An Interview with

SIDNEY MICHEL RUBENS

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Conducted by Arthur L. Norberg

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Abstract

Rubens discusses his career through his employment with Engineering Research Associates (ERA). He reviews his education in physics at the University of Washington, his work in ionization techniques, and his teaching position at UCLA beginning in 1937. In 1940, he joined the Naval Ordnance Laboratory, where he developed magnetic mine detection devices. There he met Howard Engstrom, Robert Gutterman, Howard Daniels, and William Norris. In 1945, under the sponsorship of the Office of Naval Research, this group formed ERA to continue their war-time work, and Rubens joined them in 1946. He first worked on magnetic techniques for computer storage as part of the Goldberg project, under the direction of John Coombs and C. B. Tompkins. Rubens discusses the magnetic tape equipment he used, some of which was war-time capture from German laboratories. He also discusses his contacts with the University of Minnesota computer center.

SIDNEY MICHEL RUBENS INTERVIEW
DATE: 6 January 1986 INTERVIEWER: Arthur L. Norberg

LOCATION: Charles Babbage Institute (Minneapolis, MN)

NORBERG: Dr. Rubens, in looking at a number of the usual sources of information about a scientist's career, we know a good deal about you from approximately 1929 on, but not much before 1929. Can you tell me something about your birth date and birthplace?

RUBENS: I was born in Spokane, Washington, March 21, 1910. I was educated in Spokane elementary and high schools and went to the University of Washington in 1929.

NORBERG: What did your father do in Spokane?

RUBENS: My father had a stove and furnace parts business and built a foundry in which he manufactured all the stove and furnace parts made by American manufacturers and furnished them throughout the Northwest and West in general.

NORBERG: When you say American manufacturers, that was a particular firm?

RUBENS: No. The name of his firm was Spokane Stove and Furnace Repair Works. But he manufactured the parts for all American-made ranges and furnaces. And also manufactured fireplace fixtures, and later my brothers, who were in the business, expanded it to include lighting fixtures and metal furniture and things of that sort. The business still exists in part today that my nephews have, in Spokane.

NORBERG: I see. Did your father come to Spokane, or was he also born there?

RUBENS: My father came to St. Paul, Minnesota from Lithuania, where he was born, and married my mother here in

St. Paul, who also came from Lithuania. He started that same business here in St. Paul on Seven Corners, and I think it was called the St. Paul Stove and Furnace Repair Works. But in those days it was a jobbing business. One bought the parts from the manufacturers and furnished repairs where needed. My father had a brother who lived in California, in L.A., who wanted him to come there. My folks moved there, approximately, I would say, in 1905, and after a short stay in the Los Angeles area decided that it was no good for his sort of business, and then he moved to Spokane, where he had friends.

NORBERG: Well, that's very interesting. What did your mother do during these years? Did she help with the business, or...

RUBENS: She raised a family.

NORBERG: Raised a family.

RUBENS: Yes, I had three brothers and three sisters. That was a good-sized family.

NORBERG: Seven in all. Now they moved there to Spokane in about 1906 or so would you say? And you were born in 1910?

RUBENS: I was born in 1910.

NORBERG: So from the time of your birth, you remained in Spokane. Now, what sort of elementary school did you attend?

RUBENS: The only kind of elementary schools we had in Spokane were the public schools and the Catholic parochial schools, and I went to a public school, called the Irving School. It was a very good one. And I went to Lewis and Clark High School after that. And Lewis and Clark High School had a scientific course in which I was enrolled. When I was about twelve or so, I became interested in radio, and I became a ham radio operator when I was about fourteen or fifteen.

NORBERG: So 1925.

RUBENS: So that started my interest in electricity and electronics as we know it today. Before I graduated from high school, just before, I joined the U.S. Naval Communication Reserve. And when I went to the University of Washington in '29, I became an active member of that Reserve.

NORBERG: What was the U.S. Naval Communications Reserve in Spokane?

RUBENS: The V7, which is the U.S. Naval Communications Reserve, attempted to get ham and commercial radio operators all over the United States interested in the problems of operating naval communication equipment. Incidently, Bill Norris was a member of the Naval Communications Reserve in Nebraska. So it had nothing to do necessarily being near a ship's port, but wherever the Navy had active naval reserve operations, as they did in Seattle, then they had an active unit of the Reserve, and we actually trained on board ship.

NORBERG: Okay, let's step back a minute before the reserve and come back to that. I'm interested in what sort of rig you had and where you got the parts for this rig.

RUBENS: My first rig was typical of those used by most operators when we all started out; it was a type 201A transmitter tube, in a Hartley circuit, which I made by myself. And I think my power supply was a transformer and an electrolytic rectifier, which I built myself out of the parts that you can buy in a dime store and so on.

NORBERG: Just out of curiosity, was the 201A an RCA tube, or a Cunningham?

RUBENS: It was probably an RCA in those days. Then I bought a DeForest H tube, one of the first and only ones

I've ever seen, and used that to increase the power of the transmitter. Eventually it burned out, and then I replaced that with a 210 tube, which almost all the hams in those days used.

NORBERG: Was there an amateur radio club in Spokane?

RUBENS: Oh, yes, and a very active one. As a matter of fact the man who got me interested, a man by the name of Tom Baird, who was a commercial operator in our only broadcast station in those days was the President of the club. Baird was an unusual guy. I think he had had polio, and was partially paralysed, but he was very active in this club. There were a number of us in the Lewis and Clark High School that were interested in ham radio and we actually formed a radio club in the high school.

NORBERG: Do you remember how many members?

RUBENS: Oh, I would say, the Spokane club as a whole had the order of twenty or thirty members, and I guess half a dozen to a dozen of them were in my high school. Now, there was an Amateur Radio Relay League to which we belonged; they had a convention. They had the Northwest League convention alternatively in Portland, Seattle, once in a while in Spokane or Tacoma, and once, when they had it in Spokane, is when two people from the Navy came from Seattle to recruit members of the Naval Communication Reserve, and that's when I joined.

NORBERG: Okay, that makes it fit very nicely. One added question: How many students in the high school?

RUBENS: I'd have to guess, the order of a thousand.

NORBERG: Sizable. Yes. Therefore, it must have had some reasonable science, mathematics courses.

RUBENS: Oh, very good, excellent in fact.

NORBERG: Can you remember any of those?

RUBENS: Oh, absolutely. It had a very good faculty. In physics there were three men. They were all good. And in mathematics there was a woman by the name of Catherine Bell, who, I think, had a masters and I think almost a Ph.D. from the University of Chicago, who taught math. She actually gave us college algebra and college trig in our junior year, which is probably done frequently today, but wasn't in those days. The man who was the head of the zoology department... They didn't have just biology. They had botany and zoology. He used to get a Sigma Xi grant every year.

NORBERG: That's early.

RUBENS: Yes.

NORBERG: Let's see, you would have been there '26, '27?

RUBENS: Let's see. From '24 to '29 actually. Actually I spent four and a half years, because, although they had graduation twice a year, it wasn't a good idea to start to college in the middle of the year. So I took an extra half year of advanced German and math there.

NORBERG: What about extra-curricular activities during this time, besides the radio club.

RUBENS: Well. I was, of course, a very avid ham radio operator. There was a science club, and a great many of the people who were interested in science went on to scientific careers from there. My interests outside of that were primarily things like fishing, gardening, and so on. Outdoor activities.

NORBERG: Did you work at the repair shop during this time?

RUBENS: Oh, yes, at the foundry.

NORBERG: The foundry.

RUBENS: Beginning with about my junior year in high school and through my college undergraduate career, I spent five or six summers working at the foundry. During that period my brothers... My father passed away when I was about eleven, but my brothers had it, and they built a new foundry, and I got involved with building one of the cupolas, which was a very interesting occupation.

NORBERG: Tell me a little about the foundry.

RUBENS: Well, the foundry primarily was a mild steel foundry for making furnace and stove parts, but they also had an aluminum foundry and a brass foundry.

NORBERG: How many people were working for the foundry?

RUBENS: Oh, I would guess the maximum number was something like forty or fifty, including machinists, and they had a plating works, and they also had a man who was expert at making wrought iron.

NORBERG: Was the ore shipped in, or did you buy purified stuff?

RUBENS: We bought pig iron and scrap steel. I remember spending a summer unloading a railroad car of pig iron. [Laughter.]

NORBERG: Was the...

RUBENS: So I learned a bit about metallurgy when I was there.

NORBERG: I would think so. What were the elements that went into deciding to go to the University of Washington?

RUBENS: Primarily, at that time--it was Depression--it was close by and it was inexpensive.

NORBERG: Well, when you were making this decision, it would be the first half of '29.

RUBENS: Yes.

NORBERG: No Depression yet.

RUBENS: Well... Things were tight. Actually it was in the fall of '29. And the real, you're right, the true Depression wasn't on, but one of my brothers had gone to Stanford, and the other had gone to the University of Washington, and I was interested in both places. But Seattle was far enough away from home to be on your own and still close enough to get home in six or eight hours if you had to in those days. I only made one trip by plane in all the time I was in school, which was about an hour. It turned out to be a very good school for me to go to, for what I wanted to do. Also, an interesting thing; originally my intention was to study electrical engineering. And we had a good friend who was a tubercular patient in Boulder, Colorado. At that time my sister and brother-in-law were living in Boulder. My brother-in-law was going to law school there. And this man had been an electrical engineer who had also been in the Navy--in Naval Communications--during World War I. And he asked me, "What do you want to do when you get through school?" And I said, "Research." And he said, "You don't want to be involved in sales and manufacturing and that side?" I said no, I wanted to do research. He said, "Then take my advice. Don't study electrical engineering." And he said, "Study physics or chemistry or math and any other physical sciences you want to, and when you get through, if you still want to study engineering then you can go into that." So I started out in physics with the idea of switching to electrical engineering. I never did.

NORBERG: How did you make this decision early? Was it because of the radio activities?

RUBENS: Yes. I was very much interested in radio activities at that time.

NORBERG: What sort of school was the University of Washington when you arrived in 1929?

RUBENS: It had on the order of eight thousand students, of which I would guess two-thirds to three-quarters were undergraduates. It was not a large graduate school. As a matter of fact, while I was there, they awarded their first Ph.D. in physics. It was just beginning to grow. The man that I did my doctor's thesis under later arrived the same year that I did. Joseph Henderson.

NORBERG: Where did Henderson come from?

RUBENS: Yale. He and Lawrence were classmates at Yale.

NORBERG: I thought the name was familiar.

RUBENS: As a matter of fact, because of that I used to make trips down to Berkeley and met many of Lawrence's people when he built the first cyclotron. There was a man there by the name of Sloan, who built their vacuum tubes. A ham radio operator, whom I knew.

NORBERG: David Sloan. I know David well.

RUBENS: Really?

NORBERG: Yes. I interviewed David.

RUBENS: Really? Yes, he had a bad accident.

NORBERG: Had a back problem for a long time.

RUBENS: Right, right. That's the guy.

NORBERG: Yes, that's the same one. Okay, I would assume that in the first couple of years in physics you were taking the standard courses in general physics and general chemistry.

RUBENS: Except not quite. There were two types of physics you could take. One, the general physics which you would need, let's say, if you were in general science or going into medicine or something like that. Then there was engineering physics, which the engineers started in their sophomore year. And because I told the faculty that I might want to switch to double E, they suggested that I take this instead of the other, and it was rough at first.

NORBERG: It was. Why?

RUBENS: Because these engineers had already had a year's training in the type of problem solving they were going to be involved with. But I did all right, especially when I got into electricity.

NORBERG: Who were some of the staff there besides Henderson?

RUBENS: Well, Professor Brakel, who always was in charge of the Engineering Physics courses.

NORBERG: How do you spell Brakel?

RUBENS: B-R-A-K-E-L. ...was in charge of all the engineering physics and the engineering physics labs. He was probably... Ah, let's see... The head of the department when I was an undergraduate freshman was a Doctor Osborne, whose field was optics. And Brakel's was electricity and magnetism. And Brakel ultimately became the head of the department. It was when he became head of the department that the graduate school really began to grow. I was very fond of him and we became very good friends and friends with his family.

NORBERG: As an undergraduate, now, did you take courses from all these men?

RUBENS: Let's see... I think I took courses from all the people that taught undergraduate courses in physics.

NORBERG: Well, that I would expect. But who were the people who were teaching undergraduate physics?

RUBENS: Brakel, to begin with.

NORBERG: Yes.

RUBENS: And Osborne. Osborne taught nothing but undergraduate courses, although he was the head of the department, primarily the courses in optics and physical optics. Bob Brattain, who was a brother of Walter Brattain, and I were lab partners through a good deal of our undergraduate physics courses. And we took a special course in spectroscopy from Osborne and, of course, Brattain became a spectroscopist.

NORBERG: Now, in the laboratories, were they reasonably well equipped, do you think, for that period?

RUBENS: Well, for the time, I guess I would say, quite well equipped. Some of the demonstration stuff that we had was far, far better than I found at UCLA and USC when I spent time down there later.

NORBERG: Can you give me an example or two of that type of equipment that would have been better there than at places like UCLA?

RUBENS: Well, I wouldn't say better in terms of... I'm talking about demonstration equipment.

NORBERG: Yes.

RUBENS: One of the things, of course, that fascinated me was the experiment of pumping down a large discharge tube and showing what happens to the glow as you get down to lower and lower pressures. I saw nothing like that at UCLA or USC. In the elementary lab, we had very good equipment for teaching mechanics and electricity and magnetism and especially electrostatics, magnetostatics, and that sort of thing.

NORBERG: Why didn't you go someplace else for graduate work?

RUBENS: Well, I guess the Depression had a lot to do with it. I applied at the University of Chicago, at Yale, and MIT, and Princeton. I was accepted at all but Princeton, but the University of Washington was the only one that offered me a fellowship. And I either had to have that or not go to school at the time.

NORBERG: Did you work during your undergraduate years while at Washington?

RUBENS: Only summers.

NORBERG: Only summers.

RUBENS: Yes.

NORBERG: So what did you do with your spare time while you were there?

RUBENS: Well, what most people do. I was interested in music. I went to concerts. I wouldn't say anything extra-curricular that was professionally oriented necessarily. I went fishing once in a while. We used to have pretty good fishing right off the docks of the canoe house, which belonged to the University. And swimming. Played tennis once in a while.

NORBERG: No organized team sports, though.

RUBENS: No, no. No, I got into trouble with that. One of my difficulties of belonging to the Naval Communications Reserve was that it was voluntary. You went to the drill when you felt like it, or when you had time. And you also got credit for cruises. I went on one cruise one summer, way up to Juno, Alaska and back, which gave me a full semester's or a quarter's credit. However, when I was a senior, I found I was short the equivalent of one quarter of ROTC, and they said I could make this up by taking any kind of organized physical education, such as, tennis, or golf, or boxing, or swimming, or whatever. I decided to take wrestling, which was a mistake. [Laughter.] I ended up with a back injury that took me out of school of the rest of the year.

NORBERG: Why did you choose wrestling?

RUBENS: Did you ever hear of a physicist named Walter Barkas?

NORBERG: No.

RUBENS: At Berkeley?

NORBERG: No.

RUBENS: Well, Barkas was a good friend of mine. We were lab partners a lot of the time. Barkas was bigger than you are and huskier than you, but he told me, "Take wrestling. It would be good for you." Well, there was a friend of mine--I can't remember his name right now--was about the same size and weight as I, and we both signed up for it. And one of the first things that they had one do is to learn to pick up a man and throw him down on his back. And about the fourth or fifth time this happened, I came down sharply on my back, and I got a bad sacroiliac subluxation, it turned out. It took a long time to find it, but I finally got it fixed.

NORBERG: Can you recall any texts that you used, as an undergraduate now?

RUBENS: I have some of them still. We used Kimball for elementary physics, especially in the engineering physics. Osborne had his own text for elementary that I didn't use. I have a copy of it. I never did use it. We used Wood's *Physical Optics* as a reference book a lot. Let's see, I'm trying to think of... I took a course in thermodynamics, heat and thermodynamics, which was primarily a laboratory course, but I can't remember the text for that. Let's see, what else did we use...

NORBERG: What did you use in electricity and magnetism?

RUBENS: Starling. Did you ever see that one?

NORBERG: No. That one I don't know. Was there anything special used for circuits and instruments as opposed to electricity and magnetism?

RUBENS: Well, the Starling book was especially good for its discussion of the development of instrumentation and the use of it. And we had a different course, which was unusual, from a man named Paul Higgs. Paul eventually became the president of the American Physics Teachers' Association. He was a very inventive guy. We had no text, but the course was called, let's see, experimental atomic physics. And we did things like the Millikan oil-drop experiment, and the Stark effect and the Zeeman effect. It gave a very good introduction to thermionic emission and electron tubes and measuring some of the fundamental constants of tubes, and things of that sort.

NORBERG: Considering how difficult it is to do the Millikan oil-drop experiment with equipment today, I can imagine that in 1929 it was hell on wheels.

RUBENS: No, it wasn't too bad. Oh, the toughest thing, and this had a lot to do I think with my future interest, we had a sputtering experiment, in which we had to make a half-platinized mirror and then make a Michelson interferometer out of it and then adjust it for white light, for instance. And that was tougher than the Millikan oil-drop experiment. But that was fascinating.

NORBERG: They had a sputtering device where you just did it yourself.

RUBENS: Oh, yes. And then we sputtered it until it was half-platinized.

NORBERG: That's remarkable, because I would guess that many schools at that time...

RUBENS: That would be my junior year. That would be about 1933. '32, actually.

NORBERG: Very good. One last question on the undergraduate years, Sid. Do you recall visitors coming to the campus, in physics?

RUBENS: In my graduate years a lot.

NORBERG: I'm thinking of pre '34, now.

RUBENS: Well, I used to attend the colloquia, beginning about my junior year. And I can remember especially some of the visitors, but I can't tell you which year. So some of these may have been my graduate years.

NORBERG: Fair enough.

RUBENS: One that I'll never forget was Felix Bloch. He had just come to this country.

NORBERG: Yes. It has to be after '34, then.

RUBENS: Yes. And I think Haas was there once. I remember Count Korzibski coming to talk on generalized semantics at one time. When we get to the graduate school years I can tell you a couple of remarkable ones.

NORBERG: Well, go ahead. As long as we are on the subject.

RUBENS: Well, Arthur Holly Compton spent a summer there and part of a year. Oh, and Professor Lochridge. Donald Lochridge. Did you ever hear of him?

NORBERG: Yes.

RUBENS: Well, Don Lochridge was one of my favorite graduate professors. I took courses from him in electricity and magnetism, in which we used Jeans, and also one on statistical mechanics. And he and Compton were interested in studying cosmic rays up and down the Pacific coast, at that time. They were building counters that they put on board ship that went up to Alaska and recorded the cosmic ray intensity between Seattle and Anchorage. Let's see. Who else was visiting there at the time. Okay, during my graduate work Kirkpatrick from Stanford came up and looked at the research I was doing. As a matter of fact, it turned out that right after I went over to the Naval Ordnance Lab, he telegraphed Henderson--who was at the Bureau of Standards at the time, during the war--and asked--this was before we got into the War, actually, in the spring of 1941--if I'd be interested in coming down to do instructing at Stanford. Of course I couldn't get away at that time.

NORBERG: Did Lawrence ever show up?

RUBENS: We may have had a Physical Society meeting in Seattle where he came. My contacts with Lawrence were primarily at Berkeley.

NORBERG: At Berkeley.

RUBENS: And Lawrence and Malcolm Henderson and the whole gang down there, at that time.

NORBERG: Joseph Henderson.

RUBENS: Yes.

NORBERG: Just to make the distinction here from Malcolm. Joseph Henderson was doing work in ionization, was he not?

RUBENS: Several things. His primary interest, I think, was in his graduate work. And he had three or four graduate students, one of whom was my roommate later. They got their Ph.D.s doing research on electronic field emission. Gertrude Felming, who shared a lab with me, Frank Abott who shared the same lab with me, and Kurt Dahlstrom, all did work on field emission. I think Kurt built one of the first field emission electron microscopes. Walter Dyke, who was my roommate while I was a graduate student, also did his work on field emission. He later went back to work with Wiesner at the Radiation Lab and then founded a company called Field Emission Corp. down in Oregon.

NORBERG: So that was one area that Henderson...

RUBENS: One that he worked in. My work was in plasma--you wouldn't call it plasma physics today, but we did in those days--and ionization measurement techniques. My doctor's thesis was on the anode spot phenomenon and doing a Langmuir probe investigation of it. But at that time I was the only one I think working on that with Henderson. And he had some people doing master's work, I think, in ionization, earlier.

NORBERG: What other sorts of research programs were going on among the other faculty in the Ph.D. program?

RUBENS: Okay, Lochridge had people working with cloud chambers. Harold Scramstead did his doctor's in that.

TAPE 1/SIDE 2

RUBENS: Hanrahan (?), who did his doctor's thesis on magneto-metallurgy, I would call it. In other words, he was trying to determine carbon content in carbon steels by measuring magnetic properties. Evidently Lochridge had worked for U.S. Steel Corporation for a time and got interested in that problem.

NORBERG: Were these the only two people doing advanced work?

RUBENS: No, no, no, no.

NORBERG: Sorry.

RUBENS: Let me finish.

NORBERG: All right.

RUBENS: We had a small Van de Graff generator. Barkas did his doctor's thesis on that, and I think he worked with Roy Kennedy. Roy Kennedy was there for a while. This was Kennedy of the Kennedy-Thorndyke Experiment. As a matter of fact, Ed Uheling came in the middle of my graduate work and gave courses in theoretical physics and had a few people working in theoretical physics. Later, many more. Incidentally, he just passed away this past year. Utterback was interested in primarily in ionization problems and also he got involved with physical oceanographic optical work. There was a man by the name of Jorgenson who was doing his graduate work with Utterback on that. I think the rest of the faculty were primarily involved with undergraduate teaching.

NORBERG: How did you come to choose Henderson?

RUBENS: It had to do with the discharge phenomena in gases. What happened was, I was fascinated by the striations in a glow discharge in the positive column. Henderson I think was demonstrating this once and I was talking to him about it. We were speculating about why the striations? He said, "We don't really know. A lot of people think it has to do with charges that build up on the walls." And I proposed an experiment--this one when I was a senior. I said, "Why not build a discharge that has no wall, to find out." So my proposal was to have a discharge between concentric spheres, or nearly concentric spheres, and then go in there with a Langmuir probe and see what happens. So I actually started my doctor's thesis when I was a senior.

NORBERG: How did you know about Langmuir probes...

RUBENS: Oh, I took a course in conduction of electricity through gas.

NORBERG: And other such techniques?

RUBENS: When I was a senior, what happened was I told you I lost a year, or a half a year. As a result I actually started my graduate work in my senior year when I went back the fifth year. One of the courses I took from Henderson was conduction of electricity through gases, in which we used Karl Darrow's text. However, I got a hold of Gunterschultz's two volumes, and with the help of a couple of NYA students we translated the two volumes into English, and we used that as a text. And in the course of doing that I got quite interested in electronic phenomena in gases.

Now, there's a little detour in 1937. There was a special electronics symposium on gases given the summer of 1937 at the University of Michigan, which I attended. I was in the middle of my research then. And that was fascinating. The courses were given by Leonard Loeb, Louie Tonks, Slepian, and Dow?? Among others in the course I found out were Jerry Wiesner. That had a lot to do with shaping my interests there after.

NORBERG: In what way?

RUBENS: Well, I decided that as long as I was interested in this stuff, I'd like to look at other tools for looking at what happens in a discharge, and that's how I happened to go down to UCLA afterwards to work with Joe Kaplan.

NORBERG: UCLA or USC?

RUBENS: No, UCLA. I taught at USC while I was working at UCLA.

NORBERG: UCLA. Okay. Let's not go that far ahead though. Can you describe briefly for me the Ph.D. program that you set out to solve, trying to understand what caused the striations.

RUBENS: Well, first of all we built this discharge chamber, which consisted essentially of two spheres. The outer sphere was also the envelope of the discharge. It was twenty-two inches in diameter. It was big. And the inner sphere was of sizes from one to four inches in diameter, mounted on a copper rod and surrounded by a glass sheath over the rod. And then I had a glass tower above through which I could drop a probe with a windlass. The hope was if there are striations, we'll measure the potential fall and all the other things you can measure with a Langmuir probe technique. The first thing that happened was there wasn't any striation at all in a discharge of nitrogen. Now, we used nitrogen because it was cheap and available and easy to get relatively free of water by the usual liquid air techniques. I kept making the inside sphere the cathode and the outside the anode. Well, the discharge rarely filled the whole thing. In that case you usually had a discharge around the small electrode, and then it would fan out into a cone to the big one, and it would move around and all sorts of things. One day Henderson suggested, "For the hell of it, why don't you make the inside one the anode and see what happens." Well, I did, and it turned out with carbon dioxide you could get striations. But meanwhile, the first thing I noticed when I did this, when the current density got high enough the anode glow broke up into a beautiful array of hemispherical spots. It looked like a golf ball at first. I put in other kinds of electrodes. I put in a flat disk, and you'd get a ring of these spots around the edge and maybe one in the middle and maybe another small ring around the central spot. I got interested in this phenomenon and what the mechanism was. I found that it had been discovered a long, long time ago in Germany. And it wasn't until I went to that symposium in Michigan that I learned that--I can't think of the professor's name--one of his students, who later went to RCA, had examined the same thing and wrote a paper on it. By using the probe technique I think I was able to come up with a pretty good theory of how the spots arose and what their function was.

NORBERG: Is that what is included in this publication in 1940?

RUBENS: Exactly. Now one of the things that was interesting is there was a following letter to the editor of *Physical Review* on this. I found that in oxygen nobody had ever found anode spots. But I found that if you got a high enough current density you could produce them in oxygen. You needed a very tiny electrode to achieve a sufficient current density.

NORBERG: This was an interesting phenomenon that was discovered quite accidentally when you were looking for some other explanations. What was the subsequent ramification of understanding the mechanism that produced these little spherical effects on the anode?

RUBENS: I'm not sure there is any. I think it was just a beautiful phenomenon, and we have an explanation for it. I don't think there's ever been any use for it as there were with cathode spots, which is a very interesting thing that has high technological import.

NORBERG: But as I recall, when you went down to work with Kaplan the two of you reproduced some auroral effects, which--

RUBENS: Now, let me tell you how that came about. Kaplan had been interested in the light of the night sky and aurora borealis spectra. And there are still some things about this that are fascinating to me. I think one of the reasons that Joe Kaplan was interested in me is that I'd had experience in building special discharge tubes, and I built all kinds of them for him. One of the things that he used to do is that he built a small globular discharge tube. You know, it looked like an old X-ray tube, with electrodes at each end. To get active nitrogen you start with fairly pure nitrogen, which you evolve by heating--oh, what is it--sodium azide. First, you pull as good a vacuum as you can. He could never bake out his tubes, because he had this sodium azide he was heating in the darn thing. So, when you're starting out, you've got a discharge with maybe some water vapor and even some more or less oxygen in it. And what he would do is he'd pulse the discharge periodically. He had a rotating sector that would turn this thing on and off. And after several days of this, the oxygen and water vapor would clean up by adhering to aluminum electrodes in there, more or less. And then you'd get a fairly pure nitrogen discharge, and then you'd begin to see an afterglow. You'd pulse it and then take a spectrum some time later. I got interested in several things.

NORBERG: This is Henderson's work now we're talking about.

RUBENS: No, this is Joe Kaplan's. This is when I first went to work for him, that was... He had been doing this for some years. He was one of Wood's students from Johns Hopkins and got interested in the metastable states back there. Now one fascinating thing about the aurora as you observe it visually, the vast majority of light is due to one transition in oxygen; it's a forbidden state, and so the aurora looks pretty green. Why only that one? How did all that energy get into that? That's an interesting problem in itself. The other thing is that, there was a physicist in India by the name of Mitra, who published a paper saying there wasn't any helium in the upper atmosphere because you never see it in the auroral spectrum. So, one of the things that I suggested to Joe is, "Let's build a tube and fill it with helium, flush it with helium, and we'll make it up to 90% helium if we want, and then we'll introduce a little pure nitrogen in there and see what happens." We got fascinating aurora spectra in the afterglow, but no helium.

NORBERG: No helium?

RUBENS: In the afterglow. Of course not. Those helium states are much too high before you initiate the spectrum of the nitrogen. That was published as an abstract from a meeting where we read the paper on the experiments. But one of the things that I got interested in down there was why take days to get these darn pictures. If you get a bright enough discharge, you can do it in an hour or so. I built long slender cylindrical tubes, put side things on them, side tubes that had the electrodes in them, and then put a mirror at one end of the tube. With a spectrograph at the other end and the rotating sector in between the discharge tube and the spectrograph, we obtained beautiful pictures of the afterglow spectrum in minutes that way and we actually began to study the spectrum as a function of time after the discharge is extinguished. In other words, we were getting a temporal effect. This was fascinating. But the difficulty of that was that there was not enough money to really support it. I got a letter from Barkas, in March of 1941, saying that the Navy was in trouble. The Germans had sprung a big magnetic mine attack on the British, and they were recruiting people to help. He asked me to write Ralph Bennett, which I did, and ten days later I was back working at the Naval Ordnance Lab.

NORBERG: Okay. Let me pick up a couple of things from the earlier period. How did you come to work with Joe Kaplan?

RUBENS: Joe I met, I think, at Physical Society meetings, and I think there was one just before I graduated that was at the University of Washington. He saw my work, and I told him I was interested in learning some spectroscopy to use as a tool to supplement the Langmuir this probe work. The original intention was to continue to do Langmuir probe work with some spectroscopy as an additional tool, and he invited me to come down. Meanwhile, there was a man who was ill at USC, and Lochridge used to teach at USC summers, and he arranged for me to have a temporary instructorship there while I was working with Joe. So that first year, that is, from 1939 to '40, I had this instructorship, and then '40 to '41 until I went to the Navy, I was working full time with Joe as a research associate at UCLA.

NORBERG: As a research associate. One of the things that we have left hanging here is that I noticed from your vita that the Voluntary Reserve with the Navy extended through to '34, but I didn't see anything beyond '34.

RUBENS: That's right.

NORBERG: Was it only for undergraduates?

RUBENS: No, no, no. If I had stayed in Seattle, I could have stayed on, but after this injury I had, I just gave that up.

NORBERG: I see, because you did stay in Seattle until '39.

RUBENS: Right. Because of this back injury, and then when I started my graduate work I was busy, and I just put it aside.

NORBERG: All right. As a teaching fellow at the University of Washington, what were the responsibilities?

RUBENS: Primarily, two things. You were a laboratory instructor under the direction usually of an assistant professor. In which case you gave the students their lab assignments. You corrected their reports and graded them. And also as a quiz instructor for lectures that were given by either Osborne or Brakel to undergraduates.

NORBERG: Do you recall what the stipend was for this?

RUBENS: Yes, I do. If you were a full time teaching fellow...

NORBERG: Which meant?

RUBENS: I guess, fifteen hours a week. That means, half your time was spent doing that. I think it went up... Let's see, it went up a little bit each year. The first year it was $54 dollars a month and tuition. You paid no tuition.

NORBERG: And this lasted for about nine months?

RUBENS: Just nine months.

NORBERG: Roughly, five hundred dollars a year.

RUBENS: Yes.

NORBERG: Plus tuition.

RUBENS: Right. And that was not bad. This is really Depression days. My last year I think it was $72 a month.

NORBERG: Were you getting any assistance from home?

RUBENS: Oh, yes. I had to. My brothers were helping.

NORBERG: You mentioned one brother-in-law going off to law school.

RUBENS: One brother went to Stanford...

NORBERG: And one went to Washington.

RUBENS: Yes. But when my father died, they both dropped out...

NORBERG: I see.

RUBENS: And went into business.

NORBERG: As you were coming up on this year in Los Angeles at UCLA, and teaching, and...

RUBENS: 1939 and '40.

NORBERG: What did you foresee as a career for yourself, assuming now...

RUBENS: I hoped to stay in academic or government research work.

NORBERG: But did you consider government research work?

RUBENS: Well, as a matter of fact, I had applied to Civil Service, hoping to go maybe to the Bureau of Standards. And after I got to NOL I periodically got offers from Civil Service to do work for less money than I was getting at NOL. The first offer came from Scott Field, from the Air Force, to teach radio communications. And later I got offers at the Bureau of Standards, as a result of that earlier application.

NORBERG: Can we turn then to the Naval Ordnance Laboratory? What was the problem that the Navy had, that they were interested in bringing you in?

RUBENS: First of all, let me tell you a little bit about the history of the Naval Ordnance Laboratory.

NORBERG: Fine.

RUBENS: It was started during World War I. And they had a building in the Washington Navy Yard called the Mines Building, where they actually built mines for the Navy. Also, something not very many people know, is that all the torpedoes and all the mines that they Navy used in World War I and through most of World War II were built on the Navy premises. They were never made by commercial firms until the latter part of World War II. During either 1939 or '40, Congress appropriated money to build a new fleet, because all the fleet we had then was left over from World War I. In the fall of 1940, the Germans sprang magnetic mine warfare on the British. And it was extremely, unfortunately, very effective. We had begun to build some of these new ships and had never considered protecting them against magnetic mines before this happened. So, all the people who were hired by the Naval Ordnance Lab, to my knowledge, that were being hired as contract employees in the fall of 1940 and the spring of '41, were hired to protect ships from magnetic mines, even though they did other things like design mines, and what not.

NORBERG: Tell me, Sid, how does a magnetic mine work?

RUBENS: You don't know?

NORBERG: I don't know.

RUBENS: All right. It isn't what most people think. It has nothing to do with magnetic attraction, believe me. But imagine there is something on the floor that's got a magnetometer on it, and something up here of iron or steel that goes over that. It's going to change the magnetic field down there, and...

NORBERG: And it's going to pick it up.

RUBENS: It's going to pick it up and set the mine off. Now, the other thing is that if a mine goes off under water, under a ship, not touching the ship, but *under* the ship, it can quite often break the ship in two. Whereas the contact mines used in World War I and maybe in the early part of World War II, would blow a hole in the ship, at best.

NORBERG: So this is what made those magnetic mines...

RUBENS: Extremely effective.

NORBERG: Such a problem.

RUBENS: Now, so, the idea of protecting it is you make the ship appear as if it's made of wood, as far as that mine down there is concerned, but up close to the ship it is quite different.

NORBERG: So now you can tell me about what you did when you came into the service.

RUBENS: Now, the interesting thing is, did you ever hear of a man by the name of Ellis Johnson?

NORBERG: I don't think so.

RUBENS: He was a professor at MIT before the war. Well, anyway, Bennett, who was at MIT, brought him down. He was the first man I reported to, and he said, "Well, we have this model program." I didn't know why the model program, or anything. And the man in charge was John Krauss, the radio astronomer? He said I'd like you to work for John and see what we can do on this model work.

NORBERG: This would be models of ships, I take it.

RUBENS: The whole idea was, to degauss a ship you have to put up to three sets of windings in to produce a vertical, transverse, and a longitudinal field, because all these have components that add to the vertical field under the ship. In those days, all they worried about was the perpendicular field, because we were working primarily in the northern hemisphere and not around the equator, as being a part that the mines would respond to. You see what I mean?

NORBERG: Yes.

RUBENS: So actually, they built models of ships ranging in size from mine sweepers to aircraft carriers. And the biggest models were like twelve or fourteen feet long, the smallest maybe so big.

NORBERG: Three feet.

RUBENS: And they were building these models out of material they hoped would represent magnetically the three kinds of steel that ships were built out of. Now, these three kinds are, so called, mild steel, high tensile steel, which was used primarily in the hulls, and special treatment steel which was armor plate. Special treatment steel is interesting. It's a soft steel with a lot of bits of tool steel thrown in. So, they were using pie plate material to represent mild steel and high tensile steel, because they knew that those were rather similar. What you're interested in is the effective permeability of these things at low fields. And the thing we had to model is a product of permeability times the thickness of the material of which the ship is constructed. I think my first job was to measure modelfields. In those days what we had to do was string wires around in these models and then measure the fields under them with magnetometers when the wires carried dc. The interesting device we used for this was called a aeroid magnetometer, which was invented at NOL. What it was a toroid coil--that's where the "oid" comes from--and an air-driven turbine... Do you have a piece of paper, I can sketch it for you better. You have a toroid that produces a uniform field inside the toroid. Right? It looks more like this. And down here you have a test coil which is rotated by the air-turbine.

NORBERG: At the bottom of the ring.

RUBENS: The bottom around a shaft. And it's spinning around this way.

NORBERG: Counter clockwise.

RUBENS: And it's of course measuring the flux through there. Right?

NORBERG: Yes.

RUBENS: And the way you drive this without having a motor or anything is at the end of this shaft we had a brass disk with a bunch of holes in it, and you blew air through those, compressed air. They were spraying at about a thousand cycles and they'd drive you crazy. But we could measure the magnetic field to within a tenth of a milligauss with this thing. And you could also orient the thing this way. in other words, you could use the second harmonic off of that and null it. What we would do is, this picked up the field and this was producing a field that would buck it. So by measuring the current in the toroidal coil and where nullifying the output of the rotating coil you were making a measurement of the earth's field, or the field under the ship, or whatever. The summer of 1941, Bill Brown, who later came here, William Fuller Brown, was brought to NOL. There were two people who I think were very important in staffing NOL. One was Quimby, from Columbia, and the other was Rumbaugh, from here. Rumbaugh brought many of his students, and he also convinced John Bardeen to come back. John Bardeen was Brown's boss. I worked for Bill. We had an unusual array of people back there at that time. There was a guy named Simon, who used to work with Bennett, I guess, at the University... No. He came from the University of Chicago. One of the first cloud-chamber people. And Shortley was there. And Atanasoff was there. And that was when I first heard about electronic computers from Atanasoff. Atanasoff headed the acoustic work. And Brown headed the magnetic work. Both worked for Bardeen.

NORBERG: Now, you were talking about measurement using this toroidal mechanism.

RUBENS: Right. Then somebody found a German publication on a fluxgate type of a magnetometer and we built a miniature one. And that was a big relief. I was the first one to try that out. But the thing that I got involved with, which really got me into magnetics, beyond this particular application, was that the laboratory had--I guess it was Krauss--had contracted with Bell Labs to build a portable plate permeameter. So we were going to go around and really measure the magnetic properties of the ship's steel both before and after it was fabricated into the ship.

NORBERG: Speaking now of actual ships, as opposed to models.

RUBENS: Absolutely. I am talking about battle ships; I am talking about destroyers, and whatnot, that I worked on. And I got stuck with the problem of working with Richard Bozorth and his two people at Bell Labs that were developing this permeameter and taking it over and using...

TAPE 2/SIDE 1

NORBERG: You just said you had taken these things over and were measuring...

RUBENS: Bozorth was in charge of the development of this portable permeameter. The reason he went there was because Bell Labs had the permalloy and they could have it in any amount or shape or size they wanted, I think. They had a permalloy yoke that was roughly about six inches long with some width to it. And this would be set down on a plate of steel. Now, it had a winding, which if you energized it would magnetize the steel somewhat down here. They also had a shielding yoke made of three layers of permalloy that went in between this and that. I shall indicate it this way. These were about an eighth of an inch thick. And then a small yoke here, which was essentially a pick-up yoke, to pick up part of this. And this went to a flux meter. And you'd try to demagnetize this to begin with in the usual way. What we were interested in was the normal magnetization period, or the initial part of it. Well, the big difficulty with this was that you have a skin effect here. You couldn't really make good magnetic contact. And the way I calibrated this was I used this on a big sheet of material and then cut a toroid out of the middle of it and found that it disagreed by about 20 or 25 percent. So Bill Brown and I came up with a scheme to get around this by actually taking one foot wide by three foot lengths of ships steel, and we made a thing called a double-yoke permeameter. I think you've got a paper of mine, a reprint of this. We published it.

NORBERG: In 1945.

RUBENS: Loeb made us publish this stuff. Leonard Loeb was sent to NOL as a Captain toward the end of the war to make sure we published whatever was worth publishing. And we had another weight under here. Now, our theory was that if this first yoke, which I'll show separately here, is wound in such a way that you produce a field like this that oppose each other here. Then down here you've got a uniform field.

NORBERG: Yes, fine.

RUBENS: So, if you had two plates, one above and one below, and these plates were cut from the same material, you could assume they're the same basic magnetic properties that an air-core coil placed in here would measure the field being applied; whereas a winding-around one of these would measure the B field.

NORBERG: I see. Yes.

RUBENS: With that, we'd usually get a one to two percent accuracy. And we'd check that against this too. So, we found that this was reading off by around twenty percent, so it was pretty good when checked with that, with samples, to give us an idea of how far off we were anyway. And we finally found that the materials we were using to build these models were probably good within about ten percent of the properties we needed, for the initial permeability, and so on.

NORBERG: Did this help to solve the problem of the magnetic mines?

RUBENS: Oh, hell, yes. It cut down the cost and the time. You see before that, what the British were doing were taking ships and actually winding huge experimental coils on these big ships and trying to find out whether they were nulling the field properly. By doing the models we could actually lay out where the coils were to go, how much current they were to carry for each heading and latitude before they were ever put in the ship. So it was a tremendously important economic thing as well as timesaving thing.

NORBERG: How long before you had an effective method for defeating the mines?

RUBENS: I don't know. The thing that gave us fits I guess, were the mines we may have lost by our own mine sweepers, I mean, the ships we lost. I don't think we lost very many large ships from mines after we degaussed.

NORBERG: Fine. But when did you begin degaussing them?

RUBENS: Well, I was on board a cruiser a week before Pearl Harbor that was degaussed. And when I came back, two weeks later--it was an aircraft carrier--it was still degaussed. So my guess is that from '42 on we didn't have too much trouble with magnetic mines, but that says nothing about the troubles we may have had from acoustic mines. You know that. And I won't go into that, because that's still classified.

NORBERG: Now, what then did you do for the remainder of the war?

RUBENS: Well, let me see. About the time we had finished this model work, the Mark Six torpedo problem exploded. Did you hear about that? I got right in the middle of that one. Ellis Johnson, that I mentioned I first went to work at NOL for, was the one who solved part of it. There were two troubles with the Mark Six torpedo. [I'm quoting here from unclassified stuff. This is written up in a book put out by the Naval Institute I think called *Pig Boat*.] The Mark Six torpedo had a magnetic exploder consisting of a rod of permalloy a yard long and a big coil of #40 wire around it. It had its own generator that put out power from a little water wheel that ran when the torpedo was running. The difficulty of this was it measured total longitudinal field and as the torpedo went too deep nothing happened, if the torpedo porpoised the exploder might go off prematurely, all sorts of problems. There was also a mechanical exploder that was defective in its design, that didn't work even when it hit the ship, especially if it hit a head-on blow, which Johnson figured out and found a replacement for it.

I'm not sure that we ever did come up with a good dc magnetic mine exploder after that. But I was involved with several of them. Another chap and I invented one that was classified when I last heard about it, and we didn't finish it when the war was over, so I can't say what it had, except that it did have a permanent magnet in it. And also we spent some time trying to devise a permanent magnet-type exploder for a small torpedo that was dropped out of air craft. A little device called "Fido," which would hunt the submarine and bump it. Again if it bumped it at a glancing blow, it didn't go off. So they were looking for an influence [?] exploder then. All right. The next thing I got involved with the war was practically over, and they set up an optical division of NOL, and I got interested in that because of my former interest in optics. I told Bill Brown I'd like to work in that. The man in charge of that was George Sabin, and I went to work for George. We had an infra-red division that I was acting head of for the time being. And one of the things we got was some evaporation equipment and I began making films. We were interested in developing an infra-red interference filter. I had a lad by the name of Mac Oldhaur, who worked on that with me, and I understand later he got his doctorate doing work on this. Also, and I can't remember exactly whether it was while I was still in the magnetic work or in the optical work, but there was an Australian attempt to build a torpedo that would find a ship's wake by interrupting a beam of light that was between a lamp and a photo cell out in front of the torpedo. And I was asked to look into this. I decided, after reading what I could about ship's wakes and crud in the water, and so on, that this wasn't a very good way of doing it. Another chap and I invented a thing called an acoustic impedance bridge. We put it in the head of a torpedo, and it looked like a pretty good way to go. And I worked on that almost until I left the lab...

NORBERG: An acoustic impedance bridge...

RUBENS: I got a patent on it.

NORBERG: But such things were around before. This would not be...

RUBENS: No.

NORBERG: Is that right?

RUBENS: The idea was to build a Wheatstone bridge with a transducer in one leg, and you put this in clear water, balance it. Fine. Now it goes into bubbly water and it's unbalanced.

NORBERG: Just as simple as that.

RUBENS: Just as simple as that.

NORBERG: I see.

RUBENS: The fact is I got patents on both the use of piezo electric and magnetostriction transducers. The first one we built used a magnetostriction transducer.

NORBERG: What was the cube square coil developed for?

RUBENS: I bypassed the magnetic shacks. When we started out, we were working in a poor environment. When Brown came, they decided that we better have a better magnetic environment to work in, and they built a number of wooden buildings at the receiving stations over in Anacostia which were called magnetic shacks. The floor space was around 10 or 12 feet by 20 feet all put together with copper nails. We needed a coil system to simulate the earth's field at any latitude or longitude we wanted. So there were a set of coils that would give you a vertical field, longitudinal and transverse, and I started out with square "Helmholtz coils". This consists of a pair of square coils spaced a little more than one half the length of one side apart. I think it was 0.6 of a side apart. Bill Brown told me that McKewon had written some papers on a way of improving the Helmholtz coils by putting a third coil in the middle. He asked me to investigate what would happen if we put the third square coil in, and before we were through we were putting five coils up. I had worked out the equations for getting the field on the axis of this system, and even off the axis. And I had a lady working with a Fridan calculator to check this out. And just for the hell of it, after I had all of this information on the axial fields of square coils, I said, "I wonder what would happen if we take, now that I've got this field all over a square coil, say if I take a square coil, and I put another pair out here, so that these are the edges of the cube--you would have three that way--but then I'll put another pair halfway in between. So I have five coils, and I will make this pair symmetrical. I'll have the same number of turns in this pair. What kind of a field would I get? Well, what you do is you set up an expression for the field on the axis, and I said, "All right. I will make the field here and here and here equal." And so you get three simultaneous linear equations and when you solve it, it turns out that if the middle coil has ten turns, and the intermediate pair has four turns each the outer pair (at the ends of the cube has 19.1 turns. Or for 100 turns in the middle coil and 40 in each of the intermediate pair the end coils have 191 turns. I'll make that 190, and build a coil like that. It gave a very uniform field over a large fraction of the volume. And it's easy to make.

NORBERG: Now, were these then used by the Navy in any way?

RUBENS: We built them all the way from a couple of feet to forty feet on edge. With one of the big forty footer, one of the interesting things we did was to use that as a test coil to measure the magnetic moment of a mine. You just set the mine at the middle of the cube coil, and turn the mine over and measure the deflection you get off a fluxmeter and you've got the magnetic moment. And with that you can make calculations. And we did that also for models of ships and parts of ships.

NORBERG: Let's go back to something you said a few minutes ago about Atanasoff. What was your relationship with Atanasoff in these years?

RUBENS: Well, when it came to this acoustic impedance device... Remember, I said he headed the acoustic division, or the department of the research division.

NORBERG: Yes.

RUBENS: Rumbaugh was in charge of research. Bardeen was his deputy. Atanasoff had a man in his division by the name of Gayle White, who was very clever in designing instrumentation for acoustics. I came up with this idea of a bridge for measuring acoustic impedance and I went to Gayle and studied this idea of this impedance bridge together and began working on it. And it had Atanasoff's blessing, but when it looked like it might go into a piece of ordnance equipment, Atanasoff wanted me to work for him, otherwise I had to stop working on it. And that's when I stopped working on it and went to work for Sabin. Not that I didn't like Atanasoff, but I didn't want to leave the group I was with. And actually we were cordial enough to each other after that. As a matter of fact, I remember going to a meeting in New York, I think, in which he had me out to dinner. And that's about the last time I saw him, I think.

NORBERG: But you mentioned that's where you learned about electronic calculator's first.

RUBENS: Oh, no. We used electronic calculator's from the day I got there with all this coil work.

NORBERG: Well, maybe I misunderstood--

RUBENS: Oh, I'm sorry. He mentioned this work he was doing at Ames. And also I think he had in the back of his mind that if he stayed on at NOL, he would build some sort of an electronic computer.

NORBERG: But you don't know that.

RUBENS: No.

NORBERG: That's speculation.

RUBENS: That's speculation.

NORBERG: You apparently were content at NOL?

RUBENS: Yes, until the war was over. I was happy enough doing this optical work, and I had a good friend. In fact, it turned out I met him the day he walked into the Naval Yard. His name Robert Gutterman. Bob had just graduated from Yale, and I guess he was hired by NOL. He said to me, "Hey, mister! Can you tell me where NOL is?" I said, "Sure, I work there." And I walked over and showed him the building. My roommate at the time, at NOL, was Arthur Hemendinger, who later went to Los Alamos. It turned out that Arthur and Bob Gutterman were working together in the mine unit. And the mine unit, at that time, was not allowed to talk to the counter-measures unit that I was in.

NORBERG: Yes.

RUBENS: Later, when the research division was formed, the two units were put together. One day Hemendinger brought Gutterman home, and we became good friends. It turned out that Gutterman was a former student of Howard Engstrom's, at Yale. And they were also very close social friends because they lived in the same apartment house in Arlington. And Engstrom told Gutterman about this ERA that they were considering, and he told me about it. I suggested, "Gee, as long as this is going to be in Minnesota, there's one other man here that you ought to tell about it and that is Howard Daniels." I don't know if you've run into any of his...

NORBERG: Only the name.

RUBENS: Because I knew that Howard was from Minneapolis, and I thought he would like to get back more. Daniels and Hammindinger and I were good friends. We spent quite a bit of time together. At that point, both my wife and I were not interested in staying in Washington forever. Both of us came from the West. My wife was from California. That's where I met her. And we figured, well going to Minnesota would be one step toward going back to the West Coast. If it didn't work out, we'd look for something in the West. And that's how I happened to come here.

NORBERG: Let me try and pick up some little notions that I'm interested in here. Your work at the University of Washington and later with Kaplan at UCLA did not involve magnetism in any direct way. Is that correct?

RUBENS: That's correct. I learned my magnetism at NOL. I didn't even know how to measure permeability when I got there. I learned it all from Bill Brown, really, and Krauss.

NORBERG: How did this process of learning go on? Was it just through interaction, on the job training, or was there any special attempt at lecturing, reading materials, anything of that kind?

RUBENS: There wasn't much reading material. We did have, however--and this is important--the laboratory early on started writing what were called NOLMs--Naval Ordnance Lab Memoranda--and NOLRs. We had a big library full of this stuff. And this was the place you would go to, and it had references to texts, if there was need for it. Mostly, it was the tutorial method. Bill, literally, showed me how you measure permeability and how to use a Type 2290 galvanometer as a fluxmeter, which I'd never heard of before.

NORBERG: Did you have any connection with magnetic recording during those years?

RUBENS: No. The only magnetic recording I knew of was a kind of wire recording that was used for music, and that's all.

NORBERG: That's it.

RUBENS: As a matter of fact, my first introduction to magnetic recording was the day I got here.

NORBERG: I'll come to that in a minute. All right. So there is this interaction going on with Hemendinger, Daniels, and Gutterman, eventually. What did they tell you? What could they tell you about...

RUBENS: About ERA?

NORBERG: What Engstrom was planning.

RUBENS: Well, number one, I knew from what Gutterman told me that Engstrom was involved himself with this decryption work, during World War II. I didn't know how it was done. I didn't even guess that computers were involved at that point. I knew that it would be digital, but that's about all.

NORBERG: How did you know that?

RUBENS: Well, by digital I mean that it involved things like gates and flip flops, rather than cw types of things.

NORBERG: Yes.

RUBENS: Because I knew about counters. We built them in Washington, these cosmic ray counters. As a matter of fact, I wrote documents early pointing out that the real basis for computer technology was the work that cosmic ray physicists had done with electronics together with some kind of storage.

NORBERG: Now, when were you writing these documents?

RUBENS: At ERA.

NORBERG: So it was post 1946?

RUBENS: Yes.

NORBERG: Turning back now to Engstrom.

RUBENS: All right. So I knew that. Now, when I got here, however...

NORBERG: Well, let's not go quite that far ahead. How did you get the offer in the first place?

RUBENS: Oh. Gutterman. He introduced me to Howard Engstrom and William Norris. Let's see, now, I first met Engstrom, I think at what was later called the Naval Communication Annex... It was a place on Nebraska Avenue in Washington, DC. I don't know what its name was during the war.

NORBERG: CSAW.

RUBENS: Yes. I met him there, I think. But, as soon as the ERA was established, he had an office on 14th Street and about F Street, on the second floor, just a one or two room office where he was interviewing people. And then Engstrom had me interviewed by Norris and, I think, maybe C. B. Tompkins. I'm not sure. And I got the offer.

NORBERG: Now these guys had already been in business almost seven months by that time.

RUBENS: Oh, wait a minute. I got this offer in March of 1946. Definitely long before I came. Gutterman actually came in the fall of '45. And I know that I met them before he left Washington. So it may have been in the late summer of '45 that I was introduced to them, before they had set up this office.

NORBERG: They didn't actually set up an office until January '46, though.

RUBENS: Now wait a minute. There were several offices. I'm talking about the Washington area.

NORBERG: But which Washington area office? The one for ERA in Arlington?

RUBENS: No, that's much later. First there was a small office, only for Engstrom and, I think, a secretary, used part time when he was still in uniform, on 14th Street. Then there was an office that was around Rhode Island, or something, where they actually were at work as ERA. And then later they went over at Arlington.

NORBERG: So you think the company was formed in late '45?

RUBENS: I know it was, because I know that's when Gutterman came here. No, wait a minute. Gutterman's first pay check, and mine, for that matter, came from Northwestern Aeronautical. So, evidently, they had approached Parker, I think--I'm guessing--in mid '45 and made the arrangements to form the ERA. And when Gutterman got here, his work had nothing to do with computers or computer technology. He had a patent that he had gotten in the Navy, on sort of an ultrasonic laundry. He was working with acoustic mines and magnetostriction oscillators and so on. And the company got a contract with the Warner Swazey Corporation, which makes lathes and mills to see if you couldn't improve the cutting properties of a tool by vibrating it at ultrasonic frequencies. And that was the first contract that Gutterman had. He worked on that.

NORBERG: So you received an offer in March after having been interviewed by these people?

RUBENS: Right. Right.

NORBERG: Did you have any idea what it is you were going to do?

RUBENS: Engineering research.

NORBERG: Period.

RUBENS: I had no idea that it had anything... You know, I should have guessed that if there were any magnetic stuff involved, they would make use of mine and Howard's background in magnetics and acoustics and so on, but no, I had no idea.

NORBERG: Then why did you take the job?

RUBENS: It appealed to me.

NORBERG: But *what* appealed to you? If you didn't know what it was that was going to happen, what is it that appealed?

RUBENS: We were going to do engineering research on a contract basis. We did this all through the war. I started out as a contract employee. Well, okay. I went to NOL, I guess you would say, for two reasons. One, it was work for a salary, which I needed, and b, for patriotic reasons, a combination of the two. That's why most of us went and why most of us didn't stay. Bardeen had left, Brown had left, and there was no immediate work on the coast, and this looked like an interesting thing. As a matter of fact, nobody in the beginning group felt that this was really going to be a serious, stable economic thing. As a matter of fact, Tompkins left after a year or so to go back into academia, and he sold all his stock for like a dollar a share, or less than a dollar a share. He paid a dollar for it.

NORBERG: You got the job offer in March, but you didn't come until August. What was going on in the intervening period?

RUBENS: I stayed on at NOL until July 1st. And then I took my wife, who had never met my family up to Spokane, to Spokane and Seattle for about a six-week vacation.

TAPE 2/SIDE 2

I arrived here the 4th of July and met the ERA people. We went on a picnic with them. And then we went out to Spokane and Seattle, and came back here in August and started looking for a place to live, and I went to work immediately. And on my first job here, interestingly enough, I guess it was called Project Goldberg. And, actually, it had, I think, two bosses on that. One was C.B. Tompkins and the other was John Coombs. They weren't sure how they were going to build this device. The original specifications called for a digital-store using photographic film, which could not be altered, of course, but would be useful for one run. They had a captured German Magnetophon tape recorder here with lots of good tape, a couple of good heads on the machine. The electronics was a mess. We never did use it as a recorder. And they asked me to look into ways of proceeding. One is magnetic recording of some sort, and the other is to investigate the possibility of solid-state delay lines, because there was some work being done I think in the Air Force at that time. It was sponsoring some work using glass prisms and bouncing the stuff around for long delays. These were both interesting.

As a matter of fact, I had two groups then. I had one group working on delay lines and the other were going to investigate magnetic recording. And the first magnetic recording exercise was, okay, how can you go? Well, you can take an endless loop; you can look at a disk; or you can look at the surface of a drum. All we had was this magnetic tape, which was about a quarter of an inch wide. I thought, well, the first thing I'll try is a five-inch aluminum drum, and I'll paste some of this stuff on to the drum and use these heads off the Magnetophon recorder and see how it goes.

Just as an aside, have you ever run into the name of Lou Chaloux?

NORBERG: Yes.

RUBENS: Lou, evidently, was first given this Magnetophon recorder. He took one of the heads off and he was trying to record onto a disc made of mild steel. Of course, Lou's background didn't include magnetics, so he didn't realize the importance of going to a high coercivity material. So Robert Perkins built me a small 5-inch drum and we put it on a grinding quill to spin it. I used to take the "Scotch" (adhesive) off the Scotch tape by passing the tape through Toluene and holding it against the spinning drum and put a piece of tape around it. Sure enough, we could record digits. I think I turned over to you the first complete report I ever wrote on that.

NORBERG: When you arrived in August of '46, do you recall what you were asked to do; that is, was the problem laid out for you at that time? The first project, by the way, was Orion, which became Goldberg after a short time. Was the problem laid out for you in any specific way as to what it is that the objective for this program is going to be?

RUBENS: We only knew that ultimately the specs called for so many tracks of digital information, and that they were going to use this... I knew this was going to use encoded material, in which you use binary code to represent alpha-numeric characters. I knew that. And they were going to use this as some sort of a way of sorting and comparing material. But at that point, you see, we had the order of a hundred people here, who were cleared for crypto and who had worked at NSG all through the war. And they were pretty tight with those of us who even had come from the Navy who had... You know, I had top secret clearance for ordnance work. But they were pretty tight about making sure we didn't know how this stuff was to be used. And it wasn't until much later that I got involved with what really goes on in the cryptological work.

NORBERG: But did you learn what had been done to date on the project that you were to work on?

RUBENS: Very little. Because all of what had been done was that darn thing of Lou's, which was nothing. At that point, as soon as I had showed (a) that you could get good magnetic recording material, and (b) that you could buy heads--we bought heads from Brush Development Company, and also that we could operate these on a drum that was about a meter in diameter and still have fixed heads at a mil away, and that it would be stable through reasonable temperature fluctuations it was decided to build such drums for delivery to the Navy. I was privy, then, to what the people who were building the deliverable drums were doing. But I never got involved with how they were going to use these mathematically, let's say.

NORBERG: Let me remind you of a few things. First of all, by the time you came in August of '46, that is, actually on the job...

RUBENS: You see, I don't know when Orion got started here.

NORBERG: June of '46.

RUBENS: Okay. It only had been going about a month.

NORBERG: That's exactly right. It had only been going about a month by the time you arrived. But two things had been done at that point. One, a series of letters had been sent to various places like the Bureau of Ships, Brush Development, some of the other companies that were making these various machines, to either get full devices--that is, recording devices, some of them wire, some of them tape...

RUBENS: Oh, yes. Okay.

NORBERG: Or, to get various kinds of literature, some of the German publications during the war, some of the intelligence reports that were written in English, and so on. That's one thing that was being done. And those were coming in on a sort of random basis, as you would expect.

RUBENS: Right.

NORBERG: And so, I would guess that by the middle of August a pile of these reports and machines would have been...

RUBENS: One of them was this German one on Magnetic Recording that I translated, and wrote the translated version for ERA use. Yes, the work had been done on Magnetophon influence on supersonic bias, and all this sort of thing. Sure, sure. That's true.

NORBERG: The second thing is that someone--and I've forgotten now whether it was Coombs or Daniels--had written to the NCR people--the location of the Computing Machine Laboratory still had a section back there at Dayton, Ohio.

RUBENS: That wouldn't be Daniels then, that would be Coombs perhaps.

NORBERG: Coombs wrote back and asked if they could receive, here at ERA, the equipment that Ralph Palmer had been working on before he returned to IBM. Now, that equipment arrived in about September.

RUBENS: It might be later.

NORBERG: I'll have to go back and look to see when the exact date was, but it was...

RUBENS: Do you know what that equipment was?

NORBERG: Well, I have a guess, but that's all. It was some sort of magnetic disk.

RUBENS: It wouldn't be this Magnetophon tape recorder, because that was there early.

NORBERG: Yes, yes. It was some sort of magnetic disk...

RUBENS: But that may be, that may be.

NORBERG: ...equipment. And when it was tried, it was found to be wanting, and just...

RUBENS: Well, Ralph Meader was actually in charge of ERA when I arrived. It was before Norris came. Meader had been at Dayton. Palmer was one of his favorites all during the war, and it may have been Meader who wrote for that.

NORBERG: Could be. But did you ever work with it yourself?

RUBENS: I never saw it, and never really knew that he had looked at anything other than maybe the Magnetophon tape recorder.

NORBERG: All right, let me approach this from another angle. Those are the things I know so far and we can look at the Orion reports and see what it is you wrote. It's my feeling, from reading the Orion reports, and also some other things that I'll note in a moment, that up until the middle of August '46 the people who were working on this Orion problem, the attempt to build a scanning system, really were floundering. They had no basic...

RUBENS: When did Tommy come?

NORBERG: Did he ever come here or did he stay in Washington?

RUBENS: He was here when I got here. He was the one... You see, I never heard of this under the name Orion. He put me to work on Project Goldberg. Task 13.

NORBERG: No. There are signed statements by you under Orion.

RUBENS: Really?

NORBERG: Goldberg is a name that comes up toward the end of the year.

RUBENS: We had a Task 13.

NORBERG: No, that's not until '47.

RUBENS: Okay.

NORBERG: This is under Task 1. 1 H, as a matter of fact.

RUBENS: I'll be darned. My memory's bad. Anyway, as I remember it, when Tommy... You know, I used to have arguments with Tommy. He was right, it turns out, and I was wrong. As soon as I showed that on a drum you could get good signal to noise, you know, with digital information, he said, "Why the hell aren't you using non-return to zero?" And I said, because the electronics are going to be too damn difficult. There's nothing wrong with magnetics. It's the way it's all done today. As a matter of fact, Thornton and Keye later built another storage system using that, and I was consulting with them on the magnetic part of it. And they had one hell of a time with the electronics, just as I supposed would be the case. The electronics were so damn simple, but I was throwing away half the storage density. No question about it.

NORBERG: Let me go back again. I'm going to keep pushing this point, because I think it's valuable. When you asked me about Tompkins, you deflected my train of thought a minute, and it took me a moment to get it back again. But in fact, up until mid-August the group that was working on this scanning system, this Orion, was really not doing very well. They didn't understand what their problem was. They didn't even understand how to approach it.

RUBENS: Can I ask you a question?

NORBERG: Certainly.

RUBENS: The first two guys who were assigned to me to work on Orion or Goldberg, or whatever, were Don Ammerman and Bill Horten. Ammerman worked with me on the magnetic part, and Horten on the acoustic delay line. Now, were either of these men assigned to Orion, before I got there?

NORBERG: Well, that I don't know, because the first report I have is...

RUBENS: They had no experience in magnetics, so...

NORBERG: Exactly. That really is a problem. Now, after you arrived, the first report you wrote shows a completely different approach to the problem altogether. You have set up a scheme for doing the research to get results. You broke it up into two parts, I think. Here now I'm a little fuzzy, because the reports don't show this. But it looks to me that it was broken up into two parts. One part was the medium on which the information would be recorded, and the second part was how you were going to read it, record it...

RUBENS: Get information in and out.

NORBERG: That's right, in and out. Now, once that separation is made, then you can set up different tasks-- tasks is the wrong word to use here because it confuses with the contract--but different objectives that can be met on a weekly basis.

RUBENS: Of course, of course. And that's the way I ordinarily do team research.

NORBERG: The fact that that didn't seem to be the case before suggest to me that you brought it to ERA. That's the point.

RUBENS: I may have brought that technique of approaching the problem. I did not bring the idea of doing it magnetically. That was Tommy.

NORBERG: No, that was here already.

RUBENS: As a matter of fact, Tompkins was going to do it several ways. When Arnold Cohen arrived, he was going to have him look at it with either electron or iron beams. Remember?

NORBERG: Yes.

RUBENS: But, you know, I never did know, I never asked Gutterman this, but I assume, after I got here, that they were interested in me not because I was a particularly competent physicist or engineer, but because I had some background in magnetics. It turned out that a hell of a lot of our work here was in magnetics.

NORBERG: And I'm trying to understand why that was the case. Why that decision was made.

RUBENS: I don't know if that was a deliberate decision or they just hired me because I was interested and I had a fairly decent background. For example, I put down in my vita that I did belong to this Naval Communication Reserve, when they asked for military experience. But I don't think anybody paid any attention to it.

NORBERG: They may not have known what it was, although Norris should have if he was a member in Nebraska.

RUBENS: He knew, but then he wasn't involved. Norris wasn't here until Meader left, which was almost a year later. As a matter of fact, Norris did his damnedest to persuade me once to leave and come back to the Arlington office. I think this was when there was trouble brewing between Norris, Parker, and Engstrom.

NORBERG: All right. I'm going to try again, yet again, Sid. When you came, how many different ways were being considered to do this task? That is, to record data and to read it back again.

RUBENS: All right. Are you talking about the magnetic way?

NORBERG: No, no, no. The magnetic way is only one. How many other ways...

RUBENS: I was assigned two and only two, and those were delay lines.

NORBERG: Yes. You mentioned that.

RUBENS: Preferably solid delay lines. The mercury delay line was really... I don't know what they used them for, but they bought a few that they were using for other things, that I think went into another project. You know, one of the tragedies of ERA was that in those early days they didn't have a long enough research and development program to afford to follow up on some of the things they found. I'm going to tell you a few things that will startle you, and I'm not going to be egotistical about it. In the course of this solid-delay line approach, I discovered, to me, a way of producing surface waves, Raleigh waves, on a square aluminum rod, and I had a variable length delay line. I never did anything with that. I never published it.

NORBERG: Did someone else do something with it?

RUBENS: I wrote a report on it. No, we dropped that when we went into the magnetic drum work.

NORBERG: But did other people outside ERA?

RUBENS: We never published it. This was classified at that point, and we never bothered. We had no money for publication just to build up a reputation at that point. If there had been an immediate commercial application, I think they might have done something about it, as they did with some of the drum applications. Do you see what I mean?

NORBERG: Yes, but what I'm trying to learn is how did it get into the commercial field, if ever, by any other company or person?

RUBENS: I think it was incomplete. The uses of acoustic waves today are completely separate. I don't think anybody ever knew that I did this.

NORBERG: I guess all I'm trying to do is to understand why you think that it was so important.

RUBENS: Well, at the time I don't think I did, except that it was a unique way of getting a variable delay line. What I did is I had a z-cut crystal at one end of a rod and an x-cut crystal on the surface, and I could move it along. Now, it had difficulties. You put on a thin film of oil, and it's all gone. But it's interesting. And the velocity was much different on the surface than it was through the rod.

NORBERG: Other people, I take it, later on have used this in effective ways.

RUBENS: For other things. For other things.

NORBERG: Fine. That's what I was trying to understand.

RUBENS: The solid delay line was being used, I think, in the radar work, and so on. They were using cast prisms of quartz, and so on. That's how I think Tommy got interested in it.

NORBERG: I see. How about a second example of this same sort of inability to capitalize on new ideas?

RUBENS: Oh! I had a contract with the Air Force through Arnie Cohen's brother-in-law, Chuck Colberg. Chuck had been a naval officer involved with radar, I think, during World War II. Anyway, he was in a radar counter measures group in Dayton, and they had a contract out to try to develop a technique of recording high enough frequency information to record radar information. Video recording. And this was about 1948 or '49, I think. It was after we finished project Boom, I'd say, about that time. they figured that at that point there were enough people working on magnetic recording to do this by some kind of magnetic means that they'd like to look into other means. And somebody at Mining Company had made a tape, a plastic tape coated with barium titanate. Why, I don't know; or what for, I don't know. But anyway, they gave us--Gutterman, I think, wrote the proposal to investigate dielectric recording. He suggested that this tape would be a way of storing information. They got a contract and I got stuck with being in charge of it. So, I got samples of this tape and you couldn't do a damn thing with it. Actually, I had some barium titanate crystals grown for me by Brush Development Company. I began to investigate their properties and I had some fun with that. Meanwhile, I was just thinking about the problem and Joe Keller and I wrote a patent disclosure on using a wide piece of magnetic tape that is going to move this way--here's the edge view of it--and here's a wheel--this way--on an axle, and its going to have heads on it, revolving this way its going to write this way and read that way to obtain helical recording. We wrote a patent disclosure and nothing was ever done with it. That was long before it was reinvented, otherwise, and produced. As a matter of fact, this was the first good way of video recording on magnetic tape. Well, that's another one.

NORBERG: This is rather unfortunate from ERA's point of view anyway.

RUBENS: Well no. In a way. It was actually before '52. It was before we merged with Remington Rand. And even after we'd merged they still had this, you know, this disclosure. They weren't interested in this sort of thing.

NORBERG: Okay, Sid, let's try and get back to your tasks in '46.

RUBENS: Let's go back to the Goldberg one. The only three media that had been suggested were the magnetic, the ultrasonic, and the ion technique that Arnold was going to look at. I can't remember that any others were there. Did you find any reference?

NORBERG: No, I did not. In fact, most of the things that appear in the record have to do with the magnetics.

RUBENS: Well, because that's what was really used and available.

NORBERG: The beam, the charged-beam particles, the beam of charged particles didn't work, because by January '47 Arnie wrote a report that essentially showed that it wasn't going to be effective. I don't have any idea of what happened in the case of the solid delay line.

RUBENS: I wrote a good report on that, and I think I may have given you one, and, if not, I think I still have it, over at Univac, or at home maybe. Nothing more was done on that. Once they decided to go with the magnetic drum, they dropped all investigations of other ways of doing it. The big problem there then was two-fold. One was, okay, to make sure we had a good source of heads, which I arranged with Brush Development Company to provide. And the other is to get the medium. Now this is interesting. Doctor Will Wenzel at Mining Company was at Bureau of Ships when I was at NOL. We became good friends. I didn't work with him there, but I saw him a lot. He hired two of my associates at NOL, namely, Robert Herr and Byron Murphy to come to work for him at Mining Company when they hired him. Well, when I got here, I was surprised to find that Wetzel was at Mining Company and looking into how you make magnetic tape. He was mixing this stuff up in test tubes and pouring it on paper and stuff like that. And I gave him some of this German Magnetophon tape samples, and I said, "Bill, when you can make it like this, you will be in business." Meanwhile, they were furnishing me a black iron-oxide tape that they were making, which was on paper in those days to begin with. Later I think it may have been on plastic. And I was pasting it on this little drum. The first ones we furnished to the Navy were on the big drums, that same stuff. Now, what we wanted to do was to spray a plastic vehicle with these magnetic particles in them on the drum, which would be much better. I went to Bill and I said, can't you give us the stuff you coat and we'll spray it. He said, "I'd like to Sid, but I can't do it. The reason is we gave some of this as a sample to some academic institutions and it got in to the hands of our competitors. The Irish and some others were in business as a result." I said, Bill this is of very great importance to the security of this country. He'd been working for the Navy and he understood this. So he agreed finally to make me a dispersion of the stuff in lacquer that you never make tape out of. And that's what we used for a long time. We'd spray the stuff on; all our drums were made this way.

NORBERG: What sort of research were you doing on the effectiveness of the spraying?

RUBENS: None, really. We had something worse than what you spray paint on a car with. It was terrible. How it worked I'll never know. But we did buff it some and it worked. My guess is the sprayed drums were about... First of all, we were only shooting for eighty digits per inch on a non-return-to-zero basis. We weren't shooting for a high density. If we had been, that wouldn't be good.

NORBERG: Were you aware of any of the German tape production during the war, by this time?

RUBENS: Oh, the week after I got here...

NORBERG: You said you had samples.

RUBENS: I had samples of the Magnetophon tape, and I had the long article which I translated, which told something about how you make... No, they didn't tell you how you make it, only how you use it. Right. No, I never had any information on the techniques of fabrication of the German stuff.

NORBERG: Because I've seen some reports of the joint intelligence committee, the U.K. and the United States, where they had interviewed German production people...

RUBENS: Sure. Sure.

NORBERG: And had drawings of the equipment, and photographs of the equipment for processing and knew about the arrangement of domains and particles along the tape in the German models.

RUBENS: And of course the Mining Company in later years used magnetic fields for orientation, and so on. But, no, I didn't get into that part of it.

NORBERG: You never did any of that. In producing the drums by spraying, when did you want to have a change in density such that the technique had to improve, and how did you go about doing that?

RUBENS: All right. Let me tell you about a little experiment we did. First of all this gets... If you're going to improve the density, you're doing two things. You're changing the media, and you're also changing the frequency at which you are recording. You're upping it.

NORBERG: Yes.

RUBENS: All right. Now, the first heads we had were permalloy or something like permalloy, like mu metal and seven to fourteen mil thick lamentations. So we were limited by eddy currents to going much over something like 10/20 kc. I read an article about the work they were doing in Holland an ferrites during the war, and I got a copy of Snook's little book, and I ordered one sample of ferrite core, about an inch in diameter, from Philips, and measured it's properties, wound it as a toroid. It looked good. I gave it to Bob Herr. He broke it in two, and he made a recording head out of it and published it in *Electronics*. That was the beginning. All we needed to do was to get good ferrite and make ferrite heads, which we did. I also decided to see how high we can go in frequency with this sprayed surface. And I had them build me a small drum that we were going to run at 50,000 rpm in a safe, with these ferrite heads, and showed that, sure enough... And in those days all we had was vacuum tubes, but we could go to one megacycle bit rate without any troubles.

TAPE 3/SIDE 1

NORBERG: Let me ask you something about Herr. It's my recollection that the paper on reproducing heads with ferrites was not until 1951 in *Electronics*.

RUBENS: Right. That's about right. But I would guess then that I got that ferrite about in '50 or maybe earlier. I hadn't had it for more than a year. It took some time to get it published. So I gave that to him either late '49, early '50, I think.

NORBERG: Early '50. Because as I remember in your files down at Sperry there's a manuscript of his that's dated December 12, 1950, something of that kind, and then it's published in the spring of '51. But again, you remember we were talking a few minutes ago about the 1946 situation. The problem of the magnetic surface and improving density, and so on, that you've just been describing is one aspect of it. The heads are another. But isn't there also a problem with the circuits to the heads?

RUBENS: Of course.

NORBERG: For reading and writing.

RUBENS: If you're going to go higher than a megacycle, you've got to produce fractional microsecond pulses, and this was damned difficult to do with vacuum tubes in those days, because of the inter-electrode capacity, and that sort of thing.

NORBERG: Did you use basic circuits that were already available, or did you go about designing new ones?

RUBENS: I never designed any new circuits. I think we had flip-flops and gates that were essentially available.

NORBERG: But for use with the heads? I mean, flip-flops and gates would be used the counting and reading, and so on.

RUBENS: For the reading. Oh, you mean for the writing circuits? I think a free running, what do you call... There was a circuit, and I've forgotten the name of it, that you could us for... You would set it off, and it just free ran it at a given frequency, giving your pulses.

NORBERG: I think of a blocking oscillator.

RUBENS: Blocking oscillator. I'm not sure what the circuits essentially were. Now the first recording that I did on this drum was really static recording, using dc. As a matter of fact, what we'd do is the following: we had this drum set up, and we could turn the drum a given distance. And just turn on dc, turn it off, and move it a certain additional distance, and so on, and then just read it with a reading head and amplifier giving a display on a C.R.O.

NORBERG: Actually moving it manually?

RUBENS: Yes, to begin with. In other words, one did the recording statically. As a matter of fact, the Goldberg drum was loaded that way, because it was loaded off teletype tape.

NORBERG: I'm going to try and stimulate some more memories for this period, around '46, because I believe that the period from November '45 to November '46 is very critical, and it's less known than what happened after '46.

RUBENS: Oh, I'm sure. I'm sure.

NORBERG: Because in November '46, the Management Committee begins to meet and so does the Planning Committee, and so on.

RUBENS: Right.

NORBERG: So there is some record for that period.

RUBENS: I understand.

NORBERG: But it's in that early year that it's not quite clear yet, so, let me come back. You've mentioned a couple of times now that Norris was not here yet, that Meader was the man in charge of St. Paul.

RUBENS: When I arrived. Yes. That's correct. And then technically the people I reported to, I think, were John Coombs, C.B. Tompkins, and John Howard.

NORBERG: Let me ask you about each of those fellows individually. Tell me something about Tompkins.

RUBENS: Well, Tompkins was a remarkable person. First of all, he didn't have the usual background of most mathematicians. I think he had a degree in chemical engineering to begin with. And also, because of his work with Engstrom's operation during the war, he knew something about the hardware aspects of digital data processing. I think he realized quite early that this magnetic stuff had real potential. Because of his association with the military community and the cryptological aspect of it, I think he was quite interested in what I was doing. He showed more interest from the scientific standpoint in what I was doing than most of the other guys that were around there.

NORBERG: How did you interact with Tompkins? I mean, what was the give and take between the two of you at this time?

RUBENS: Well, we had a lot of arguments about this return-to-zero and non return-to-zero. Other than that we were just good friends.

NORBERG: Well, that's fine. I'm not challenging that. But how did it go on. What did you do, sit down with a pencil and paper? Stand at the black board? What?

RUBENS: As a matter of fact, I think he gave me very great freedom to do pretty much as I wanted to go, once he got my plan of going. And I can't remember very many discussions with him about the Goldberg work, for example.

NORBERG: Did you notice him interacting with other people?

RUBENS: No. Not particularly.

NORBERG: No.

RUBENS: Because a lot of that, I think, was classified, behind-the-door stuff on, you know, what you do with this stuff.

NORBERG: How about John Howard? I don't know much about Howard.

RUBENS: Well, John was very conservative, as I remember.

NORBERG: Technically or politically?

RUBENS: Probably both. Rather quiet. And I don't really remember having any either exciting or casual technical conversations with him. I think he was mostly involved with my work administratively, and that's all. Coombs, on the other hand, I don't think he understood the science approach to this work. Coombs was, I think, one of Vannevar Bush's people at MIT. And it took a long time before you began to appreciate the importance of basic measurements on this stuff, before you did much with it. But I remember having a conversation with him just before he left to go to IBM, saying that he's sorry he didn't get to know me better earlier.

NORBERG: If this is the case, what you're suggesting is that you were really on your own.

RUBENS: Pretty much. As a matter of fact, at the time that I arrived here, I think I was the only one who had an advanced degree in physics in St. Paul. Of course, Wakelin and Bryant were in Arlington, and I interacted with them quite a bit. But most of the people here were engineers and had a different philosophy of how you go about magnetic research, I think, than I did, at that time.

NORBERG: Did you have any connection with the University here, at that time?

RUBENS: No. Not in '46. But when I became the head of the so-called Physics Department at ERA, I tried to get the University interested in having night courses in first year graduate work in physics. No interest whatever. Primarily for some of my people who wanted to work on advanced degrees. No interest whatever, at that time. My first real connection with the University came... I can't tell you the exact year. But the University decided that it ought to do something about getting a computer and invited me and a number of people--I had the title of Director of Research of the Defense Division of Univac, at that time--invited me and a number of people from industry who had similar titles, along with most of the heads of the departments of IT and a few other people to come down and spend a weekend at the Lowell Inn. The first thing they started out by saying, "Well, what should the University be doing that it isn't doing," but then it finally turned out that maybe the University should do something about getting a computer. It turned out that I wrote a letter, as a result of this, to Norris, and they finally got an 1103 as a result.

NORBERG: I see. Was Stein here by that time?

RUBENS: Well, Stein worked for us before then, and then they went down to California. But no, Stein, I don't think he was here yet.

NORBERG: Because he came back in '55.

RUBENS: He came back to run that thing. That was about when it was, I guess, '54 or '55 that this thing was... Or '53 or '54, something like that?

NORBERG: Did you have any interaction with Meader?

RUBENS: Only administratively, and we got along quite well. Not technically.

NORBERG: What about Norris?

RUBENS: Oh, quite close, as a matter of fact. As a matter of fact, just before the merge... let me see, which one was it? Just before the merge with Sperry, I think it was, Norris had an advisory committee made up Arnie and myself and Jack Hill and maybe Daniels. We used to meet with him periodically.

NORBERG: But what about in the early days? What I'm searching for...

RUBENS: When he first came here, you mean? I had little to do with Norris except that before Norris came here I used to go down to Arlington once in a while in the course of some work. Now, I'm going to diverge from computer history for a moment. The ERA did a job for the Army Engineers, called Project Boom. You know about that.

NORBERG: Yes.

RUBENS: Royal Bryant was in charge of this for the company. Have you run into his name? Bryant was a physicist, a good one, who worked with Wakelin and Art Sloan at Goodrich before the war. They were all at ONR just before ERA was formed. Now, Project Boom was a fascinating thing. Of course now you know what it was for. In those days we didn't really know what they were worried about, but it was easy to guess after a while. But when it turned out that they were going to blow up to half a million tons of TNT under the ground and have to make measurements of it at the end, you know, of long cables... They were worried about noise and stuff. I was given the job of looking for non-electronic or non-electrical instrumentation that might be used to supplement some of this. And now we run into another thing that we never followed up. This is fascinating. I got to know some interesting people in the course of this, namely, Louis Statham of Statham Instrument Company. And we got to be good friends he wanted me to lay off some of this stuff and go fishing with him, but I was too busy. Anyway, I went down to Statham, and at that time he had a little outfit in Beverly Hills, and he was making these Statham unbonded strain-gauges. I bought a few of them. And one of the things we were interested in, of course, is what's going to be the pressure during and after an explosion in the little wells that you drill in the ground and fill with water. They were going to use tourmaline gauges for this with these long cables and amplifiers at the far end. I came up with the design of what I call a hydrostatic impulse gauge. It would measure the integral of the product PCH, using no electronic parts that you could recover. This consisted of a means of forcing mercury from one little rubber bag, through an orifice, into another one, and you weigh the amount of mercury you collect, and that's it.

That didn't get used for a very funny reason. But the other thing that was fascinating was we built a couple of hundred of them for the Bikini test. It was a self-recording accelerometer using magnetic recording. I got the job of calibrating all of those, which was interesting. This was a device that just took a wide piece of magnetic tape. You pre-recorded a rather wide constant frequency signal on it. Now the accelerometer part was nothing more than a leaf-spring with a permanent magnet on it, which with no acceleration would erase half of the recorded track. If there was acceleration, it would either raise more or less of the records. All you had to do was recover the tape and then you'd take it back to the laboratory and play it through a wide reading-head, and you've got your record of acceleration.

And in the well, the tape was driven by a spring-run motor. No electronic parts whatever, or electrical parts in the initial use of the thing. As I say, we built a couple of hundred of those, and Byron Murphy used them in the Bikini test.

NORBERG: Now this suggests that you were involved in a number of project-related things which were not necessarily along the same stream.

RUBENS: Not the digital computer developments. No.

NORBERG: Can you sort of balance the amount of time you would have spent on various kinds of projects? What I'm interested in is, did you spend, say, fifty percent of your time on digital computer related activities, and fifty percent elsewhere?

RUBENS: No, no, no. I think I ran around like a yo-yo for a time. From August '46 until I wrote that report that I gave you, I was full-time only on Goldberg, full-time. Two aspects of it, both the magnetic and the acoustic part. My next job after that I think was Project Boom, immediately. Now in connection with Boom there is a lot of other stuff. For example, I went out and hired some people for Boom. As soon as Boom was over, however, we had a task called Task 23. And I'm going to get back to digital processing again. Task 23, I think, on that first big contract was a catch-all. You could do all sorts of things. And one of the things that we were going to do was look into magnetic amplifiers as a substitute for vacuum tubes, primarily on logic. And the other reason's obvious: if you get that many vacuum tubes in logic, you're going to lose reliability. Now, in those days the only published information--and this is before the Remy devices, pulse type magnetic amplifiers--became known as the rf amplifiers, or the ac amplifiers that the Germans had used at low frequencies for fire control and things of this sort. I got a hold of some... 1/8 mil thick Permalloy cores made at Bell Labs.

It was very interesting. There were two very interesting metallurgical problems. One has never been solved to my satisfaction, in this regard. There was a man at NOL named Elmen. Now Elmen was the one who invented Permalloy at Bell Labs. He had retired from Bell Labs and he was at NOL all through the war, and after the war they sent him to Germany on a team to find out what the Germans had done. And he brought back the material that the Germans call Permanorm 5000 Z, which is 50/50 nickel-iron alloy. If you heat treat it in a magnetic field it acquires a very square hysteresis loop. So it was used for low-frequency magnetic amplifiers. So people were trying to roll this down thin enough to use it in higher frequency applications. When you get down to about a mil thick, it begins to lose all its squareness. Meanwhile, during the war, Bell Labs had made some 1/8 mil 4-79 Permalloy cores, which had a very square loop, especially with magnetic anneal. And you wonder, "Why the hell should this 50-50 alloy that is very square even when it's this thick lose it's squareness when you make it thin, and the other material (4-79 Permalloy) gets squarer as you make it thinner?" The answer to this just occurred to me recently. The 50/50 alloy has got a large amount of magnetostriction, and as you roll it thinner you begin to line up the crystallites, and the combination of the magnetostriction and the alignment is blocking each other. Whereas, in the case of the 4-79 Permalloy, which has very low magnetostriction, you're just making it thin enough to make it one domain thick, essentially.

We did some investigations with a couple of samples we had of this 4-79 Permalloy at something like a 100 kc frequency. And when we learned about the Remy circuits, and Seymour Cray began to be interested in this for a logic circuit he designed, it was limited, of course, by eddy currents to the frequencies you could work with at 1/8 mil thick. I got interested at that point in the possibility of making magnetic films that were thinner primarily to get away from the eddy currents. I had no interest in those days whatsoever in film memories -- and this was in 1948. I've got documentation of this. I wanted to deposit thin Permalloy films in order to see if you couldn't make a type of core material that would give you a very high frequency pulse-type magnetic amplifier. I proposed this to the Navy, Bureau of Ships, and they asked me to propose it to Office of Naval Research, which I did. Nothing happened. I met Richard Bozorth at a Physical Society meeting and he asked me, "How are you doing with that magnetic film work you're doing?" I said, "What magnetic film work?" He said, "You proposed this to ONR." Van Fleck, W.F. Brown and Smaluchowsky and Bozorth and Elliot Montroll were on the O.N.R. solid state advisory committee, and they recommended, I think unanimously, that it be supported. He said, "Look. Elliot is here at the meeting. You'd better ask him what happened on this." So I ran into Elliot Montroll and he said, "Gee, you'd better go down to ONR ? and find out what happened." It turned out that Bryant set up a meeting with me with--oh, gee, what was his name--the man who was head of physical sciences at that time, who more recently was a vice president of IBM? ... Manny Piore. So I went down to talk to Manny, and he said, "Well, we only had a limited amount of money for magnetic research"--I was asking for something like $20,000 in those days--"and this didn't seem fundamental enough, and we were supporting two projects." One with Fowler and Freier out at Pomona, and the other was with Hoffman and Crittender at the Naval Post-graduate School. Both Fowler and Freier were going to look for domains using the Kerr effect on the surface of magnetic materials, and incidently, I gave them film samples later. And the others were going to look at thin iron films. So it wasn't until after the ferrite core memory was invented that later-on there was a project set up out at the Bureau of Standards (later NOL) at Corona for very high speed computer development that the Navy got interested in film work for memory.

NORBERG: What is the relation between this idea and some subsequent developments called the Cray switch? Wasn't that part of magnetic amplifier work, too?

RUBENS: I don't know what you mean by the Cray switch and when it was.

NORBERG: Well, obviously it would have to be after 1951 when he came to the company.

RUBENS: I think the Cray switch is his version or improvement on the Remy circuit. That used a type of core that we built at Norwalk. I got involved with testing those before Cray got them. These consisted of a little tiny stainless steel bobbin wrapped with 1/8 mil thick 4-79 Permalloy ribbon.

NORBERG: Now were these used in the 1103, later on.

RUBENS: No, no. The were used in Bogart in a very large quantity. Bogart was a very remarkable machine. It had very few vacuum tubes. It was slow compared to modern computers. It was extremely reliable, and think we built nine or ten, or so. And, as I understood, about 10 years ago there was still one running someplace.

NORBERG: It certainly wouldn't surprise me.

DATE: 15 January 1986

TAPE 4/SIDE 1

NORBERG: I'd like to go back to something that we left a little bit hanging last time. I'd like to ask you why it is that you received a job offer in March but didn't come until July. Do you recall the circumstances?

RUBENS: I still had work to do at the Naval Ordnance Lab that was interesting and valuable I thought. I needed that much time. And also I'm not sure that... Let's see, that was March of '46, right? We were designing a new laboratory for the Naval Ordnance Lab to be at White Oak, Maryland. I was the acting head of the Infrared Division at that time, and part of my job was to help lay out the new lab. I didn't want to leave until that was done.

NORBERG: So *you* decided not to leave?

RUBENS: That's right. I'm not sure that there would have been an opening to actually work here much earlier.

NORBERG: But during that period while you were finishing it at NOL, was your clearance being checked and so on, so that when you..

RUBENS: For this other work?

NORBERG: Yes.

RUBENS: I have no notion. I had top secret clearance with the Navy at NOL. And I don't know whether I needed any different clearance. As a matter of fact, I never got involved with crypto until several years later.

NORBERG: I just wanted to clear up that point because several other people had to wait several months before they could actually go to work.

RUBENS: I think they transferred my TS Naval clearance directly. The work I had here was not very highly classified until we got involved in the underground explosion work that I told you about which was a year or so later.

NORBERG: Okay. With that said then, I've sent you a series of reports which were written for project Orion in the last quarter of l946 and you mentioned to me that these brought back some recollections about exactly how those projects proceeded.

RUBENS: Right. As a matter of fact, I remember that Lou Chaloux was the project engineer, I guess, on Oxion with John Howard being the supervisor before I arrived, and actually probably the first month that I was there. One of these says that it was being transferred from Chaloux to me. The thing that I remember that sort of tickled me, or I would say seemed rather peculiar to me, was that the day that I arrived and first saw what Chaloux and Howard were doing was that they had a mild steel disc, and they had been attempting to record on it with one of the heads taken off the Magnetophon recorder.

NORBERG: Directly onto the metal surface?

RUBENS: Yes, which is low coercive material, and of course wasn't worth bothering with. There's a statement in here saying that the recording wasn't very good on one of these. Also, this part I do not remember at all, but it states that a Brush Wire recorder was dissembled and studied for pole piece design. The reading head was modified for use with a high speed spinner. I never had any experience with a head that had been taken from a wire recorder to my knowledge and attempted to use with either tape or any other surface of that sort.

NORBERG: Had you only acquired new heads from Brush Development that would have been designed for..

RUBENS: That came later. My first work was done with a head right off the Magnetophon recorder, using it to both record and read from the spinner using the Magnetophon tape and other samples that I had. Now you have a copy of a report that I wrote in 1947 covering all of this work.

NORBERG: June of l947.

RUBENS: Yes. That states that I got many different samples from many different people: Brush, Indiana Steel Products, and others. A very interesting thing was the Indiana Steel Products tape, because it was made up of very fine iron particles, carbonyl iron, on paper. Of course, the signal intensities were very large on it compared to what you could do with Magnetophon tape, but the noise was horrible.

NORBERG: Were you using only one Magnetophon head to do both jobs at the same time?

RUBENS: That's all that was necessary, because if you remember from last time I told you that, at first at least, the recordings were made with that head with the spinner at rest and then we'd move it to the next spot and record and then read with the same head with the spinner running. Later, and I don't know the exact date, we acquired some Brush heads, then I could use a head on one track to write and read, and another on another track to write and read, and we'd pick off the signals on one track and record them on the other track to show that they were transferable. Later on I did some work, that I think led ultimately to work that Arnold did on addressable material, in showing that I could write and then write over with opposite polarity and still get reasonable signals-to-noise situations. Of course, that was done I think with the head at rest in both cases to do the writing.

NORBERG: We were just looking at this list of projects that had been sent around in November and we saw a couple of others for earlier months, November l946, and in there is a listing of three parts to Project N-l0ll, parts A, B, and C.

RUBENS: Entitled Goldberg.

NORBERG: Entitled Goldberg. Right. Now, initially part A was Orion, part B was Goldberg, and part C was Venus.

RUBENS: Correct. I don't know what part B was at this point? That was Coombs and Howard wasn't it?

NORBERG: But you worked on A and C.

RUBENS: I should tell you one thing. I was led to believe that the original specifications for Goldberg called for a movie film. It was to be a non-erasable, non-alterable store. I don't know whether that may have been the B and some studies generated for that or not.

NORBERG: I think your sense of it is correct, because when Hill first came, his task, while he was waiting for clearance had to do with improving the speed of photographic film for scanning purposes.

RUBENS: There was one other program we got into that Daniels ultimately became the project engineer on. I don't know what the date was. That was a microfilm Rapid Selector. Have you heard of that?

NORBERG: No.

RUBENS: Well this was a contract that we had with the Department of Agriculture Library to build a scheme for doing the following: they would put an abstract on each frame of the film, and beside that a black and white dot code. The dot code corresponded with a punched card. The idea was to move this as rapidly as possible, and go through and, say you wanted to pick all the abstracts having to do with the diseases of Durham wheat, the punched card would be punched in in such a way as to address those abstracts. Every time they would match a light would flash that illuminated the film and you would get another negative on a camera, so you could sort that way.

NORBERG: Yes, I have seen reports of that.

RUBENS: All right, the only thing is whether or not the work Hill was doing ultimately had to do with that or had to do with the original specifications for work we called Goldberg.

NORBERG: He wrote a report, which he showed me, on the speed of film, and essentially, once he got on to Goldberg full time, that is working on the drum, that ceased. All activity with the other film ceased.

RUBENS: That checks with, I think, what Tompkins told me, that the original specs called for this --I don't even know if it was to be a loop--so let's say a long tape with the data on it, and the hope was to be able to go through it as rapidly as possible. But it would not be alterable at all or reusable; you would have to make a new film each time.

NORBERG: Then A was the magnetic head work that you were doing.

RUBENS: Well, the magnetic drum work, with the head in the beginning...

NORBERG: The first part of the task?

RUBENS: Right. As a matter of fact, I think I wrote a memo to myself, maybe, or wrote it in my logbook, that there were several ways of doing this, such as a drum, a disk, an endless loop. But since the drum ultimately had to be pretty big to fill the specifications, and I did not even consider any kind of contact recording/reading because of a wear problem and so on, that it turned out that the size of the drum was almost a meter in diameter, and that the others you could forget about at that point.

NORBERG: What do you mean the others could forget about it?

RUBENS: The disks or endless loop.

NORBERG: Why is that, I'm a little confused?

RUBENS: In order to have as much data as was required on one track, if you're going to do it on a drum, the drum had to be about a meter in diameter. I wouldn't hope to do a disk a meter in diameter.

NORBERG: Why not?

RUBENS: Well, mechanically it is just more difficult. Also, remember, this thing had a very strange set of specifications: it had to be loaded from teletype, which means it's loaded statically--baud by baud or bit by bit. And the drum with a gear all the way around it looked like a better way of doing it than a disk. Also, we could wrap the tape and stick it to the drum, which we did; later of course, we got suitable material in a lacquer dispersion and sprayed the drums.

NORBERG: But do I understand that the disk would have to be very much larger than one meter in order to store the same amount of data?

RUBENS: Well, you would only use a small part of it, too.

NORBERG: Then we're talking about a great big disk in diameter...

RUBENS: Oh, huge! In those days...

NORBERG: ...as opposed to a meter drum.

RUBENS: Right.

NORBERG: Okay, now I understand what the problem was.

RUBENS: Either that, or it would have to be able to store on the inner tracks at a much higher density than on the outer tracks.

NORBERG: Yes. Well, wait a minute, on the inner tracks?

RUBENS: You would have to have a higher bit density than on the outer tracks.

NORBERG: For the drum or the disk?

RUBENS: For the disk.

NORBERG: Why?

RUBENS: Because it's going to turn at a given rate.

NORBERG: I'm sorry, I thought you were filling the information on the outer part of the disk, on the edge of the disk; but, you're talking about as we do it now with a regular disk.

RUBENS: Yes, if you did it that way you would call that a drum disk; also, I think you need 24 tracks anyway.

NORBERG: Ok, now I understand what the problem is. Given that you've made that decision...

RUBENS: Also, we had already had this experience with a 5 inch spinner, which looked very good. We showed we could make the tapes stick to the drum. As a matter of fact, we delivered the first Goldbergs with tapes cemented to the drum surface.

NORBERG: You were the one that built the 5 or 6 inch diameter drum, were you not?

RUBENS: Bob Perkins I think actually sent in the shop drawings for me. I told him I wanted a 5 inch drum on a spinner, and then I received it from the shop, and I worked with it, yes.

NORBERG: When it came time to build the 34 inch diameter drum, though, that was done by the group heading B.

RUBENS: Whoever was the project engineer, I think Dolan Toth ultimately was involved with that, and I don't know if he was working for Hill at that point or what. I think so.

NORBERG: Yes, it was under part B of the project. Let me ask you one other thing: in June of 1947, actually three reports were written; you wrote two and Hill and Coombs wrote a third one. I'm sorry, you wrote one; Hill and Coombs wrote one, that is signed one.

RUBENS: Horton wrote one, and I don't know if I wrote that and signed it or not, but it was the work Horton and I had done on the solid delay line.

NORBERG: That's not what I'm talking about, I haven't seen that one. That may very well be there, but I haven't seen it. I've seen three other reports. One of them signed by you about storage on magnetic tape; a second one of a model system, the 34-inch drum.

RUBENS: Okay, that was probably Hill.

NORBERG: Right, Hill and Coombs. And a third one done by Arnold Cohen. That is, the big report essentially on a system.

RUBENS: Did Arnold's have to do with magnetic recording?

NORBERG: Yes, it did. These were all magnetic recording.

RUBENS: I guess it would have to do actually with the actual handling of the data electronically to get it on a drum and take it off a drum, and so on.

NORBERG: What interaction did you have with these other two people?

RUBENS: I had very little interaction on the big drum, except for the following. We were ultimately having trouble, and I think we got involved with what became the 1101 later. We were having trouble with the idea of tapes sticking. I was able to negotiate with Dr. Wetzel at the Mining Company to get materials we could spray on. Other than that I wasn't involved with the big project.

NORBERG: But spraying is later. As I recall, spraying doesn't come up until the spring of 1948.

RUBENS: That's probably right. But in the period let's say until the spring of 1948, let's say... Well, it was probably early in '48 that I was dickering with Wetzel to get that sprayable material. And one of the difficulties was that he had given samples of their coating material to Harvard, I think, and it got into the hands of some competitors. This was material that was capable of being coated onto a tape, and there was a strict ruling over at the Mining Company that no more samples were to be given to anybody. I was able to persuade him to make a lacquer dispersion of the particles so that you couldn't make tape out of it but you could spray it on a drum. And that's what we used.

NORBERG: One of the Task 13 reports, report number 14 of 1 October 1948.

RUBENS: Okay, so it was probably the summer of 1948 that I was involved.

NORBERG: And I quote, "magnetic sound recording tape bounded to the drum surface has proved unsatisfactory, because of its tendency to pull away from the drum at the surface speeds employed on Atlas."

RUBENS: Right, that's much higher speeds than they were on Goldberg.

NORBERG: "..investigation was started to determine the practical ability of spraying the drums surface with magnetic material." In report number 17 in January 1949, "the most significant development of the period being reported," which would be December 1948, "was the completion of tests of magnetic surface spraying techniques. This work will be reported in detail," and so on. You were involved in that I take it?

RUBENS: I was involved in getting the material for them. I sort of kept an eye on the spraying technique. But I was not involved in any of the fabrication of the big drums that went out, at all.

NORBERG: Were you involved with Atlas?

RUBENS: Only I think on a consultative basis with regard to magnetic recording. The first Atlas, I think, was delivered using Brush heads, wasn't it?

NORBERG: Yes.

RUBENS: And of course I was instrumental in bringing ferrite into the company at a later date to replace Brush heads with ferrite heads. I worked with Jack Hill to develop a suitable spacer. We finally ended up with fine silver for this in order to get better results, rather than using any other non-magnetic material.

NORBERG: Let you go back to the middle of 1947 and recount for me what you remember about what you were doing in ERA after that report was written in 1947, when you've established that the technique was possible, you've been doing it on a six inch drum, and you've passed the information onto someone else, and they are now building Goldberg. What is it that you turn to?

RUBENS: I have a little difficulty remembering exactly what I did. I remember that after the work that led to Goldberg...was there a task situation on Atlas?

NORBERG: 13.

RUBENS: All right. I was involved with task 23.

NORBERG: But that was later.

RUBENS: That was sort of a catch-all task. The one thing I can't remember was when Project Boom came up. Whether it was in 1948 or 1949 or about the same time. I was involved heavily in Project Boom for a period, and I think it was before I was involved with 23. So I would assume that aside...oh, when did Arnold start working on the ONR report that led to his patent on selective alterations?

NORBERG: The patent was filed in the spring of 1948 and the first report was written at the end of September of 1947, so the work was going on during the summer of 1947.

RUBENS: Right after my work on Goldberg, I was also on B 3001. In other words, the ONR thing. I worked separately but collaborating with Arnold on that.

NORBERG: What was B 3001? That was selective alteration?

RUBENS: No, it was continued magnetic recording technology, I think, primarily. I don't know what the title of the contract was, but it was a contract with Mina Rees, who was at ONR at the time. I'm trying to think of what accomplishments, if any, I made at that point. One thing I'm sure is that I made a trip to Brush Development and I got some Brush heads. By that time, we were able to use sprayed surface on drums. I think most of my efforts went into

Project Boom pretty soon after that.

NORBERG: Let's see, we talked about Boom last time...

RUBENS: Yes. It had very little computer technology except for one thing that was very valuable, and that was a self recording accelerometer that recorded material on magnetic tape without using any electrical components in the recording. As the tape was recovered then it could be played back electronically. You could measure accelerations quite accurately. The other thing--I think I mentioned this last time--I was involved with the development of a hydrostatic impulse gauge.

NORBERG: Yes, we did talk about that last time.

RUBENS: Right.

NORBERG: I was just looking for a couple of dates here...

RUBENS: If you could find the date that Byron Smith came to work for the company that will establish when Project Boom started. Because I hired him for the company. For that he was to do the analysis of the data.

NORBERG: We can determine that; that's not too difficult. The Cohen project on selective alteration went from 1 July 1947 to 1 December 1947 as a special task under contract ONR 240. The listing of people who were on the project, though, does not include you. So, this means, as you said, you were giving consulting advice more than anything else.

RUBENS: That's right.

NORBERG: Two things are of interest to me in addition to that, Sid, and then I stop badgering you about magnetic recording. One of them is, there was a meeting in Norris' office on 31 January 1947 on magnetic recording. The group was Norris, Tompkins, Cohen, Hardenbergh, Gutterman, Rubens, Coombs, Kalb, and Horton. It was decided, and I'm quoting from that report, I'm sorry, memo to the file, that "top priority would be placed on completion of design of recording and reproducing heads for Goldberg. Gutterman will be responsible for the aforementioned heads, and Mr. Hogan and Mr. Christianson will assist him in this work. S. M. Rubens is responsible for getting experimental data necessary for Goldberg, and specifically head designs, and Mr. Horton and Mr. Eulberg are assigned to him to assist in this work," and then progress reports follow.

RUBENS: What's the date of that?

NORBERG: This was 31 January 1947. Norris wrote the memorandums. Now, weekly progress report 9 February 1947 on Goldberg: estimated completion was December of 1947, and it was 15% completed by estimate; aspects of the problem, and I just listed down the titles, I didn't bother to do any more than that: "Testing Writing and Reading Circuits with a Disk; Development of an Electronic Counter; Design of Film Passing Equipment," which I presume is what Hill was doing; "Construction of a Model Film Punch," which he disavowed any knowledge of; "Writing Specifications for the Control Circuits for Goldberg; Experimenting with Methods of Applying a Magnetic Coating to a Wheel." Those are the aspects of the problem that were listed.

RUBENS: That means you're beginning to look into magnetic coating other than just the tapes cemented to a drum.

NORBERG: That's not clear from what I have written down. I'd have to look back again at the report.

RUBENS: Initially, it must have been cementing tape to the wheel.

NORBERG: No doubt. Were you only working on that last one or did you have anything to do with the writing and reading circuits?

RUBENS: I had nothing to do with the electronics basically, except, if you read the report I wrote in 1947 there were some electronic circuits that Ammermann and I used before Eulberg came. Eulberg, incidentally, was one of Nier's students, who had come from Oak Ridge at that time. He had quite a bit of experience in electron pulse circuitry... The ultimate circuits that went into the big Goldberg thing we delivered, I had nothing to do with them.

NORBERG: Do you know who had the responsibility for those?

RUBENS: I would imagine Hardenbergh, because that was one of his preeminent fortes: electron-tube circuitry.

NORBERG: Where does Cohen fit into this then?

RUBENS: System maybe. You said he was involved with systems earlier.

NORBERG: Logic circuitry, yes. Second point: In May of 1947 there was a symposium on high speed storage and analysis of numerical data in St. Paul. Norris chaired this session. Introductory remarks were made by Engstrom. Arnie Cohen gave a talk on "Storage Problems in Digital Computing"; Howard on "Photographic Storage and Selection"; Coombs, Rubens, Gutterman on "Digital Storage by Magnetic Recording;" Hill, Berkhoff, and Erickson "Demonstration of a Magnetic Storage System."

RUBENS: What's the date on this?

NORBERG: 17 May 1947.

RUBENS: That must have been beyond the Goldberg technique then.

NORBERG: Is it possible that it was done for airline purposes?

RUBENS: Well, ultimately there was an airline reservation system. You see, Goldberg was unique in that this was a large drum that had to be loaded statically. Now, right after that we got involved with dynamic loading and reading of drums, and that, I'm sure, is what they were recording on, and that led to the drum that went into the first Atlas.

NORBERG: That could be.

RUBENS: I'm guessing.

NORBERG: I can't confirm or deny what you are saying.

RUBENS: Do you have the dates on Atlas?

NORBERG: Well, the Atlas contract isn't let until the 1st of September 1947.

RUBENS: What!! Then hell, yes. Right after it was let...

NORBERG: This is 17 May of 1947.

RUBENS: Oh hell...

NORBERG: You see how complicated it is for me?

RUBENS: Let's turn this off again, and give me a piece of paper.

TAPE 4/SIDE 2

RUBENS: You have a timing track on this 5 inch drum in order to dynamically record and read and know where the hell it was. Timing track was put on the Goldberg drum, too, I think. I think it was an extra track.

NORBERG: Timing track was run by some sort of pulse circuit, I assume?

RUBENS: Correct. Timing track was running on a free-running flip-flop, right? There is a name for it. But anyway, you record pulses all the way around. I mean there is a series of pulses, completely closed. There is no dead space. The reason for that, of course, was you couldn't depend on the speed of the drum, the electric circuit, unless it was being run by a separate generator. The timing track would control all of the electronics.

NORBERG: The timing track controls all of the electronics.

RUBENS: I first got into this by putting it on this little one [drum].

NORBERG: You were saying that the first magnetic tape recording was with the 5-inch drum in Orion, the second was the 34 inch. Now, what were you going to add to this list?

RUBENS: What ultimately became the Atlas drum. I don't remember how many or what experiments led up to it, but the Atlas drum had 24 tracks.

NORBERG: Yes, the 34 inch drum for Goldberg.

RUBENS: 24 data tracks. There might have been a timing track besides, I'm not sure. The Atlas drum, as I remember, also had a timing track, I think 36. I remember something like 18 inches in diameter.

NORBERG: That's about right, but there were several of them by that time, so it's hard to remember all the dates. But that's right, 36 tracks of data.

RUBENS: What experimental drums there were between this and this, but not including this, I'm not sure...

NORBERG: ...the 5 inch and the 18 inch. That's because you were not involved in any other experimental drums?

RUBENS: Correct. To my knowledge. Except later, under Task 23 or so, I did a crazy experiment. After we got the right heads, we did an experiment to find out what the ultimate frequency considerations were on both circuits for heads, by running a 5 inch drum in a safe at 40,000 rpm.

NORBERG: In a SAFE!

RUBENS: Right.

NORBERG: Why?

RUBENS: It might blow up. I'm not sure of the speed, it was the highest speed we could get with a grinding quill. It was 25,000 or 40,000 rpm. The bit rate was a half megacycle, megahertz, which was fast for those days.

NORBERG: Sid, in this period in the late 1940s, what would you say was your most significant contribution to ERA?

RUBENS: In that period, I would say it was the magnetic recording technology primarily. And the contributions I made to Boom, which were quite successful, I guess.

NORBERG: Were you in any way involved in the IBM affair, when IBM came in 1949 looking to have some drums designed for their new machines?

RUBENS: No. By that time the techniques for making the drums, the techniques for making the head and the circuits were pretty well done. I think I made a very significant contribution by bringing in the ferrite for making ferrite heads, and making the magnetic measurements on it, showing that it would be much preferable to what we could get in the way of permalloy or mu metal, and things of that sort. By that time, I think Jack Hill's work and others on this thing pretty well established our capability of fabricating reliable drum systems.

NORBERG: When you were out dealing with Brush Development Company and other makers of materials that could be used in making heads, did you simply buy their heads and bring them back and use them, or did you modify them in any way after you brought them back?

RUBENS: I'm not sure whether we just bought the cores or bought the heads, and I'm not certain at this point whether we had windings to our specifications put on by them or whether we made our own.

NORBERG: So there are four possibilities here all together.

RUBENS: The only people I dealt with for heads as completed structures was Brush. The other problem that came up when we were dealing with ferrites was to find suitable suppliers of ferrite, and there I had a lot to do in dealing with the supplier. But there, we were essentially buying bars of the stuff, and fabricating them out of the bars for ourselves.

NORBERG: Did you know to go to those companies, or did you learn from their product literature or maybe things in publications about what sorts of materials that were available from them?

RUBENS: Well, let me think... There was a man by the name of Horstman who worked for Westinghouse, who was their general expert on magnetic materials. We used to get our pulse transformers from them or their other suppliers. I think through my dealings with him I learned about people I could deal with in the ferrites. Later there was Ferroxcube, but there was an earlier one. I can't think of their name, because we had a lot of dealings with them. We dealt with them in two things: one in getting the heads for magnetic recording and later for getting ferrite cores for ferrite core memories.

NORBERG: I can't remember it either, but I've seen some product literature in your files about it. I'll check that.

RUBENS: I've got to go over and check those files.

NORBERG: What sorts of publications or printed materials did you find useful in this period? Were you keeping up with IRE proceedings and things of that kind?

RUBENS: In those days I was a physicist, and I used to read the Journal of Applied Physics and Physical Reviews regularly. There wasn't much in Physical Review, but there was quite a bit in JAP that pertained to this stuff. In those days, I rarely read the journals of the IRE.

NORBERG: Did you continue to have contact with people like Bozorth at Bell?

RUBENS: Yes. When did you say Task 23 began?

NORBERG: I didn't. I think it was 1949.

RUBENS: When I got into Task 23, one of things we began to look into were magnetic amplifiers as a substitute for vacuum tubes and logic circuits. I obtained, I think through the Naval Computing [Machine] Laboratory, two small 4-79 permalloy cores, 1/8 mil Permalloy-tape cores. I had contact with Bozorth to find out who made those. Originally, I think they were made for radar circuits of some sort. Then I was aware of the fact that Elmen had been sent from NOL to Germany and brought back the Permanorm 5000 Z material, and then people in this country that were beginning to roll this 50/50 alloy, and I obtained samples of that. This was all under Task 23, I think. What we were trying to do was make high frequency magnetic amplifiers. In those days, even with 1/8 mil stuff, a carrier frequency of 100 kc was about all we could do anyway. Although maybe they would have withstood a megahertz if we could have provided it out of vacuum tubes. In 1948, I got interested in the problem of eddy currents with these materials. I wrote a memo to the Navy, and later a proposal to deposit permalloy both by vacuum deposition and electroplating to make cores that could be used instead of the rolled material. This had resulted from a conversation I had with Bozorith at an American Physical Society meeting, where I asked him what limits how thin you could roll this stuff. I wrote this proposal, and the Bureau of Ships liked it, but they said they did not have the capability of supporting essentially pure research, and said perhaps we should take the proposal to ONR. We did that. I think I told you the story about why it was turned down?

NORBERG: Yes.

RUBENS: Under Task 23 we did some other things.

NORBERG: Before you leave that, what was the intended purpose of magnetic amplifiers?

RUBENS: To replace as many logic circuits as possible, and vacuum tubes. Of course, I think I told you this is what Cray did in the machine that became Bogart.

NORBERG: Yes, you did.

RUBENS: Now, if you could find the date of that Bogart contract, it would help to tie this together.

NORBERG: I don't know if I have that. I think I have when Bogart was delivered.

RUBENS: Couple years before.

NORBERG: What else did you do under that contract?

RUBENS: I think I obtained the first transistor for ERA under that contract. Under Task 23, if I'm not mistaken, Dolan

Toth built a flip-flop, and wrote a patent application. When transistors later became available - and I don't know what the date was - was the invention date of the transistor something like '52?

NORBERG: December 1947?

RUBENS: I think in about 1952 they became generally available. I got a contract with the Navy, having written the proposal. This was right after the delivery of the first Atlas. There was a request for bid that came to us for a study of transistorization of a large scale parallel computer such as Whirlwind or Atlas. I wrote the proposal and we got the contract. Then, I ran a transistor school for the company.

NORBERG: Tell me about that. Tell me some more about that. How did you learn about transistors?

RUBENS: Well, about that time Bell Labs published a thick, gray covered book on transistors. Remember, in those days there were only point contact transistors. The junction transistor had been invented, but nobody could get any. The Navy, in this contract, was to deliver a considerable number of point contact transistors and a few junction diodes, which we never got. All the experience around the company in circuitry had been with vacuum tubes up to this time, except the small amount of work being done on the Bogart machine using pulse magnetic amplifiers. When I got this contract with the Navy, we proposed to do as many of the circuits as possible using transistor circuits, and what we couldn't do with transistor circuits for we'd use vacuum tubes. We got these books that gave the theory and operation of transistors. There were three or four of us working on the contract, and we all pitched in to give talks to all the engineers that were interested in explaining what we could glean from this book. Also, what we were learning in our experience with the point contract transistors. Now the project engineer under my direction was a man by the name of James Kelsey. Have you run into his name?

NORBERG: I have.

RUBENS: We had two other people--a man by the name of Pram, and another man (I can see his face, but I can't remember his name -- I think it was Crosby). We worked very hard on this. First of all, the transistors we got were so bad--by bad, I mean they were so non-uniform in their performance, some worthless--that one of the Navy people, who was involved in monitoring our work, recommended that our contract be canceled.

NORBERG: Because it couldn't be performed?

RUBENS: With the point-contact transistors we had.

NORBERG: Yes. Where were you getting the transistors from again?

RUBENS: These were all furnished by the Navy from Bell Labs. There was a tri-service contract, Army, Navy, Air Force, with Bell Labs to deliver a large number of transistors and junction diodes to the services that they could use any way they wanted. A man by the name of Richard Lilly, in the Navy, found out about this and decided he would sponsor this particular effort.

NORBERG: Here at ERA?

RUBENS: Right.

NORBERG: What sort of research were you doing on the transistors yourselves?

RUBENS: Just trying to make circuits out of them. Never attempted to build a thing like a transistor or a diode in the lousy environment like we had. It would be impossible. We didn't have anything like a clean room.

NORBERG: Was there any success in this attempt?

RUBENS: The contract, I think, lasted a couple of years, maybe three. Before it was over, we were able to purchase from other people than Bell Labs, a number of junction transistors and junction diodes. In the final report, we showed that we had built typical circuits of everything needed to do the Atlas job, and using transistors for everything except the writing on the drums, which was done with the same vacuum tube circuits that were used in the Atlas drum. Such a computer would fit in a typical commercial desk-sized cabinet, and use a total of a half kilowatt. About the time we had written that report, Cray was involved in the proposal for the Athena computer. He proposed a two part effort, one to do it transistor wise, and one to do it magnetic amplifier wise, because there still were not enough good transistor suppliers and reliable enough efforts to depend on the transistorization. But after he had finished the first phase of his work, that is building things like MAGTEL and TRANSTEC, he was convinced that the transistor was sufficiently reliable to go ahead from there and build the Athena computer using transistorization. That was probably, other than the magnetic recording, the second most important thing I did for ERA.

NORBERG: How did ERA change over the years between 1946 when you arrived and 1952 or so?

RUBENS: You mean between the time I arrived and the time it became the ERA Division?

NORBERG: That's correct.

RUBENS: I don't think there was too much change in that period. We had a big growth in the variety of programs and projects in that period. I'm not sure when we established a physics department or on applied physics department to work on almost anything. I think that might have been after 1952. After Bill Norris came--I think I told you when I arrived Meader was in charge--after Meader left the company and Bill Norris was in charge here I think there was a greater collaboration of management with the top engineers of the company, and general plans than there had been earlier. Earlier, I think we were largely driven by the contracts we had with the Navy and with Navy supervision of their performance. As a matter of fact, we had almost daily visits from Naval officers.

NORBERG: Is that right?

RUBENS: At first, oh sure.

NORBERG: How was your interaction with them?

RUBENS: My personal one was excellent.

NORBERG: I wasn't thinking in terms of problems so much as I was what the give and take was in terms of the oversight of the work.

RUBENS: Right. Mostly in explanation of what we were doing. How well we were doing it and what our problems were, if any. I guess the biggest problems in those days were the acquisition of suitable instrumentation. If I'm not mistaken, I think I was the first one to advocate that we buy some Tectronics oscilloscopes instead of the Dumont things that came with NCML.

NORBERG: I have one other loose end here to pick up. In September of 1946 you were asked to prepare immediately, the document says, a set of specific objectives for the phase of the Navy program to be covered by project N-1011-A from Tompkins.

RUBENS: Right.

NORBERG: I have the draft report program for Orion. Now this is clearly your writing.

RUBENS: Absolutely.

NORBERG: Do you recognize this writing, which is a continuation of the report, but obviously written by someone else?

RUBENS: I don't think this was written by me or any of my people. (reads it) It could either be Coombs or Tompkins.

NORBERG: Why do you say it would have to be either one of those two?

RUBENS: What's the date on this?

NORBERG: September 1946.

RUBENS: Jack Hill wasn't here, right? I was reporting to Tompkins then. Originally I was reporting to Howard, I think. It could have been Howard. It's got to be one of those three.

NORBERG: That's all I've found by the way on that report; I haven't found the actual statement of the objectives in its final form.

RUBENS: Ok. [reads document] "..please...a set of specific objectives.." Ok. "...this program requires special...development..." That's because I didn't now yet, whether it would be a drum or disk or tape, right? [continues to read] "...upon which pulses can be recorded, reproduced, and erased. It would be desirable to use this medium for recording up to 5,000 bauds at 10,000 per second..." Wow, that's pretty low speed. I think I got that from Tommy. "...materials to investigate...ferrous oxide...plated ferros magnetics..." We never did that. "...and solid ferromagnetic tapes, wires, disks, rods, and so forth...Investigations must yield the following information: recording and erasing technique giving the greatest signal-to-noise resolution at various recording speeds...Best recording and reproducing head design, gap, core materials, inductance, and so forth...for each recording medium, and mechanical properties of each medium which influence the electromechanical design of any system in which the medium may be used..." Right. Those were the objectives of my part of the project.

NORBERG: So you would say that that first sheet is complete as far as your concerned?

RUBENS: Absolutely.

NORBERG: So this could be additional things...

RUBENS: What are you going to do with the best of this.

NORBERG: Ah...Now we see why it's in a different hand, because you had already said to me that you were not involved with the application.

RUBENS: Correct.

NORBERG: So this would have to come from some other group...

RUBENS: This has to do with the specific application. Not only that, but whoever wrote this pretty much decided it was going to be a drum, not a disk, and not an endless belt. And it's going to be magnetic.

NORBERG: We are assuming, since these were all attached to each other, that they were all written at the same time?

RUBENS: Right. It might be that what Tommy did is to get this from me, and something else from the other people that were around at that time, and he wrote this, maybe. He said, disk or drum hasn't been settled yet.

NORBERG: Yes.

RUBENS: [reads document] "...various reasons for interest in this type of medium are...it's possible to obtain a high set of speeds...finite length which can be placed around a drum of reasonable size...It might be possible to use multiple band or closed helical tape on the surface of a drum to increase the length of data...Wear for repeated passes..." Of course, this was eliminated by a non-contact recording.

NORBERG: Ok, but this suggests that contact recording was being considered at this time?

RUBENS: Oh yes, oh sure.

NORBERG: But, you said you never considered it?

RUBENS: The point is, I realized, one of the things I did in this was with very thin shims. I was actually able to measure the head displacement away from the tape. I had been able to show that the closer we could get to the head to the tape, the higher density we could get. So the contact recording ultimately might have been better, but there's a wear problem.

Now, I'm going to tell you this for what it's worth. We had a man in the department, who a short time later became my supervisor. His name was Bertil H. T. Lindquist. He was trained as an aeronautical engineer. He suggested, "Why don't you put the head in gimbals and float it?" I didn't realize what he was saying, because I didn't realize the nature of the boundary layer at that point. I got into this later in Project Boom when I got involved with the hydrostatic impulse gauge. But I think he was the first one to really suggest a floating head.

This incorporates part of what I put down. [reads] "...Whether a drum or disk...the following investigation is needed: resolution, signal-to-noise ratio, high speed erase...and...and record limitations technique for organizing of pulses...the width of the magnetic track for sufficient signal...and maximum distance required between adjacent tracks to avoid what we would call cross talk...investigation of factors influencing recording and recording head design..." He talks about the gap and the coil design for high speed recording and reading and material. Wait a minute. Is this the same writing?

NORBERG: It's a little more cramped, but I would say it is. It looks like that third page could have been done in a meeting, perhaps, as opposed to the others, which could have been done at someone's desk, but it would be the same person.

RUBENS: That's interesting.

NORBERG: Isn't that a nice document? That was in your files down at Sperry.

RUBENS: I better go down and look at 'em.

TAPE 5/SIDE 1

NORBERG: Sid, one last point: were the 1101s that were delivered to Tennessee solid state machines? The ones that went to the Arnold Engineering Development Center.

RUBENS: No. To my knowledge, no 1101 ever had a transistor. 1101s were vacuum tube machines, with about 6000 of them.

NORBERG: I want to go back to something else you said last time when you mentioned for the first time in the interview about non-return-to-zero, and its use in ERA considerations. As I recall, you mentioned the first time you heard that was from Tompkins?

RUBENS: Right. He didn't call it that. Oh, I guess he did. I guess I was the one who concocted the term return-to-zero. You see, when you record a situation like this, (drawing something) if this represents the magnetization, the signal you get out looks like this...

NORBERG: Sinusoidal.

RUBENS: Right. If you integrate it, it looks like this. I actually did integrate. I had an integrating amplifier. I thought of this as a non-return-to-zero.

NORBERG: That's one spike above the horizontal axis.

RUBENS: Right. What I didn't realize at the time - Tommy kept harping at me on it, but it didn't make sense - was if you thought of these as flux changes instead of total flux, a flux change up or a flux change down could represent two different states. However, that took electronics to handle that, that I was not particularly familiar with it. My own experience with electronics before I came to ERA was all cw, never any digital, even though I had been involved in laboratories where they had done some pulse counting, in my graduate school days, I never got involved with that. We did get a contract later. As a matter of fact, Tompkins left the company in order to award the company this contract. I guess there was just one machine of this sort built. It was, I guess, funded by ONR.

NORBERG: Built with what?

RUBENS: By using non-return-to-zero electronics and the project engineers on that--and I consulted with them--were Bill Keye and Thornton. Now this was after the Atlas 1, I'm sure. At least after the engineering was well started on Atlas 1. This was a drum--I'm not sure how much of the machine that Keye and Thornton were involved with, but they were involved. They were involved with a non-return-to-zero electronics on this drum. The machine was used for war games and was delivered, I think, to George Washington University.

NORBERG: But didn't non-return-to-zero become a common technique in magnetic recording?

RUBENS: After that it did. Not only a common technique, but about the only technique.

NORBERG: But you're suggesting this is after 1951 or so?

RUBENS: Right.

NORBERG: IBM had a technique for non-return-to-zero, which they patented.

RUBENS: When?

NORBERG: They submitted an application some time in 1950.

RUBENS: That could be. The point is, that in 1947/1948, there was no work that I knew of going on in NRZ.

NORBERG: That was my next question. When does this become reasonably common knowledge for the community?

RUBENS: The first use of it at ERA was in this machine. The machine that Keye and Thornton were involved with on the storage. It was done in the ERA days, because I remember the room they worked in. Tompkins must have urged me to consider this as early as September 1946.

NORBERG: Non-return-to-zero. Did you tell me last time that that was a term that came from earlier activities, maybe from telegraph? Was this new at that period?

RUBENS: No. I'm not sure. It could have. I get the impression I invented the term return-to-zero to describe these things here. Tommy says that ought to be two digits instead of one, and he didn't use the term NRZ, I don't think, but he may have.

NORBERG: It came up later on, because there are documents in 1947 about this question. Selective alteration is one patentable idea; non-return-to-zero is a second patentable idea. And then there were two circuits which were...

RUBENS: Right. The point is, selective alteration with NRZ is a little different than what it is with the other.

NORBERG: Can you tell me what the difference is, I don't know it?

RUBENS: How do you do it? The point is, this represents a digit in return-to-zero, right? And this represents another digit.

NORBERG: Yes. Two opposing vectors on a horizontal line.

RUBENS: Right. Let's say in NRZ, this is one digit and this is another one.

NORBERG: We split one of the vectors in half.

RUBENS: How do you change this one without altering that one? I don't know.
You see what I mean?

NORBERG: Yes. How does that provide us with a difference? All you've done is increase the density of the data.

RUBENS: That's right, but the storage itself... In other words the magnetic representation of the data is quite different from the... Let's take a situation: in return-to-zero these are bit spaces. The magnetic representation of these are like this, and this, and this, and this; say it's one, zero, one, zero, one, zero type of pattern. In the NRZ, I begin at the same point...the head signals are this.

NORBERG: Sinusoidal curves.

RUBENS: Essentially.

NORBERG: And one complete cycle per digit.

RUBENS: One complete cycle per bit. Whereas, on the NRZ now, let's extend this back a half...on the NRZ, these are bit spaces. The same magnetic representation, but by the time you've gone through the electronics, these are the output pulses you are going to get our not get.

NORBERG: Right. Essentially half your earlier space.

RUBENS: Say you want to change this one, which let's say represents this direction, and not change that one, how do you do it? If you did, then this becomes two of these, right? So, I guess you can alter the magnetization in such a way that this becomes a longer...

NORBERG: Can't you just double the pulse rate and essentially do the same thing?

RUBENS: Right. I guess you can. These are the problems that worried me originally about it. It's true that I think that Tommy was campaigning for this from the very beginning, for the non-return-to-zero. The first instance of investigating it laboratory wise, was done by Thornton and Keye. It would be interesting to get the date on it.

NORBERG: I'll ask Thornton. He's easily accessible, whereas Keye is not so easily accessible. That's useful. We are talking about non-return-to-zero because I'm interested in something that appears in the ERA book, *High Speed Computing Devices*. At one point, the question of non-return-to-zero is broken up into two parts. I wish I brought the book with me now Sid, I'm a little embarrassed that I can't repeat this easily... But there was two level non-return-to-zero and three level. There is a question about non-linearity of signal in this two-level area. We might have to get the book, I think, to...

RUBENS: When you talk about levels, I'm wondering if this has to do with intensity?

NORBERG: Yes.

RUBENS: You realize that the interesting thing that happens is I was studying the Magnetophon reports in those days, and how you get linearity by using supersonic bias, by using so called anhistoretic magnetization. It didn't occur to me then, later it occurred to a lot of people, that if you used this, you could have more multiple level recording. As a matter of fact, in recent years, such recording was investigated very carefully at the Sperry Research Lab, three level recording on a standard UNIVAC disk.

NORBERG: But those things would have all been done after 1950?

RUBENS: Right. But it would have been possible back in those days, but nobody ever thought of it. Maybe they did.

NORBERG: We'll have to look at that reference downstairs.

RUBENS: I think simple three levels you could do without the anhistoreisis.

NORBERG: Let's turn to the sale of the company. When the first announcement was made that it appeared as if Remington Rand was going to pick up...

RUBENS: The merger with Remington Rand.

NORBERG: Right. What was your private reaction?

RUBENS: I wasn't really quite sure at that point. I knew that Norris was disappointed.

NORBERG: How did you know that?

RUBENS: Later, I found out that he thought it would be better if the originators maintained control. Also, a personal friend of mine by the name of Leopold Pistner, who was the president of Fuel Economy Corporation here, who had built power plants up and down the Mississippi Valley in particular, told me that it could easily have been local financing for enhancing the capital of ERA if we wanted and needed it. On the other hand, my first contact with Remington Rand people came within a month of the purchase, I think, when I made a trip back to a meeting that was called by General Groves - I may have mentioned this to you earlier - to tell Groves, essentially, what we were doing in the way of magnetic research, primarily. The reason for the meeting it turned out was that Eckert had become interested in exploiting magnetic amplifier techniques. They were interested in acquiring necessary means of fabricating and annealing such cores. Of course, they wanted to make sure there wasn't unnecessary duplication going on. At the same time, I found out to my surprise--and we had already had some experience under Task 23 and in the Bogart work and so on - on devising circuits... I'll have to digress for a moment--circuits using magnetic pulse amplifiers. We got a contract with the Navy called Bricks. Bricks were logic blocks containing magnetic amplifiers that could be wired together to make all kinds of circuits; all sorts of logical devices such as: adders, counters, accumulators, and what not. All our work had been done with silicon diodes and permalloy cores. Now I went back to Eckert and Mauchly, aside from discussing the problems of fabrication of magnetic cores and the like, and found that Eckert and his people were all using copper oxide rectifiers instead of silicon diodes. I couldn't understand why.

NORBERG: Did you raise the question with them?

RUBENS: Oh yes.

NORBERG: And what was the response?

RUBENS: It was mixed. Of course, copper oxide diodes in those days were cheaper. They were also slower. I don't know. Ultimately, the company at Philadelphia built a thing called the solid state computer, which was made up of magnetic amplifiers using, I think, copper oxide or silicon diodes, I'm not sure. Silicon diodes were difficult to get also. They were being used primarily for the highly important military work. The supply was limited. When we got this Bricks contract, the first thing I did was to call Sylvania, who was then about the best supplier of these. I wanted a thousand. They said I could have 50. I said, how many are you making here? And he said, oh about a million, but all you can have is 50.

NORBERG: Why? Did he give you any justification for that?

RUBENS: There were higher priority jobs that needed the others. They were hard to get. The interesting thing is that that all turned around after the merger with Sperry. Sperry was one of the best suppliers of silicon diodes in the world. They know a lot about silicon technology. As a matter of fact, soon after we were acquired they decided to go into the business of making transistors. They wouldn't even consider making them out of germanium, which was then what most transistors were made of at that time.

NORBERG: So they went to germanium I take it?

RUBENS: No, they didn't.

NORBERG: What did they make them out of?

RUBENS: Silicon.

NORBERG: I'm sorry, they went to silicon. You said they did.

RUBENS: Now there's a good reason, militarily, for having the silicon over germanium, too. One is they work at much higher temperatures, that's the most important one. There is less trouble with moisture.

NORBERG: Back to 1952, it seems that what you're telling me is that you found this merger with Remington Rand to be quite an acceptable phenomenon?

RUBENS: Amongst certain people. Actually, financially we were all better off; we got some Remington Rand stock, which turned out to be ultimately quite valuable, for our ERA stock. I don't think it changed my... If anything, it enhanced my professional contacts, and it made my professional work more interesting.

NORBERG: How so?

RUBENS: Frequent trips back to Philadelphia to work with their people, and so on.

NORBERG: Who were the people you worked with back in Philadelphia?

RUBENS: A very important one... It wasn't just Philadelphia, I'm sorry, it was also at Norwalk. They had a man by the name of Jacob Randmer. Jake Randmer was a vacuum tube designer. In the earlier days, I guess during World War II, Remington Rand actually had some classified vacuum tubes design work for the government that he had worked on. Randmer was the one that was chosen to develop the magnetic cores for the solid state computer that was later built. I consulted frequently with him on this. It turned out later on that between Cray, myself, and Randmer that there was a design for the core of this Bogart computer. So, I had frequent trips back there, and found that was very enjoyable and stimulating.

Also, a few of the original ERA people had left to go to work for Remington Rand, including Lou Chaloux and Hugh Duncan. They were good friends of mine, and I use to see them frequently when I was back there. In general, my own interactions in particular with Eckert and his associates were very amiable. I was encouraged by Norris to work with these people.

NORBERG: Would you say that there were three levels of interaction between St. Paul and Philadelphia. I'll leave Norwalk out of this for the moment. First, there was what I'll call the research level people like yourself, who worked on essentially fundamental problems in this area. There is a second level which is involved in design and production of systems. The file computer on the one hand and the solid state computer on the other hand.

RUBENS: Which may have had some competition as ultimate commercial items.

NORBERG: Exactly. And thirdly management, the management level. Now you're telling me that at your level everything was...

RUBENS: Was fairly smooth.

NORBERG: It was fairly smooth, professionally oriented, and so, the same way you would expect any laboratory activity.

RUBENS: Right. Not only that, I was received as cordially back there as I would be walking into any of the labs at ERA.

NORBERG: Why do you think that is true?

RUBENS: I don't know. Maybe I didn't represent any threat. Maybe this first contact we had with them that I was involved with, a very interesting thing took place there. This meeting was called by Groves, and there were four of us who went back here to attend that -- two from here and two from our operation in the Washington area, wherever it was then, maybe Arlington. Dr. Engstrom and Gordon Welchman from Arlington, and Tomash and myself from here. As I said, there was great emphasis on the magnetic core business, which I got involved with primarily. I remember Engstrom was asked, at the opening of this meeting that Groves called, to say, "What are your prime efforts and goals at ERA?" Engstrom said it was primarily engineering development, with no particular reference to a computer or anything of that sort. I think that he was trying to point out that we merely represented a considerable technological capability without any particular product in mind. Later, but not very much later, I learned that some people in Norwalk learned about the 1103, which either we had just delivered or were about to deliver - I can't remember the date of the first delivery. They had decided this might be a useful commercial product, and ordered 10 or so to be built. They would provide the funds for doing it. This sounded like a stable thing to me.

NORBERG: Did you have a counterpart in Philadelphia, someone who was doing...

RUBENS: Essentially what I was doing? There were a number of people I interacted with primarily in Philadelphia to begin with: Eckert and Ted Bonn, do you know who he is?

NORBERG: No.

RUBENS: Ted Bonn, I think, had a lot to do with the group that was trying to develop magnetic amplifier technology. When I first met Hugh Duncan back at Norwalk, if I'm not mistaken, Eckert had had him involved with looking into some magnetic amplifiers using these copper oxide devices. In those early days, most of my contacts were with Eckert and Bonn, I think. I'm trying to think of who else, perhaps.

NORBERG: Let me shift the question a little bit, then you may recall some of the others. This suggests that the man who was at the top in Philadelphia, namely Eckert, was also the chief design man.

RUBENS: Oh yes.

NORBERG: And the chief researcher, if we want to go that far, which is different than at the ERA division where the people at the top were in the usual sense management or sales people or financial people, and the research was at a somewhat different level.

RUBENS: Don't forget, historically, at the time that Eckert and Mauchly was acquired by Remington Rand, Eckert was primarily everything: sales manager, what not; cost accountant, the works I assume. After they were acquired, I think his chief job was not to sell more UNIVACs, but to get the one that was contracted for finished.

NORBERG: Out the door.

RUBENS: And it made a difference. Because, as I remember, that first UNIVAC was delivered a year or so after the merger, and the ERA division had the responsibility of making damn sure the thing got installed and serviced properly.

NORBERG: I think that's not right, because the actual installation of the first UNIVAC at the Census Bureau was in 1951, a year before.

RUBENS: Right, right. There were problems I know, and the ERA and the office in Washington got involved, and ultimately... Let's see, it must have been about 1954 that we got involved with replacing those mercury delay line memories with magnetic cores in the so called UNIVAC IIs, or whatever, and that was in...

NORBERG: That was the one that there was so much difficulty with installation.

RUBENS: Oh yes, terrible difficulty.

NORBERG: So, we have a difference. Did you observe the same sort of difference between St. Paul and Norwalk?

RUBENS: Well, Norwalk at that point... Don't forget, let me list the operations in Norwalk that I was involved with. First of all our patent office was there. I made frequent visits there involved with patent problems, one that lasted several years. I invented a static reading head, and we got into an interference suit on that one. The second level, I think the most important level, was my interaction with Jake Randmer and the development of magnetic core materials and core devices.

NORBERG: But he was at Philadelphia?

RUBENS: No. Norwalk. Always at Norwalk. After this he left Norwalk to work for an x-ray tube company. Never did move back there.

TAPE 5/SIDE 2

RUBENS: In the first year or so of our relationship with Philadelphia, my concern, or their concern with me, was primarily in connection with the magnetic amplifier. Later there were other developments. Where are we annually now, what year?

NORBERG: We were talking about 1952. So 1952,3,4.

RUBENS: I'd like to revert to the magnetic film business going back to my proposal in 1948. When the Korean War came along, and as the result of the Korean War, the Defense Department people realized it was very important to have faster computers and faster memories. This was after the ferrite core memory had been developed. My experience with ferrite core memory was primarily making basic measurements on the small ferrite cores themselves, and making sure that when the cores arrived they were suitable for use. Jack Hill and others got involved in developing automatic core testing machinery. The Navy set up a program in about 1953, I think, at Corona, California. It was in the Bureau of Standards, Corona, on a Navy premise. The chief concern of the people at Corona at that point had been in rocket control and that sort of thing. There was a man in the Navy by the name of M. Scott Blois. Have you ever heard the name? A remarkable man.

Scott had worked under Engstrom during the war, and the Navy had sent him to Stanford where he got a Ph.D. in physics. He did his doctorate on very thin film x-ray tube targets, copper, gold, and the like. Right after he got his degree this work was set up at Corona and he was put in charge of it. The objective was to develop, if possible, memories and circuits that would work at higher speeds, than with the 1103, which was about the best we had then. As I remember, the 1103 memory cycle per bit was something like 12 microseconds. So, they were shooting, hopefully, for 100 times that fast, if possible, in those days. Transistor circuits were around, but good magnetic memories that were fast enough, or transistor circuits that were reliable enough and fast enough for such higher speed memories weren't available then. One of the things Scott proposed to do was to vacuum deposit permalloy in the hope of making cores that could replace the ferrite cores in computer memories. He was sort of naive. He thought these should be toroids, and so the substrates were little ceramic washers that were glazed over with glass, and he would deposit the material on these. He thought in terms of feeding wires through washers like you do through a little toroid. However, he found, I think it was about that time that Dr. Joseph Eachus, who was in the Defense Department in charge of this work, and knew about my earlier proposal - suggested that I be a consultant to Blois. I was. I suggested to Blois that he do this deposition in a magnetic field. Of course, when he did, he got a nice built in - uniaxial anisotropy with square hysteresis loops and so on. Right after that, during 1954, Howard Campaign and Eachus approached me and said, "How would you like to try to build a memory out of this material?" In about June or July of 1955 we got a contract to do this. There was some problem with Eckert on this. He wanted to do the same thing in Philadelphia.

NORBERG: Based on your research?

RUBENS: No, based on what he knew that Blois was able to do, and the fact that the proposal came to Remington Rand, and there was a big problem as to who would do this. I think Eckert was claiming they already had some of the vacuum equipment. We had none of it here in St. Paul at all. But I did want to do this, and as I pointed out to you earlier, when I was at the Naval Ordnance Lab, the last year I was there, I did have considerable experience in making thin films by vacuum deposition and sputtering and the like. The decision was made that the work would be done here.

The first thing we did on that, which would be an improvement over what Scott had done, would be able to control several things. He had no temperature control of his substrate whatever. He had no control of his composition. He put essentially permalloy into his crucible and got something that really wasn't a non-magnetostrictive material. It didn't matter for what he was doing at that point. We made several important studies. One was the effect of composition of the melt. We found that if you melted 83 parts nickel and the rest iron and evaporated it at a reasonable rate you got 8119, which is what you want for non-magnetostrictive material. The other thing is, we made a careful study of what happened. All the depositions were on glass substrate. Now, if you carefully cleaned and heated a glass and controlled its temperature during deposition that was the important parameter for controlling behavior. We found that the best temperature of the substrate was about 300°C, plus or minus about 10°. We developed an awful lot of the techniques that were necessary to be able to get reproducible stuff. We began to study its domain structure using the Kerr effect, and so on.

This work continued under other contracts until about 1962 or 1964. So almost a nine year period, I think. We actually were able to develop two kinds of memory under this. One was a suggestion by the people in the Government, but not suggesting the materials for doing it, that if you had a core made out of two films, one being a low coercivity material such as permalloy and the other a higher coercivity material, the higher coercivity material would store the information, but you could read it out by switching to low coercivity material. So it would be a non-destructive readout. We built such memories, and they were actually used in military operations.

NORBERG: When you say used in military operations, do you mean in computing machines?

RUBENS: Yes.

NORBERG: But not computing machines that were commercially designed?

RUBENS: No. No. However, the first commercial use of the film memory by our company was in the 1107. It used a small permalloy core memory. I don't know, something like 512 words. I think, as a scratch pad type of memory. We also developed a memory that was tested in aircraft that used both types, the non-destructive and destructive readout, small. But it showed that it was possible.

NORBERG: One of things that puzzles me about your tale, when you start talking about Eckert and the competition as to where this work would be done, if the equipment is in Philadelphia and the personnel who are adequate to this task requirement are in St. Paul, after all this is the same company, why not move you to St. Paul? The company moves people around all the time.

RUBENS: Like move me to Philadelphia?

NORBERG: Right.

RUBENS: It could have happened. Number one: contracts that ERA had were primarily with one group, as you well know. They were originally with the Naval Security Group which became NSA. To my knowledge, Philadelphia never had such a contract, or were never cleared to do that sort of thing, and didn't want to be. That's part of it, I think.

NORBERG: Then why would Eckert even have been receiving an RFP?

RUBENS: Because it came to the company.

NORBERG: And the company distributed it?

RUBENS: I think so.

NORBERG: Then it wasn't a classified document.

RUBENS: Oh no. The first contract wasn't classified.

NORBERG: Then how does your argument about classification hold up here?

RUBENS: I think it gets into immediate applications maybe, or special applications, and having the cleared people near by without transferring facilities, and so on.

NORBERG: So it would be the Navy, then, that would have made the decision as to where the work was going to be done, not the company.

RUBENS: I don't think so. I think they may have had an input, I don't know. You'd have to ask Norris. That I'm not sure of. Let me tell you the sequel to this. Eckert was so anxious to do this that he went out and got a contract with the Air Force to do it by plating instead of evaporation. They did a lot of work in Philadelphia. The man in charge at that point, and I should have mentioned this earlier - oh what's his name, it avoids me now, I can't think of it -- anyway there was a man by the name of Edelman, who was the first one to come up with a good way of electroplating permalloy. It turned out that electroplating permalloy is much more difficult than evaporating it. Although you start out with what you think is cheaper capital equipment, you've got this problem: when you're going to plate an alloy such as 81 nickel, as soon as any of this is plated onto something, you get a different potential drop. Now you aren't plating 8119 anymore unless you have violent stirring of the material to break down that gradient. So you have to have violent pumping of the electrolyte, and you have to have a suitable substrate. What were they doing? They started out with glass substrates, the way we did, and then they would sputter or evaporate gold on to those, and then electroplate onto that.

NORBERG: An expensive proposition.

RUBENS: Really, yes. It turns out that Control Data has invested millions of dollars into devising a very carefully controlled way of making so called magnetic heads by electroplating. Because you get more reproducible results that way then by sputtering. It turns out ultimately to be cheaper, because they used very careful microlithographic techniques to cut these things up, and so on. If your going to use wet chemistry anyway you might as well go wet all the way.

NORBERG: Sid, what was the attitude in St. Paul toward Philadelphia?

RUBENS: I think that depended on who you talked to.

NORBERG: Well, can you give me some examples about the attitudes among the people then?

RUBENS: Certainly among those that were without mentioning names, involved in the commercial projects - it wasn't really hostility, but it was... I think the attitude was they do things differently than the way we do, and we prefer to do it this way.

NORBERG: Another example?

RUBENS: This depends on what time period we're talking about. I told you that from the start my contacts with the Philadelphia and Norwalk people were very amicable, and I think actually, I never saw them as a threat of any sort. I think there were people who considered them to be a threat.

NORBERG: What did you think of the technical capability at Philadelphia?

RUBENS: No question but what there was a great deal of ingenuity. I had no reason to believe that they weren't at least as competent as we were.

NORBERG: Did you hear any tales around St. Paul about their inability to do certain kinds of jobs?

RUBENS: Oh yes, but I can't remember specifically who made these comments, or what jobs they were talking about.

NORBERG: Did you take them seriously at the time?

RUBENS: As I say, I wasn't really concerned with projects that had to deliver commercial equipment with deadlines. We knew there were things like LARC, the project they did later for the atomic energy people... A huge computer...

NORBERG: LARC?

RUBENS: LARC, yes. All kinds of disparaging things about LARC. As I remember, when the LARC request for proposal first came out, I think ERA had a chance at it too, is that right?

NORBERG: Yes.

RUBENS: I think the fact that it was granted to Philadelphia, created some unhappiness. I wasn't involved at all.

NORBERG: Sid, do you remember a Department of Defense symposium on magnetic recording in 1953?

RUBENS: Sure do.

NORBERG: I'd like to know more about it. I've seen two papers only from the proceedings.

RUBENS: This gets all the way back to Task 23. I had always wondered why you can't record... During World War II - I think I told you this story - I had some experience with a flux-gate type magnetometer. I always wondered why can't you build a flux-gate type reading head that won't erase the tape. Finally, it occurred to me one day you could do it. We had no project to support this at all. I managed to get enough money on Task 23 to prove this, so. Say you built a head by taking a piece of permalloy and you cut it like this (draws).

NORBERG: It looks like a clothes-pen with a shaft on it.

RUBENS: This is a square tube that has an opening here and an opening here.

NORBERG: Ok.

RUBENS: This bottom part is also bent down to form this... Let me put a gap here. Now we put a winding in here and drive that at some high frequency f and we put a winding actually on both legs, but I'll just show it on one, then we put a filter here that filters out 2 f and an amplifier follows the filter. You stick this on a piece of tape, what happens? Every time this thing goes to saturation -- this will be saturated either this way or this way periodically. When it goes to saturation, the reluctance around here is maximum, when it goes through zero it's minimum, so you get this 2 f signal out. You stick this on a piece of material that's magnetized like that, and you get a 2 f signal out. It turns also, if you put a small dc bias in here on the one leg, but not big enough to write, you can tell the difference between ones and zeroes. I wrote a disclosure on this, and we got enough money out of Task 23, I think, or some other task that was set up under that contract, and spent a couple of months showing what you could do with it. There was a group at DOD that said, "Fine, we want you to build us the following equipment. We want you to build a... Let's see, we had two different contracts. The first one was to get a tape recorder - analog now - that could record or read dynamically at 15 or 30 inches a second both, and also play back at those speeds plus play back at 1/10 of inch per second. We built such a thing, delivered it to the Defense Department. It was called a symposium of acoustics in air.

NORBERG: Which was?

RUBENS: The symposium.

NORBERG: The one I'm talking about?

RUBENS: Yes.

NORBERG: No, it was labeled a symposium on magnetic recording.

RUBENS: Really? What is the date?

NORBERG: 1953. Here in St. Paul. It actually occurred here in St. Paul. You were one of the speakers, Benning was another. He described the device. You described the head, he described the use of the head in an NSA machine.

RUBENS: They used, of course, silica gel RFP signals.

NORBERG: RFP signals? Radio Frequency -- what's the "P," Pulses?

RUBENS: "F" is Finger Print.

NORBERG: I missed that one! Are we talking about the same thing, or are you talking about something else?

RUBENS: Benning and I both talked, and the meeting was in Washington. The symposium was on acoustics in air. He gave a general talk about the machine, and I talked about the results we obtained from the machine in the laboratory. The point is, the interesting thing about it was, this was to have a flat frequency response over a wavelength from one mil to a foot. In order to get the flat response up to a foot, we had to put big long wings on this damn thing, and it worked. The one we delivered was much easier to build then this. We found that, hell, you don't have to do that, all you have to do is...

NORBERG: Do what, put the wings on it?

RUBENS: You don't have to have the tube and windings through it.

NORBERG: Oh yes.

RUBENS: All you have to do is this (draws). I'm going to over simplify a little bit. You pass a current through this.

NORBERG: That's the head that was reported in your paper. That second one, not the first one.

RUBENS: Of course.

NORBERG: The reprints of those two papers that I have seen, have at the head of them, the "Department of Defense Symposium of Magnetic Recording."

RUBENS: That could be. But I don't think that was the invitation to the people that attended, I think it had to do with acoustics in air. The point is, I may have mentioned this to you, right after that meeting, we were still ERA before 1955, ERA got a letter from Westrex Corporation asking for a license. We had a patent on it, but never replied. I learned from a guy why they wanted it. Do you know, in those days, how they edited sound recordings, the sound strip?

NORBERG: No, I have no idea.

RUBENS: I didn't either. I wouldn't believe this. They had a pendulum, a long pendulum with a magnetic reading head on it, and they'd swing it back and forth along the recorded sound track. They could cut the tape right at the beginning of a syllable. With this thing you wouldn't have to do that.

NORBERG: I see. Was this pendulum oscillating perpendicular to the tape, or longitudinal to the tape?

RUBENS: Here's the tape, here's the head, here's the pendulum, and it's swinging actually along the tape like this.

NORBERG: Along the tape? That's what I was asking. Then they could cut it off at the syllable.

RUBENS: With this thing you see, they wouldn't have to do that. They could just look at the output on the oscilloscope and back the tape up to where the syllable began and clip it off. We got this flat frequency response, it was nice. We never tried to get below a mil, because we had very crude gaps in these damn things anyway.

NORBERG: Were you knowledgeable about the interaction between Eckert and Norris?

RUBENS: Only to the extent that Norris asked me to cultivate Eckert's friendship, because I think he thought it was necessary that we remain amicable.

NORBERG: Do you have any knowledge of what was occurring in 1956,7?

RUBENS: Is this the days of the LARC thing now?

NORBERG: In part, yes, but I'm thinking about the interaction between Philadelphia and St. Paul, notably in the persons of...

RUBENS: Wait a minute! Now that you mention this, it wasn't just Eckert... Do you mean Eckert or the whole...

NORBERG: I centered on Eckert and Norris when I asked the question, but indeed, there were others involved.

RUBENS: Oh absolutely, I'm sure. I think, in retrospect, that Eckert influenced top management at Remington Rand a great deal. Let me tell you a story that I don't think you've heard before. Arthur Draper, who was sort of a top dog for old man Rand in acquiring companies, told me this story one night of how he helped to acquire Eckert-Mauchly, and how Eckert was the one who suggested that Remington and Rand acquire ERA.

NORBERG: Is there more to the story than that?

RUBENS: No. Not much. The question is, if you think of Eckert as being a power grabber, did he want ERA so he could control it, or did he want ERA because he thought it was extremely competent and aggressive and would assist in building the company?

NORBERG: One would have to look for documents to see if there was any written justification for what was going on at the time. I ask that question, because the next obvious question to ask you is about whether or not you knew anything about the founding of Control Data before it was publicly announced?

RUBENS: Yes, quite a bit. This was before I knew that Norris himself was going to leave. Do you know a man by the name of Arnold Ryden?

NORBERG: I've met him.

RUBENS: Arnold Ryden, as I remember, was a controller of ERA before we were acquired by Remington-Rand. Ryden, evidently, and Norris got along pretty well, and Norris...

END OF INTERVIEW