing these out as things I remember.

NORBERG: Go ahead.

DUNCAN: The basic operating rate of this thing was around a 100kc, a 100,000 pulses per second. And IBM itself was rather dubious that the insulation would stand up at that frequency. But I was an old ham and knew from experience that 100kc was so low that the losses would be trivial. I'm sure I talked to John about various things, but I

can't remember any things substantive now.

NORBERG: Okay. How about the people below you. Who were they and what were their capabilities?

DUNCAN: There were a couple of engineers, two or three engineers, and probably half a dozen technicians. One of the engineers was a guy by the name of Greenwood, who had been a friend of George Smith's at CalTech and had been looking for a job after the war and he did just about what George did. He came out to ERA and worked for perhaps a year or two. And then went back to CalTech to get a doctor's degree, which in his case, is in mechanical engineering. And then he taught at Michigan, University of Michigan, for the rest of his career as far as I know. But he worked for me, a smart guy and did some of the circuit analysis, although what was required was not too profound. A young guy ERA hired off the street, whose name escapes me now, did some transient analysis -- and then a number of technicians whose names I can't remember. I think our greatest weakness was that I had no mechanical engineer and used a technician. His selection of the angle irons that we used in the frame was probably not big enough. The frame sagged slightly in the middle. So later I got a mechanical engineer who did a quick study and then he put in a pad - a lift - in the center and that took care of that.

NORBERG: Had you ever turned to anybody like Jack Hill who certainly would have had some mechanical engineering ability?

DUNCAN: No. You know, the problem was not that profound. The problem came later when we started loading the chassis. By that time, I had a mechanical engineer. The fix it was quite easy.

NORBERG: How did ERA change over those early years from the time you arrived in mid-'46 through to say '49?

DUNCAN: Well, I have to give it, you know, tell you in terms of my own experience, and that's very personally oriented. The John Howard affair came and went, which was fairly disturbing, but...

NORBERG: What were the details of the Howard affair?

DUNCAN: Well, he decided, I'm sure you've been told about this before.

NORBERG: I have. Now I want to hear your side.

DUNCAN: He decided that John Parker was a bad guy and not doing the right thing for ERA and he got a bunch of the guys who worked for him together and went through this. I was quite impressed, and I think, in retrospect, naively so.

NORBERG: Impressed with Parker or with Howard?

DUNCAN: With what Howard had to say, naively so. It led to naught. I remember going down to the Navy Bureau of Ships and being lectured by them at that point. They made it clear that they were not entering this fray in any way, which seemed a perfectly reasonable position to take. John eventually was invited out. He went through a period where he had psychiatric care. Later, that happened to him again. Interestingly enough, I'm just following this thread because I'll forget it otherwise. John continued his ways, I think. He got mad at his bosses in various places, or he didn't like them. He went through another period in the late '50s, early '60s where he went to a psychiatric treatment place. I saw him in 1960 after I joined IBM. He was working for Burroughs then, I believe.

NORBERG: Do you remember what his arguments were about Parker's management techniques?

DUNCAN: The ones I remember now were sufficiently trivial to be embarrassing. Essentially what he was saying was that Parker was spending money to aggrandize himself with fairly trivial things, like private bathroom with towels in it, and perhaps a lot of traveling. Nothing which in retrospect, I would defend. I was very fond of John and respected him technically, and I suppose that influenced me heavily.

NORBERG: This is John Howard we're talking about.

DUNCAN: Yes. I did not find him, as I became a manager, very sympathetic person, perhaps but that's why I didn't bother to go see him.

NORBERG: Did the Howard affair have a substantial impact on ERA for a short time?

DUNCAN: I don't believe so, really.

NORBERG: So it didn't disrupt things at all.

DUNCAN: I think there was a lot of handwringing perhaps in the upper levels of the Navy because John was well enough known, for example at MIT, so that people could ask questions. Other than that, my impression is no.

NORBERG: Then let's go back to what you were saying...

DUNCAN: When he was replaced by John Coombs. I believe we were very good friends. Coombs let me be because I didn't get into any trouble. But I could always go in and talk to him and in general my recollection is talking to him more about personnel problems than technical problems. The next project I completed was one which had the name O'Malley. You can look up the number on it. Essentially, this was a multiplying machine. It's interesting to me, because essentially it was multiplying and summing machine. And we interfaced with some IBM equipment, essentially the printer from a tabulator, which was probably the 407, and a card reader. After the card reader came the multiplying gear of ours and its output went to the 407 printer. Shortly after we delivered that, which was in '49, IBM came out with the 604 which would have done the job perfectly well. IBM was being more secretive than they are now. They're secretive now only to the extent that they won't be accused to making life unnecessarily unfair to competition. At any rate, it was quite a different company in those days. And I worked with Rowley from IBM who had been my immediate boss in the Navy during the war. The work on O'Malley was done through triple C,

Commercial Controls Corp. in, I believe, Rochester, New York, which was a subsidiary of IBM. And the Bureau of the Ships liaison was John A. Skinner.

NORBERG: Okay. You were talking about the changes in the company over these years.

TAPE 2/SIDE 2

[Recording Interference - 12 Minutes]

DUNCAN: The three laboratories had a rotating meeting in each of the three places. The theory of these meetings was that if you mix these guys together they'll agree on technical approaches by method agreement. Unfortunately this runs counter to human nature. No one in charge had the technical background and drive to force decisions. Saddled with these three outfits, what does Norris do? Does he spend all of his time negotiating and putting out fires, because Eckert could make four more fires in a week than Bill could put out in ten years. So I could empathize with him. His solution was to create committees. Well, okay, that keeps them off his back, but does not produce solutions.

NORBERG: Why didn't you leave earlier then?

DUNCAN: I ask myself that. I suppose because when Norris called in Sorenson and Sorenson called on me, I thought I'd see what I'd do and the answer was not much.

NORBERG: So how did the offer to go to IBM come about?

DUNCAN: I asked them, IBM, because I was reasonably sure of getting a positive response. The timing turned out to be good because they had a project coming up for which they wanted to put my name in as the engineering manager.

NORBERG: Which project?

DUNCAN: 466L, which was an Air Force proposal. And guess what -- field, intelligence.

NORBERG: Yes, that's not a surprise, actually. Now, back again though I don't want to move into the IBM years, at least not yet. I'm still a little puzzled at thinking about Remington-Rand and the various types of managers that they had available. Groves doesn't strike me as the kind of person who could come in and run a laboratory like Norwalk anyway. It just seems to me to be a bad choice. Now I know you were not part of the choice, so you haven't any idea why he was chosen.

DUNCAN: He was there when I came.

NORBERG: That's right, yes. You have no idea why he was put in that position. But why he was in it for so long is very striking. Who was he relying on? Who was he relying on to back him up?

DUNCAN: I have seen all of these people, but I'm not privy to the day-to-day work at Rockledge, which is where they all had their office, as you probably know. I would have to say that by and large what it was Rand and the seven dwarfs. Rand ran the company. He achieved success by buying smaller concerns and attaching them to Remington Rand as he did Eckert-Mauchly and ERA. My impression of A. M. Ross is that I never saw his hand in anything. I think what those people did was largely to try to decide which of these groups they were going to bet on, that was my impression. And I think Ross decided he was going to bet on Eckert-Mauchly -- good choice as compared to the Norwalk Laboratories. He hadn't had much luck with that over the years and its predecessor in Brooklyn was essentially the same people, moved. This was before ERA. Then when ERA came in, I don't think he knew what to do.

NORBERG: What was your interaction with ERA after? We talked about the interaction with Eckert and the Eckert-

Mauchly division, how about with ERA division?

DUNCAN: Very little, really, very little is the answer. You know, they were doing their thing, we were stuck with the old-line commercial stuff. Eckert-Mauchly was doing new computers for commercial work. And ERA was essentially a military operation which, if they had a machine that was saleable commercially, they would sell.

NORBERG: Was there ever any discussion about coordinating all of this to get a single machine out there or a string of machines? Any idea why not?

DUNCAN: Because there was nobody that was prepared to do that. I don't think Eckert... The machine would not have been tolerable unless it was Eckert's design, in my opinion. And nobody was strong enough to say...

NORBERG: But why not even use his design then, if that's the case? If what you said about the machine a few minutes ago was correct, then why not adopt that machine and try to coordinate all of these different kinds of products?

DUNCAN: Well, I can only speculate on that. And my speculation is Norwalk wasn't in that business, so they weren't even a factor unless, you know, maybe you wanted them to build punch card equipment to go with it or to build a small scale calculator. That's all they knew how to do. There were probably enough guys there, after all, there were guys that come from ERA and so forth. And they were later successful. So Norwalk really wasn't a factor. But Eckert-Mauchly and St. Paul were two different worlds. St. Paul was primarily motivated by the machines that they could build which were saleable to the military and might have some commercial fallout. And they looked to somebody in Remington-Rand to sell them. Eckert-Mauchly was interested in pushing the frontier. You know, the LARC, which is a computer I mentioned, was pushing a frontier. You know, they'd succeeded because they could sort thousands of transistors and get ones that had selective performance. Good job. And Herman Lukoff, this guy we talked about who wrote *From Dits to Bits* or whatever it was, dits and bits, he made sense out of it, I'm sure. It wasn't Eckert. Not because Eckert is dumb; he's a very brilliant man, but Eckert simply wasn't interested in nuts and

bolts. He had the idea. He created the concept and somebody else implemented -- "I worked out the design now you work out the details." And Lukoff did. And more power to him. I have a high opinion of him.

NORBERG: Wouldn't an effective sales department have helped a good deal?

DUNCAN: Sure, but there was a vacuum there, too.

NORBERG: Why?

DUNCAN: Because in Remington-Rand? Rand ran the company and the others did his bidding. That doesn't make for a kind of management to tackle these difficult jobs, very difficult jobs. If you have two groups, physically removed, who don't like each other very well, who have a different orientation, it's a hell of a tough job just to get anything through to them, but you sure as hell have to know what you're doing.

NORBERG: But the man in charge of sales of electronic computing equipment at that time was John Parker.

DUNCAN: Oh sure. You know, he sold the first two UNIVAC's, you know, at bargain basement prices, but he left shortly after that. He didn't stay long at that job.

NORBERG: It was four years.

DUNCAN: Was it that long?

NORBERG: Yes, '52 to '56.

DUNCAN: Well, it came out I suppose. I guess he sold the 1103.

NORBERG: They sold lots of UNIVACs certainly, UNIVAC Is.

DUNCAN: Yes.

NORBERG: And they sold a credible number of 1103s.

DUNCAN: Well, John was not, nobody ever accused John of being the kind of manager, coordinating manager. What they needed was a product planning department. There was one under the sales department, which was headed by Zettlemeier. In fact, he was one that we interacted with when we were working on this, in Norwalk with this 604 competitor, so called. He was the one who came out and gave Bill Keye the specifications for the File computer. As far as I'm concerned, case is closed. That was a kluge. I don't fault Keye for that. I mean these guys were supposed to know what the hell they were doing, you know. And all they did was look at the machines in the field that were giving them fits and said you want some of that and some of that and let's tag them together and make it work better. A camel. It's very hard to get the kind of coordination that's needed and it requires a great deal of talent.

NORBERG: Can you contrast this, then, this problem we've just been hearing about in connection with Remington-Rand, with what you found when you got to IBM?

DUNCAN: Sure. IBM was used to success. At least the part of Remington-Rand that I was in was used to failure. That's a very over simplified statement, but let me give you another one. My experience at IBM is if someone got into trouble, there was a sea of people that came in to help. The company went all out. Boy, it's going to be fixed. If you wanted the right guy, you got him. The guy who got into trouble, depending on how bad the trouble, might or might not survive in that job, but there would be somebody who would fix whatever needed to be fixed. And at Remington Rand if you got into trouble, there wasn't anybody in the horizon. Everybody went into his bomb shelter. Think about the 360 for which Evans was responsible. Before the 360 there was some of this same sort of thing: Endicott built its machines, Poughkeepsie built its machines. They weren't necessary compatible. Poughkeepsie

tended to build the smaller ones, Endicott the larger ones. And somebody like Evans, who had a great deal of talent and was a tough manager and technically competent, came along and sold management on the idea that we should have a coordinated line which is compatible from top to bottom. As Evans said, "We played you bet your company." He had. There was enough talent in IBM to go off and work on this and come up with a line which did it reasonably well. In turn, implementation is never as good as the idea. And with that as a given, and I think it must be, the 360 was a superb creation.

NORBERG: So you would contrast this difference as being the capability of IBM to have both talent and product.

DUNCAN: There was no Evans, and there was no Evans support??

NORBERG: In Remington-Rand.

DUNCAN: In Remington-Rand. Who was, you know? Sperry didn't know what to do. They had Charlie Greene come over, perhaps if he had lived and he had been a younger and a well man he might have done what was needed. He probably had the kind of talent. I think Norris didn't have the stature to do it.

NORBERG: Did he have the talent?

DUNCAN: Probably. I think you needed more technical talent than Norris had. He had the guts to do it for sure, and he had the drive to do it, but he didn't have the stature or the technical talent. That's my assessment.

NORBERG: So he needed not only to fight Eckert, but there were quite a number of other battles to be fought simultaneously.

DUNCAN: Look at Eckert. Here's a guy, you know, who's Mr. Computer, Franklin Medal, you're going to wipe him out? How are you going to do that?

NORBERG: Well, I would guess and here it is only guessing, you'd have to somehow neutralize him. And I think that's what IBM would have done. They would have fixed the situation so that they could use his ideas, but they would have blocked him from controlling anything else.

DUNCAN: Probably. It could be managed. But I think it needs more stature than Norris had at that time. A great deal more. And in IBM other people don't fight it. I mean, people aren't sabotaging other people. I'm not so sure that was the case in Remington-Rand. I don't know how much of this is actionable that I'm telling you, but that's the way I see it.

NORBERG: No, I don't think any of that is actionable.

DUNCAN: That's the way I see it.

NORBERG: Yes. Let me just finish up by asking you a couple questions about the IBM years. Did you stay in intelligence work throughout the remainder of your career at IBM?

DUNCAN: Not entirely, no. I stayed in the military work. As a matter of fact, it turned out that I had a whole series of projects which involved establishing overseas groups, which were clearly military oriented. So I've seen a lot of the world a number of times. And I enjoyed that, you know.

NORBERG: Yes. But what I was going to ask was if the type of problems, and you don't have to describe the problems, the type of problems that you were working on, were they similar to the type of problems that existed in the late 1940s that you worked on for ERA?

DUNCAN: Much more macroscopic. I was not doing engineering design. I was managing a system. I was not creating hardware in those days. I was using IBM hardware, and other hardware for that matter, and software,

installing, testing... The one -- I don't know if you care about this -- the job in Thailand, for example, we managed a consortium which included Ralph M. Parsons who made the site, cleared the site. Radiation Inc. (which is part of Harris now) did communications interface and storage. 360s were of course IBM, the programming was IBM. We had a test site in England Air Force Base where we tested out this facility. So we had to establish all these, build the buildings, put in power, put in sewage disposal plant, work up operating techniques. That's the kind of thing that I did in the latter days.

NORBERG: So this is an entirely different magnitude, as you said.

DUNCAN: One other job was to go over and try to sell an air traffic control system to the Russians. The system was to be a part of FAA on-route traffic control system in the US, built by IBM. They did not buy it; I believe the price was too steep.

NORBERG: Given your later experiences at IBM, what would you say were the significant aspects of your ERA Remington-Rand days that contributed to your ability to do those projects well?

DUNCAN: Well, two things I suppose. In ERA days I think I learned to stand on my feet, because as long as I did it nobody bothered me. That's probably over simplified. In Remington-Rand days, things were constantly combative and I learned to withstand pressure and people. How to deal with a meeting where people came in to throw rocks at you...

END OF INTERVIEW