An Interview with

ARNOLD A. COHEN

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Conducted by James Ross

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Abstract

Cohen provides information about relations of Engineering Research Associates with the Navy, and with Remington Rand management after their acquisition of ERA. He also describes ERA projects in detail. Specific topics include: early research on magnetic drum storage systems, reports to the National Bureau of Standards, the Atlas I project and the commercial by-product (the 1101), the Atlas II project and the commercial by-product (the 1103), the 1102 built for Arnold Engineering Development Center, the 1104 built for Westinghouse/BOMARC, the Remington Rand Tape-to-Card Converter, the File Computer, ERA non-computer projects, ERA's design contract with IBM and its relation to the IBM 650, UNIVAC II, and patents and their defensive use in litigation.

COHEN: Date of Birth, August 1, 1914, Duluth, Minnesota.

ROSS: Where did you attend high school?

COHEN: Duluth. A year ago we had our fiftieth reunion, class of 1931, Duluth Central High School.

ROSS: Were you attracted to the sciences as early as high school?

COHEN: Yes, and after high school I went to a school that no longer exists because there's a now Duluth branch of

the University of Minnesota there which was not there at the time --Duluth Junior College, a two year school. Junior

colleges in those days were different from the junior colleges or community colleges that you see around today, quite

different. This was definitely the first two years of a college program. People coming out of two years of Duluth

Junior College usually went right into the third year at the University of Minnesota or elsewhere.

ROSS: So from there you went to the University of Minnesota?

COHEN: Yes.

ROSS: And you entered into which program?

COHEN: Electrical engineering was what I took at Duluth and at Minnesota.

ROSS: And you graduated in...

COHEN: 1935.

ROSS: From the University of Minnesota. Did you go through a five year program?

COHEN: No, they didn't have that at that time.

ROSS: How long had you intended to go to college? Can you remember as early as high school knowing that you were preparing for that?

COHEN: Oh, yes. I guess most of the people I associated with had that expectation, even though that was mid-depression.

ROSS: That's a good reason to be thinking of it, though, to strengthen your appeal in the job market.

COHEN: Yes, and going to school in those days was inexpensive. Tuition, and even living away from home when you got to the third and fourth year --you could do that very cheaply, too. Tuition didn't amount to anything, and room and board was cheap. Everything was cheap, even if you didn't have much money.

ROSS: Where did you go after the University of Minnesota? Did you take employment or do post-graduate work?

COHEN: I went into graduate school, majoring in physics, and, although I suppose electrical engineering might have been a little bit more appealing, the kind of program they had here at Minnesota didn't appeal. Applying here and there, the thing that turned up was a teaching assistantship in physics, and that kind of made the decision.

ROSS: After graduate work at the University of Minnesota, what did you do?

COHEN: You jumped through the whole graduate work period. I think it's significant that what I concentrated on was mass spectrometer applications for the doctorate area, but for my master's thesis I worked up a whole monograph on the analysis of beams of charged particles, the principles that are used or are applicable in mass spectrometers, and so forth. That was a pretty good exercise. I learned later that the master's thesis turned out to be a useful reference for graduate students who came along later. My research advisor in the mass spectrometer work was Al Nier. Well, we don't have to go into the details of what that was, except that nobody was talking about digital computers and nobody knew anything about the subject at that time, so that was out of the picture at the time entirely. But I did get experience in building scaling counters, which are binary counters, but we didn't call them binary counters in those days. Things of that nature would later be useful to some extent.

ROSS: And that graduate career ended in what year?

COHEN: Well, in 1942 I went to work for RCA in electron tubes, specifically gas discharge tube development -- thyratrons, rectifiers, and so forth. Some of that work was classified --hydrogen thyratron radar modulators, as well as things that weren't classified but were nevertheless important. They were developments, in general, for defense purposes. We didn't call it "defense" in those days because there was a war going on.

ROSS: But also very much in line with what you would later do theoretically?

COHEN: Well, this was physical electronics --the development of actual devices like tubes as opposed to systems, which are a little different in a way. Occasionally in conversation, when people learned that I was with RCA before coming to ERA, without realizing the state of the computer art at the time that I joined ERA, they assumed that I had worked in computers at RCA. RCA Labs had done early work in the digital area, and I had heard about that but I wasn't in it. I was in quite a different area, and that's the only point I want to make.

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ROSS: Why don't you give me some highlights on some of ERA's history --successes and failures.

COHEN: All right. I won't even make these chronological. Let's call this a check-list of things that we ought to get back to and make sure we cover in more detail. One of them is the exercise with IBM that occurred in 1949 through 1950, and even after the company was acquired by Remington Rand, when there was some patent follow-up.

ROSS: You're talking about the development of magnetic drum storage?

COHEN: No. This was a special study for IBM. I think Erv Tomash and I covered it in the *Annals* paper on ERA briefly, but perhaps that's worth a little special attention. We entered into a design study contract, but, actually, without anyone ever admitting it, the reason this came about was that there was a sort of little courtship going on with IBM, I suppose as a prospective purchaser of the company. It was never mentioned. This was simply a design study for a magnetic drum based computer that used punched cards for its input and output.

Another important topic is the hiring away of a group of people by Remington Rand. These people were entirely or mostly of the founding technical group. I won't have a lot to say about that, but I think it is a check-list item that we ought to explore a little bit.

ROSS: I think that has as many ramifications as the IBM tale does. You have told me, off the tape, last time, just to remind you, that many of those people who were raided by Rem Rand before it acquired ERA were still there once the acquisition took place, but in higher positions, so that we need to come back to that, in terms of competition with the Eckert-Mauchly group.

COHEN: I don't know if this is a single check-list item, but it's something to bear in mind, and that is the context in which we became better acquainted, as time went on, with our relatives by marriage. There were two kinds there: the relationship with the Eckert-Mauchly people in Philadelphia; the other one, strangely, was really getting acquainted with management, or having the management at Remington Rand headquarters become better acquainted with us. That's kind of an interesting story. As part of the process, there was at least one specific project that was started before the merger was completely approved. This again was sort of a design study. We had priced it as though we were doing it for an outside customer, so if the merger fell through, everything would be clean and above board, with no legal complications. They would simply pay us for the work that was done. I guess the title of that check-list item would be the "Tape-to-Card Converter."

ROSS: So that would have been in the early 1950s, late 1950s?

COHEN: That would have to be in 1952, because, you see, we got word of the impending or proposed merger, the agreement in principle, in December of '51. And then for the next five or six months there were hearings and investigations by the Department of Justice, the SEC, and perhaps the Federal Trade Commission. There were at least two federal government agencies that had to give the merger their blessing. The Department of Justice was concerned with questions of possible monopoly and anti-trust.

We undertook a project to design, and ultimately build, a tape-to-card converter. This was one of the missing elements in the Univac line, which did have a card-to-tape converter. This device would take Univac tape as its input and produce punched eighty-column cards as its output. The reasons that was done are worth discussing.

I think it is also worth discussing the contractual relationship with the Navy in the first year of the company, when there were two companies in parallel to circumvent certain legal restrictions. That's another item.

ROSS: I think we can go beyond what you wrote about in the article and talk about specific projects. You had good access and good advocates. Mina Rees, for example. Something that struck me in the pre-Rem Rand merger days was your hosting of conferences. It seems that built into the ERA organization was a determination to educate and train.

COHEN: Yes. Not conferences so much as just tutorial presentations to groups of customers. I guess that's standard practice even today, when there is new technology in any company. Sometimes it took place on the customer's premises and sometimes at the company, where we would take them on tours.

ROSS: Sometimes it went beyond that to include publications such as "High-Speed Computing Devices."

COHEN: Well, of course that book had a very special history and it's sort of in a category all by itself.

ROSS: I think we need to consider things like that.

COHEN: You know, that has been selected for the reprint series [of the Charles Babbage Institute]. I wrote the introductory chapter for that, which attempts to explain how the book came to be written. I don't know how thorough it is, but it's fresh in my mind now and it's at the printer.

ROSS: I think we should follow that up. Then there were specific projects with which you were involved, such as the 1101 series, and we should come back to that.

COHEN: Right.

ROSS: Also keep in mind associates --I have a good long list that we should pursue.

COHEN: There were some typical equipments listed in the Tomash-Cohen article, and that's part of our check-list. We can use that as a reminder. We can just go down a list of those things and there might be some interesting points and anecdotes.

ROSS: Yes. To back track, you were telling me about your association with RCA during the war years. Where did you go from there? You were with them through the war?

COHEN: And beyond, up until the fall of '46.

ROSS: O.K., so you went right from there to ERA?

COHEN: Right, RCA to ERA. I heard of ERA through an advertisement in the newspaper.

ROSS: Oh, really?

COHEN: It appears in the *Annals* article.

ROSS: So you came at the time when they were really expanding?

COHEN: Right. I stopped in for an interview during a vacation visit here somewhere around Labor Day, 1946, and then went back and wound up things at RCA. I actually came to work early in December 1946.

ROSS: What projects were you immediately involved with?

COHEN: The first thing I was asked to look into was an idea that Tommy had, C. B. Tompkins.

ROSS: You'll have to tell me who Tompkins was.

COHEN: I believe he had the title of Director of Research at the time. He was one of the founding group and a good friend of Mina Rees. The contract with ONR, the smaller of the two contracts, was a multi-task contract, but it was almost a contract between Mina Rees at ONR and C. B. Tompkins at ERA, because he had technical responsibility for everything going on at our end of that contract. The first thing I was asked to look into, because I had tube development experience, vacuum tubes, electron physics and so forth, was the feasibility of storing information on, as Tommy put it, "a beam of electrons." If you generalize it to say, "a beam of charged particles," you very quickly come to the conclusion that you reduce the dimensions of the apparatus if you don't deal with light-weight particles like electrons, but consider something like hydrogen ions or protons traveling as slowly as possible so that you don't end up with a linear accelerator a mile long, or something like that. And so I ran a paper analysis on this and considered all of the factors involved --diffusion and the highest frequencies, the pulse rates that one might use, and so forth. The conclusion was that it was not too feasible, but marginal. Then we got into other things.

ROSS: Would you characterize that kind of research as task-oriented?

COHEN: Yes, because that was a specific task. We reported on it in the progress reports.

ROSS: Do those progress reports and papers exist?

COHEN: I don't think they are too accessible, but we do have, among my papers at least, copies of relevant material. One of the good resource documents I think I have, and there might be a second copy around here too, is a catalog, or finding aid, from Dartmouth College of their collection of George Stibitz's papers. I have learned recently that Hank Tropp had a hand in organizing that material. But it turns out that Stibitz has in that collection many pages of engineering lab notebooks of mine and of other people at ERA. And the reason for that was that he was a consultant in some litigation that I was involved in also --the case of Technitrol vs. Sperry Rand, 1958 --and so apparently

among his papers is all of this consulting that he did for various purposes. He simply kept them, and there they are, at Dartmouth.

ROSS: So, from that first analysis project you moved on to what?

COHEN: O.K. Let's kind of block out the period from the time I came to the company until approximately 1 July 1947, because a number of interesting things happened in that period. Later on so many things were going on at the same time that I don't think you want to take them up chronologically necessarily. In that first period, as I indicate in the foreword to the reprint of "High-Speed Computing Devices", Mina Rees and Tommy had concluded, in a conversation on the first day she was in Washington at ONR, that there was no good place to turn to find out what the state of the art really is. There were many projects and much government money supporting work in the field, much of it an outgrowth of work that might have been started during the war. So, as a result, probably in late '46, they agreed that it would be a good idea to include a task on this ERA-ONR contract to conduct a survey (they may not have used the word survey) to report on the state of development of some computing machine components. They used the word "components" in a very broad sense. Components can be subsystems (the word software was yet to be invented) and actually all technical aspects of the digital computer field.

ROSS: Did they restrict it to digital?

COHEN: Not just digital, not just electronic.

ROSS: Electromechanical devices as well?

COHEN: Well, from a historical basis, yes. The reporting, for example, would have to include punch card devices and things like that.

ROSS: So they were asking you to do some historical work as well as report on the state of the art?

COHEN: Insofar as the past clarified the present. Where I and a few others were involved was that arrangements were made for us to visit various places that had work going on under government sponsorship, and the easiest doors to open were those that had ONR sponsorship.

TAPE 1/SIDE 2

COHEN: In March 1947 I visited MIT and met Jay Forrester and we discussed their electrostatic storage developments in the Servomechanisms Laboratory. We probably discussed a little bit of Project Whirlwind. Incidentally, we were receiving all of the memos, reports and documents from Project Whirlwind. It was ONR's policy to disseminate these things widely and we were on their mailing list. It would be wonderful to have a full set of that stuff now. You know you don't think of saving these things. I'm sure they were tossed out by the company.

ROSS: The military and the government clearly learned during the war the utility of involving more than one institution.

COHEN: The important thing here, Jim, is that this was part of the educational process. We were being educated by visiting and by reading reports from many projects: The Moore School Reports, the MIT Servomechanisms Lab Reports, the ONR supported projects. I think these were mostly sponsored by the ONR Training Devices Center, later called the ONR Special Devices Center. All of their large development projects had stormy names like Whirlwind, Cyclone, Typhoon, and so forth. So visiting MIT was important, but at the time their particular version of electrostatic storage was a major discussion item.

I also visited Brown University where, under Professor Lindsay, one of his graduate students, David Arenberg, was doing work on solid acoustic delay lines, particularly quartz delay lines. And, as a matter of fact, I think one of those things turned up in one of the two most recent issues of the *Annals*, with a diagram of the multiple reflection paths.

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So, you see, one important thing to recall from an historical standpoint is that it wasn't clear which of these horses were going to be winners, which technologies were going to be practical for the storage of digital information. So everybody was looking at everything, and that is one of the reasons that ONR and Mina Rees wanted to have a report on just what was going on, just what is the status of everything.

ROSS: And out of that came...

COHEN: And out of that came the book *High Speed Computing Devices* later turned into a book is, again, ONR's policy of encouraging wide dissemination. Even years later Bernie Galler told me, just recently, that that was one of the books from which he first got his introduction to computers.

ROSS: You said that ONR contracts didn't make up the total work load of ERA.

COHEN: Maybe this is a good time to pick up the contractual structure.

ROSS: Yes.

COHEN: O.K. When the company was formed there was some work that was a continuation of work previously carried on at the National Cash Register Company in Dayton, Ohio.

ROSS: Who had been unwilling to continue the war work.

COHEN: No, they wanted to get back into and catch up with their commercial development, their commercial product line. The monitoring Navy organization down there was called the U.S. Naval Computing Machine Laboratory, USNCML, and that was moved to St. Paul in 1946. It was already on board and in operation at the time that I joined ERA.

ROSS: In the contract they actually served sort of as a watchdog or supervisor?

COHEN: Yes. And partly because so much of the work was classified, you just couldn't have an ordinary kind of inspector organization. You had to have discipline, and they supplied discipline. So, a contract was set up, multi-task and fairly large in scope. The Bureau of Ships was the purchasing agency in the Navy. The customer, the ultimate user, was the CSAW, which stands for Communications Supplementary Activity --Washington, but purchasing was by the Bureau of Ships. These people at NCML were representing the Bureau of Ships in carrying out that contract. That contract was not with ERA. It was with Northwestern Aeronautical Corporation.

That first contract must have been dated about August 1, 1946, because by August 1, 1947 that contract was replaced with a new one with ERA from the Bureau of Ships, with completely new task numbers. The first ten or twelve tasks were continuations of what were in that first year's contract and then new ones were added. In parallel with that Bu Ships contract the first year, was a smaller contract that was let directly to ERA from the Office of Naval Research. It was a smaller contract, and that was the one for which Mina Rees had technical responsibility, and Tommy Tompkins was principal investigator for ERA. Some of the tasks were classified because they involved his consulting services on specific things, and others were not classified at all. They were assigned very easily as the need arose.

You asked if the Navy was the only customer. There were these two contracts, but Northwestern Aeronautical was struggling along trying to fill the gaps in its business created by the cancellation of the wooden glider contract. So there were several things going on. They had a multi-task (this was going on at the time that I came on board) Air Force contract. They had a lot of interesting things going on, mostly under Dr. Bertil H.T. Lindquist, Director of Research for Northwestern Aeronautical. He stayed on with the ERA operation for some time. I don't know at what time he left. He died some years later, but he was a very flamboyant fellow. I knew him when I was an undergraduate at the University. He was one of those fellows who told stories that are hard to believe but nobody could ever prove

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that they didn't happen. I remember the names of a few of those jobs. One was maintenance dock skis. This was a scaffold structure for use in the Arctic, mounted on skis --you'd pull this thing around. Another was a cargo container where you could parachute a large bomb -like structure that contained cargo. It had the aerodynamic characteristics of a bomb so that it had stability and would land in a certain way. They had big models of things like that around, and they used to play around with little ram-jet engines. I don't know whether or not they were part of a task. There was another group in the company that was obscure to me. On these things Sid Rubens would have better knowledge, concerning people like Sloan and Wakelin. There was an ONR project called SQUID, I believe. It had to do with solid propellants. Now that was ERA, not Northwestern Aeronautical. Not everybody was thinking in terms of there being a computer business some day. A lot of people didn't take that prospect very seriously, even for a long time afterwards. You know, electronics for various kinds of applications was O.K., but computers? Yeah, O.K., sure.

ROSS: I'd like to return to that because [Robert E.] McDonald made some very specific remarks [in a CBI interview] concerning just that topic. O.K., were there other programs that were going on either in NAC or ERA that didn't have ONR sponsorship?

COHEN: Yes, commercial. One of NAC's products that was already completed, and being marketed as an ERA product at the time I came, was what they called the Gasporter --the ERA Gasporter. It was a miniature fueling truck for fueling small aircraft at small airports. And at the time, right after the war, did you ever hear of the Crosley car? There were several subcompact cars, we call them subcompact today, that were on the market. One was the Crosley, that you used to see running around. There was also a Crosley truck, and here they attempted to fit the Crosley truck with a large fuel tank so that they had this miniature servicing thing. I guess that was too much of a load for the Crosley truck because they were always stripping the transmission or the differential or something every time they tried to accelerate, so it didn't work out as a product. But it was being promoted. There was a lot of product literature, and I would suspect that in the stuff that Russ Headley archived over at Univac we should be able to get our hands on some of that if they don't care to keep it. If they want to promise to keep it, fine, that's good. But it

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ought to be catalogued. There was a lot of that early product literature (and a lot of non-product literature) -- advertising, feelers and testing the market to see if there were a market for such a product, based on some things that might have been done in the laboratory just up to a certain point. There was a parallel thing to this ERA Gasporter. It was officially called the ERA Lavatory Porter, but it got to be known affectionately as the privy-porter. It was built on a sturdier chassis, on a Jeep. I guess they sold some of these things. These all related to the aviation industry, and John Parker, of course, had good connections throughout that industry.

Through that first year, even in the early days, I remember once in a while on fairly short notice some visitor, some VIP, would be brought in and be shown around. I remember General Jimmy Doolittle, who was a Shell Oil vice president at the time, being brought in, and I gave him the rundown. This was at the time of one of our magnetic drum developments. I was waving my hands around and pointing at the scope and throwing switches, trying to get him to understand what this idea of magnetic drum storage was. But there were visitors of that kind because of Parker's close connections with so many of these management people from the aviation industry.

ROSS: You've talked about some pretty diverse products that NAC was involved in. Were those attempts to establish new marketplaces?

COHEN: Yes, for NAC. Efforts along those lines began before Parker made a deal with the ERA founding group.

ROSS: That makes sense. What other kinds of work went on?

COHEN: Now you're speaking of a very particular period of time.

ROSS: Very early.

COHEN: As part of the Bureau of Ships contract, from the very beginning in 1946 some of those tasks dealt with pretty much straight communication equipment. This work had been done by other contractors, such as NCR, before ERA was formed. This was communications security equipment. A lot of it had to do with straight radio and telegraph techniques, frequency-shift keying and the like.

ROSS: As I understand it, many of those tasks were assigned for pretty established technologies.

COHEN: Yes, variations on established technologies.

ROSS: Yes. That is something that has not been recognized. People make the same mistake about ERA that they made about your association with RCA --computers all the way, and you're arguing that, at least in this early period, that was not true.

COHEN: Yes.

ROSS: It's very diverse.

COHEN: Right. Some of the tasks were almost boondoggles. There was one project which they called (all these tasks had code names) ALCATRAZ. I guess they picked that name because of a multi-cellular aspect.

ROSS: And what was that?

COHEN: Well, this was a cryptological application, and as in most of these things, you keep tally of situations, and the way you would do it with even the most rudimentary computer you would simply record counts in some storage medium, or storage device. Well, here they had electromechanical counters, a whole wall full of these things. In those days race track totalizators would look the same way. Then, when certain conditions occurred, there were

printers to which the counts would be transferred, and as part of the development, the printing unit of a Sundstrand

adding machine was modified or adapted for the multiple printers. I believe the counters were sort of low inertia

high-speed for that time, relatively high-speed mechanical counters, that were developed pretty much from scratch,

and later were attempted to be marketed.

ROSS: Commercially marketed?

COHEN: Commercially marketed as counters for various purposes, but without great success. More importantly,

there were a lot of people who had skills in electromechanical technology, in the use of various kinds of relays, IBM

high speed relays, tape punches, and so forth. Well, the work going on at ERA had continuity, of course, with the

work that had been going on during the war. The National Cash Register work, I believe, involved building many,

many copies of the same thing for use in the field, things like that.

ROSS: Cryptographic?

COHEN: Cryptographic and cryptanalytic. And printing devices with variations on IBM's electric typewriter, which

in those days, bore the trade names Electromatic and Flexowriter, I believe. For many years you saw strictly Navy,

with Navy name plates on them, IBM typewriters and all this good rugged gear. So that was part of the technological

skill pool that ERA had. A lot of the technicians who came out of the Navy and who worked for ERA had those

skills. Some people came out of other experience, communication, radar, and things like that, with electronics skills.

ROSS: What other projects were you specifically involved in? You were about to tell me that when I interrupted and

asked you to go back. After the publication of the handbook...

COHEN: No, the publication of the book went on to others.

ROSS: Oh, that came later.

COHEN: Yes, actually the publication date was 1950. There were a lot of gaps to fill. On that I'd refer you and us to what I've written up in the introductory chapter to the reprint edition. We can use that as a basis for discussion.

The next thing I was involved in on the ONR project was to test out the feasibility of certain kinds of recording for magnetic drum purposes. You see, the magnetic drum work was going on along several parallel lines. The first work was done in conjunction with a project called GOLDBERG. You may have seen references to the GOLDBERG drum, which was a thirty-some inch drum. In the earlier version it had magnetic tape cemented to the drum. The drum would be loaded up by stepping it mechanically, creating a lot of noise. The information from a punched tape reader would be transferred to the drum, and then the mechanism that stepped the drum along would be disengaged and the drum would be shifted into a high-speed, continuous running mode. Not terribly high-speed, but high for that kind of drum. That was the analysis mode. The information would be read and then re-recorded. There was a reading head, an erase head, and a writing head on each track, and by interposing some shift register delays, it was possible to cause the information on one set of tracks to precess relative to that on another set of tracks. This was part of the standard stock-in-trade in regard to certain cryptanalytic operations, you see. And as another part of that project, of course, there was the comparison electronics, and they would look for certain matches --they would count the "hits", so called. So this was part of the GOLDBERG thing. The people who did instrumental work on that were Sid Rubens (you can talk to him about it), Jack Hill, and others, under the supervision of John Coombs. John presented what I've called ERA's "maiden paper" at the National Electronics Conference in Chicago, November 1947. We have copies of that paper available so that we can pin down the details there. I mention that GOLDBERG work because this ONR sponsored work was to be a little different.

One of the things that Tommy was very interested in was the possibility of achieving a greater density within a given track by using what he called at the time a "non-return-to-zero" wave form. The concept is explained in the ERA book.

There's another thing that was rather important if you were going to use magnetic storage in a computer. You didn't want to use it in the way that it was being used in the GOLDBERG project but rather you wanted to be able to selectively get at one particular item on the drum, one particular angular location, and either read what's there or write something new there. It really isn't selective erasure, so I used the term "selective alteration of digital data." On the basis of an address, you'd pick out a location and you'd read it, if that's what was called for, or you'd write something new on top of what was there.

TAPE 2/SIDE 1

COHEN: So, this project was for purpose of checking out the limits and the feasibility of selectively altering data recorded digitally. Also, if one crowded the information, packed the information on the drum so that you were reading an envelope of the pulse train could you achieve a higher order of information storage density? I had responsibility for this work as project engineer. Bill Keye had just joined the company and was assigned to work with me. Arnie Hendrickson, also new, was our electronics technician. Well, we set up a little spinner type of drum and some elementary electronic counting circuits, and reading and writing using the same kinds of magnetic heads that were being used, basically the same kind that were used on the GOLDBERG Project. We checked out what one could do. I guess we achieved some 220 or 230 pulses per inch, which was very gratifying because these were not aerodynamically floating heads as they have now. They were rigidly mounted, with spacing of perhaps one mil from the surface, and the surface going by pretty fast. I remember we were very impressed with ourselves because the surface was moving along I suppose something like sixty miles per hour, and yet you could very accurately pick out a given bit position and change it on the run. We were very impressed that we could do this very accurately, and reproduce it every time.

ROSS: Were you manufacturing your own tape?

COHEN: No, tape was available. Oxide coated paper tape was new. I believe that was originally a German development. Earlier audio recorders were largely wire recorders, and then, with the end of the war, oxide coated paper began to be seen for audio recording.

ROSS: I'm leading somewhere, because later you did some instrumental work in different...

COHEN: Well, we worked with Minnesota Mining and I believe we bought some oxide suspension from them and started coating our own drums. Was that what you were referring to?

ROSS: Yes.

COHEN: We were sometimes asked such things as (it seems kind of ridiculous in retrospect) "Well, gee, if you have tapes cemented to a drum and if they're running that fast won't they eventually just fly off?" Now the object of this particular experimental work for ONR was to establish certain feasibilities.

ROSS: What I was leading up to was you said that Tompkins had asked you to do the research, and the goal was really to figure out which kind of storage had what parameters, and you were working on magnetic drum storage techniques.

COHEN: Let me amplify what I mentioned earlier. Bill Keye came to the company in early '47. I don't know what his title is at Control Data now, Vice Chairman or something high up.

ROSS: His last name again?

COHEN: Keye, William R. Keye. He had come to RCA not too long before I left RCA, and since he was a native of St. Paul, he was interested in what kind of company I was going to and he followed me here, as did at least two others from RCA out there in Lancaster, Pennsylvania [Howard Shekels, Joseph Kelar].

ROSS: And he worked on the project with you?

COHEN: Yes. And we had a very bright technician who rose in the company later, Arnold P. Hendrickson, who, as far as I know, is still with Univac. He moved to Salt Lake City, the Salt Lake City operations.

ROSS: What I was trying to get at was to place this specific research program that you were developing in the picture of ERA as a whole and try to see what Tompkins' design was in having you follow up questions like that.

COHEN: Well, it's a little complicated because somewhere in the first half of '47, he and his family moved to Washington. He stayed with the company but he conducted some of these things from a distance by commuting out to see us. We differed on what ought to be done next. He wanted to run certain kinds of experiments that some of us weren't too interested in running. We felt that what had to be established had been established and let's get on to the next useful phase. So we had some differences on that, and it got to the point where we reorganized things a little bit and I reported, as result of that, to John Coombs. There was a kind of shift in the chains of command. So Coombs became sort of a buffer between me and Tompkins.

ROSS: When you say something had been established could you be more specific?

COHEN: As far as we were concerned, we had established that it was safe to go ahead and use this kind of recording for a drum storage system in a computer. Tompkins was impressed with the experiments that Howard Aiken had performed and with his magnetic drum work. There was magnetic drum work going on elsewhere, in particular at Harvard, and Aiken had a routine of altering and precessing data, seeing if patterns would be preserved, over and

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over again, and he was able to say that by running this exercise a certain length of time, that the information had been manipulated thousands and thousands of times, millions of times, and was still intact. We felt, yes, this is good, but in our case, the information was going to just sit there on the drum most of the time. I suppose that if something was going to deteriorate it might be due to temperature or whatever.

ROSS: But not use?

COHEN: But not use. One could have been very scholarly, I suppose, and run all sorts of exhaustive experiments, essentially repeating what Aiken had already established regarding the stability of rewritten digital information. We felt that the risk of instability was minimal, and that we could go on to the next step and try to build a larger system using these techniques.

ROSS: Let me turn my questioning around a little bit and ask you what convinced you and at what point were you convinced that magnetic drum storage was a possibility? That is, had more possibilities than alternative methods? You were arguing that there was a good deal of work going on with a number of alternatives.

COHEN: Within the company, Sid Rubens had some of this work going on. These were kind of side projects related to the GOLDBERG project where he and his people were measuring the characteristics of pulses going down a metal rod, different kinds of metal rods, for the dynamic storage of information. I mentioned having visited Arenberg at Brown University. These kinds of delay lines had their origin in radar moving target indicators. The wartime use of these things was, you'd store the pulses of the last scan and...

ROSS: And compare the next one.

COHEN: ...and compare the next one. If it canceled out it was the same. You could cancel it out in such a way that your display could be made to show only that which had changed since the last scan. So it showed a moving target.

The techniques had been developed for another purpose. The mercury delay line was the strong contender in the acoustic or sonic delay line field. And, of course, that survived through a number of different computers, including the UNIVAC and Raytheon's computer.

ROSS: European ones as well.

COHEN: Yes, right. EDSAC was another one. Well, having written up a strong sales talk, I had myself pretty well convinced. Here you had something that would stay put, that was nonvolatile. Incidentally, as far as I know, I invented that term as applied to storage. I thought it up one morning while shaving. We were putting on one of these seminars that day for airline communication engineers. We were giving them a tutorial about the elements of computers, storage and so forth, and the magnetic medium had this unique property of electrical alterability combined with nonvolatility. That's an important property. Its disadvantage, relative to some other techniques, is that it is a cyclic type of thing where you have an access time, a latency period until the next time the drum comes around to the address you're looking for. But when you compare that with the number of words of storage in mercury delay lines or electrostatic storage tubes, where you have the very limited capacity of faster access storage, this looked like it would be a pretty good workhorse.

ROSS: Where did you go next? So far, this has been pure research.

COHEN: Well, we can follow the line of my personal involvement, bearing in mind that there were many things going on in parallel within the company. You know there were something like four to six hundred employees at that time, and many different things were going on in parallel.

ROSS: Let's focus mainly on your area of involvement.

COHEN: I think a pretty good check point here is the date that the Bureau of Ships contract was changed, was transferred from NAC to ERA --new contract, new contract number, new task numbers. That happened early in August 1947. The task numbers looked entirely different from those in the early BuShips contract. The GOLDBERG project was listed as Task 1H on the old contract, but GOLDBERG turned out to be Task 9 on the new one. I think there were twelve tasks at that time. Number thirteen was added; that was a new one. That was for preliminary work on the ATLAS machine. They never called it a computer. They were very careful, our sponsors, never to call these things computers because there was something stirring in the government that all government computer acquisitions would be coordinated by the Bureau of Standards. So these people had two controls. In the first place, there was the secret classification. In addition, they called it an "analytical machine", which made it special purpose. They also called for a few special purpose instructions, which helped make it both classified and special purpose.

ROSS: And you did what?

COHEN: The assignment was to produce a paper design. This was a paid-for proposal, really. It was done under a task, and we were not authorized to build anything yet. This was a very instructional period for me personally, and for others, because we worked with certain people down at the Naval Communications Annex, as it was then called. Incidentally, it wasn't the National Security Agency at that time. This activity was at 3801 Nebraska Avenue in Washington, D.C. on the campus of a former school. The work was contracted and administered by the Bureau of Ships. So, under Task 13, to begin with, I was under a chain of command where I reported to John Coombs. One of my associates on that job was George Hardenbergh. That's a name that you ought to pursue, and he's still around town. He served as County Commissioner of Ramsey County for a time. You might have seen his name around.

ROSS: I actually went to school with his son.

COHEN: Really! That sets me back a whole generation. George was very bright. He could see things very quickly. Very inventive. I don't know, just many fantastic skills. He had gotten his Bachelor's degree in electrical engineering

at Cal Tech, had been in the Army during the War and was assigned over in England for a time in radar development. He was on board at ERA when I arrived. We were thrown together, and it was very interesting, very educational. So, he and I and John Coombs visited Washington several times and some of those people visited us. The people we dealt with mainly were Howard Campaigne, Joe Eachus, and Jim Pendergrass. Prior to the war, Campaigne was on the math faculty here at the University of Minnesota. He's not a native Minnesotan. He comes from Chicago. He's now retired in New Mexico.

ROSS: What were the positions of these people in Washington?

COHEN: Howard Campaigne was a civilian, but a Commander or Lieutenant Commander in the Naval Reserve. Jim Pendergrass was career Navy, Lieutenant jg as I recall, with a mathematical background. Dr. Joe Eachus was a civilian mathematician, perhaps a notch higher on the civil service scale. Incidentally, after we shipped off the Tomash-Cohen paper in the *Annals* in 1979, it occurred to me, in rereading it, that the impression could have been created that the 1101 was strictly ERA. I felt that some more credit ought to put in there, so I wrote up two or three paragraphs that elaborated on the roles that some of these people played, and that appeared in the letters section of the very next issue of the *Annals* as an addendum to our paper. It mentioned Pendergrass and Campaigne and Eachus.

ROSS: What came out of the task assignment?

COHEN: The result was a fairly detailed plan, I think in the Spring of '48, for the architecture of ATLAS, and we were then authorized to go into the development and construction phase. Somewhere along in there Jack Hill, having finished up another project...

ROSS: Do you remember what that might have been?

COHEN: Well, he had been on this GOLDBERG project and they delivered that. Then he was put in charge of a test project, DEMON I, which is a legendary part of ERA history. Ralph Meader, a former Navy captain, was a Vice President of ERA. As the story goes, he was having a drink at the Officer's Club down there in Washington, and he was insisting in this alleged conversation that ERA was a "quick response resource" and if the Navy ever needed something in a big hurry, "just try us." So shortly afterward they said, "O.K., here's something we need in a big hurry", and this turned into a big crash project, this DEMON thing, where they took and adapted the GOLDBERG drum and some other electronics from here and there. The fellows worked very long hours, and I think I can pinpoint the time. That would have to be in the spring and summer of '48. They worked like dogs. You, know, seven days a week, long hours and everything, just to prove this point that came out of a barroom conversation. It was delivered on schedule. We heard some time later --they didn't want to share this with too many of the people on the project -- that the particular need --many of the needs in that field are kind of ephemeral --that the need had disappeared before it was delivered. But there was a celebration banquet when they delivered it. "You fellows did a tremendous job", and all that. I think that answers your question about what Jack Hill had been doing.

ROSS: One of the things that we didn't include on our agenda of future topics is the relationship or conflicts that actually existed between the philosophies of ERA and Eckert-Mauchly people, and one of the different approaches certainly was the lack of quick response on the part of Eckert-Mauchly people to orders that they had contracted for. We'll come back to that. I just wanted to get that on tape.

TAPE 2/SIDE 2

COHEN: I think we side-tracked a little bit to pick up the DEMON thing, the crash program, as sort of a footnote. I think we finished...

ROSS: We finished the logical design, and they had actually authorized the next phase.

COHEN: Yes. And among my papers I should have a copy of that.

ROSS: Your proposal?

COHEN: Yes, the system design. We wanted to get a copy of that unclassified later by removing a couple of instructions that really weren't classified, but they were part of a little game that made it classified. So, we brought the idea to Captain Creasor, the Navy commander of NCML, and asked, "Is it O.K.?" "Well, let's see what you want." We even changed the name, "ATLAS", and took another name with the same number of letters, "MABEL", and we put that in. And for a time, he wanted to classify that. So there was no way of getting out of it, because it always related to something previous that was classified. We finally somehow got a version that satisfied him. We wanted to use that for possible showing to others, for potential marketing purposes, and it eventually led to the 1101, which wasn't very marketable anyway, as it turned out.

ROSS: Let's hold off on that. We do need to return to that subject. But first, did you continue with the ATLAS project?

COHEN: I kept on being pulled off of whatever I was doing to get into new areas. That was both interesting and frustrating. And in the summer of '48, I would guess, we (John Coombs and I) were brought into conversations with the Bureau of Standards. The Bureau of Standards was responsible for coordinating computer needs of various government agencies. On that occasion we met George Dantzig, who was a pioneer in the operations research technique of linear programming. He had devised the simplex method of solving large scale linear programming problems. He was at the time with the Office of the Air Controller together with Emil Schell, Marshall Wood and others. George Dantzig was later at the Rand Corporation and then after that at one of the universities out there, Berkeley or someplace. George, for his linear programming work, was interested in the inversion of very large matrices, and he wanted a computer that could perform many, many multiplications in a row but not spend more than, I don't know, maybe ten microseconds per multiplication, which was out of the range of what we were doing, certainly with magnetic drum storage. Tommy and Mina Rees were there. We weren't speaking very much about our work on

ATLAS. We were required to keep that compartmentalized. But what happened is that we were given a contract for a small design study, $25,000 or something like that, a paper proposal again. That was a common way of doing business in those days. It was a paid-for proposal for the Bureau of Standards. It was there that I met and worked with Harry Huskey who was at the Bureau of Standards for a time, and of course we had known Sam Alexander before. The Bureau of Standards was building a computer right there at the Washington location. Suddenly, in the middle of these meetings we were having during that study, Harry Huskey moved to UCLA. The Bureau of Standards had this branch setup called the Institute for Numerical Analysis, which Tommy later headed, but Harry Huskey was brought in there. The Bureau had a computer under construction there too, and these two computers eventually became the SEAC and the SWAC --the Standards Eastern Automatic computer and the Standards Western Automatic Computer. Harry Huskey had been with one of the groups in England, the National Physical Laboratory I believe, before he was at the Bureau of Standards. He said to me, "If you want to make a magnetic drum computer fast, why don't you use a two-address structure, where the second address simply indicates where the next instruction will be found?" That is one way of approaching the minimum latency problem. Fine, as far as we were concerned. That was part of the requirements specification from the customer, in this case the Bureau of Standards.

At this point, Warren Burrell was brought in to work with me, and he and I kind of, just two guys, worked on this one. We may have conferred with George Hardenbergh and others, but we worked up a paper design which was really a variation on ATLAS, but with a two-address structure. I introduced some things to speed it up, new instructions like "index jump". Here, we begin to see some of the first responses to the needs of programming, software, in the architecture of the machine.

Meanwhile, Tommy, with one or two other people at our, by then, Arlington, Virginia, location had other variations. He felt if you put two heads in a track and if you read something off of a set of tracks and completed a multiplication, or whatever it was, in time for the same part of the drum to appear under the next heads, then you'd write the result in the same place without having to wait a whole drum revolution. Logically, it was a good idea. There are some problems here. I don't think it was ever successfully done. I don't know if we ever really tried it. There was a serious

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question of taking something that was written on the drum with one head (let's confine our attention to one track at a time), a pulse that is laid down with one head, and now you're going to, splat, land exactly on that same spot with another head. For the first time you had mechanical tolerances which you don't have with a single head if you want to hit the same position accurately. In that case, it's just a matter of electrically reproducing time without jitter. That problem had been solved.

ROSS: I see exactly what you're saying.

COHEN: But mechanically, you might have a little different problem there. So that was a variation. So, two or three different variations on the design and architecture of a computer came out of that contract, and I think about three different reports were submitted, bound separately. That's what went in to the Bureau of Standards. That was the end of that, except that in the *High Speed Computing Devices* book, one of those variations was described, and was called "the ERA computer", thereby creating no end of confusion. Not that it makes any difference at this late date, but it did as late as 1976 in the *IEEE Transactions on Computers*, December 1976, twenty-fifth anniversary issue. In that issue, the first article happens to be by the editor of the special issue, Harry Huskey, with his wife, Velma Huskey, on the "Chronology of Computing Devices." And in that chronology he mentions this... well, let's see what he does. I can probably turn to this. Page 1190. Incidentally, the article is very good on history of techniques and specific equipments. He says: "In 1948, Engineering Research Associates had a contract with the National Bureau of Standards for a magnetic drum computer." (Actually, it was for a design study, and Huskey was, for a time, the man we dealt with there.) But he continues: "A commercial version called the 1101 was later marketed by Sperry Rand." As you know, the 1101 was a commercial version of ATLAS I, the Task 13 computer, and not of the 2-address NBS design.

ROSS: I see your point.

COHEN: He jumped to an understandable but incorrect conclusion. I had the opportunity to pick up the correction in the introductory chapter to the *High Speed Computing Devices* reprint edition, pointing out that people could easily be misled by the fact that the NBS paper design was called "the ERA computer" in the book. This is strictly trivial, but in the interest of historical accuracy I thought I'd mention it.

ROSS: O.K. Who was it that kept calling you off projects?

COHEN: Well, there were several of us who were called key people around there, you know what happens. The question is, "who should we send, who should we put on this thing?" So some of us were always being pulled into things of this sort. John Coombs was my immediate supervisor. At his level, he participated in decisions of this sort.

ROSS: All right, so you then did?

COHEN: So, that was an example of the kind of short exercise, like from July or August through November of one year, while doing a few other things at the same time. That was the sort of thing that we got into.

ROSS: All right. So the ATLAS project proceeded how?

COHEN: At some point it turned into the development of specific equipment, a development project, and Jack Hill was project supervisor, Frank Mullaney was project engineer. An interesting thing about that. The construction wasn't the lowest cost but by gosh, it was built to Navy wiring specs and things like this, and was very conservative in both the electrical and mechanical design, and it was pretty good. It was delivered in December of 1950 down at the Nebraska Avenue location, and the big question --legend has it people were betting on this one --was whether the ERA installation crew would be home for Christmas. And it was set up and the power turned on and it ran. So,

they let it run through various test routines for a week or two, and nothing happened so they went home for Christmas.

ROSS: Uneventful.

COHEN: Yes. That was kind of a new thing in those days compared with many other kinds of projects. A point that has been made with regard to a number of the systems that were delivered by ERA was that most of the computers that you were hearing about or reading about were developed and run at the same site for a long time. Even Univac I, when they said it was delivered to the Census in 1951, spent another year at Eckert-Mauchly. It belonged to the Census Bureau at the time and they were running census problems there, but it was still at its birthplace. With ERA, the equipments were shipped out and, in the case of the Navy certainly, installed and left to be maintained by customer personnel. That was an important kind of discipline, which became part of the ERA reputation. It stayed on in the commercial times later.

ROSS: Now how was ATLAS turned into a commercial product?

COHEN: The company authorized some money to build a commercial version, and that's where the term 1101 originated. Jack Hill thought of that one. That's thirteen in binary.

ROSS: My impression is that the 1101 really was a transitional product in that you were balancing commercial with military and government markets.

COHEN: Yes. But first let me include some background. As far as our NSA sponsors were concerned, ATLAS was from the outset a precursor to something more powerful that would ultimately be built when acceptable high-speed storage would become available. When ATLAS II was later authorized, by that time there was enough confidence in

the Williams Tube, so-called Williams Tube CRT storage, to build in, say, 1K words of that storage in addition to a larger capacity of drum storage.

ROSS: O.K., and what I'm trying to have you help me identify...

COHEN: The architecture was very similar, but not exactly the same. There were some improvements in electronics and practically every aspect of the machine, but basically there was a family resemblance. ATLAS II became the prototype of the 1103.

ROSS: I understand, but why extend a machine like ATLAS I to the commercial market.

COHEN: Well, it was sort of a trial balloon, I would say.

ROSS: But a big one.

COHEN: Yes, and that first 1101, the first commercial one that turned out to be the only commercial one, the only non-Navy one, was set up in a computing center in a store front location in Arlington, Virginia, at 555 North 23rd Street. There was a good group of people staffing the center. George Cramer, who is now deceased, was in charge. He had retired from NSA, and then worked for the company for a while. I guess the painful truth soon became apparent to everybody --you can't run a thing like this as a service bureau, which is what they wanted to do. You've got to have one heck of a lot of software, which didn't really exist. And even with it, if it didn't have the competitive capacity you had nothing. You didn't have a competitive service to offer. That particular machine wound up either as a donation or as a bargain sale to Georgia Tech. There were Serials 1 and 2 of ATLAS I that went to NSA, and these were eventually retrofitted with magnetic core storage. They lasted a long time. One of them wound up over at the Navy Training Center in Spezia, Italy.

ROSS: ERA did flight plan computers for the Civil Aeronautics Administration.

COHEN: Right.

ROSS: Now that is a more commercial market than...

COHEN: Well, it's government, but not military.

ROSS: What kind of installations were those?

COHEN: That was called a Flight Plan Storage System, and the drum was huge. We have pictures. That was one of the largest drums that we ever built, maybe that anybody ever built, but it was built with the same rigorous head positioning design as the ones on the 1101. That was in response to CAA, which had a development center in Indianapolis, and that contract was let out of there. They wanted regional flight plan systems. These were to be special purpose devices, you see, not programmable computers, that would talk to each other over communication lines and perform a number of different functions. Somebody would file a flight plan over here in this region and he's going to fly through or to a different region. All these things had to be coordinated. Very, very complex. They later simplified their requirements. That was one of these instances where we responded and followed the purchase specs to the letter. It got a little wild but we stuck to it and cranked it out accordingly. Other bidders took exception to a number of things and departed from the original procurement specs. Under the rules of government procurement, if one bidder is responsive and the others are not, then you can negotiate a contract with the one bidder. We were in that position. Technically, that system had the feature of being able to search the entire drum very quickly because there were parallel paths and the information on the drum was displaced in a certain way so that as soon as you established that what you were looking for was not there, you would switch electronically over to the next set of tracks and go on down the line. That system actually was built and delivered. I don't think that it was ever used for flight plans. For a time they used it for weather data. I don't know what ultimately happened to it.

ROSS: We should try to find out. Who were some of the bidders you competed against in an instance like that?

COHEN: I couldn't tell you for sure, Jim. I think it could have been people like Sylvania, companies in the communications and electronics business. Certainly companies of this kind had basic digital experience by that time, from military projects I suppose for the most part.

ROSS: Now, this project went on in the early '50s?

COHEN: Yes. If I were pressed for a delivery date on that one I would say '51, give or take.

ROSS: You referred to some of the inadequacies in the 1101 as a commercial product.

COHEN: It's in the Tomash-Cohen article. There might be a citation in there, too, so that you can nail it down if you wish.

ROSS: In fact, I'll just refer to it. I think it's on page 90, where you're writing about the 1101.

TAPE 3/SIDE 1

ROSS: This is a tape recorded interview conducted with Arnold Cohen, Senior Fellow of The Charles Babbage Institute, at the Charles Babbage Institute on February 1, 1983.   After you delivered the ATLAS I, you tried to seek other customers for what became the 1101. Can you tell me a little bit about those projects as well as the pluses and minuses associated with the machine?

COHEN: Yes. ATLAS I, serial 1, was delivered in December of 1950, and while it was in development, we in the company were very active in seeking out other opportunities to supply computers that hopefully might have some market potential. Let me preface any specific examples by saying that the company in its Navy contracts, both the ONR and the Bureau of Ships contract, was operating under the usual patent clause in which the company owned the patents, with the proviso that the government would have a royalty-free license to those patents. This was a pretty liberal policy. In effect, the government indicated that it would not stand in the way of exploitation of technology it had paid for. I recall particularly from conversations with various people in our customer agency that the company was encouraged to find other customers in order, somehow, to spread the load of supporting all this development. Another factor operating here is that in a company with little capital (and this company was certainly undercapitalized), the incentive is to spend as little company money as possible to adapt a design for the commercial market.

The initial planning on that machine working with the customer's people, began in 1950 or perhaps earlier. Early in the planning of ATLAS I, it was recognized by everyone that there would eventually be faster storage and perhaps other technology that would enhance the performance of a system, so that ATLAS I was always regarded as sort of a preliminary exercise. A useful machine for their purposes, but nevertheless preliminary to something bigger.

ROSS: That makes it a lot clearer.

COHEN: Yes, and that's an important point. In the meantime, the company management was very active in seeking markets for whatever technology the company had. There was a variety of things that we haven't mentioned here because they weren't even related but they were part of oddball things that were regarded as marketable in one way or another. There was a shaft-position indicator for example; I think literature on this is still there at Univac in St. Paul. The people there, I understand, have preserved a lot of the product literature and what I would call non-product literature, a lot of things that were trial balloons to see if any of these things were marketable. But if we confine our attention in these conversations to digital storage and digital computers and adaptations of these things,

that was a whole lot in itself. Bill Norris at the time was vice president for marketing and Jim Miles worked with him. Also Bill Winget, who was primarily a contracts administrator, and Bill Butler (he's in town and available for conversation)--William W. Butler. And then it became apparent to some of us, and to me in particular, that the company was concentrating on trying to sell magnetic drum storage systems, variations on this, to potential customers who may have had applications which involved other computational needs. I remember going in to see Bill Norris and suggesting that perhaps the focus was a little too narrow on drum systems, and that by now we had a capability that went well beyond that to computer systems and we thought that we could do something there.

ROSS: Do you remember what his reaction was? I assume it was positive.

COHEN: Positive, yes. But I think it wasn't something entirely new to him. I think what it did was to give him a calibration point on how ready we felt we were at that time. I can't nail down a time too well on this, but it could have been '48 or earlier, because certainly by the summer of '48 we went into an exercise with the Bureau of Standards for something that was more than just a storage system.

ROSS: And we've spoken about that already.

COHEN: Yes.

ROSS: Tell me a little bit about the origin and production of the computers that you built for the Air Force under a contract with Arnold Engineering Development Center.

COHEN: AEDC.

ROSS: Located?

COHEN: In Tullahoma, Tennessee. The very large test facilities that they were developing there would demand huge amounts of energy. They needed to be close to a high capacity source of electric power and that is why the center was located right in the middle of TVA country.

ROSS: Was this done on a competitive bid?

COHEN: Yes, but let's go back to how we went about bidding on that job. I would guess it was in the Spring of '49. I don't know at whose initiative, whether we were invited by Howard Aiken to pay a visit but there was a little party of visitors to Harvard from ERA. I know Howard Engstrom, John Coombs and I were there. Jack Hill might also have been present. (He could verify that for us.)

Jim Miles may also have been on that visit, or more likely he came on a follow up visit. I do know for sure that it was Coombs, myself, and Howard Engstrom. Now I recall that Tommy Tompkins was also there. That's probably how the visit was arranged. It was probably Mina Rees who urged us to go, because what was going on at Harvard at the time was the development of the Harvard Mark III, which was a Navy sponsored project. Mina Rees very likely said why don't you go there and have a look? So we spent a very pleasant day getting acquainted with Howard Aiken and getting a tour of the laboratory and getting the sales pitch from Aiken as how he was testing all the parts of it and how rugged everything was. For example, they had modified the battery of Underwood typewriters that served as the printing output, and they were running a test where these typewriters were running at full speed around the clock, and they were able to tell the Underwood Typewriter Company where the weak parts were, where they perhaps ought to put in some better metal alloys, or whatever, in these typewriters. And, of course, the architectural features of the Harvard Mark III are a matter of record and everybody knows about them. He had some very curious unusual features. There was a tremendous gearbox that drove a set of four drums from one motor. I guess the gearbox itself dissipated a couple of kilowatts of power, and then there was a separate large drum, with a vertical shaft, for the instructions. Aiken kept data and instructions in entirely separate systems. Well, one of the things that Howard Aiken was very proud of is the fact that he ran a completely open door operation in keeping with Harvard tradition, and he said that Harvard has a policy of not applying for patents. I guess they did copyright, though. Everybody

had access to anything that went on at Harvard. For that reason he said that he kept on repeating the phrase that if we wanted "to get into the computin' machine business, other people come and go all the time to see what we are doing, if you want to send somebody down here to live with us for a period of time, we would welcome that person, give him full access to all of our information, and let him learn how to build a computin' machine." The company took him up on that and Jim Miles went to Cambridge and remained there in residence perhaps a month, maybe two months, during which time he dictated several cases full of dictaphone cylinders, these wax cylinders that were used for dictating in those days, copying out many many pages of the Mark III manuals, and the reports that were available at the time. He brought all that stuff back and had the dictated material transcribed.

ROSS: The internal reports and manuals?

COHEN: Well, there were manuals of a sort, I suppose the plans for the machine, with detailed descriptions of how they were doing things. Since the system was pretty much there and standing already and much of it had been converted into hardware, this was the definitive descriptive material about the Harvard Mark III system. So that would have been in '49.

In September of '49, we were back at Harvard to attend the second Harvard symposium on large scale computing machinery and Howard Engstrom was on the program. He presented a paper on the merits of special purpose computing devices. On that occasion we had a number of meetings arising out of --let's call it a marketing opportunity.

ROSS: Who attended from the company?

COHEN: In addition to Engstrom, there were Parker, Meader, Jim Miles, John Coombs, myself, and Jack Hill. I don't know if Parker and Meader registered for the symposium. The ERA officers stayed at a fine, old hotel in Brookline, the Sheraton Beaconsfield as I recall, where Parker had a large suite which we used for strategy meetings, and as it turned out, for a lot of proposal writing.

ROSS: Who was the customer?

COHEN: The prospective customer was Dr. Elbert P. Little, who had just gone to Wright Air Development Center near Dayton, Ohio, to organize a computer laboratory as part of the Office of Air Research, and he was looking for equipment that would serve the needs that he saw. I believe he may have gotten his degree under Aiken. In any case, he had spent some time in Aiken's laboratory for the Air Force. Howard Aiken was a consultant to the Air Force on various procurements, and on this one in particular.

ROSS: So it was quite expected that Little would be there, no better place to solicit bids.

COHEN: Right. Sam Alexander of the Bureau of Standards was there. He was probably serving in an advisory capacity to the Air Force. There was a government regulation in effect that delegated the coordination of government computer procurements for scientific purposes to the Bureau of Standards.

ROSS: We've already talked about how the Navy got around that.

COHEN: Right. Bert Little, since he was very familiar with the wonders of the Harvard Mark III, having worked on it, would like nothing better than to get a copy of the Harvard Mark III for his purposes but his funding was limited. Jack Hill had an expression for this: He had a champagne appetite, but a beer pocketbook.

ROSS: That's perfect.

COHEN: His budget didn't begin to cover anybody's estimate of what such a procurement would cost and at one point it was even decided, well, suppose you chopped off this and chopped off that, no output printers, eliminate certain input tapes, and just somehow make a barebones machine. What he and perhaps some others were thinking

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was that he would buy with his limited funds some part of the machine and then at a later time he would, perhaps in another fiscal year, be able to get the parts that he couldn't get the first time around.

TAPE 3/SIDE 2

COHEN: Well, with regard to getting a part of the machine now, and perhaps at some future time getting the rest of it, Howard Aiken didn't think very much of that idea. I remember his saying that all you have to do is drive around New England and you'll find many examples of churches that thought they would start to build an edifice. They would build the foundation and the basement, and they would move into the basement, then at a later time, the idea was to complete the structure. He said there are many of those churches that have been around for years, and they're still in the basement of the building. The rest of the church never appeared. He didn't think that was a very good approach. I don't recall that Elbert Little was ever able to buy anything at all. We submitted several revised proposals. We sat there in the hotel room and wrote up some descriptions of what we might provide. At a later time in history, after we were part of Sperry Rand, he was a good customer of ours for the 1103 but at that particular time I don't know what he bought, if anything, from anybody to get started. (Note added in editing, by Cohen: Probably the OARAC, from GE.)

Now along about 1950 there was a request for a proposal that came out of the Arnold Engineering Development Center at Tullahoma. This request for proposal was something that looked interesting enough to us to bid on, and after a lot of internal conversation about the thing Jim Miles put together a proposal. He had all this material that he had transcribed about the Harvard Mark III, so he proposed something that resembled the Harvard Mark III for this proposal to AEDC. I don't know how he was able to go ahead on that. I don't know how anybody in the company felt. Maybe nobody was shown the proposal. What he had there was something that I believe used ERA drums for storage but for the most part had the Harvard Mark III logic and architecture. How a price came to be put on that, I don't know, but we found ourselves in a favored position. We got the job.

ROSS: Did you seriously plan to build a Howard Aiken machine?

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COHEN: Well, on visits to Tullahoma we looked over the place, saw their needs and I guess they, the customer, had ideas that were rapidly maturing, as to what they would have to do going from batteries of transducers which would be scanned as inputs to the machine. There were to be three machines for three different test facilities. There was a propulsion wind tunnel, there was a jet engine test facility, several large separate facilities. One of these machines would be used for data reduction in each facility. Not only data reduction, but they had some ideas that were probably ahead of their time with regard to the computer controlling the shape of the throat of the wind tunnel. I guess there were actually two different wind tunnels. All extremely large scale, very impressive.

ROSS: So the computer was actually to vary the tunnel parameters.

COHEN: Yes. It would control these parameters. So what we built for them was a variation on the 1101. This model was later called the 1102. The word size was 30 bits, which is just in between the 24 bits of the 1101 and the 36 bits of the 1103, and with magnetic drum storage. The circuit packaging, chassis and so forth, were essentially those of the 1103, which development was pretty far along by that time. There was no high speed memory. The magnetic drum was the only storage at the time. I believe they later modified those machines. They may have put in some core storage on site at a later time.

ROSS: Supplied by St. Paul?

COHEN: By somebody else. I don't recall who the vendor was. So that was an example of seeking out markets, and how we managed to get our foot in the door in various places.

ROSS: When were those machines delivered?

COHEN: Warren Burrell was project engineer. He can give us an exact date.

ROSS: Early '50s?

COHEN: Probably in '53. I think a lot of things came to a head in '53. ATLAS II, serial 1, was delivered in September of '53, as I recall.

ROSS: This, incidentally, is after the Rem Rand merger?

COHEN: Right. The merger became official I think in May of '52.

ROSS: What other computer projects were going on about that time?

COHEN: Another example. This might have come up a little tiny bit later. Westinghouse was the subcontractor to Boeing on the Bomarc Missile program. Westinghouse in Baltimore. They wanted a computer to serve as part of the control system for that missile's track-while-scan and so forth. A special variation, which really didn't have to be special, was a 30-bit variation on the 1103. This was later called the 1104. Frank Mullaney was project supervisor, Noel Stone project engineer. I don't know how many of those there were. One or more of those were delivered (there's an error in the Tomash-Cohen article, incidentally) to Patrick Air Force Base down at the Cape. If you look in the article, it says it was delivered to Eglin and that's not so, not the 1104. That was pointed out to me by Leo Bock after the article appeared. I remember that the Westinghouse people, after this was pretty far along, when they learned about the ATLAS II or 1103 development they said well, wouldn't one of those have really served us admirably? Well yes, but we were following your specs. There was no good technical reason why a separate development had to be undertaken.

ROSS: Well you said to me during an earlier session that you won some contracts by following the customer's procurement specifications very closely.

COHEN: Yes, and that's undoubtedly what happened with Westinghouse. So that was a 30-bit variation.

ROSS: Well it also goes well with what you were saying about not thinking in terms of developing product lines.

COHEN: Another example of a project: there was an ACM conference in the spring of 1950 at Rutgers University. It was at that conference that the acquisition of Eckert-Mauchly by Remington Rand was announced. While we were there, a couple of engineers from the Air Force Cambridge Research Center at Cambridge said we'd like to talk to you about something that you might be interested in. They told us about their need for a logic and arithmetic unit. They thought we had the capability, and they liked the way we built equipment. These people were Ted Kalin (Theodore A. Kalin) and Bill Burkhardt. That initial approach led to a contract for the construction of a unit. There wasn't an awful lot of development involved. It was pretty well a straight adaptation of a part of the 1101, the arithmetic and certain controls. We built it into a free-standing cabinet which was painted Air Force blue and delivered to be part of a very large system involving a huge radar, and so forth. These were all located up at Hanscom Air Force Base, at Bedford, Massachusetts. This was another example of the kinds of things we built and sold. Custom engineering, I think, was the term somebody used, and that was the business we were in, custom engineering.

ROSS: Would it have been hard to organize your business differently at that time?

COHEN: Well it kept us alive, and not only alive, but moving forward in terms of experience.

ROSS: Do you think at that period you could have actually tried to come up with product lines? It seems as if producer as well as consumer were pretty unclear about the role of the computer.

COHEN: That's right. I think most of us felt in our bones that, considering the very general capabilities of the computer, there was a great deal of potential. This was largely untapped, what you could eventually do with control

systems and everything else. I have a feeling that at the management levels, even management with technical

background, I think they probably didn't fully appreciate the momentum that would eventually show up in that kind

of business. One thing they did appreciate was that in order to seriously develop a product or products with your

own money you'd need a lot more capital. There were several ways to go, and as Bill Norris discovered later with

Control Data, you could sell equity without losing control, but that possibility certainly wasn't taken very seriously.

But Parker, in particular, had always made a point of bringing in people from other companies just to show off what

he had. Whether he explicitly talked to them in terms of possible merger or acquisition, I don't know, but a lot of

people were just brought in. I remember a series of meetings with Honeywell, management and technical people, in

which ostensibly the proposition was that we had complementary capabilities, and perhaps there were things we

could do on some sort of a joint basis.

ROSS: Can you be specific about what sort of arrangement was implied?

COHEN: Well what was said was one thing. What was really in mind was acquisition by Honeywell of ERA.

ROSS: They were being courted.

COHEN: Yes.

ROSS: What kinds of things did you talk about? What kind of joint projects?

COHEN: Nothing specific, as I recall, but there were presentations to groups like these, you know, tours of the labs

and a little bit of show and tell about what we had done and what we were doing to the extent that we could talk

about these things under security restrictions. At the time, we were developing experience in digital computer

techniques and in particular magnetic drum storage, and perhaps other skills which Honeywell didn't have at that time, so it was natural to show it off.

ROSS: Let me ask you something. ERA, as you indicated, at least for the very early period, was involved with a good deal of other products that either related only peripherally to computing or had nothing to do with it. One of those that we touched upon was radio communications. We talked a little bit about that. What you're saying is that ERA was really feeling its way at this early stage.

COHEN: Right.

ROSS: Do you think it could have made it without those other research areas and products?

COHEN: Probably yes, because none of them ever materialized into anything. There were some one-of-a-kind things. In the very early days (it almost seems prehistoric when I think about it), there was an adaptation of an idea of Vannevar Bush called the microfilm rapid selector. One of these was developed and built and delivered to the Department of Agriculture Library. When I say early days, I'm talking about '47, '48 maybe at the latest. This was a device where on each frame of a continuous film like 35mm film, there would be, like a microfiche, reduction of abstracts of papers or articles or whatever, and then over in another area of the frame, alongside of it, there were optically encoded dots and spots that would indicate certain key words or whatever, and then this thing would move along and scan against the code that was sought. Here, we're looking for these categories, and whenever a frame of interest came by there was a strobe light that made a copy, on another piece of film, of the selected frame. One of those machines was built and delivered and much publicized by Ralph Shaw at the Department of Agriculture Library.

ROSS: You're saying ERA built and delivered this?

COHEN: ERA built and delivered this and in the publicity by the customer there was never any mention made of ERA.

ROSS: All Vannevar Bush?

COHEN: Yes. He first described this in a non-technical article, I think it was in the Atlantic, way back, I think pre-war. We can find a reference on that. I believe the title was, "As We May Think."

ROSS: How did you come to produce a Bush design?

COHEN: Well there's a connection there in that John Coombs and John Howard, and Larry Steinhardt I believe, all came out of MIT. They were students, not necessarily Bush's, but they worked on his projects, and Bush may have suggested to Agriculture that here was a company where some of his boys were located, and they had the capability. Howard Daniels, incidentally, had project responsibility for this one.

ROSS: That's an interesting product. I'm taking what you say to mean that practically everything you offered, no matter what the nature of the customer, was produced under task-oriented research.

COHEN: Right. There was some company money put into something that never had any outside support. One of the founding group, Bob Gutterman, who had been at Naval Ordnance Lab, was kind of an idea man on many things largely mechanical in nature, and he had some bright ideas that might have gone somewhere. This is completely unrelated. He dreamed up an ultrasonic tool post holder. By vibrating the tool holder on a lathe at ultrasonic frequencies, you get a smooth cut without getting a chip, you can take a bigger cut and, in principle, complete the job more rapidly with this vibrating tool than you could with a stationary tool. And in another variation, a wire-drawing die. When you're drawing the wire, you vibrate the die ultrasonically, and in this way you might eliminate some of the stages required to draw a wire down to the ultimate size. Another one: The company listed itself in the Yellow Pages under several different categories, and I believe the Consulting Engineering category was one where they

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bought a listing. So from time to time they would get inquiries and one of them that happened, I suppose somewhere around January 1947 (This one was very early), the Pearson Candy Company in St. Paul called. They had a candy bar which still may exist called "7-Up" in which there were 7 pieces of candy. I guess the chocolate was poured over the whole thing to make a single candy bar and they wanted to make certain improvements or wanted to automate their production line. So we wound up with a contract, Bob Gutterman again, in which there was a model of a machine that would take these seven components and put them all together and put them through the steps and the process. I don't know if that was a successful job or not, as far as Pearson Candy was concerned.

ROSS: But it shows the diversity of the operation. I'm really impressed.

COHEN: Nobody knew at that time which of these things would turn into some solid line of products and services.

ROSS: I'm unaware that any other company was operating quite like that.

COHEN: I guess the feeling at the beginning was that gosh we don't know where the world is going and we'll take on anything and there were some people who would prefer to be involved in things other than the digital computer idea, which didn't grab them particularly. These people were off in other areas. Howard Daniels, the late Howard Daniels, was one of them. He did a communication job which was a task under the NAC-BuShips contract. Well, there was a technique that I think later got to be called Delta Modulation where you could make better use of the channel capacity or bandwidth by simply transmitting the increments, the changes, in the signal. If you sample a waveform instead of just transmitting the whole waveform, digitizing or whatever, you would simply indicate whether you send a +1 or a -1 to signify an up or down increment. You get certain problems with noise, but there are some benefits, and to this day that technique is applied, but with a little more sophistication. The interest on the part of the customer, that part of the Navy in this case, was that you take this digital encoding and you could do something with manipulating the signal so that you encipher it or encode it, and then unscramble it at the receiving end. So this was an example of something that was strictly in the communication area.

ROSS: Might that have been part of another task?

COHEN: It had its own task number.

TAPE 4/SIDE 1

ROSS: You were telling me about some of the communication devices ERA produced.

COHEN: Okay, let's talk about one very notable one. I don't think I can recall exactly in what year it had its origin, but it very likely was in the period before the merger with Rem Rand. From the very beginning there was an engineering group that specialized in communication projects.

ROSS: Can I interrupt to ask you who might have been associated with that division?

COHEN: I believe Bob Kalb was originally hired because of his background in telephonic communications in particular, but then he got off into other things. I can remember some of the names. Vernon Vogel was the supervisor in charge of a number of projects of that kind. That was his specialty. Walter Moe, who's still here in town, Ken Bergan, who is with Control Data now or perhaps retired, and a number of others. These were communication projects. Some of the projects they were on were tasks under the main Navy contract.

But at some point, and this is very interesting, at some point, Vernon Vogel followed up on a procurement request or a request for an expression of interest, from the Air Force, for development of an antenna coupler device. I remember another name in that group. Francis J. Biltz, who if he's still around is certainly long since retired from some other company. But Vernon Vogel wrote proposals, and pretty much on his own without anybody paying a great deal of attention to the effort, got some preliminary work going on this antenna coupler device. The device was for use in

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aircraft. If the transmitting antenna were as simple as a piece of wire, tuning the antenna optimally as you change frequencies would not be too bad. However, with the appearance of jet aircraft, antennas were beginning to appear as insulated sections of the tail cap or the wing tip or part of the aerodynamic structure of the aircraft, which meant that they had, as far as electrical impedance is concerned, a very arbitrary shape. In order to tune an antenna like that, to get a good impedance match from the transmitter to the antenna for a number of different frequencies this required a device. If you were to tune it manually this is quite a job. You spend your time tuning. So what is needed is an automatic device for matching impedance. In fact, I think they used to call it automatic impedance matching before they simplified the nomenclature and called it an antenna coupler. Vogel and the fellows working on this project stayed with it. There was competition in the field, RCA, Collins and others, and they carried this through many years, I guess, and many stages of development, all sponsored development, Air Force money, to the point where in the final qualification acceptance test, here's ERA with a qualified product! I believe about two companies' products were down to the wire. The ERA product was the sole survivor, which meant there was a pretty good market for these things. There were many variations of these because they were for different aircraft. The success phase of it came well after the merger with Remington Rand, and regular production facilities were set up for producing and testing this device.

ROSS: At ERA?

COHEN: At ERA. It had to be built into a pressurized container because there were high voltages involved at high altitudes, not a trivial thing. Pretty good price tag on it too, as I recall. Production facilities were set up at one time at a separate rented location on University Avenue. For a number of years it turned out to be what you would call a bread-and-butter item. Here was something that had a continuing market and it held up very well. I guess where it ties into the present discussion is that it had its origins back in the ERA period but the success of the product went way beyond that up until certainly within the last 10 or 15 years.

An interesting thing is that Vern Vogel was a little bit disappointed at some point before all of this success was achieved. It was technically successful but he didn't have the patience to stick it out. He didn't think that the company was moving in that direction, and his interests were along communication lines, so he left. Whether it was premature or not it's up to him to say, but he went to Motorola or someplace. When I last heard of him, he was with whatever company is out in Fullerton, California. My memory needs jogging as to what company it was. He left ERA, and his baby became really a good money maker and supported the company through rough times.

ROSS: Did the coupler bear any connection to the rest of the work we've already talked about?

COHEN: No. It was an electronic control device for a communication application.

ROSS: So it was considered to be a completely separate...

COHEN: An isolated product. One more thing: When the Boeing 707 came out, you remember the distinctive way that you could recognize a 707 was that the tail had the antenna, it had the point sticking out forward. That was the antenna, and mounted in that tail section underneath the antenna on every 707 that flew was an ERA antenna coupler. We didn't have a very good PR department at the time because when the 707s back around 1960, began flying transatlantic flights, every company that had something to do with the 707, such as Honeywell with the electronics and all that, really took advantage of that as a publicity opportunity, and played it up big. By that time we were part of Sperry Rand.

ROSS: You didn't take full advantage of the opportunity?

COHEN: I suppose Sperry navigation equipment must have been publicized, but the ERA antenna coupler somehow was not. I remember asking about that.

ROSS: Why should it have been publicized?

COHEN: Well, it was not publicized. Why should it have been publicized? I think it was important to do everything you could do to show that here, by gosh, was an internationally marketed and successful product, made right here, like so many other things.

ROSS: So purely for public relations.

COHEN: Yes, and establishing a company image, a pretty solid image. Whether that image transfers over into other product areas, that's probably behind your question.

ROSS: Yes.

COHEN: Well, IBM gets into medical applications and things like that. Sure it's all part of a big complex image.

ROSS: You mentioned some other devices. Did the company ever deal with analog devices?

COHEN: Yes. There was an analog recording technique, and I'm sure it was patented, or at least patent applications were filed. Howard Daniels and others developed a technique known as boundary displacement recording. And for getting a very accurate reproduction of a wave form, what happened was roughly this: You had a magnetic head of a particular design which I would have difficulty reconstructing right now. Sid Rubens might be able to. Think of, say, a quarter-inch magnetic tape and an invisible wave form running down the middle of it. It was invisible but, on one side of this wave form the tape was magnetized in one direction, and on the other side it was magnetized in the other direction, to saturation, so the recorded signal didn't depend on the magnetic characteristics of the medium at all, but on just where that boundary was. There was a magnetic head design that was able to produce this variation. Playing it back it was a simpler proposition because the total flux picked up would depend on the position of that boundary. I

don't know where that was sold. It might have found a few applications in that form, but one in particular brings us around to something that I'd almost forgotten about.

There was a contract that the company undertook which had the code name of Boom. Project Boom. This was a project to produce instrumentation for a large underground explosive test, or test series, at Dugway Proving Ground in Utah. What it obviously was, in retrospect, was instrumentation for handling what later would be nuclear underground testing. This involved the procurement of, or development of, or supervising the development by subcontractors, of various things. Cable that would withstand that kind of an environment, sensing devices, and one of the devices was an accelerometer that would give you a waveform of what went on wherever you planted this accelerometer. This had nothing electrical in it. There was a slowly moving spring-driven drum whose rotation was triggered by a mechanical shock. There was a mass attached to a permanent magnet adjacent to the drum, which had a magnetic surface that was magnetized in one direction. The permanent magnet moved back and forth parallel to the axis of the rotation, and would lay down a boundary-displacement record of the waveform. These were made in quantity, and apparently worked very well.

ROSS: Do you know the period of production of that device?

COHEN: Possibly in the late '40s.

ROSS: Well, again it shows the diversity of the company.

COHEN: Yes, and that was a large project. A lot of people were on it. I think Byron Smith must have been on it when he first came to work for the company so, if you get around to meeting him, ask him about it. This was Project Boom, and Army Ordnance was the sponsor. They had a large field crew that was out there in Utah during these tests. I remember there were pictures. I remember a framed picture of ERA management people, including John

Parker, at the ground breaking of a field station out there in Utah, all these people standing around. I think the shovel was saved; the inscription on it memorializes the event.

ROSS: Did the company produce any other analog devices?

COHEN: Something just occurred to me. I think a model of this boundary displacement recorder was sold for recording power-line transient faults. You have one of these things that's sitting there, and there may not be a fault that comes by, let's say, for a year and a half, and then suddenly there is one and you want to have taken a picture of it. Well, this is a recording thing that somebody expressed interest in as an application. There were other analog devices, under the heading of analog-to-digital converters.

ROSS: For instrumentation?

COHEN: For instrumentation purposes. There was a shaft-position indicator that was sold. I think one customer must have bought a number of those but it was an expensive way of getting an accurate indication. The device would produce a digital count, a digital signal which would vary with the angular position of the shaft, but there are easier ways of doing that today. As for analog devices, analog computers as such, I don't know of anything of that nature. There were things that involved control systems and servomechanisms. But I don't know of anything very significant in the way of analog devices.

ROSS: Tell me a little bit about the raid of ERA personnel conducted by Remington Rand in '49 long before it acquired ERA. Who went, and why was ERA a very attractive acquisition?

COHEN: Yes, that was after General Groves was hired by Remington Rand to head up its research laboratory.

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ROSS: That's an interesting incident that McDonald and I talked about. They clearly saw him as one competent to organize a high technology operation on a large scale and what did he do? Did he conduct raids of other corporations as well?

COHEN: I don't know, I don't think so. I got to know him later after we were in the family and could tell you some stories about that, but that's another chapter. That comes under the heading of things we did for Remington Rand while they were trying to figure out who we were, or what we were. What they had bought. About the raid: Some of the original founders, quite a group of them were hired away. Names, Hugh Duncan...

ROSS: He was a technical man, wasn't he?

COHEN: Yes, one of the founders.

ROSS: He had been in the Navy?

COHEN: With CSAW, and I believe Dave Noble was among them, and Lou Chaloux. Louis Y. Chaloux.

ROSS: He again joined very early.

COHEN: He was a founder, these people were almost all founders. Don Ammerman, and Harry Zimmerman. I think he was one of the founder group but at least he was with Rem Rand later. I don't know if Cliff Olofson was among those that went to Rem Rand. He may have been, but it was basically the real core group of the founders, some of these people were technical supervisors at ERA, and they all up and left, or were lured away.

ROSS: What was the inducement?

COHEN: Well, they left at a time when the continuing support of ERA by the Navy was in doubt. That wasn't too visible to those of us who were a little lower on the chain. I guess the Navy at one point did trim back and there were some layoffs. That happened to coincide in time with the Rem Rand recruiting exercise. I don't know if Groves had the benefit of knowing of this or not, but it's possible. If a company is in a hiring mood and hears of a cutback in another company, you know, they move in for the kill. So I they must have had a few people come down and sit in a hotel room and call these people to come down for an interview.

ROSS: Had you talked to any of them?

COHEN: The people who were leaving? A few, perhaps. You remind me that another was Al Fenaughty, who now heads up a company called Information International out on the West Coast. He was not one of the founders, but an early engineering employee.

ROSS: Later on after the merger of Rem Rand and ERA, did you ever come in contact with those people enough to find out whether they thought the move had been good for them, to leave ERA?

COHEN: I knew them, sure, but I don't recall discussing that aspect. There were other things to talk about.

ROSS: More exciting things.

COHEN: Because you see, they had a ringside seat on the inside when Remington Rand then later acquired ERA. There was a fair amount of talking and speculation about what went on, why Rem Rand was motivated to buy ERA. I never got a clear story on that.

ROSS: That's too bad.

COHEN: That's a good one to pursue.

ROSS: I'd like to.

COHEN: Some of those people are around. I think I mentioned to you and Arthur that one of the men, I believe he was on McDonald's list too, Art Draper, Arthur F. Draper, should be interviewed.

ROSS: Marketing for Rem Rand.

COHEN: Well, various things, I think he's got to be retired by now, but most recently, perhaps in the sixties, he was branch manager up at Bridgeport for the Rem Rand product line. He had gone through various stages of management, and then wound up running a regional branch. That was his job when I last saw him.

ROSS: How did the Rem Rand raid affect ERA?

COHEN: Some of us moved on up to fill in some of the vacuums that were created.

ROSS: Including you?

COHEN: Yes. We had plenty of talent around there. Whether it was viewed somewhat as a disaster or not I don't know. I suppose management wasn't too happy about it, but I don't think any of the work suffered.

ROSS: Well, certainly from the projects we've discussed it wasn't devastating.

COHEN: No.

ROSS: When did you first hear of the proposed Remington Rand acquisition of ERA?

COHEN: December, 1951. Bill Norris shared the information with some of us at that point.

ROSS: What was your immediate reaction and the reaction of your colleagues?

COHEN: Well my reaction was one of dismay, as I recall, because among the companies in the field that one might consider being associated with, if any...

ROSS: That's a good point.

COHEN: If any, that's very important. Remington Rand certainly was not high on my list. Mine or any other engineer's.

ROSS: You've already said Honeywell was, potentially.

COHEN: Only in that there were meetings.

ROSS: They showed interest.

COHEN: Well, not openly. John Parker arranged meetings so that management and technical people in small groups could get to know each other. I think we made little presentations to them as to what we were doing, things of that nature. No mention whatever in any of these meetings of any possible merger or acquisition. That was only left to your own speculation.

ROSS: My impression is that Parker forced the acquisition on the company.

COHEN: Probably.

ROSS: Did that breed some resentment at all? My impression is that it didn't, but people were apprehensive.

COHEN: Reading between the lines, yes, I think some people were a little resentful of it. Of course, one has to consider the, what would you call it, the venture capital climate of that particular period. It was largely non-existent and I recall that Bill Norris, in explaining this move to us, said the company needed more capital and there's a limit to the amount that you can borrow. If you go after equity capital you lose control. I guess he discovered how to do that without losing control later. So that option was kind of dismissed. A third possibility was through merger with a well-established company.

ROSS: What was Parker's object in trying to recapitalize this company? He must have had some plans.

COHEN: I can't answer that question because who knows whether his main interest was to enjoy a personal capital gain and get out, or whether he thought some sort of plateau had been reached in what this company could do? He may not have perceived the real potential of the business that was beginning to emerge, the new computer technology. I don't think many of the management people around had an idea of the real potential.

ROSS: You've said that before.

COHEN: Those of us, the technical staff, engineers, who in the great innocence of no experience in the financial world, didn't think too much about that side of it, but certainly recognized that the field of application had tremendous potential. The application of the computer technology that we were getting in on the ground floor of.

ROSS: Could he have been looking forward to actually establishing product lines? I know that people within the ERA organization had actually proposed that to him.

COHEN: Yes, but it may be that he recognized that as just being out of the question, at the time. I guess he'd be right.

ROSS: If anything changed after the Remington Rand merger, what was it? What changed and how do you assess those changes?

COHEN: Well, we found ourselves part of a larger family, but family might be too cohesive a word. You see, there was a period from what you might call the announcement of the engagement, and the time at which the marriage became official, which I think was along about May of '52, when all the government agency approvals were in. But during that period we went through a get-acquainted period. Meetings, visits back and forth with Philadelphia and Norwalk, and with various men who were the top management in Remington Rand at the time.

ROSS: Can you attach names to those personalities?

COHEN: Gosh, I can see faces but I've got to work on the names a little bit. There were some very pleasant people. Bill Butler here in town would know some of these people. I don't know what role he played, but he was involved in arrangements with some of these people and their visits, so he got to know some of them very well. I recall, in particular, Beverly Bond and Albert Ross. One, in particular, who was not really top management was Arthur Draper. I've mentioned him earlier. He seemed to be sort of a liaison kind of a management person between the Remington Rand management and the Eckert and Mauchly people. And he had played some sort of detailed role in the acquisition of Eckert and Mauchly.

ROSS: Were you looking forward to working with Eckert and Mauchly people? I don't mean that as a leading question.

COHEN: I don't say that we're looking forward to it. It was sprung on us. You know, we were taken quite by surprise when that whole thing happened. But then once the merger was under way, no, I can't say that we looked forward to it. I think we were interested in seeing what might come out of it, sort of an unknown quantity.

ROSS: It seems that that's the perspective Remington Rand management took as well. Both Eckert-Mauchly and ERA were unknown quantities to some degree.

COHEN: Yes, I agree.

ROSS: What changed after the merger came about in May of '52?

COHEN: Let me make an observation that I think applies to that whole period and right up to the end of '52, because I can put certain events on the calendar. This was an observation that I made over coffee once to Erv Tomash and some others, that here the Remington Rand management people had never apparently asked what we were capable of, just what we were doing. They were always dreaming up things and saying would you like to do this, would you like to do that? And I likened it to a game of "twenty questions" in which we were only permitted to answer yes or no, but not amplify anything about our capabilities. And that persisted right down to about December of '52, when we managed to make this presentation to them showing that we had a very powerful, competitive machine all developed and ready to go, developed with government money, and which at that point we were ready to call the 1103. But there hadn't been any presentations. They were asking us. We made presentations only in response, when they said how would you like to build this or build that?

ROSS: What kind of suggestions did they make?

COHEN: Well, one in particular was the one that led to a tape-to-card converter for the Univac system. We were invited to propose how we would do this. That was almost like an Alice in Wonderland experience. Some of the Eckert-Mauchly people were along at the presentations, and one of the things that we were supposed to get from the company was a card punch. This tape-to-card converter was not to produce the so called 90-column cards, the round holed Remington Rand cards, but to produce what most people were calling IBM cards, the 80-column card. Now Remington Rand's only source of such equipment, of 80-column card equipment, was the arrangement they had with the Bull Company in France. And the Bull readers and punches, and perhaps other things, were being marketed or planned to be marketed, I guess, in one way or another, by Remington Rand in this country. They showed us, on one occasion when they first asked us if we thought we could develop a tape-to-card, they showed us a couple of these 80-column card punches. They were quite different models, both made by Bull and, this Alice in Wonderland thing, the man heading up the Remington Rand group in these discussions was none other than General Groves himself. I don't know if it was he who made this memorable statement or somebody else, but somebody said, "Have they seen the Bull punch yet?" "Well, we can't show them that." You see, that was kind of a confidential thing. "We can't show them that until they tell us which one they want to use." These may not be the exact words, but that was the general gist.

Do you mind if we digress from the tape-to-card a bit? I'd like to include a little about the atmosphere in which Remington Rand management operated in the first year or so of our association.

ROSS: O.K.

COHEN: The company headquarters was at Rock Ledge, the beautiful mansion in Rowayton, near Norwalk. If you were at a meeting at Rock Ledge as opposed to the laboratory at South Norwalk, you would have a chance to join management people in their served lunch, very elegant. They had their own chef on the premises, and around this table they would do a lot of lunch table inventing, and it was always in the context of putting things together that they knew about. There was the 409 series of electronic calculators. What if we put a magnetic drum on a 409 calculator? Or, what if we put this or that printer on? Mr. Rand and others would engage in this kind of inventing.

ROSS: But it was always within the context of their previous experience in office machinery.

COHEN: Yes, but without taking the trouble to visit a laboratory or to go anywhere to learn something. Or more significantly, to delegate this function.

Another aspect of the Rock Ledge meetings: I remember that Mr. Rand brought in General MacArthur as Chairman of the Board at what must have been the annual meeting in the summer of '52. So after that point, General MacArthur would be seen from time to time at these lunch sessions at Rock Ledge. I never had the privilege of being at one of those. Jack Hill did, and perhaps a few others from out here, but as I gather, the pattern was that somebody would say, "Well General, what do you think?" And he'd take a couple of drags on his corn cob pipe and he'd say, "Well, that reminds me of the time I was..." and then he'd tell some sort of war anecdote not having anything to do with the question, but that was his charm.

ROSS: And he did perform in international functions, in terms of Rem Rand marketing of office machinery, especially in Japan.

COHEN: I really don't know much about that.

ROSS: But I certainly understand where you're headed.

COHEN: I think the general impression that we had in what I described earlier and even in this last anecdote was sort of a vast isolation on the part of the then management. Although some of the senior men in management had come out to St. Paul to visit us, and had a fair idea, old man Rand never took the trouble. I guess he went to Philadelphia on occasion, but not once did he come out to St. Paul to see what he bought. And that's symptomatic of the isolation.

Here was a man that was obviously in a decision-making position, but not providing himself with all the inputs that he really should have had.

ROSS: It's my impression that ERA was able to benefit in some ways from that isolation. For example, you continued to deal directly with government agencies.

COHEN: Yes, the general pattern is for government agencies who are interested in procuring developmental services or equipment would advertise the fact. They would have a list of potentially interested companies who would be sent these requests for proposals, or requests for expressions of interest, if it's for a rather complicated thing, then that pares down their mailing list for the eventual RFP, the request for proposal.

ROSS: Were you, ERA, always dealing directly with the government or sometimes through Remington Rand?

COHEN: Usually directly. On occasion, I suppose there would be an opportunity to do something through company headquarters. I guess what you're asking is whether they had sort of an approval or veto authority over what we were bidding on, and I would have to guess at that--I would guess on commitments, on bids that were very large, above a certain dollar level, and I think this is true in most companies, below a certain dollar level you handle things on a local level, local management, but once they exceed some level, you get the participation of upper management.

ROSS: Let's get back to the tape-to-card development.

COHEN: O.K. So we said we'd go back and think about it. We being myself and Warren Burrell. I don't think there was anybody else along on this trip which was early in '52. We said we'd go back and think about it. It so happened that we had just gone through an exercise that I may have described to you earlier, or in any case it's described in abbreviated form in the Tomash-Cohen article. The exercise was a paper design for IBM, and this particular design for a computer had a way of getting input information off of 80-column cards and output information back on to

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80-column cards with intermediate magnetic drum storage. It seemed to us that since we had gone through the thinking on that and we certainly had the rights to it, even though a patent application was in the process of being written up for IBM, we would use that technology for a quickly approach. So here you have information coming off of a Univac tape and going into buffer storage, and then coming back out to cards with some options with regard to rearrangement of data, or whatever, on the way through. So we wrote up the thing in the form of a proposal. We were to abide by certain rules, as spelled out by our management, however.

ROSS: Your management at ERA.

COHEN: Bill Norris. We were told the rules of the game at that point, since we were not yet part of Remington Rand and there was always a possibility that this might not go through. We were to act just like an outside vendor bidding on something for Remington Rand, and therefore we should do our cost estimating and so forth just as though it were for an outside customer and make sure that we had a realistic cost estimate to go with it. So then we went up at a later time, to the Norwalk Lab, and presented this thing to General Groves and an audience that consisted largely of our old friends who had left ERA back a couple of years earlier, Hugh Duncan, Lou Chaloux, and others. This was to use a Bull card punch. I remember in starting the presentation, I said this was something that we could quickly put down on paper, and that it wasn't terribly sophisticated. General Groves stopped me on the word sophisticated. He said, "What do you mean by that?," and he kind of ridiculed my use of the word. We had the feeling that the Eckert-Mauchly people didn't exactly like the idea of our doing this job for the Univac line. And now this apparently negative reception by Groves was likely to kill the idea altogether. But to our surprise, a short while later they said okay, go ahead, build it, we'll buy it. Ward Lund was assigned as the project engineer to develop and build one copy of this device. We took some subsequent trips to Philadelphia to get more about the tape unit and the Bull punch, and so forth. So you see, there was interaction with Philadelphia and with Norwalk on that one. It's typical of how things were beginning to shape up in the Spring and Summer of '52.

TAPE 5/SIDE 1

ROSS: One group within Remington Rand that we haven't touched upon was the tabulating machinery group. What associations did ERA have with them, if any? Or what was your impression of the work that they were doing?

COHEN: We were shown on our visits to the Norwalk Laboratory the then-current models of the 409 series. I can't recall the various suffixes. There was a 2, and a 2R, things like that, and these had used vacuum tube circuitry, and punched card input and output, with a certain amount of plugboard programming that was typical of much equipment of that period. For example, the IBM 604 was programmed in that way, too. Do you mean what we thought of the quality of it?

ROSS: No, not so much the quality of it. It's been my impression that people in the history of computing have concentrated on two rather than three divisions of Remington Rand, as they relate to computers, for the history of information processing and the two divisions they concentrated on have been ERA and Eckert-Mauchly. But the tabulating people were also active, and it's my impression that they actually proposed that ERA produce a file computer.

COHEN: A little further down the road, yes. At some later time, Bill Keye looked into a possible special purpose device for the Northwestern National Bank in Minneapolis that would use magnetic drum storage to keep track of transactions and account balances in savings accounts. He went through a number of variations on a particular design. There were presentations to some of the bank people, and I believe that was sort of a mutual educational experience both to the customer and to the ERA people involved, both sides recognizing that perhaps life isn't as simple as they thought. And then at some point, to add greater computational capability, logical processing capability, the thought was that if you took one of the current 409 models, and somehow added magnetic drum storage, that you might have a useful product for the business world, and for the banking community in particular. Bill Keye was involved, and others by that time I suppose, and the design evolved so that a series of models of what became the File Computer was set up. There was File Computer model zero, and then other models that followed.

There were also peripheral things such as rather cumbersome buffered tape units where a whole buffer storage cabinet just operated one tape unit. I think the technology, even at that time, would have indicated that several tape units could have been controlled and buffered through a single control. Some of us in the company viewed this as a manifestation of one of the problems that we had at ERA, even at that period, of compartmentalization within the company, not making use of the best technical information available within the company. The cross-fertilization of projects, if you will, could have been better. That's my own prejudiced view.

ROSS: Do you think that problem stemmed from the task-oriented research that you'd done in the past?

COHEN: Very definitely, yes.

ROSS: It's a hazard of that kind of contract.

COHEN: Right.

ROSS: So what role did the tabulating machine people play in that whole story? Did they market the devices for you?

COHEN: Well, by that time there were some marketing specialists, analysts, who were based in New York. They knew the application fields, the needs of the customer community, whether it would be insurance companies, or banks, or whatever. These people were very good constructive critics, and indeed were responsible for, I suppose, many of the design features, many of the properties of the business-oriented product line.

ROSS: Helping you to define the parameters.

COHEN: Yes. The file computers, I believe, sold quite well for a while. They probably could have done better, but they were in competition with IBM in particular, and it was a pretty rough environment. The guiding hands in shaping the characteristics of the File Computer were a couple of people, Art Geilfuss, and for a time an older man was with him, John Zentgraf, and these people were the experts on the application of tabulating equipment to the problems of the business world. As long as we've talked about them, I wanted to get their names in.

ROSS: Off the record you told me a story about some fingerpointing that went on within the Rem Rand management concerning ERA's understanding and representation of the Univac I. Could you rerelate this for me?

COHEN: I think the main thread of what you're seeking here is some examples of a form of competition or rivalry that began to develop fairly early between ERA and the people in Philadelphia. From my completely impartial vantage point, I might say that during that first year of our relationship, 1952, they may have regarded us as a little bit of a threat. And some of these matters that I'll relate may bear that out. Just to bracket the timeframe, the acquisition of ERA by Remington Rand, Inc., was announced in sort of a limited way in December of 1951 and the necessary approvals were completed several months later so that the acquisition became formalized in, I believe, May of 1952. From early 1952 on there were visits back and forth by technical people. Some of us visited the laboratories in Philadelphia, became acquainted with the people there, and some of their people visited St. Paul, so there was a general familiarization process going on.

During that particular period, ERA, in addition to its principal Navy contracts which have been described in several places, was busy submitting proposals to various government agencies and in some cases successfully, so that there were various jobs, many of them relatively small, that were being done for government agencies. One example was a series of design study contracts. The initial contract was for a study, which was later renewed and expanded, where the customer was a group based at Wright Patterson Air Force Base in Ohio. These people were primarily concerned with fire control systems, airborne fire control systems, where the technology up until that point had been analog control systems. These people had felt that they should be looking into digital techniques for the purpose, and that

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was the subject of an initial study contract for that group. A lot of what we had to do was to educate them. Every time we visited them they'd call in a number of their people and we'd conduct an informal seminar at the blackboard as to what some of the basic concepts were in making up a digital computer. That work must have started early in '52. By the fall of '52 our sponsor, that particular sponsor, felt that we were giving them a little bit of an education and indoctrination into digital techniques, and perhaps we should get the feel of what else was going on under their sponsorship in the fire control area. So they urged us to send a few people on a little visiting tour to some of their contractors on the West Coast, and they arranged visits with several places. We visited several companies, of which I think only some of these were actually contractors to this particular fire control group in the Air Force, so I may not have the companies properly categorized. But the visits were arranged and a couple of days before election day 1952, Byron Smith, his full name is Byron D. Smith and Wally Moe, Walter J. Moe, and I, the three of us set off on a railroad trip to Los Angeles. We arrived there the morning of election day of 1952. We visited North American Aviation at Downey, California. I recall that one of the persons that explained the Airborne Fire Control System that they were doing for the Air Force was Lester Kilpatrick, who was later the founder of CalComp, or California Computer Products. Another company was Hughes Aircraft at Culver City, California, who were doing, I think, two projects for the same sponsor. One of them was called Digitac, which was not only an airborne fire control system, but it was an airborne digital computer performing, or intended to perform, a number of functions, only some of which were airborne fire control. There may have been some navigation and perhaps other data reduction functions involved. I remember our principal hosts on that occasion were Dr. Eugene Grabbe, Eugene F. Grabbe, and Dr. Eldred C. Nelson. Both of these names are well known and became even better known in the industry later. I think there was a brief visit to Douglas Aircraft at Hawthorne or one of the other suburbs adjacent to the Los Angeles Airport, and I don't recall any particular people there. In addition, we did visit some other places.

One was Northrop Aviation at Hawthorne, California. Our host there was Rex Rice. That was my first meeting with Rex Rice, incidentally, and we run into each other occasionally every few years. Rex at the time ran the computer center for Northrop and we discussed their particular manner of using the IBM Card Programmed Calculator, the IBM CPC. He also showed us the BINAC which had been delivered to them by Eckert-Mauchly. The engineer who was

trying to make the BINAC work was an old friend of ours who had been an ERA employee, one of the founding group of engineers, named Jim Sprong. He was called Jim Sprong, but his full name was Dorval C. Sprong.

ROSS: Let me ask you a little bit more about BINAC. Northrop would have had BINAC now for a couple of years and you said the man was trying to make it work. What was your impression at that time about how successful that project had been? Were they impressed with the machine?

COHEN: Impressed in a certain way, not necessarily favorably. I may be quoting Rex Rice, but I'm certainly quoting somebody out there, who said that Eckert-Mauchly at some point decided to deliver it to the customer and it was up to the customer to finish building it. And I have a feeling that if you asked Rex Rice about that today he would probably agree that that was not only his own feeling, but that of his colleagues as well. Sprong was trying to make the thing work. I don't know how far that particular effort was carried. However, that was just incidental to our trip. We were certainly not there to solicit opinions on what other people thought of our new relatives or anything like that.

ROSS: Oh certainly, I understand that.

COHEN: We visited Lockheed at Burbank. You're probably familiar with that airport, which used to be called the Lockheed Air Terminal. The principal Lockheed plant was there. We were introduced to people in the computer center, and one of our hosts was Julian Goodpasture. Some years later he came to work for Univac in the software area. They had their particular internally standardized way of setting up the plugboards for the IBM CPC. Apparently everybody that had a large computational load for engineering purposes, certainly in the aircraft industry, what is now called the aerospace industry, everybody in that field had their computation computer centers set up around the CPC. So we had seen Rex Rice's version of the CPC, with his adaptation of the plugboards, and also the Lockheed approach. We may also have discussed the CPC usage with the Douglas people at Hawthorne. I want to stress that this was not the Douglas plant at Santa Monica. This was the Hawthorne plant in the airport area.

Incidentally, Douglas at that time was set up so that all of their Navy development and manufacture was at one location, Air Force was at another location, and so forth.

ROSS: So you were visiting the Air Force installation.

COHEN: They may have been one of the contractors that we were sent to visit. My memory is hazy on that one. I have a feeling that it was Rex Rice who encouraged us to see the CPC installations at Lockheed and Douglas. The Douglas visit must have been fairly drab compared with some of the other places. I don't recall very much about that particular one.

Going back to the visit with Rex Rice, Northrop apparently had not encouraged some of its engineers on some of the innovative things that they had been doing. It's awfully easy to pick up and go out and start a company in California. There was one such group, Floyd Steele at Northrop and some of his friends, who had designed a really novel idea in a computer. It was called the MADDIDA, which stands for Magnetic Drum Digital Differential Analyzer.

TAPE 6/SIDE 1

COHEN: The MADDIDA has been described in the literature. It was the equivalent of an analog differential analyzer of many integrators, except that each integrator was in effect a register at an addressed position on a magnetic drum. So there were arithmetic circuits that caused these registers to be incremented according to certain rules, and the outputs of these integrators could be combined in various logical ways, so as to be exactly equivalent to what goes on in a large differential analyzer, for the purpose of providing solutions to differential equations. In particular, those differential equations might relate to control systems. Come to think of it, that may have been the primary reason for our visit, because they probably had Air Force money in that original project, but Floyd Steele and others, I believe, were either in the process of leaving, or had just left, to start a company of their own, which we did not visit.

Through the persistence of some of the Northrop people, we became a captive audience and were driven out to visit one of their former colleagues at Rodondo Beach, California. Well, Glenn E. Hagen, a former colleague of theirs at Northrop had started a company called Logistics Research, Incorporated, and he had among other things a new approach to the design of a magnetic drum, in which he avoided expensive machining. He was able to make the drum out of sheet metal, with a seam welded together, just ground smooth. He got away from the dimensional precision required, through the use of pneumatically supported magnetic heads. I believe he may have been the first to do that, and he does have, I think, dominating patents in the field. Hagen told us the story of how, on a cruise, he had met Axel Wennergren, the Swedish industrialist, the Electrolux man, and Wennergren became very interested in some of Hagen's ideas, about this concept of a computer, and ultimately provided backing. So in honor of Axel Wennergren and his funding of Hagen's company, Hagen called his computer the ALWAC. I think he had a couple of designs, ALWAC I and II perhaps. I don't know how many of these he sold, or where Hagen eventually wound up, but I don't think his company lasted very long. In this visit we were spirited out to his company by his friends, and we had no say in the matter. We were never shown any equipment. He had a storefront operation, and we visited with him at some length in the outer office. We never went into the back room, but we heard all about his plans, and how his drum worked, how his machine worked and where his backing came from. It was very interesting. Well, where does all this lead to? Simply that it was a very educational trip, and we learned what many people were doing. This is what our Air Force sponsors wanted, and this was an important input mechanism for ERA.

ROSS: How long was the trip?

COHEN: About three days. We lined up maybe three or four appointments per day. Oh, I remember another company. Whenever we realized we were geographically near somebody, we'd call them up. In one case, we called from a phone booth outside of the Lockheed Air Terminal and confirmed that we were not too far from a company called Telecomputing. They had been advertising a digital plotter. We were interested in digital plotters for some of our Air Force contract work. This was before we got the hang of renting cars, so we waited on the street corner and somebody from Telecomputing came down to pick us up and we visited them. The president of Telecomputing at the

time, as I recall, was Bill Bell, William G. Bell, who later wrote a book about business applications, published by McGraw-Hill. He had a lot of photographs and descriptions that he got from ERA. As I recall, Telecomputing Corporation was his company, or he was one of the principals in that company. That was one of the companies we visited.

ROSS: You were going to mention one additional.

COHEN: Yes, because of some other Air Force work, we were interested in on-line data reduction. We had made arrangements in advance to visit the Cooperative Wind Tunnel, which was a joint operation sponsored by aircraft companies who were otherwise competitors of each other, and operated by Cal Tech in Pasadena.

ROSS: I'm not aware of it.

COHEN: I'm sure it no longer exists. It would be obsolete. I recall the day we visited North American in Downey, California. The official host, the PR department or whoever it was, gave us a company car and driver, and told the driver to take us to Cal Tech in Pasadena. Well, the Cooperative Wind Tunnel was elsewhere in Pasadena, but this fellow wanted to let us off at the main gate of Cal Tech. He said those were his orders, but we conned him into taking us the extra couple miles over to this other place. Just to give you an idea of the state of the art at the time, the equipment we saw, and this was November '52, the equipment we saw at this Cooperative Wind Tunnel was various lash-ups for taking the readings off of manometers and the things that one has in a wind tunnel, sensing things of various kinds, and transferring that information to punched cards, going to a card punch at certain intervals bang, bang, bang, and data were punched out on cards. The cooperative part of the wind tunnel was very interesting because these fellows were kept very compartmentalized. I remember they had these large curtains, just like curtains around hospital beds where you have a number of beds in one room, except just visualize a huge room with large curtains separating various areas one from the other. That was the way things were being done in 1952.

Okay, perhaps I've given you a little bit of the flavor of the educational process that we were being exposed to, which was very educational for the three of us. From these conversations, and from what we were shown, we were learning a great deal, and perhaps this is typical of what ERA was doing.

Well, I guess one of the things that this leads up to is that the following week, when we were back in St. Paul, we learned a lesson. We had committed an error of protocol. Here we were new members of a larger organization, and although we had our own contract and sales or marketing operations for our own particular kinds of contract work with the government, I guess it never had occurred to us that we were now part of a larger organization where there really was indeed a nation-wide network of marketing people. The Remington Rand marketing organization in Los Angeles had gotten wind of the fact that there were some fellows from ERA that were visiting a number of their customers and potential customers. They had no idea, really, of what we were up to but they naturally assumed that we were up to no good. So Bill Norris in a few days had heard about it from topside. The word got up to Remington Rand top management and somebody, one of the vice presidents in charge of marketing or whatever, had called St. Paul and complained to Bill Norris about this --what would you call it--contraband or illegal selling operation. Bill Norris at the time seemed to be a little bit shaken by this, and for those of on the trip it seemed so absurd we couldn't take that seriously. It was kind of funny. I just said to Bill Norris, "Bill, do they want my resignation now, or later?" That seemed to clear the air, and we all agreed how absurd an allegation it really was.

This next item gets into another important subject area, but in a sense it's a continuation. Among the ERA activities going on at this time, l952, was the development of a computer under Task 29 of the BuShips contract (incidentally BuShips is the standard abbreviation for the Bureau of Ships). Under the BuShips Contract, Task 29 was for the development of what was called ATLAS II.

ROSS: We've talked about that.

COHEN: Have we? Fine. Also along about that time IBM had already I believe announced, at an ACM meeting, something that was originally called the Defense Calculator and later would be called the IBM 701. And we realized while we were developing Atlas II that we had a system that in many respects was a much more powerful machine than the IBM 701. Furthermore, it was developed with government money and if Remington Rand wished to put a few more dollars into development they would have a commercial product that would be highly competitive with the IBM 701. This was something that we began to think of, by we I mean those of us on the technical level in St. Paul. I have already told you about observing in one of these bull sessions that Remington Rand management kept on asking ERA "could you build this for us, could you do that, could you do this and that and the other thing?" without ever asking us what we really were doing or were capable of doing. It was very similar to a game of "twenty questions", where all that we seemed to be able to do was to answer yes or no, or yes, we'll try, or whatever.

ROSS: You have already said that to a large degree most of the projects they proposed were well below your abilities or your capabilities.

COHEN: Probably so, yes. Some of the people thinking about this were Erwin Tomash and Gordon Welchman, who was not an employee but a consultant to the company at the time (brought in by Howard Engstrom), and very well versed in state-of-the-art programming and computer architecture and what not. He had spent some time at MIT, and he had worked closely with Charlie Adams when Adams had been at MIT--Charles W. Adams. Gordon Welchman had done some scratching around on paper as to what the actual speeds and capabilities of ATLAS II might be in certain kinds of problem situations, and the more he tried these various examples the more everybody was impressed that we really had something there. So what developed was a plan to make a presentation, if we somehow could get an audience, to Remington Rand management, and give them a sales pitch and comparison of the capabilities of the 1103, as we suggested it be called because we already had an 1101 and I guess we were talking about 1102 being assigned to the data reduction devices that I think I've already talked about for the Air Force at Tullahoma.

ROSS: You have, yes.

COHEN: So it was suggested that we call this the ERA 1103. The people involved most directly were Erwin Tomash and Gordon Welchman and myself, and we had a lot of support from the home base. I remember Bill Drake and Larry Reid were involved in getting up the supporting material.

ROSS: Being on the technical writing staff.

COHEN: I think by that time Bill Drake was involved in preparing materials for marketing purposes, whatever they were. Larry Reid was a senior mechanical engineer. But I remember one of the things that came out of this was a wooden model, a scale model showing cabinets and things. As a model, it probably was not very impressive--a lot of little blocks representing cabinets. But to create an image of tangibility, this was included as part of the show. Then we had some large flip charts, black on yellow paper, in which, among other things, there were side-by-side comparisons of the various properties--word size and memory capacity and speed of this and that, of the IBM 701 versus the proposed ERA 1103. There was a heavy bullet showing which column was superior on each of those items. That was very impressive. I'm not sure how the meeting came to be arranged. Of course, Norris was with us, and what started to be put together in St. Paul was not widely known. Some of the people from our Arlington office, the late Howard Engstrom, and Hank Forrest, who is now with Control Data, were involved. As far as they and the people in Norwalk were concerned, this was to be a capability presentation. I guess what was requested was the opportunity of giving sort of a little run-down, sort of a show-and-tell, on the various things that ERA had done and was capable of doing.

ROSS: Who might have been responsible for requesting that? It had to be somebody who knew of your military and government contracts, for instance.

COHEN: Well, no it came from ERA management, whatever the channel was? -Parker, Engstrom, Norris. We had various guardian angels upstairs at the time and whoever was our guardian at the moment was probably the channel for the request.

ROSS: O.K.

COHEN: So the ostensible purpose of this trip was to run sort of a dog and pony show about ERA capabilities. And in addition to management, a lot of people in the audience were former ERA colleagues who were now up at the Norwalk laboratory, which was the site of this presentation. It was in the large conference room at the Norwalk laboratory. Arthur Draper was there, John Mauchly, Grace Hopper, among our old colleagues who were among the founders of ERA who had defected to Remington Rand in 1949. I remember, in particular, Hugh Duncan, Lou Chaloux and others. Those of us who went out on this trip were Erv Tomash and I and Gordon Welchman and John Parker and Howard Engstrom. I remember Erv Tomash and I spent a good chunk of the previous day in a room at a different hotel from the one Parker and Engstrom were at, because hotel rooms were kind of scarce in New York, rehearsing Erv Tomash in this presentation--getting the proper punch and so forth. I wasn't involved in the direct presentation. I was simply helping with the coaching. It turned out that Howard Engstrom, Henry Forrest, and the others did not know anything--apparently Norris chose not to tell them--of the fact that we were going to be so specific, that we were going to propose a specific product. And Howard Engstrom had his introductory speech worked out that this would be a presentation on our capabilities. Which is essentially true. He didn't know what was coming along, but he was going to introduce us. He was the research-oriented member of management, you see. I guess Erv and I were a little late getting over to the other hotel, which was the Lexington. We were at the Essex house, and we had completed a run-through of his material until we were satisfied about what was going to be said. So by the time we got over to the Lexington to meet the rest of the gang for dinner, it was late. So we wound up in the doghouse. So I suppose that's an example of tensions developing even within ERA. I don't recall what the tension was about, except that it turned out that Engstrom and Forrest and others were planning quite a different kind of a show. Putting it kindly, there were some awkwardnesses that evening. I don't recall any detail except that there were awkwardnesses.

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The next morning we took the commuter train up to Norwalk for the presentation. Mr. Rand was not able to be there for the first part of the presentation. He could only attend part of it because he had to be somewhere else for a luncheon in which General MacArthur, who was still chairman of the board, was to be given an award of some kind. So that took precedence--competed with our bid for his attention. But we still had a pretty good audience.

But he was there for part of the show and I'll never forget, from where I was sitting I could see Mr. Rand almost in profile, and when Erwin was showing those flipcharts and the point-by-point comparison, Rand moved further and further forward on his chair and then took out his pocket glasses so he could really see that chart better. He was the picture of total concentration. Well, the net result of that presentation was that management, very conservatively, agreed that we should go ahead. They gave us whatever conservative amount of money we had suggested would be sufficient to add the necessary trimmings to make a commercial product out of it. They told us to proceed with the construction. ERA management was told later, not right then and there at the meeting, to proceed with the construction of two machines, and to make or buy parts for another two, for a start. Well, we were extremely elated, of course, at that. That presentation ran from the latter part of the morning, essentially through the lunch period after which we broke for lunch. Then Arthur Draper requested of us that he would like to have a meeting with us for an unstated reason.

ROSS: Us meaning....

COHEN: Well, the technical people who were present from the three laboratories primarily, in a smaller conference room after lunch. We could talk about technical matters. Well, we gathered for that meeting and Gordon Welchman with all his enthusiasm about the capabilities of the 1103, slipped in some impromptu preliminaries by demonstrating on the blackboard the speed of computation--multiplications and divisions and things like that--including the time for converting between decimal and binary. Even with the conversions included, the 1103 had a very impressive speed, and without his actually saying so it was clear that it outperformed Univac, Univac I, which was called Univac at that time. Arthur Draper started the meeting by saying he would like to have some of the Univac people explain to us, and

to me in particular--I was singled out--to correct some false impressions that I personally might have had about the Univac. I don't recall what properties of the Univac he brought up but what came out was that he said that on a recent trip to the west coast, I and a couple of colleagues had made some representations about the Univac to one of the aircraft companies that were not true. And it turned out that this story was alleged to have come from people at Douglas, the Douglas plant in Santa Monica.

ROSS: Which you've already said you didn't visit.

COHEN: Which we did not visit. And I might add, parenthetically, that now these many years later I still haven't ever seen the Douglas plant at Santa Monica. But Grace Hopper had insisted that somebody out there told her that we had made a presentation of some sort in which we had made some incorrect and presumably disparaging comments about Univac I, about Univac. So that led to a great deal of tension. I personally feel that this type of thing, and Arthur Draper calling a meeting to chastise some of us for this, was a manifestation of the feelings of some of those people that ERA with its 1103 and perhaps other capabilities represented some form of threat. That's a long story leading up to that particular conclusion. Well, I guess in telling the story I brought out two manifestations of it. One is the marketing and the other was the technical.

ROSS: Very, very true.

COHEN: The marketing people didn't like our going into their territory without their coordination, without their being present, and the other was the fear, or whatever, of the Eckert-Mauchly people that we were somehow disparaging Univac I to potential customers on the west coast.

ROSS: What was Draper's position at that time? Do you remember?

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COHEN: I couldn't tell you exactly what it was, but whatever the local management was of Eckert-Mauchly in Philadelphia, he seemed to be in the chain between them and Remington Rand management. He was not an officer, as I recall, of Remington Rand but he might have been an "assistant to" type of person, and he presumably had a good deal of influence.

ROSS: Well let's go back to, I got you started on this by actually giving Lukoff's impression of Univac II and in essence he came down to saying in his book exactly what Draper said to you before, that the ERA people didn't understand the Univac I, or the Univac.

COHEN: Yes. I believe you once told me, Jim, also that--I haven't seen this passage in the book, I really didn't look at it--that he used the term "underbid" that ERA had underbid the Eckert-Mauchly division on Univac II or something like that?

ROSS: Well, I don't know if he says underbid but you got the contract and Philadelphia didn't. What he says is that was too bad because the ERA people in St. Paul didn't understand the design of the Univac I so it was beyond ERA's capabilities of producing the Univac II without Philadelphia's help and you've said that in some sense that's true.

COHEN: I don't think I said in some sense that's true. I think I'd dispute that.

ROSS: Well, it's a complex issue.

COHEN: Yes, a very complex issue.

ROSS: And you welcomed the help, I believe you told me that you welcomed the help of the Philadelphia people.

COHEN: Well we certainly didn't reject any help. No, I think there was very good communication.

ROSS: Why the pointing of fingers then?

COHEN: I don't know that there was any pointing of fingers, and I would just as soon not personally get too deeply into this because I think you can get a pretty good story from Jack Hill. Jack Hill, who was the project manager on that, took an increasing amount of heat from Pres Eckert personally on this particular project, and finally had had a belly full of the whole thing and left the company in 1956, so we have a time marker on that program.

Let me indicate for the record here that the principal feature of Univac II that was different from Univac I was the core memory. This made it possible for the Univac to have a larger capacity high-speed memory. I don't recall how large this particular memory was, but there are some definite problems in taking what is essentially a parallel kind of memory, where all of the bits are handled in parallel, and putting that into a machine that was designed on a purely bit-serial basis, which fit the mercury delay line memory very well. Of course, there are many other features that are different too, but that's the outstanding one. The power supplies were completely redone. There was quite a large disparity between the electronic design philosophy of the two development laboratories, Philadelphia and St. Paul. St. Paul had gotten its battle stripes, and battle scars I guess, by having had to conform to Navy specs, and whenever Navy specs were incompatible with electronic requirements such as higher speed pulses and things like that, it was necessary for ERA to do a great deal of battling to get a waiver of a given spec. But nevertheless, ERA electronic equipment was built in a very conservative, rigorous way, conservative design relative to tolerances of components, tolerances of voltages, temperatures and all that sort of thing. Ruggedness. This was certainly not the case in the Univac. Univac I was an entirely direct-coupled kind of a system in which there were many many different supply voltages, some of them with extremely critical tolerances. Quite the opposite of the way ERA built things. The more that the ERA crew got into this the more they had to change and redesign. I suspect that this incursion into what Lukoff and others may have regarded as well-established design, might be what Lukoff was referring to. What was viewed by St. Paul people as not being the best engineering design approach, that particular view could be interpreted by Philadelphia people as a lack of understanding on the part of St. Paul.

ROSS: Exactly. So you've got two groups of people who technically are convinced that their way of going about things is superior to the other.

COHEN: Whether they really believed that out there or not, I don't know. But that was the stated opinion.

ROSS: That's my impression. Well, what came of the Univac II? It was late in delivery. A lot of redesigning had to be done.

COHEN: Yes, I believe it was late, late relative to some expectations or promises. Before the whole project was dropped, I don't know how many were built. I don't think very many, and I don't know who got them. It may be that Jack Hill doesn't know either because he left before the whole operation was completed. So this would be an interesting part of history to pursue. Herb Mitchell might provide some insight. Herbert F. Mitchell, who was coordinating the systems requirements for marketing.

ROSS: Well I will definitely follow it up with other people. You said that Hill left in '56, so the Univac II wouldn't have been delivered before the Sperry merger.

COHEN: No, the Sperry merger was in '55.

ROSS: Right.

COHEN: I don't know when the Univac II development was started, but it could have been early in '55.

ROSS: Well, I think it was earlier. I think it was around '54.

COHEN: Could be.

ROSS: You questioned my rendition of Lukoff's story that ERA had bid on the Univac II project.

COHEN: Right.

ROSS: Any idea on your part as to why ERA got the contract, why you built the Univac II?

COHEN: The word contract is inappropriate, because within the company it's really the assignment of the project.

ROSS: Why did you get the assignment?

COHEN: I'm sure there were a number of factors, and perhaps Jack Hill would have the best feel for that. I think one factor must have been the commitments and the problems of project overload at Philadelphia. In that time frame I think the LARC was in development. That was a major commitment. They were also doing various other things, what was later called the Solid State 80 and Solid State 90, and things like that using magnetic amplifiers as logical elements. I think they had their hands full down there; that was certainly one important factor.

ROSS: And they were still delivering Univac Is.

COHEN: Yes, but that was definitely in a manufacturing phase by that time. It may be that some peripherals were still in development, I don't know. But at the same time St. Paul had had experience in core memories, as well as in digital computers in general. Perhaps a better reputation at delivering on commitments, for whatever that was worth. I'm speculating as to why the assignment was made to St. Paul. Those could be some of the factors.

ROSS: Keep speculating.

COHEN: So much for that particular speculation. In retrospect, and perhaps even at the time to some extent, I feel

that the development of Univac II should not have been undertaken at all because at best it would not be a

competitive system compared with what was already available in the field--IBM, ERA 1103, which I believe the

management by that time insisted on calling the Univac Scientific. There were other companies that were entering the

field. I'd have to do a little more thinking to decide who else was in the market at the time, but certainly Univac II did

not appear to have the potential of being competitive. I suspect there was a lot of pressure from marketing on

management to do something that would preserve or that would keep Univac customers in the fold, Univac I

customers in the fold, by giving them the Univac II. The Univac I customers, I'm pretty sure by recollection, were

certainly bringing pressure to bear on Remington Rand marketing people to come out with an improvement that

would still use whatever software they had, and here again I don't know if the word software applies to that particular

period of time. But it was almost a dare, I suppose, on the part of the customer community to have us bring out an

improved model of that Univac, and Univac II must have been an attempt to respond to that pressure from the market.

ROSS: You hinted at something interesting that's a totally different subject. ERA made scientific computers as far as

Remington management is concerned, and Eckert-Mauchly made commercial computers?

COHEN: The marketing people made this distinction, and this is true I guess in IBM too, because IBM had the 705,

which was for commercial applications. The 705 and the machines that were primarily alpha-numeric and were set up

for handling files and things like that were regarded as business or commercial computers, and systems that had both

large memory and high computational speed were thought of as scientific. So this kind of thinking was not unique to

Remington Rand. It took a while for these distinctions to fall by the wayside. The differences, as we now know, were

resolved primarily as software evolved for the various application areas.

ROSS: As well as random access.

COHEN: Well, random access is good for everybody. You mean disc files for example?

ROSS: Yes.

COHEN: Well before the today's kinds of disc files evolved there were various attempts to make use of magnetic drums as random access file stores and Eckert and Mauchly were very large in this. They had several. There was the Randex, and Fastrand. I've forgotten in which order historically these came along, but there were efforts to build, and successful efforts I might say, to build large rotating memories that allowed you to process files and databases by direct access rather than sequentially by passing tapes.

ROSS: And the development of that kind of access to the data and manipulation of it really made the distinction between scientific and commercial rather arbitrary.

COHEN: Right.

TAPE 7/SIDE 1

ROSS: This is a tape recorded interview with Arnold Cohen conducted at The Charles Babbage Institute on March 2, 1983.

ERA had had associations with IBM over a continuing period of time. There was litigation in the early to mid-50s between Remington Rand and IBM. Tell me a little bit about the process there and some of the results.

COHEN: Well, to start such a discussion why don't we lay down a few events? One was an agreement between ERA and IBM, in late 1949 probably, which was done in connection with a design study contract undertaken by ERA for IBM, with IBM as the client. In this agreement IBM gained access, in the form of a license, to the ERA magnetic

drum patents, and perhaps other things, and ERA had certain limited rights, as I recall, to IBM technical information or technology. Let's just use the term, "punch card technology." They were fond of using the term "record-controlled accounting" to specify this technology.

ROSS: Card calculators?

COHEN: Any devices that were operated by information punched on cards, and which in turn were capable of producing similar cards. IBM had the option of paying royalties on whatever it wanted to use. They were to pay royalties on licenses on various patents that ERA might have, or, at their option, could pay a lump sum royalty, which is the one they eventually opted for. A fairly small amount, maybe $150,000, something like that. I recall the time they exercised that option and sent in the check to the ERA division. That would have been after we were a division of Remington Rand, so it would be probably in '53. That one check put the division, and perhaps the whole company in the black for the quarter. But that was still a pretty puny amount, and there is a comment in the Tomash and Cohen paper on the history of ERA about that in vague, general terms. Another event that figures here is the IBM consent decree of 1952 to the Justice Department. I'd have to check that date. I believe the main change in the way IBM did business as a result of that consent decree was a broadening of the way they marketed their equipment and services. They hitherto had leased only; you could not buy any IBM equipment. As a result of that consent decree, customers henceforth had the option of purchasing or leasing.

ROSS: Did that leasing policy extend to the tabulating machines as well?

COHEN: Yes, in particular. My statement may not be accurate. It may be that IBM electric typewriters always were sold, I don't know, but certainly for the tabulating equipment this was new. And here again I reserve the right to have a faulty memory. This may not, this particular change may not be due to the consent decree of 1952, but that's a matter of record. It's in the open literature. As a result of the design study performed by ERA under that 1949 contract for IBM, even before the work was undertaken, it was agreed that any patents that came out of that work

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would belong to IBM, but ERA (because it was ERA at the time) would have a royalty-free license to any patents that might result. But the ownership of the patents would remain with IBM. As it turned out, a couple of fairly massive patents did come out of that work, and the preparation of the patent applications continued after the acquisition of ERA by Remington Rand. Two patents came out of that work. The inventors were listed as Warren Burrell, Arnold Cohen, and George Hardenbergh on both patents. I don't recall the date of issue, but I think we put those patents into the footnotes of the Tomash-Cohen article, so we could establish the dates of issue.

ROSS: So the nature of those patents would be in the Tomash-Cohen article as well?

COHEN: Yes.

ROSS: That kind of cross-licensing arrangement wasn't uncommon.

COHEN: That's true. But IBM wanted to make sure that these conditions were set before any technical work was undertaken, and that's what happened. Actually, the exercise that we went through, as we indicate in the Tomash-Cohen paper, was a design for an electronic business computer with punch card input and output using the magnetic drum for performing several of the functions, not only the internal storage but certain buffering relating to the input and output operations, as well as the actual performance of arithmetic calculations. We were told at the time that this work would be judged in competition with the work of at least two different groups within IBM. What came out a few years later was the IBM 650, so we always did assume that the computer that they had in mind was what they eventually produced as the IBM 650. But we don't think that anything of the ERA design was incorporated into that machine. It may be that some concepts here and there might have been used. We never found out. But it's worth noting that ERA did not invent and design and produce the IBM 650, as some people around the country, or some of the folklore around the country, seem to have it. Things tend to be overly simplified in cocktail talk, I suppose. I just wanted to set that record straight.

Another important legal event: Remington Rand elected to sue IBM under provision of the law that allowed for triple damages by anyone who claimed injury due to anti-trust practices. In this particular case, where IBM may or may not have admitted to these practices, there was a basis for Remington Rand bringing suit, which led to a fairly complicated settlement, which I believe culminated in 1956, around fall of '56 or maybe going on into '57.

ROSS: After the Sperry merger.

COHEN: Yes.

ROSS: Was this over announcing products that were not on line?

COHEN: I'm trying to remember some detail here. Each company would have access to certain patents and technical information and complete manufacturing specifications for products that had been announced prior to a certain date.

ROSS: This is the result of the decree?

COHEN: Not the result of the decree, the result of the settlement of the suit.

ROSS: Prior to that, none of that cooperation had taken place.

COHEN: That's right. And by way of implementing this settlement (I'm going to jump around a little bit), there must have been a good solid truck load of engineering drawings and manuals and all of this sort of thing that Remington Rand Univac received from IBM. Bob Kalb had the job of organizing this material in a way in which selected parts of it could be retrieved for the use of any part of the company that might want to use it or examine it. A room was set up in one of the buildings that we were using at the time. There's an old building directly across Ford Parkway from the

Ford plant in St. Paul. We were leasing the whole building at the time for various groups. There was a room down the basement where they built some frames out of two by fours and set up a lot of these files or transfer cases for holding that stuff. Bob Kalb, with the help of two clerical persons, two women that were employed for I don't know how long, spent some substantial chunk of time just organizing and indexing this material. I mention that to indicate that it wasn't just something that happened and was set aside; somebody had to go through some big motions. I don't know if IBM paid any great attention to what Remington Rand had. The feeling was that they weren't much interested but Remington Rand people, particularly the tabulating machine people, were interested in what IBM had. I participated in some of the meetings that were held between IBM people and Remington Rand Univac people in which there were lists of products of various kinds. This is what the lawyers call a discovery process where there are names of projects and developmental numbers, project numbers that they got wind of--when I say "they" I mean both sides. There was sort of an interrogation in going down these lists. What was this? What is that? What does this mean? Is this the same as that? Jim Birkenstock was at one of the meetings that I attended. This was not my first meeting with Jim, but I recall that he was involved in that process. This might be worth mentioning to him, to see how he remembers this particular thing. As a result of these meetings a list of each company's developments and products was arrived at, and this would be the basis for an exchange of information or at least an indexing of information that would be made available, each to the other. I remember one of these meetings taking place in IBM's conference room at Lever House on Park Avenue in New York. It was after that particular meeting I was talking with Mr. Frank McNamara who was Vice President and General Counsel of the Remington Rand part of Sperry Rand, and here my memory is hazy except that it was clear to some of us that he had agreed to certain concessions.

ROSS: In the settlement.

COHEN: In the settlement. Certain concessions had been made to IBM. I recall asking him why that was done, and his reply was "well we had no choice; we had to if we wanted access to...(whatever it was)." I remember at the time mentioning to him that we already had access to whatever specific category it was, as a result of the ERA-IBM agreement. And he was not aware of that particular detail. I believe this is all worth mentioning for the benefit of

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anyone who might want to do some research on the history of the relationships between companies like Remington Rand and IBM in particular. There's a trail of documentation here that would be interesting to fit together.

ROSS: It's also evidence of, if it can be documented, of a lack of communication between divisions of Remington Rand.

COHEN: Well, since that agreement, the earlier ERA agreement, was written before Remington Rand was in the picture, it's understandable. I know they were aware of that agreement, but perhaps not the details of what rights the parties had to each other's technology.

ROSS: Do you have any hints as to what Remington Rand did give up?

COHEN: I don't recall. It may not have been terribly important but it was at least a little bit shocking to see something happening that didn't have to happen. The significance may not have been terribly great, and the fact that I can't remember the detail right now means that it probably wasn't very great.

ROSS: What changed, if anything, when Sperry merged with Remington Rand, from your standpoint as a technician rather than a manager?

COHEN: On the technical side, probably not an awful lot. I don't know if others would agree with me on this. I'm sure there was a tightening up of how engineering things were managed.

ROSS: A closer watch...

COHEN: Dr. Zike Barrow--his first name wasn't really Zike, it was Wilmer, I believe. Barrow had once been an MIT professor, and he was at this time the Research and Engineering Vice President at Sperry. He came out and looked us

over with an objective eye. He and others had formed a special ad hoc committee for the purpose, and then made

certain recommendations relating to the management of engineering development in the various Univac centers. I

don't recall much about that. It is pretty clear that Remington Rand at the time had a pretty good collection of

computer engineering talent, perhaps in some respects the best collection in the country, at least in the commercial

sector. Management either didn't know that, or didn't know what to do with it, so whatever the suggestions were by

Barrow and his associates, I'm sure were intended to do something about that. The suggestions were primarily about

organizational structure. The Tomash-Cohen paper amplifies this point.

ROSS: But in terms of products and customers it was pretty much business as usual?

COHEN: Yes, I don't recall that there was too much influence by Sperry management on the way Remington Rand

Univac products were marketed, or how products were planned. I certainly don't think, in retrospect, there was much,

if any, influence. I believe Sperry had some problems of its own in that regard, as became apparent later.

ROSS: Can you elaborate on that a little bit?

COHEN: Not really, but, other than if you look further down the road, there was a sort of steady contraction of

Sperry activity in the fields in which they operated. They were perhaps slow in picking up digital techniques in

control systems, and so forth. I believe the Univac operations over a period of time grew and the Sperry operations

diminished. And that's all I have reference to. I think they may have had some problems of their own.

ROSS: You mentioned to me two personalities that need discussion, when we were talking off tape. Marcel Rand

was one--the son of James Rand or no, nephew?

COHEN: Well, let's see. No, there's something tricky there. Half brother, maybe? There were several Rands around

and I don't recall. Well, historians can dig these things up themselves, but there was H. J. Rand who had a separate

company called the Rand Development Corporation in Cleveland. I believe the Bendix washing machine, the thing that would squeeze the water out of the clothes by compressing a rubber sphere, I think was his. Also, later work on artificial hearts, substitutes for tobacco that would not cause cancer, and all sorts of things like that, but that was H. J. Rand. Sid Rubens might be able to recall the relationships of all these people. The head of Remington Rand at the time they sold the company to Sperry was James H. Rand, Jr. His father obviously was James H. Rand, Sr., who started the Kardex business, and things of this nature. I believe Marcel Rand, who was considerably younger than James H. Rand, Jr., was a half brother of Jim Rand's. I know there's a half brother relationship in there somewhere, and I think that's probably it.

ROSS: What was his company role?

COHEN: He was plugged into management somewhere along the line and he was certainly in that capacity, perhaps even as head of the Remington Rand operations, that half of the company, along around '56 or '57, certainly in '57 at the time of the Control Data breakaway. I have a personal impression, which I know was shared by a large number of people. I remember I sat in a meeting where Marcel Rand was present, or perhaps in charge. His questions and comments certainly indicated a limited understanding of the business, and how one goes about arriving at decisions as to where to put developmental money in a high stakes business. He apparently did not appreciate what he had hold of.

ROSS: So your comment about his being in a top management position at the time of the CDC break is no accident. You're leading us somewhere.

COHEN: Yes. I don't know if I have the guts to put all of this on tape. Let me temporarily switch now to Dr. Thornton C. Fry.

TAPE 7/SIDE 2

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COHEN: Dr. Thornton C. Fry was well-known as a mathematician at Bell Telephone Laboratories and I believe vice president of one of their research operations, perhaps mathematical research. He was a mathematician, originally from the University of Wisconsin, I believe. A textbook of his published by McGraw-Hill enjoyed kind of a durable popularity. It came out some years before World War II, and was still selling after the war. The title was *Engineering Probability*. It was for a long time the ranking textbook in that field. During the war he was in charge of some sort of applied math research for the government. I seem to recall that either the work that he was doing was based at Princeton University or perhaps Princeton University was one of a number of places where work in this particular area of his was done. Well, along about 1956 he was retiring from Bell Labs and the president of Bell Labs, Dr. Mervin Kelly I believe, mentioned to Sperry, perhaps to Zike Barrow, that Dr. Fry was retiring and would be available, and that his services might be of interest to Sperry Rand. I believe he was taken on as a consultant for a time, and he visited us out at St. Paul, and looked over everything and asked questions, and I believe shortly afterwards he was put on the payroll as vice president and I believe they put all the research and engineering in what was by then called the Remington Rand Univac division under him, in a direct line capacity.

ROSS: So they were actually creating a new position at that time then.

COHEN: It may have been. The organization was in constant change anyway, so I suppose it could have been new. Well somewhere in there (and this is a matter for others to fill in the details about), Bill Norris was pretty much in charge of many of the things in the Remington Rand Univac division. Almost as soon as he was assigned all of these responsibilities, various functions began to be removed from under him, one by one.

ROSS: Any specific examples?

COHEN: I believe Carol Pine has it in her chapter on Bill Norris in the book *Self Made*, which came out in 1982. So in what sequence these things were removed, whether it was marketing and then, I don't even recall which functions,

but they were removed one at a time. (When I was interviewed by Carol I likened it to removing feathers from a chicken one by one, a painful process, and that metaphor was misquoted in her book.) Well, you can see that that, and a number of other factors, created some pretty poor morale, particularly in the St. Paul organization. This made the environment ripe for something new to happen, and Arnold Ryden came along with a plan, just at the right time. He was able to interest Bill Norris and others to join with him in this.

ROSS: That plan of course being...

COHEN: The forming of Control Data. It was not at all clear--from the viewpoint of those of us on the technical staff of the company, and I'm not exaggerating--it was not at all clear at that point whether the company or at least our part of the company would even survive. The management at that time was so obviously poor. Marcel Rand, Fry, and the people around Marcel Rand. It was just disastrous. Not only poor management from a competence standpoint, but the politics of personal relationships that entered in were becoming quite obvious.

At some point there, Bill Norris resigned. Marcel Rand and Thornton Fry immediately came out to St. Paul, to save the place, I suppose. And the story, as I know it, was that they approached Frank Mullaney to succeed Bill Norris as the general manager of the operation, and Frank turned them down. I don't know if he told them that he had plans to leave himself, but at least he turned them down. He was asked whom would he nominate--did he have any ideas? He suggested Bob McDonald. The position was offered to Bob, and Bob accepted.

ROSS: Mullaney did leave.

COHEN: Yes. I'll relate the story that I heard--this was sort of a miniature "two or three days that shook the world" sort of thing. Carl Swanson was always kind of close to Norris, sort of an assistant to Norris or whatever. Bob McDonald may have been the manager of manufacturing operations in St. Paul at the time, and his morale by this point was extremely low. I suppose it related to the large unknown of where he stood, where people at that level

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stood, relative to the management in charge. And the story, if I remember it correctly as I got it from Carl Swanson, was that McDonald was feeling particularly low and was expressing his feelings to his old friend, Carl Swanson. I guess he felt this was the end of the line, and that he might do well to go elsewhere. At this point he was summoned to meet with Rand and Fry, and was offered the General Managership. This relieved his mind considerably, apparently, and he accepted. That's the way I recall certain events that happened in a short span of days in August or September of '57, probably late August of '57.

9 March 1983

ROSS: This is a continuation of the Arnold Cohen interview conducted on March 9, 1983.   Tell me a little bit more about the contract work that ERA did for IBM. We talked about it before.

COHEN: Right.

ROSS: What kinds of demands did they make on you?

COHEN: Well, as I mentioned earlier, I think what they had in mind was to go a step beyond the 604 electronic calculator by producing a system that had some internal storage and at that time what they had in mind was magnetic drum storage. They were definitely aiming this at--a relative term--the mass market in the business world. They were aiming at production that might be comparable with that of their various tabulating devices.

ROSS: Certainly, they were trying to pull in the same customers they'd had over the years.

COHEN: Right, or preserve the customers in their camp before they wandered off into something else. It became evident, in this more or less competitive design study that they had us do, that low manufacturing costs, that is, cost-effectiveness in terms of performance versus low manufacturing cost was going to be an extremely important factor.

ROSS: Did they define any parameters involved there, for example power?

COHEN: Well, in those days I guess throughout the industry an easy measure of manufacturing cost or complexity would be the number of tubes, the number of electron tubes estimated to be in the equipment to perform the specified function. The senior man on the IBM team that we dealt with, a man in their Future Demands group--name was Dave Rubidge--was fond of using the word "tubage" to represent the number of tubes, the tube count in a system. Our obvious response to that, or part of our response, was a slogan on the wall reading "tubage for Rubidge". We went through this exercise on sort of an iterative basis. We came up with a particular system design, a roughed-out approach, and then we would submit a system drawing and a few pages of descriptive matter, and these went in by installments. I hope I have some copies of that around for the archives. If not, perhaps Warren Burrell does. There were three of us on this project: George Hardenbergh, Warren Burrell, and myself, and every time we had a meeting we found they were receptive and we received a certain amount of praise for the work so far, a pat on the head "but the tubage isn't low enough." This was a very excellent exercise for those of us at ERA who, for the most part, had been doing work under government contract, and the incentives were a little different there: performance, reliability, but not necessarily the lowest cost, and particularly if you were only going to build one or two or three of something. You have a different set of incentives in that kind of environment than you do in the mass market type of environment. I guess we kept on being amazed at how much fat could be wrung out of a design even though we thought it was already down to the bone, by going back and doing things, taking another look, finding ways of combining functions, or leaving things out, whatever. I'd like to mention that our continuing IBM technical liaison man was Robert Walker, of Watson Scientific Computing Laboratory at Columbia University. Very capable, and a pleasure to work with.

ROSS: It would be really interesting to document that design project. We should try to locate whatever documentation we can come up with, including the contract.

COHEN: They'd be a little harder to read, of course, but the two fairly massive patent specifications that came out of the work are referenced in the Tomash-Cohen paper. I may have some of this material among my papers. I think we should make it our business to find some of that. And even that is incomplete because as I indicated it went in in installments and it doesn't read like a book with a beginning and an end, or conclusion, or anything like that.

ROSS: Did the experience gained by ERA on this design venture lead to any suggestions within the company that ERA had the potential to produce on a serial or mass scale?

COHEN: Well, let's see. If we go over the particular time frame, that was in about '50. We started talking with IBM on this one in late '49, and the work was done, I suppose, in the first half of 1950, somewhere in there. I know that the effort that was expended on patent applications, involving other people later on, extended on into the period after which we had become part of Remington Rand, which would be 1952, so these things dragged on awhile. I think ERA, as a company by itself, recognized that it could not gear up for large production runs without some solution to the problem of sufficient capital for the purpose. The solution that our management, or in particular John Parker, chose was to merge, sell the company to Remington Rand.

ROSS: Do you think Remington Rand was capable of using their already established manufacturing units to mass produce computing devices?

COHEN: Well, what they had going at the time that we became part of the family, was a series that I may have referred to before called the 409 series, which in some respects was more sophisticated than the IBM 604, with card input and output and electronic computation in between, but not stored program as such; it had plugboard programming. So they were producing this kind of thing and I don't recall on what scale, but certainly not a small scale, and that was being produced along with their by then fairly archaic conventional tabulating equipment.

One more comment. As we saw later on, when we sold them on the idea of supporting the 1103 as a product, they had no particular intention of doing the manufacturing at their established manufacturing locations. And the same thing is true of Eckert-Mauchly, with Univac and the peripheral equipment for Univac. They apparently had no intention of bringing the manufacture of any of that equipment into any of their existing facilities.

ROSS: That answered my next question.

COHEN: Right.

ROSS: What was your impression, as a design engineer, of the IBM 650? It's a leading question; I'm trying to get you to tell me what was exciting you at the time that product came out.

COHEN: Well, we didn't get to know too much about it. By the time it came out it was just another product of another company. Interesting, to be sure, but I guess most of us were too busy with other things and didn't take any great deal of time to look into the details of its design, its characteristics, its performance.

ROSS: Good, that's informative. Turning to another subject: A patent for which you were partially responsible for in the late, that you filed for in '49 and received in...

COHEN: Filed in '48 and...

ROSS: '48.

COHEN: ...issued in '51.

ROSS: You shared that patent with Bill Keye and...

COHEN: C.B. Tompkins.

ROSS: Okay. That patent was later embroiled in a good deal of infringement litigation. What did you learn from being called to testify?

COHEN: Well, embroiled may not be the right word. It was used as a convenient and moderately strong defense, by defendants in several different cases over a period of years. What did I learn? One thing I learned is how not to prepare a patent specification or patent application. May I go into a little bit of narrative history?

ROSS: Certainly. I was going to ask you to anyway.

COHEN: Okay. That particular patent was based on work that we did under one of the tasks under the ONR contract with ERA which is described in the Tomash-Cohen paper. It is also described in the introductory chapter that I wrote to the CBI Reprint Edition of the *High Speed Computing Devices* book. The company encouraged us to present papers in order to get the company into the public eye in the field. In those days, the IRE, Institute of Radio Engineers, the IRE annual national convention in New York was held in March every year, in this particular year, 1948, late March, and we had submitted an abstract. The abstract was accepted, and Bill Keye and I were to present this paper on this method of storing information on a magnetic drum at that meeting in New York. Well, in the course of completing that particular phase of the work, we were obligated and encouraged by our sponsor, ONR in this case, to proceed in filing for any patents that might come out of the work. And, in those days, the arrangement was that any patent which issued would belong to the contractor, ERA in this case, and the government would have a royalty-free license on it. There's also a thing about public disclosure. If you're going to present a paper or publish an article about the subject matter of the patent, then you have up to one year beyond the date of publication to file for a U.S. patent. The hitch was that if you wanted to file a patent in a foreign country, most foreign countries didn't allow that extra year, and once something was published, it immediately became public domain. So, there was a big

rush. The patent counsel, in this case the company's patent counsel, was the firm of Cushman, Darby, and Cushman in Washington, D.C., a large firm of patent attorneys, even larger today than they were at that time. But we were encouraged by them to get this completed.

ROSS: Let's go on to another tape.

TAPE 8/SIDE 1

COHEN: The problem within the company was that the liaison with the patent law firm was not directly with us in St. Paul who had worked on the system, but with certain people in our, at that time, Washington office, in the Portland Building on Vermont Avenue near Thomas Circle.

ROSS: Was it just geographical dislocation that was the problem?

COHEN: No. We had our problems with integration in those days, too. I believe there were certain people in our Washington office who regarded it as their turf to handle patent matters, and they handled many things without the need for conferring or consulting with the people in the laboratory that did the work. We did provide them with the final report to ONR on that work, which was very complete, and it showed assorted diagrams of everything that was used in the system, more than you'd really want to know about anything, which is the way it should be.

ROSS: Do those exist?

COHEN: Yes. For some reason, the patent attorney in the law firm who was assigned to work on this was not provided with that report, but was provided only with a copy of the paper to be presented, a highly abbreviated, obviously, version of this thing. We in St. Paul didn't know that that was the case. That was the internal company problem and I guess it's all right to mention the name here. The man who enjoyed this prerogative of serving as

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exclusive interface with the patent attorney was Larry Steinhardt, Lawrence R. Steinhardt. Larry was a very bright

young fellow and one of the founding group of the company. He felt that he knew things. This came up in a number

of instances even later, that he knew a lot about things and didn't have to ask. Well, apparently one of the things he

didn't know was that he had a copy of the complete report. There was a second problem. The law firm at that stage

in history had no person with technical background in this field. The field itself was too new. They didn't even have

an electronics specialist. The man that they assigned, Roger Gessford, was apparently very good in his field, but his

field was chemical engineering and chemical processes, and so he did the best he could with the material and where

an amplifier was called for, or a counter, or this or that, he simply found some reference in the open literature that he

might include as a reference.

ROSS: Oh no.

COHEN: With all of that kind of speculation going into a patent description, if you put these things all together, you

may not even have had a workable system. You would have had a poorly designed system.

ROSS: Well, and then the third level of complication was why was the patent issued in that form?

COHEN: Well, they wanted to get the thing filed, on or before the day that it was to be presented. So what

happened? This almost reads like an unbelievable fairy story, Alice in Wonderland or something. I was asked to

leave for New York a day early, stop by the Washington office enroute, and look over the patent application in its

final form--we hadn't seen it even in draft form earlier, you see--and sign it. It's incidental, but it helps to show why

it's vivid in my memory. We did most of our travelling by train in those days; it was possible to fly but you could

keep schedules better if you took the train from here. In 1948 we had one of those months of March that had many

below zero days, so at the time I left Minneapolis it was below zero, and I had an overcoat and all that, and got to

Washington and proceeded to the company offices on a day which turned out to be heading into the 80s. They said,

when you go to call on the lawyer and you move around town, you'd better take your suitcase and your overcoat and

all that and keep it with you because this turned out to be the day, the very day, that the company was moving its office from Washington over to Arlington, Virginia. So I remember that very vividly. It was quite a day.

I was brought over to the law firm which at that time was in the American Security Building adjacent to the U.S. Treasury on 15th Street. I was given the patent specification to read in final, smooth form one page at a time as it came off the typewriter. There was no opportunity, even if I objected to a statement, there was no opportunity to change anything. This was a deadline situation and I couldn't have seen it any earlier because it was just being typed at that time. That was the kind of a squeeze that we had. So these are good examples of how not to do things. I'm sure that if any of the parties involved had recognized or had been able to predict the role that this particular patent would play in the future in various litigations they would have insisted, no matter what the consequences, on our withdrawing the paper even at the last minute and doing a more careful job of writing--just rewrite that whole thing.

ROSS: One follow-up question that I was trying to hint at just a minute ago was why did the patent office approve something that to me would have looked a little bit...

COHEN: Well, apparently if all the elements were there they didn't care. It may not have supported the work in the best possible way. Too bad that Roger Gessford didn't live longer because he might have been able to help out with the litigation that came along later.

ROSS: Tell me about the first case between Technitrol and Sperry Rand.

COHEN: O.K. But first, another point or two. I signed off on that day, and Tommy Tompkins was around town there in Washington, so I'm sure they got him to sign off at some point, although I don't think our paths intersected on that particular date. And then I was supposed to carry a copy to New York, where Bill Keye would join me, and we were to get him to sign. We had to go to a notary, one of those things to be done at the very last minute. He signed and

we got it back to somebody, and it got into somebody's briefcase. What started to happen shortly after the patent

was issued in 1951 was that people began asking me about the sprocket track idea, what came to be known as the

sprocket track. The fact that there was a track of pulses that were counted to keep track of positions turned out,

much to our surprise, to be new. Roger Gessford had put it in as part of one of the claims. We, as green engineers,

probably wouldn't have thought of its being that important because, well, how else would you do it? So that turned

out to be an apparent dominating feature in that patent, the sprocket track idea. As a matter of fact, many people

referred to it later as the sprocket patent. Well, now let's jump the calendar a number of years, up to about 1958.

There's a company named Technitrol that still exists in Philadelphia that was founded by people who were at the

Moore School. The principals who were living at the time in 1958 are both now deceased. T. Kite Sharpless and his

partner headed up this Technitrol company, Stuart Eichert, not to be confused with any similar name...

ROSS: Originating at the Moore School.

COHEN: I don't know if Stu Eichert had been at the Moore School but Kite Sharpless was Moore School faculty and

we had met before. They had a patent which must have been filed somewhere around 1948 and issued around 1951,

very contemporaneous with ours, and it described a manifestation of a storage system that had a proposed

application, for keeping track of airline seat reservations. This particular thing that they claimed, and I have no reason

to dispute whether they did this or not, but they claimed that they had reduced it to practice, that they had a

complete operating system as described. They used kind of a crude counting arrangement for keeping track, for

adjusting the count, the remaining seat count or whatever. But the magnetic storage device, it was not a magnetic

drum system, used magnetic oxide coated paper disks and these disks rotated. This is the original floppy disk, I

suppose. The paper disks passed between pole pieces of a magnetic recording head so that the recording was

perpendicular to the plane of the disk, which at least at that time would have meant that their density of recording

would be very low. They apparently had included all of the necessary logic to do what they claimed the system did.

What they were doing was apparently knocking on the door of every computer manufacturer and saying, "Look, do

you have a magnetic drum storage system or disk or whatever?" (In those days it would have been drums.) "Your

system XYZ infringes on our patent and if you would like to become a licensee we would be happy to work out some arrangement." There's a patent attorney who may have been an employee of Technitrol, or perhaps he was in business for himself at the time. Sy Yuter was his name, and he's still around and probably still exploiting the Technitrol patent among other things. He was the aggressive party in this case, pursuing these licenses. And as I understand, IBM had the reputation in those days of avoiding trouble, problems, because of their anti-trust vulnerability I suppose, so if a little company came along and kind of kicked them in the shins and said "Here, why don't you take out a license?" I guess they would take out a license, that was just it. So IBM became a licensee, and I think RCA may have, and perhaps others. Some companies resisted. So along about 1958 there were conversations with Sperry Rand. This was Technitrol approaching Sperry Rand with the idea of Sperry Rand taking out a license. Sperry Rand said, "Get lost, we're in this business, we have our own patent defense structure." Then even many, many years later, as recently as I suppose 1978, Technitrol was still going after the industry, including Control Data. Yes, 20 years later. There's an interesting narrative here.

Technitrol was threatening to take Sperry Rand to court. These were pre-trial conversations, or more than just pre-trial but pre-litigation I suppose, sessions and I found myself drawn into these things a little at a time and got more and more involved. One of the consultants retained by Sperry Rand in these 1958 conversations was Dr. George Stibitz, whose name you will recognize. Stibitz amassed much material and looked it all over and gave his technical opinions. That's where I got to know George Stibitz very well, working with him on that particular case. All right, now for a trivia narrative. Do you want to use up some of your tape on this?

ROSS: Be my guest.

COHEN: When I say Sperry Rand I'm speaking of the Sperry Rand Patent and Licensing Department which was at that point located in Norwalk, the Norwalk laboratories at Norwalk, CT. And the head of that group was a very seasoned, very pleasant, older patent attorney named Clyde Norton. One of the men on his staff (incidentally, Norton is now deceased) whom I was working very closely with on this case was Louis Etlinger, who was a patent

attorney on the company staff. He later left the company to become, I believe, the chief patent counsel at Sanders Associates in Nashua, New Hampshire. He called me a few years ago on something, so I believe he's still there and still in that capacity. Okay, several times some conferences had been scheduled between the Technitrol people and ourselves. I believe there had been some meetings, but one particular meeting at which certain information was to be brought forth--both sides, you know how they do, they write to each other "we'd like to know this and that and that" then have a conference. The Technitrol people had accused the Sperry Rand people of stalling, of delaying the conference because one particular meeting had been postponed several times and they threatened to go to court to complain about this, which our people regarded as a tactic. The Sperry Rand people were genuinely trying to piece together the picture that they wanted, so I have no doubt that the postponements were very legitimate. Well, it so happens that just prior to this time of a meeting in the fall of '58, I had some trouble with a tooth requiring root canal therapy here in town. I was referred to a doctor at the University here for root canal therapy, and he got this thing under control. I had had quite a bout with this where the whole mouth got swollen. He got it under control, and here was the date for this meeting. I asked the dentist, "Okay, is it safe?"--he had it temporarily capped up--"Is it safe for me to travel?"--and this was to be by air at this time--"Is it safe for me to travel for a couple of days to a meeting?" He said, "Sure, no problem at all. This thing is under control, no problem. Go ahead, enjoy your trip." So I went ahead, and we were scheduled to get our side together the day prior to this meeting, to review what we had. So George Stibitz and Lou Etlinger and Clyde Norton and Bob Sorensen met pretty much all day and then we went out to dinner. Not all of us, but I remember George Stibitz and maybe one or two others. And suddenly, just almost coincidentally with the first course, my gum, my jaw, started to swell and in a matter of minutes I couldn't even open my mouth. It was the same symptoms that I had had prior to the root canal therapy. My teeth were almost shut together. It was a horrible thing and everybody was very concerned and solicitous about this. Bob Sorensen suggested that, well okay, we're going to have to postpone this again and we'll get you to the airport in the morning and get you back and in the meantime maybe you can see the company nurse or the company doctor or somebody until you get home. Then Lou Etlinger or somebody said, "Look, would you do us a favor? These people are going to be showing up tomorrow. We'll have a meeting, but it's obvious that you can't play too big a role there or can't do anything. Would you please make an appearance at the meeting and greet everybody before you leave, so they can

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see that this is a genuine reason, that we're just not giving them a story." So I agreed. The meeting was on the company premises and later I was amused to see when they wrote the minutes of this meeting, I was essentially Exhibit A. The fact that I had this problem appears in the minutes of that particular session for the judge or anybody else to see if necessary. Immediately after that meeting, Technitrol brought formal suit against Sperry Rand. The suit was brought in October, 1958. That's a big digression, but it establishes the date very clearly in my mind.

TAPE 8/SIDE 2

COHEN: I had mentioned that George Stibitz was consultant to the company in helping with this case. The George Stibitz collection is in the archives of Dartmouth University, the Dartmouth University Library, and there is a finding aid, a bound volume with an inventory of this Stibitz collection. We may have a copy here, and I think I still have a copy. It lists laboratory notebooks of Cohen and Keye and all sorts of things, so apparently all the material that Stibitz used in his various consulting matters were in that collection and have since been catalogued.

ROSS: Fine, that's great.

COHEN: That information was used by a patent attorney I did work for in a later litigation, going up to look over the Stibitz papers to find certain materials that he then copied for use in this litigation. In some cases the laboratory notebooks, the originals, were no longer to be found, the old ERA things that were not classified. So these are worth mentioning. And the story of the tooth reminded me of the fact that Stibitz was part of that.

ROSS: It is worthwhile.

COHEN: Let's continue with the Technitrol vs. Sperry Rand case, which was brought in October 1958. I was involved in various depositions and conferences --company attorneys and outside trial firms. Both firms, both Technitrol and Sperry Rand, engaged outside trial counsel for this particular activity. One thing that I remember

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vividly was the experience in 1960 of having to recall under pressure, under very intense questioning, recall in detail why I did this, why I did that, back in 1947. This was thirteen years later and I thought that was a long period of time. As it turned out I was exposed to similar questioning in depositions a number of years after that. The principal interrogator, or inquisitor, for the opposition was a fellow named William Benson, Bill Benson, very pleasant, very friendly type conversationally, but he could really bear down with his questioning, but always very politely the southern gentleman. He had the southern dialect that went with it, and after each such session, perhaps several hours in duration or before and after lunch, you know, the greater part of one day at a time, after each of these sessions I really was wrung out. The experience was very interesting. Having to essentially live in the past for a while, I suppose is something like a form of hypnosis. I remember that it took a few days to come out of it, to kind of shake it off. It was that intense an experience. What I learned later on, and fortunately I didn't know it during the depositions, was that Bill Benson had been part of the team of attorneys handling the prosecution of the Nurenberg trials in Germany, and he was regarded as one of the more effective members of that team.

ROSS: So Technitrol was not playing games. They were taking this pretty seriously.

COHEN: I'm sure that this was an expensive law firm.

ROSS: You said earlier that your patent that you shared with...

COHEN: Keye and Tompkins.

ROSS: Right...was a convenient defense. By that did you mean that there were other possible ways of going up against Technitrol?

COHEN: Well, it was convenient for Sperry Rand since Sperry Rand owned that patent for one thing, but apparently it turned out to be useful to others as well, as we learned later.

ROSS: My question is, was the litigation really over the counting device as a part of the stored unit or...

COHEN: Well, apparently, well there are a certain number of claims, I suppose, in the Technitrol patent, and Sperry Rand presumably would be alleged to be infringing under claims number this and that and that of that patent, whatever that may have been.

ROSS: The reason I'm confused is that ERA had been working on magnetic drums for a long time, long before....

COHEN: Yes, but you see the period of Technitrol's work and the period of ERA's work were contemporaneous. Same time frame. I was never present when the parties ever actually got down to a specific date showdown, but there was enough there to indicate that we were working at the same time, in the same period of time. You asked about other work going on at the time. It was along in, I suppose '60 or '61, somewhere in there that I heard that some of the patents of Gerhard Dirks came into the picture as work that perhaps even antedated and may have been relevant to this, and I believe Sperry Rand or perhaps even Remington Rand earlier may have become licensed under some of these in Germany. Dirks had a number of patents, some of them were Dirks and Dirks, father and son, of which the man we now know as the older Dirks was really the son. And some of these patents came to light. There was complicated handling of German patents by the Alien Property Custodian. Then there were certain rules that applied to priorities of such patents, things of that nature. It gets very complicated. Dirks had certain U.S. patents, and their dates and validity were subject to challenge. I don't know how the Technitrol vs. Sperry Rand case was eventually settled, but I think that the work of Dirks figured in the settlement in some way.

ROSS: I think you're right. CBI has an interview conducted with Dirks that's a little bit hard to get through but it's primarily concerned with that issue. We'd have to go back and look at it again.

COHEN: Dirks claims, in some statements that I've seen abstracted from his interviews, that he had been a consultant to IBM. I think this might have come up in connection with another case that I haven't discussed yet. We know that he, too, went around knocking on doors, and as part of the settlement or in addition to any lump sum settlement that he may have obtained, he obviously had a consulting contract for a few years I suppose, where he received payments spread out over a few years. I doubt that he had to provide any consulting services as such. Although it's possible, it's unlikely in my mind. He may have had some similar arrangement with Remington Rand in Europe, and perhaps even later.

ROSS: Very possibly.

COHEN: Sperry Rand reached a settlement with Dirks, at about the time of the Technitrol settlement, but quite separate from Technitrol. I don't know the details of any of these settlements actually, but shortly after the Technitrol settlement, Dirks was given a tour of Remington Rand engineering and manufacturing facilities. Bob Sorensen brought him out to St. Paul, and I was asked to help show him around, in particular the areas where the 1103 was being manufactured and the magnetic drums that were part of that system. And he expressed great interest in what would be more the manufacturing things, and didn't seem to express a great deal of interest in any more detailed technical matters of logic or design or anything like that. Which doesn't prove anything, I suppose, but some people have their own private ideas about what his contributions may have been, really.

ROSS: Did he appear to you to be a bitter man?

COHEN: No, not at all. He was comfortable and happy. He was collecting money from a lot of people. Have I disposed of the Technitrol thing? The important thing is the nature of the settlement between Technitrol and Sperry Rand, which is unknown to me. That tends to be true about witnesses, who have no particular privileges, necessarily. You've done your piece, okay, goodbye and thank you. Or if it's an outside thing, you get paid for it.

But a second piece of patent litigation relates to this same ERA patent as an element in the defense. The case was Hale Brothers Associates vs. United States.

ROSS: Where did the Hale brothers hail from?

COHEN: The Hale brothers, I believe this is the same family, if you are familiar with the stock exchange listings or if you're familiar at all with western United States department stores, there are the Hale-Broadway stores and so forth. There have been many mergers and things of that nature. I believe this is the same Hale family. And Hale Brothers Associates is sort of an investment holding company for these people. Gerhard Dirks, and his son I believe, established a company, and if you look in the IEEE directory, if you look up Dirks, I think you'll find Dirks Jr., the son of Gerhard Dirks, the man that we know. Father and son had a business that they set up in California. An electronics firm of some sort. I don't know what the principal purpose of the company was to have been. But as I understand the story, they received their principal backing from Hale Brothers Associates as their bankers.

ROSS: For capitalization.

COHEN: Yes. As I understand the story, Hale Brothers Associates didn't have equity in the company because that obviously could turn out to be worthless, but the Dirks patents were used as loan collateral. When the Dirks company folded, the Hale Brothers got title to a whole bundle of Dirks patents. And that's why it's Hale Brothers Associates vs. the United States. It's the Hale brothers, as owners of the Dirks patents, suing the United States.

ROSS: Can you place us in time here? '60s?

COHEN: Okay, I first received a call, from the patent firm in Washington shortly after I had left Sperry to come to the University, which would have been in '71, so they might have called me in '71 or in '72, somewhere in there. They asked if I would be available for testimony and consulting on a patent case. When I didn't express any great

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enthusiasm the clincher was--this is standard practice in the law business, I guess--the clincher was, well we'd like to have you come as a consultant. We'll pay you regular consultant's fees and we need you on this case. If you're not willing, we'll simply subpoena you as a witness, and the witness fees under those circumstances are considerably less. A form of legal blackmail, I guess. So, they said that this was coming up on the calendar and probably wouldn't come up until next spring, so it was kind of set aside and forgotten. Once in awhile I think I got a call from somebody on the thing and then it was set aside again. The thing came to life, obviously I have records to pin this down to exact dates, but I would guess late '76, 1977, somewhere in there would be the time when it finally became active. But there was a long period from the time of first contact on this subject, '71 or '72 until late '76 or '77. What was the name of the law firm in Washington that called me? Cushman, Darby, and Cushman. Cushman, Darby, and Cushman were the principal patent law firm representing Control Data. In this particular case, the United States government was the defendant accused of infringement on certain things like disk files and so forth which the government had purchased from various vendors. And all of these were claimed to be products which were infringing on certain Dirks patents. Now the fine print in any government purchase agreement says that if you're a vendor selling things to the government, then as part of that agreement the vendor agrees to hold vendee harmless and so forth, meaning that in the case of any litigation arising out of alleged infringement, the government would not be a party.

ROSS: The vendor would have been liable...

COHEN: The vendor would be liable. That's the way to put it.

ROSS: I assume these, the products that they had picked up, these mechanisms, had been purchased over time.

COHEN: Yes. The government had become a large user of all such things by that time in history, disk files, drums, everything. So a number of vendors of equipment were listed in that law suit. Control Data, Honeywell, Xerox, and a number of others. I couldn't say exactly which, perhaps National Cash Register (NCR). Some that were not named, such as IBM, were already covered by blanket license agreements under the Dirks patents.

ROSS: And Sperry Rand probably was as well?

COHEN: Yes, I believe you're right. Sperry Rand was not among those listed, so they were probably covered by a license agreement by that time.

ROSS: I assume so since they had dealings in the '50s with Dirks.

COHEN: Yes, right.

ROSS: We'll have to find that out.

COHEN: So here was a pooled defense. The vendors were really the defendants that had to sweat it out. They pooled their defense, and Control Data's law firm was selected to handle the case for a number of reasons. Primarily, I suppose, they might have been one of the larger vendors of the specific equipments that were involved here. The specific attorneys I was dealing with were Jim Kirkpatrick, better known as "Kirk," and Larry Nixon, the junior member of that pair. I had known Kirkpatrick from way back, probably during preparation of the ERA-IBM patents in the early 50s. He may have been the first electronics man at Cushman, Darby, and Cushman. He's one of the senior men in the firm today. Larry Nixon, the younger man, is very competent in engineering and computer matters, and a very good lawyer. It was a pleasure to work with these people and to renew the relationship after all these years.

ROSS: For a fee.

COHEN: Yes, of course. But it was hard work. I guess there's not a great deal of detail to go into except that I was there not only because of the specific ERA patent, but I was asked to go over a lot of other defenses as well, which involved Eckert-Mauchly's mercury storage system. It was possible to map the Dirks claims onto systems that were

actually reduced to practice such as the mercury, the acoustic delay-line storage system. That patent was very good at pointing out that this is not limited to mercury delay lines but could obviously be applied to a number of things in which they included magnetic drums, disks, everything. And so it was possible to reconstruct. You didn't have to invent anything, there from the description you could simply show a manifestation if you wished in some other physical implementation. And a pretty good defense was built up using that, and also using several other patents where obviously the American work clearly antedated the work of Dirks. The case was settled out of court, but as in so many such activities, at the very last minute. In October of '77--I know it was October and probably '77--we were actually to show up in court on a Monday morning and I was prepared to appear there and to testify. We were set to go over the material. We had great big blow-ups of drawings and everything prepared for use in the courtroom. They had rented rooms in the Capital Hilton. We were to spend the weekend, Saturday and Sunday, going over this material and seeing it, you know, becoming familiar with the blow-ups in their form, not just pieces of paper on the desk but the actual form in which we'd see them in the courtroom. And so that was going to be a hard-working session. Friday afternoon I was all packed, ready to leave for the airport from here, and I got a call. And it was from Larry Nixon's secretary saying that "Mr. Nixon asked me to call you to let you know that they seem to be coming to some settlement or agreement." So the message was, "Cancel your travel, that is, don't come on this flight, but don't unpack your bags. You'll hear from us later." Then I got a call, I think it was that evening, from Larry Nixon saying that they had indeed settled, and that ended that. What happens here, in cases of this kind, for the plaintiff to actually go to court and lose the case immediately invalidates his own patent. To settle out of court doesn't invalidate his patent, and allows the plaintiff to go on and seek licensing agreements from other companies, other potential defendants. So that's the way that one ended.

ROSS: Can you mention any other cases that followed?

COHEN: There was another case for which I was also engaged by Cushman, Darby, and Cushman. This was Technitrol vs. Control Data. This shows you how many years a given thing can keep going. That was the same old Technitrol patent, and by this time the principals in that company were both gone. Technitrol still exists, and

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somebody owns the stock, family or whomever, and Sy Yuter was still pressing the licensing. Larry Nixon wanted me to come in and spend some time talking with him and Bill Keye at Control Data. They took a deposition from Bill Keye, and took a longer deposition from me. That was in January of '78.

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COHEN: Part of the defense involved certain manifestations of the storage system of the Atlas I which we also have referred to as the ERA 1101. There were several points of contention, such as whether or not there was a mark or signal on the drum that denoted the beginning of a new revolution to start the counter so it didn't start at a random location, and we definitely did have that, so we were able to point that out. Also a number of other things so that we could show that that system, since its description went back into 1947, definitely was a reduction to practice of some of the points claimed in the Technitrol patent. That was done under full deposition procedure with a court reporter and all that. An interesting thing was that Sy Yuter, this same Sy Yuter who apparently was still out working the mine, so to speak, was invited to be present at these depositions, so that he could cross-examine. He didn't respond to the correspondence from Cushman, Darby, and Cushman, so Larry Nixon called him, and he said no, he didn't plan on coming out. So there was no cross-examination, which was kind of curious. We didn't know why, exactly, and I don't know the outcome of that particular case. Incidentally, in describing these cases I don't know how much of this might be proprietary to the parties involved, so in editing these interviews we might want to check that point out. As I understand it, the transcript of something that actually went to trial in a courtroom is publicly available. But if there's a settlement in advance, I don't think that's true.

ROSS: No, it's not at all. It's the property of each of the participants.

COHEN: Yes, even though a lawsuit was formally brought into court.

ROSS: Just for the record, remind me what your position at the University was when you left Sperry Rand.

COHEN: Assistant Dean, Institute of Technology, in a specialty area: Assistant Dean for Industry and Professional

Relations, which defined an area that made it distinct from the territory or the turf of another assistant dean.

END OF INTERVIEW