An Interview with

WILLIS H. WARE

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Conducted by Nancy Stern

on

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RAND Corporation (Santa Monica, CA)

Willis H. Ware Interview
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Abstract

The interview is primarily devoted to Ware's work on the Institute for Advanced Study computer project in the late 1940s. Ware details the contributions of Jules Charney, John von Neumann, Herman Goldstine, and others. He discusses the division of tasks, interaction among project members, design considerations, the pace of work, and patent issues. Ware distinguishes between scientific and commercial computers, and compares the Institute computer to others produced at the time. Associations of the Institute with IBM, RCA, and other companies and with Princeton University is also discussed. Ware also relates his move to the RAND Corporation and RAND's activities in computing.

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WARE: And I have pictures.

STERN: That would be helpful -- pictures.

WARE: But they're particular pictures. At the time the IAS was doing its thing, there were five or seven other

organizations around the country building copies of that machine.

STERN: Well that was later on. Would you say '49 or '50?

WARE: Oh, no. Much sooner than that.

STERN: Really?

WARE: Much earlier than that.

STERN: I didn't realize that.

WARE: And in particular, Rand was one of these places.

STERN: Can you pinpoint when Rand began --

WARE: Let me think a minute. Let me talk a minute and maybe it'll come out.

STERN: While you're talking and thinking, let me just say that this is an interview with Willis Ware in his office at the

Rand Corporation, January 19, 1981.

WARE: Right. So -- what I did -- I had a little deal with Rand to take pictures and send them to Rand. So I would photograph the machine as we built it. I just kept photographing it in 35mm slides, and I sent Rand a set of all those slides, and when I came here, I got to be custodian of the Rand set. So now I have -- I haven't checked it, But I think I probably have two complete sets of all the slides that I took -- but they were all engineering details and they were used by Rand as an adjunct to building the machine that they were going to build here. Because there wasn't any documentation. There were circuit diagrams and drawings, but no detailed documentation, so the pictures were an adjunct to them.

STERN: Well, at some point I really would like, if possible, to get a copy of those slides. If you can give me a copy, or I'll make a copy, or whatever would be --

WARE: Well, okay. If they're worthwhile to you. They're just nitty-gritty engineering details, close-up, resistors, tubes, and all the likes of that.

STERN: One - I think it would be helpful, and Two - when I publish some articles out of this, it will be useful to have some sort of photographs.

WARE: Well, why don't I get them organized, and see what I've got. They're all in a box over there.

STERN: Terrific. That would be great. Okay -- can we start --

WARE: Now, as to date. The Rand -- I mean, the IAS project started in the summer of '46. Right?

STERN: Well, it actually started, I think, in January '46.

WARE: But the engineering team assembled in the summer of '46. And I'll bet -- I think it was probably not more than a year or a year and a half later -- maybe two at the outside -- when the other groups started to do their thing.

STERN: So we're talking late '47 ?

WARE: So I would guess it's '47 - '48 is when the action got started. Now there's probably documentation here that I can dredge that up to pin that down, if it's useful.

STERN: That would definitely be useful. It's an adjunct to what I'm studying, but I'd be interested in it. I was under the impression that the copies came later than that, so it's very interesting that -- really, just during the development stage this was going on.

WARE: Well, the copies -- they were building stuff as fast as we got done. They were really right -- all of the groups were right behind Princeton, in a matter of a couple months or so. As fast as we did something, they did it too. In fact, some of the copies, I think, went operational before Princeton.

STERN: Yes, they did. That was one of the questions I was going to ask you about. Before we do that, can you give me just a little biographical information about yourself. Like, when you were born, where you went to school, what you majored in -- up until the time you get to Hazeltine.

WARE: Born August 3l, l920. Bachelor's Master's, and Ph.D. all in Electrical Engineering at University of Pennsylvania, MIT, and Princeton, respectively.

STERN: Were you working with the Moore School group?

WARE: I left the Moore School in l94l, and the Moore School Group was just getting started the following year. But Pres Eckert was a classmate of mine.

STERN: Really? I never knew that. That's very interesting.

WARE: And a lot of the other people that are known around the Moore School like Joe Chapline and Stu Eichert and the rest of them, were either a class or two behind me, and so I knew them at that time as undergraduates. And John Mauchly -- if I recall correctly, John Mauchly had just come to the Moore School. Art Burks I think had just come. I'm not sure of that. But at the Moore School when I was there, of course, the mechanical differential analyzer was a big thing.

STERN: Well, Burks and Mauchly came at the same time. They both took that summer course in '4l.

WARE: Okay -- then they came that summer and I left that spring -- in June.

STERN: And where did you go when you left?

WARE: To Hazeltine. To Hazeltine for the summer, and then I had a Tau Beta Pi fellowship which paid for my Master's degree at MIT in '41-'42. In the spring of '42, I went back to Hazeltine full time. And of course the war had started by then so we were into radar and radar beacons and the likes of that.

STERN: Did you work on the IFF project?

WARE: I sure did. I spent my whole time at Hazeltine working on that.

STERN: Do you think that kind of experience prepared you for the computing field? Is it a natural to move from that sort of thing into electronic digital computing?

WARE: Well, it was at the time because the digital computing field didn't exist and so it was viewed as another brand

of electronics except the circuits were different, but with the same old vacuum tubes, the same old resistors, etc. So it was an easy move for an electrical engineer.

STERN: Did you work with Pomerene on this?

WARE: Pom and I worked together at Hazeltine.

STERN: That's what I mean -- at Hazeltine.

WARE: Yes. And Pom then -- I've forgotten how Pom heard about the Princeton thing, but he went down to interview. When he came back he told me what a great deal it was and so I went down on the strength of his suggestion.

STERN: But during this period were you familiar with the computing work going on at the Moore School?

WARE: No.

STERN: So really you had no idea about electronic digital computing?

WARE: In all honesty, Nancy Stern, I went to Princeton because it was an almost-free Ph.D. The deal was, we could work for Johnny von Neumann during the day and get paid as an engineer, and take whatever time off that we needed to go off to campus and work the degree. But I was the only one who did that, as it turned out. It was a deal you couldn't say "no" to.

STERN: I can well understand that.

WARE: And a computer to me was -- I had no background in it; The IFF business was digital of a sort, -- it was really

pulse stuff, not digital -- pulse stuff like radar. And that was no preparation for computing. In contrast though, of course, Bigelow - coming from MIT, had a whole wartime experience of building analogue computing devices for gun trackers and the likes of that. And Pom's background would have approximated mine. So as far as we were concerned, I think -- as far as I was concerned, and I think also Pom -- we were simply moving into another brand of electronics.

STERN: Now he started at Hazeltine in Chicago, and then moved into the Little Neck area. Did you start at Little Neck?

WARE: I started at Little Neck. 5825 Little Neck Parkway.

STERN: I know that area. My husband worked for Hazeltine for a while -- way back -- about l0-l2 years ago.

WARE: The original Hazeltine plant was on the Little Neck Parkway just outside of Little Neck. And I don't know whether that one is there any longer or not.

STERN: Yes, it is. And they have one out in Greenvale also. Further our on Long Island. A second plant. But I wasn't aware that they had a place in Chicago until Jim mentioned it.

WARE: Well, they used to have a licensee lab in Chicago. Hazeltine of course was in the patent business for radio and TV and so forth.

STERN: So, Jim came back -- told you about the project at the Institute and you applied. You applied to whom? To von Neumann? To Goldstine? Can you recall?

WARE: No, I don't recall. I don't. But I went down there and I talked to somebody. Now, it might have been Bigelow. I might have applied to Bigelow. Because Julian was probably the first member of the engineering group to

be hired. I would guess that Julian probably came down there --

STERN: In March.

WARE: Yes, I was going to say spring. So the odds are that Pom and I both talked to Bigelow.

STERN: You said - well, it's a long time ago, so these things are hard to remember. When I spoke to Jim a couple of months ago, he mentioned that he didn't know how you got involved. He just remembered that he was talking to you about it and he was so excited to find that there was somebody from Hazeltine that was also going down to this project. I guess he just didn't recall that he had spoken to you about it.

WARE: That's the way my memory recalls it, Nancy. I feel really - reasonably confident that he came back and told me that he was just discussing it - and he told me the options. And they looked good enough for me to go down.

STERN: So essentially your interest was this Ph.D. program at Princeton. What did you think about the concept of a computer at that time?

WARE: It was a box full of electronics. No awareness at all of what the implications were. It was just another box full of electronics.

STERN: Jim commuted back and forth initially. Did you do that or did you move right away?

WARE: No. We wangled a deal to trade apartments. We had an apartment in Bayside, on Kennedy Street -- 3340 Corporal Kennedy Street -- and there was some woman and her daughter in Princeton that were going to work for the U.N. and they needed a place in New York, and we traded apartments. So we went down to Princeton right away, in the spring of '46; probably June or July. And then that summer we managed to find a permanent apartment at 274 Nassau Street.

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STERN: That's right on the main drag there.

WARE: No - 272 Nassau Street.

STERN: And when you got down there, besides Jim and Julian, who else was working on the project? Do you recall?

WARE: Ralph Slutz was there almost right away. And then I think soon thereafter Dick Snyder came. He fooled around -- and he had come -- I don't remember whether Snyder came from RCA, or whether he was a consultant for RCA. But I think he came from RCA. But I'm not sure whether it was RCA-Princeton, or RCA-Camden.

STERN: I think it was RCA-Princeton because he worked with Rajchman.

WARE: Okay. But Dick Snyder didn't stay very long. And then Rosenberg came later. I would guess that there was Pete Panagos who was the draftsman for the group and I think he must have come -- I would guess that he probably came in the fall of '46. I think that's right. Then somehow we got some shop people and I really don't know when they came or where they came from.

STERN: What were your first impressions of the Institute?

WARE: I really don't have any vivid recollections or impressions, although it became clear that we were sort of fifth class citizens around there.

STERN: In what way did it become clear?

WARE: Well, they stuck us in the second basement. And when you go to the social events -- let's see, at that time,

the Institute was run by the Quaker -- Aydelotte. So there would be social gatherings from time to time, and you would go to the social gatherings, and they's say "Well, I'm in mathematics" or "I'm in physics," or "I'm in --" -- "What are you in?" And then when one answered, it became clear that you were a social outcast.

STERN: In a sense though, you know, here there is a whole series of young engineers who were getting -- at least being invited to social gatherings with some of the most eminent mathematicians in the world.

WARE: Well, that's true, except that the people you often had such conversations with were not the permanent faculty, but the visitors that would come and they wouldn't always be so old, and they wouldn't always be yet so prominent.

STERN: I see, I see.

WARE: So I think most of us thought at the time that it was professional snobbery. That's an impression.

STERN: And it really did not seem to you from the permanent members as well as the visitors, that there was much of a commitment on the part of the administration towards this thing?

WARE: Oh, I've always felt very strongly that the only reason the Institute got involved in this whole deal was because of John von Neumann.

STERN: That their heart was not in it.

WARE: No. It was completely tangential to everything they ever did and it was not an intellectual exercise particularly. We were doing things with our hands and building dirty old equipment. That wasn't the Institute.

STERN: So you didn't get the Einsteins coming, visiting, seeing how things were going, or anything like that?

WARE: No. No. Incidentally, speaking of Einstein -- his assistant at that time, whom I knew quite well, was -- right out of my head -- the President of Dartmouth.

STERN: Kemeny.

WARE: John Kemeny.

STERN: He was an assistant to Einstein? I didn't know that.

WARE: John Kemeny was Albert Einstein's personal assistant for either one or two years while we were there. And in fact, for a while, Kemeny dated our project secretary.

STERN: Really? Who was the project secretary?

WARE: Acrivoe Kondopria. And Acrivoe is now married to a pediatrician and lives down here in Long Beach. That's -- I don't know exactly where Kemeny went after he left the Institute, but of course he finally wound up at Dartmouth and you know that history.

STERN: That's very interesting. I had no idea that was the case.

WARE: And John Kemeny -- Oh, he must have gone to the Physics Department at Princeton, because he was one of my thesis committee.

STERN: What was your thesis on?

WARE: I took a -- I had done some development work on a binary counter for the project and I used that work. All

dutifully published -- *Proceedings of the IRE*.

STERN: Were you aware of the problems with Eckert prior to your coming?

WARE: The rivalry?

STERN: Well -- that Eckert was asked to be chief engineer for this project.

WARE: I had heard that. I heard that after the fact. And as a humorous anecdote, when we heard that they were going to call the machine the UNIVAC, we figured they had a good name for a vacuum sweeper!

STERN: But you didn't have any dealings with Eckert then during the time that you were at the Institute?

WARE: No. After I left the Moore School, I probably didn't see or talk to Eckert again until - I don't know --

STERN: It was way after this period then?

WARE: It may have even been when I came back to Rand. I came on here in '5l. No. '52. And at that time we were actively following the development of a lot of companies that were working in computers and we made a lot of field visits. And it may have been on one of those field visits that I saw Pres Eckert again. I suspect that I did not see him at all during the whole time that I was at Princeton.

STERN: What were the working conditions at the Institute when you started?

WARE: When we first started, we were given temporary space in the basement under the one -- no. Our first place, I guess, was temporary space in the second basement, surrounding the boilers. You know - it wasn't bad since it was summer and they were turned off. And then we moved upstairs to the first basement under one wing, as I remember.

STERN: As a promotion -- moving up?

WARE: Yes. And we built all our work benches, and we cobbled up our work benches out of 2 x 4s, and 2 x 6s, and strung our own wires, and so forth, while the brick building was being finished.

STERN: Do you have any recollection of some of the people who came from the Moore School like Robert Shaw, Jim Davis?

WARE: Jack Davis. I went to high school with Jack Davis. John Davis it really is. Jack's his nickname. But it's really John. He and I went all through high school together. We lived about two blocks apart in Ambler, Pennsylvania. We went to the Moore School together. I used to sit on Jack Davis' bed and listen to the short wave radio on receivers that we'd built on our mothers' pie pans. Then -- I think he stayed at the Moore School. I'm not sure of this. And I went off then to MIT and Hazeltine. Then, he got hooked up with the Navy somewhere along the line, and was in their Aircraft Development Center at Johnsville, and I heard that he retired a couple of years ago.

STERN: But he came to the Institute, didn't he, for a very short period of time?

WARE: I -- I don't think so. But I'm not clear. Shaw would come up. Albino - Bob Shaw would come up, and I thought -- I always regarded them as visitors. I really don't know.

STERN: I think they were scheduled to come. They only stayed a short period of time, and then they went back to work for Eckert and Mauchly. And I wondered about that.

WARE: Oh. I think you're right about that with Shaw, but I don't think Davis ever worked for Eckert and Mauchly.

STERN: So he was visiting from the Moore School?

WARE: I'm fuzzy about that. I don't know.

STERN: I'll have to check that out.

WARE: But Shaw certainly did come for a short while. You're right about that. And then went back when Eckert and Mauchly got tooled up.

STERN: Why come for a short period of time and then leave?

WARE: Don't know. Really don't know. I suppose Eckert and Mauchly just looked like a better deal and moreover, Shaw was a Philadelphia guy so I suppose he was going back to where he felt comfortable. And Shaw, I think, probably had a reasonably close personal association with Pres Eckert, because he was at the Moore School also.

STERN: Eckert's brother-in-law, John Sims, came to work for the Institute.

WARE: Oh, John C. Sims. Gee -- I'd forgotten all about that name. That's right.

STERN: And he also left after a short period of time, but did not go with Eckert --

WARE: It was not too short. It wasn't as short as the other. I think. I think Sims stayed longer.

STERN: What sort of contribution did he make, if any?

WARE: I have no idea. It seems to me that Sims was a mechanical engineer, and not electrical. So I really don't know.

STERN: He was not working then specifically with the electrical people?

WARE: I would guess not, but I don't really remember. The way the thing sort of lined up -- Bigelow and Slutz were sort of doing the design. Pomerene and I were sort of doing the circuit development on the bench. Jack Rosenberg, when he came, sort of had responsibility for getting the whole thing put together and built. He supervised the technicians. He saw to it that the pieces got bolted together, helped conduct some of the tests. Panagos was doing the drafting. Dick Melville (?) -- I don't remember when he came. Dick Melville ran the shop. But he came later, as I remember. There are no personnel records at the Institute on all this, are there?

STERN: No. I'm beginning to develop some sort of --

WARE: Well I can tell you how you can pin down some of it because progress reports were written every six months, and the names would turn up as they came aboard.

STERN: That was the one method I used for at least getting names, but the progress reports didn't start until about l8 months after.

WARE: The first two progress reports were von Neumann, Goldstine and Burks design and whatever it was -- logical design and something-or-other for a digital computing instrument -- something like that.

STERN: And did you use this -- I guess -- working in circuit design, did you rely on that document at all? Those documents?

WARE: No. No. Bigelow and Slutz were in between.

STERN: Now, Bigelow makes comments about how initially he ran the shop in a democratic way, and then eventually decided that that really was not in the interest of the project as a whole, and --

WARE: I think that's probably true. And I -- this is conjectural -- but I imagine what happened was that he thought the rest of the boys were as grounded and had the mind-set of a computer-oriented person, and we didn't. We didn't have the experience he had at MIT during the war. So you're right, and I don't remember the period of time, but there was a period there when there was a lot of "to-ing and fro-ing" and floundering, and not much progress.

STERN: And then after that, he was much more specific about what he wanted people to do.

WARE: Yes.

STERN: And you think that that was an appropriate way to move?

WARE: Oh, absolutely. Look, it was an engineering development project. Somebody had to be in charge.

STERN: What was Goldstine's role at this point?

WARE: Well, he of course was von Neumann's representative on site. He was Deputy Director - whatever - #2 on the project. No, I guess he was the leader of the project. I really don't know what label he carried.

STERN: Associate Director.

WARE: Associate Director. And he was always there. And of course we had the guys drifting in like Jules Charney on the numerical weather stuff, and Phil Thompson, whom I discovered is now with Boulder, or some such place.

STERN: Is he? Really -- I didn't know that.

WARE: And Herman would interact with Johnny on mathematics, he'd interact with Charney and those people on

their kind of exploitation of the computer, if we ever got it done. He really didn't concern himself whatsoever with the actual engineering development. He left that to Julian.

STERN: I see.

WARE: And I think he obviously had some role in getting those initial documents written - the Design - Logical Design and whatever it is.

STERN: What about Burks? Did he make any contributions?

WARE: I don't remember Art Burks ever being around the Institute, although he must have visited, but I don't remember it.

STERN: Well, he was there for two summers, at least. The summer that you started - in '46. And then he came back in the summer of '47. And there are some documents that talk about his work on the multiplier. But nobody seems to remember any work that he actually did on that.

WARE: Well, let's go back to the "to-ing and fro-ing" business again. When Dick Snyder was there -- and I don't know why he would have thought in digital terms -- but he came up with -- he had an adder design and I don't remember much about it except that it struck me at the time that it was a little strange; but it didn't get anywhere. And it may be that Burks had some ideas for a multiplier that didn't get anywhere either.

STERN: I see.

WARE: But the way that machine is built, and the way it does its thing is exactly the way it's laid out in that Logical Design document that we're talking about. It followed that exactly.

STERN: It's my sense, from reading the few documents I have, that it was at the point at which Snyder's adder didn't work properly that Bigelow decided he has to shift his supervisory concepts. Would you say that that's a fair estimate?

WARE: It could be, Nancy. I don't really know. I really don't know. I didn't keep a diary at the time. There wasn't any documentation. I really don't know.

STERN: The people working in the circuit area really were separate from the people working in the design area. Would you say that's fair?

WARE: That's the way it came to pass. In fact, Bigelow -- if I recall correctly, Bigelow and Slutz shared an office, and Julian would have the ideas, or Ralph would kind of detail the ideas, and then Pom and I would go try and make the electrons do their thing.

STERN: So that when you did have meetings it was the kind of thing where -- it was laid out for you and then you went back and you implemented them.

WARE: I don't remember that much formality.

STERN: Not even that much. I thought -- I was presenting that as kind of informal.

WARE: Well, the part that I remember kind of clearly, because it's the work that I used for my thesis -- Julian said "Here's a rough circuit of a binary counter that I think will work. Go build it and find out if it does. If it doesn't, [find out] why it doesn't. And fix it."

STERN: You said a while back that Goldstine did not have very much to do with the engineering aspect of things.

WARE: That's my belief -- that he interfaced some with Julian, but that he never really dabbled or got concerned about the nitty-gritty details.

STERN: That being the case, why this tension between the two of them?

WARE: I don't know. Where did you pick that up?

STERN: Well, first -- were you aware that there was some tension between the two of them?

WARE: I guess I wouldn't have called -- I wouldn't have thought of the word "tension." I know they kind of had an arm's length relationship, and that they didn't -- they weren't buddies in any sense of that word.

STERN: I picked it up from the documents, as you can subtly, and then in speaking to them -- if I weren't aware of it before, I certainly was aware of it after. But I'm not clear as to the reasons.

WARE: As much as anything, it could have just been personalities.

STERN: Did they have very different ways of operating?

WARE: Yes. Herman was by training a mathematician; Julian by training was an engineer. Although I guess he had a big smattering of physics thrown in on the edge. And Julian could be kind of caustic. People -- not only I, but other people -- seeing in Julian in recent times, will remark on how he's mellowed over the years. He could be very caustic. And I really don't know. Maybe Julian saw Herman in his way -- or maybe Julian wanted to be the Associate Director. Those are all conjectures. I haven't any reason at all to say those with conviction.

STERN: Well, Goldstine seemed to think that Julian was a perfectionist, and although something could be done 99% perfectly, he wouldn't release it unless he got that extra l%, and that l% could take much longer.

WARE: I can see that being said. But I think, after the fact, that damned machine might not have worked except for that.

STERN: So you think that that was a very positive trait?

WARE: It turned out to have been. Under other circumstances, it might not be. But in that case it turns out to have been because -- hell -- we were trying to make 2000 vacuum tubes do their thing! And do it reliably and all that kind of thing, and that level of perfection was a positive attribute. Now, I guess the way the perfection might have been troublesome is if progress didn't go as fast. Let me offer you a conjecture which may or may not be true. But Julian really was the pacing item in the progress of that whole effort. The rate at which Julian could think, and the rate at which Julian could put ideas together was the rate at which the project went. And I can imagine that Herman might have chafed that the progress wasn't faster.

STERN: But from your end of things, you never felt the pressure to do things faster? You're saying this is kind of looking outside, but never from within the project did you have that sense?

WARE: I'm not sure that the whole damned thing *could* have gone much faster. We were building a different kind of machine right from scratch, and there were an awful lot of details that had to be thought through. There were a lot of circuit arrangements that had to be thought through. And until those were sort of done, the guys out on the bench trying to make circuits work had nothing to do -- so you were kind of piddling around and playing, and trying whatever seemed interesting. So that initial thinking and that initial scoping of the project, and those initial essential decisions were pretty much right out of Julian. After Slutz came, Julian -- aided and abetted by Ralph. But that was really the pacing item. Now, once they got enough of that done, then the crowd out back could pick up the pace and go as fast as the ideas flowed.

STERN: Would you say it's characteristic of good engineers to have that kind of perfectionist trait?

WARE: No.

STERN: No?

WARE: An engineer is not a perfectionist. An engineer is trained to compromise. An engineer is trained to get an answer, regardless of the matter that he doesn't know all the facts.

STERN: So it is a kind of atypical --

WARE: I think so. And, in fact, Julian, in a kind of a precise classical sense -- Julian wasn't an engineer. He was kind of more physicist and theoretician than engineer.

STERN: What kind of relationship did the computing group have with von Neumann?

WARE: We would see him, and we would go to parties at his house, which were real dingers, I'll tell you. Did Pom tell you about the story -- about him and Nick Metropolis one night after -- well it's an amusing story so I'll share it with you. Klare von Neumann used to make Fish House Punch, and I don't know what she put in it, but it was potent stuff. Nick Metropolis was visiting from Los Alamos. He was running the Los Alamos effort -- the MANIAC -- so after von Neumann's party, Pom and Nick drove home in -- I forget what car it was -- but *backwards*! The next time you see Pom, remind him of that. It'll come back to his memory. So, we would socially mingle with them -- and the parties that Johnny had were quite different from the parties that the Institute had. They were really a friendly bunch, and there was no stand-offishness and intellectual snobbery. And Johnny and Klare were just nice hosts. In fact, Marina - Whitman now, she's called -- all of us knew as two feet high, or whatever she was at the time.

STERN: But no sense of the same intellectual snobbery that --

WARE: No. Not al all. Not at all. There would always be some number of outsiders there, but there were -- it was a friendly bunch. Metropolis -- if he happened to be in town -- would visit. I'm not sure whether Richard Arens, who were in the math department at Princeton at the time. They're now at UCLA. It's A-R-E-N-S. It seems to me that they would sometimes come to those parties also. But sort of -- the sort of "in-group" - the institute group didn't turn up. This was kind of a project-oriented thing, together with friends of the project. And Phil Thompson of course would be there. And Jules Charney, and whoever Jules' wife at the time was. I've forgotten what her name was -- Eleanor. Jules and Eleanor Charney. So we had that kind of social interaction with von Neumann. And he would come down and look in from time to time to see how we were doing, and he was always very friendly, and the interface was easy. You could talk to him like you and I are talking.

STERN: And that was from the very beginning -- considering the fact that he was such an eminent man at the time --

WARE: It was a very easy interface between Johnny von Neumann and all of us on the project. Everybody -- including the people -- he'd walk around the shop and he was just as easy out there as anywhere else.

STERN: Did he visit the project often?

WARE: I would use the word "occasionally." I don't know what that is in terms of times per year, but I would say not often. Just from time to time.

STERN: He was involved with the Atomic Energy Commission and Los Alamos at that time. What other projects was he working on at the time, do you know?

WARE: No, I really don't know. But he was deep into all the Atomic Energy affairs at the time. But I don't know any explicit projects.

STERN: When he came, did he ask specific questions, or was he just looking around essentially?

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WARE: He was conversational. "How are you doing?" There are some wonderful anecdotes that I'm going to write down and send to Bernie Galler for his *Annals*, but I'll tell you one right now. I remember Johnny saying one day, in his beautiful Hungarian accent, "What would anybody want with more than a thousand words a minute?"

STERN: Seems a lot of these great people in the '40s had comments similar to that -- that really is surprising. Since he didn't come all that often, as I gather from what you're saying, and he really didn't take a tremendous interest in the developments that were going on, why did he push to have the Institute develop a machine?

WARE: In the first place?

STERN: Yes.

WARE: I really don't know. But I would assume that that was his home -- his professional home, so why not?

STERN: Why would he want to build a machine? He didn't spend the time actually working on the development. Why not have some other group that was tooled up already for that sort of thing?

WARE: I can't answer that. And the only person that I think you might get that answer out of would be Herman. I really don't know. But remember that he and Herman were acquainted at the Moore School. See -- I suspect -- this is conjectural again -- but I would guess that he figured at the end of the war, he had a whole new set of ideas that were not incorporated in the ENIAC, so I can imagine Johnny thinking to himself, "Well, here's myself and Herman and Eckert and Mauchly, and Burks. What a team to go do this thing that I want to do!" So I suppose that that's what he tried to make happen with funding from Army, Navy, and the AEC, as I remember it. It was three-part funding. Try to put it at Princeton because that was a nice home for it, and also -- I guess the Moore School probably had its own vector, or else maybe had decided it didn't want to have anything to do with this funny business. I don't know that either. But - oh - have you ever talked to J. G. Brainerd?

STERN: Yes.

WARE: Brainerd would know a lot of that history of the l945-46 era.

STERN: Brainerd -- I believe Brainerd -- I know Eckert said that there was some discussion of a three-way project between the Moore School, IAS and RCA. But Herman -- and there's no documentation on this -- Herman said that that was never an option; that there was a question of having all of them come to Princeton, but there was never a discussion with the Moore School to do that sort of thing.

WARE: Those are details that people like me and Pom were not privy to.

STERN: There has been some publication that indicates that some people are better off doing theoretical work and other people are better of doing practical or applied work, and that von Neumann would have been better off to stick with the theoretical and not to have undertaken such a project?

WARE: Oh -- I think Johnny's insight was that he saw the computer as a tool to let him do things that he couldn't do analytically. And therefore he had to have this thing.

STERN: And the meteorological group that came to work out certain equations on the machine -- that couldn't have been done by any other means.

WARE: Absolutely.

STERN: Von Neumann wanted the focus of this machine to be scientific as opposed to commercial?

WARE: Yes.

STERN: What did that mean at that time? Aside from today's concept of a scientific machine being processing-oriented as opposed to l/0 oriented, what did it mean then?

WARE: In terms of machine structure, it probably didn't mean anything. Probably meant that the project was housed in an academic institution instead of in the industrial world.

STERN: That's what I -- my sense is.

WARE: But in terms of processing organization it probably didn't mean anything. You can look back and say "Well, the machine was binary because that's the way you do arithmetic, and you do arithmetic in science and engineering." On the other hand, Eckert and Mauchly UNIVAC I was character oriented because that's the nature of that world.

STERN: So that would be a distinction between a "scientific" machine as opposed to a --

WARE: That was a distinction, yes.

STERN: Was there ever any discussion of making the machine serial -- or did it always have a parallel focus?

WARE: The word "parallel" was in from the very beginning. Absolutely from the very beginning. In fact, I think *parallel* may be even in the title of that Logical Design document.

STERN: I don't think it's in the title, but it's very close up there as to being an important concept.

WARE: It was the concept right from the very beginning.

STERN: Never any thought to making it a floating point machine?

WARE: I suspect, Nancy, that at that time, the general awareness of involved calculations was so poor that no one realized the intricacies of scaling; therefore the desirability of floating point. Floating point never got talked about while I was there. Now, remember that the project went on -- see, I was there for five after that with Pomerene as Chief Engineer. So floating point might have been talked about then. As soon as I came out here to Rand, though, I bumped into floating point right away. Because Rand had CPCs and 604s. Floating point became of interest out here, but we never put it on the hardware as such. Which reminds me that probably the discussions of floating point at that time were always in terms of doing it by software, because the hardware was a little too costly to think about - dedicating it to a special function like floating point.

STERN: And perhaps von Neumann felt he didn't need it.

WARE: Don't know.

STERN: Could the friction between Goldstine and Bigelow be a result of a kind of attempting to have von Neumann's ear? Could it have been that sort of thing?

WARE: I can acknowledge that it could have been, but I really don't have any evidence.

STERN: You have no first-hand knowledge of any difficulty really.

WARE: No. I think Johnny and Herman probably both got a little annoyed at the fact that the thing wasn't going lickety-split. On the other hand, I think neither of them appreciated the magnitude of that undertaking. So they probably both thought, without ever being precise about it, that "In two or three years we'll have this magic thing!"

STERN: Yes - the initial estimates were two or three years.

WARE: It just turned out to be a tougher job. That's all. Partly a tougher job; partly we got in trouble with the memory; partly we had the bottleneck -- the intellectual bottleneck: Julian kept it all to himself.

STERN: Do you think that really was a problem - that Julian kept it to himself?

WARE: In modern parlance, what you'd say was: Julian was the architect of that machine.

STERN: Might it have gone smoother if it was a more open kind of thing? I mean, when I talked to Julian, he said that you and Pomerene were absolutely critical to this project. Well, why not get you more involved in the architecture?

WARE: I don't know. I really don't know. What I would -- the way I would conjecture is - Julian's personality. He was that kind of a person. Now, you know, you can make all kinds of wild guesses -- but they would be wild guesses, and I'm not sure they ought to be made. Julian was that kind of a guy. You'd give him an idea and he wanted to see it through from start to stop --intellectually. That was his personality.

STERN: Anybody ever try to push him to be more open about these things?

WARE: I don't know. Herman may have. But I really don't know. And there's another way you could say it in today's parlance: Julian is not an engineering manager. He's not a big engineering project manager. He just isn't. He's an intellectual guy. And I'm sure that there's been, after the fact, discussion to the effect that "Well, maybe the whole project would have gone better if Slutz had really been the engineering manager, and Julian had been off here on the side someplace as the idea guy. And it might have.

STERN: When I spoke to Julian in August, I asked him why Slutz left to go to the Bureau of Standards, and he said that he thought Slutz really wanted to be Chief Engineer - at Princeton.

WARE: I think that's probably - that would be my intuition, Nancy.

STERN: That that would be the case?

WARE: Yes. See, Ralph was a strong person in his own right. And after Ralph came to Princeton and worked for whatever it was -- two or three years -- he was probably progressing at a rate that led him to feel that he was ready for bigger things, and it wasn't going to happen with Julian there. So I don't know how Ralph ever -- in fact, I don't even know how Ralph came to know Sam Alexander at NBS, but one way or another he wound up down there to build that machine.

STERN: Well, the input/output for the IAS machine was supposed to come from him.

WARE: Absolutely. Absolutely. And I'll offer you another conjecture: I think Ralph was probably bothered by what he considered to be too slow a pace. You know, Ralph's still around - if you haven't talked to him.

STERN: I have an interview with him. In Boulder. Yes.

WARE: Well, ask him about Phil Thompson, because I think Phil Thompson's either at the National Weather Service in Boulder, or he's at ENCAR (?) also in Boulder. So maybe you can pick up Phil Thompson, too.

STERN: Okay. That's a good idea. Did anyone replace Slutz in terms of being a right-hand man to Bigelow when he left?

WARE: I think the answer is no. I can't recall who it might have been, But I think the answer is no.

STERN: If I may speculate -- had he stayed longer, the project might have gone faster -- do you think that's a possibility?

WARE: That's a possibility, but it would have had to be Slutz pushing Bigelow, and I don't know whether Julian could be pushed. It's a possibility, but I would estimate a low probability.

STERN: When you came, there were no laboratory facilities at all at the Institute. Is that correct?

WARE: That's correct.

STERN: And you had to spend a significant amount of time getting facilities, getting test equipment, supplies --

WARE: Right. We built our own oscilloscopes.

STERN: You did? I didn't know that.

WARE: Well, one of the things that I got exposed to at Hazeltine was building, at that time, extraordinary oscilloscopes to work the TV business. We would build oscilloscopes which, at that time -- would you believe, had 5 mh .... And I learned a lot coming out of the radar and IFF business. I learned a lot about how to build pulse generators and the likes of that. And so a lot of the test equipment that we had at Princeton, I just used my -- and Pom had the same experience -- we used our Hazeltine experience to build that test equipment.

STERN: Would you say that it's fair to say that the first -- almost the first full year was devoted to this sort of thing? Even prior to doing any circuit design?

WARE: I have no sense of the timing, but it could have been that first year. It could have been even more. Of course you can tell a lot from those progress reports.

STERN: Well, for von Neumann and Goldstine to tell the funding agents it's going to take 2 or 3 years to build a

machine, and not taking into account --

WARE: There was no awareness on the part of those two as theoreticians and mathematicians that there was not an adequate instrument industry in the U.S. to supply what would be needed to work at micro-second circuit stuff.

STERN: Could you get any of this test equipment from the Army Surplus, or wasn't that feasible at the time?

WARE: We would get what was considered to be good oscilloscopes at that time. These Bumont (?) something-or-others, and we would simply use them as the tube, and build an appendage up on top that had all of the fast circuits in them. So we really built -- all of our instrumentation and all our test equipment was built there. And that's -- that, plus cobbling up benches and everything else *easily* consumed a year, I would guess. And then we had to move down to that other building. Incidentally, I did all the photographs for those progress reports, so I have those negatives around someplace.

STERN: Oh, great!

WARE: They're safely someplace. I think it's in the box. And I have also -- Also I have a lot of photographic coverage, although not as complete as the work here, in case you ever get around to doing this part of it.

STERN: As I said, right now I'm interested in the copies to the IAS project, but only in a peripheral sense. Eventually -- it takes time to move on to the other things. Were you involved with Rajchman at all in the Selectron work?

WARE: Peripherally. We knew of course -- all of us knew what was supposed to be the memory over there, and we would go over and talk to Jan Rajchman and Milt Rosenberg at the time. And we would see the demonstration, and we would see demonstrations of color TV while we were over there testing. But I had no direct involvement. Nobody really had intimate involvement with Rajchman except Julian. The project interface was really Julian to

Rajchman.

STERN: When did you sense that there was some dissatisfaction with the progress on the Selectron?

WARE: It was just plain schedule -- purely and simply schedule. And I don't know when that was. If you can find out when Julian made the trip to England to see F. C. Williams and back-date it about six months or nine months or something like that -- that's the period in which that growing awareness would have been developing, that the Selectron might not make it.

STERN: But from your vantage point, working on the circuit design, it was not the major problem at that point?

WARE: No. No. We hadn't started the memory at all. And as far as -- I would say that as far as Pom and I were concerned, one day we were kind of told, "No. We're going to use the Williams tube memory in this thing, and here's what it's all about, and let's get some action." Now, Pom started that. And I don't know -- Pom did not go to England with Julian. I think Julian went alone. I do not believe that F. C. Williams visited the project. But remember, there was also electrostatic storage tube development going on at Whirlwind.

STERN: Was there any contact with the Whirlwind people?

WARE: There might have been, but if so, it would have been predominantly through Julian, or maybe Herman. But certainly not me. And I suspect not Pom either. But according to my memory, Pom did the initial exploratory experiments with the Williams tube stuff. And then we got around to doing the big design, and I sort of tuned in with him.

STERN: You weren't involved then with the Rand decision to use the Selectron, then, later on?

WARE: No. I think that decision had already been made by the time I got here.

STERN: Would you have thought of the Selectron as a failure because it was not used in a mass production way?

WARE: No more so than I think the dinosaur is a failure!

STERN: Because some people have classified it -- that's why I ask.

WARE: In evolution, not everything succeeds. And it was one evolutionary vector that -- if the magnetic core hadn't come along, the Selectron might be the thing we'd have today.

STERN: But it did succeed. I mean -- it was used in a machine.

WARE: Oh, sure! Oh -- we got tremendous performance out of it.

STERN: Were you familiar at all with the patent policy at the Institute when you came?

WARE: Well, I can't remember being familiar with it, but I would suppose that I signed some piece of paper -- but I don't know that for a fact.

STERN: So it was not a concern of yours?

WARE: No. And really, you would have thought it ought to be because at Hazeltine, of course, patents were kind of high on everybody's list. But - no, it wasn't a concern of mine.

STERN: Patents were high on everyone's list, but there was no chance, as I understand it, of really making any profit from a patent when you worked for an industrial organization?

WARE: That's right. You got a buck? In those days.

STERN: But at the Institute, the possibility existed that if you did get a patent, you could derive some financial advantage.

WARE: I don't know. If you're telling me that's the way it is -- why, fine. But I don't remember it.

STERN: Well there seemed to have been some issues that are in the documents and have been substantiated by Goldstine and Bigelow giving me two different perspectives on this. Now, Bigelow said that there were two patent policies: one, until '48, which said that any engineer who invented anything would get the patent and that the Institute would supply lawyers to file for the patents. But the patent would belong to the inventory, not to the Institute -- as long as the government got its license-free right. Then after '48 it changed to the traditional kind of patent agreement because the funding agencies insisted upon it.

WARE: Could be, Nancy. I don't know.

STERN: But it was just not simply anything that concerned you at that point?

WARE: Well, if it did, it didn't stay with me.

STERN: Of course, Bigelow was particularly upset because that support never materialized, and he never got any patents issues. He had asked von Neumann to supply these attorneys and it just never materialized.

WARE: That's a piece of it that I really didn't -- I guess I never knew it or have forgotten it.

STERN: Were you aware that von Neumann was consulting for IBM in the late forties -- at the time?

WARE: Probably yes. I probably was aware of it, but I'm not absolutely certain. But I remember another incident that's of a similar nature. There was a development out there on the West Coast called the Digital Differential Analyzer, by a guy named Floyd Steele. And I remember hearing a story to the effect that Floyd Steele brought his digital differential analyzer in a suitcase to a hotel room in Princeton and demonstrated it for at least von Neumann and probably also for Goldstine and Bigelow. I'm not sure of the latter two.

STERN: Is this the Northrup project?

WARE: It was Northrup at the time.

STERN: And how did von Neumann react to this?

WARE: The way I heard the story, he was very impressed by all this. He regarded it as a -- as Johnny would -- the digital differential analyzer is a very ingenious, innovative arrangement -- which it was.

STERN: Did you get reports or any information about other projects that were going on at the same time?

WARE: If they were around, I don't remember ever seeing them.

STERN: Did you attend any conferences relating to other projects?

WARE: No. I would say no.

STERN: Were you aware of people coming to the Institute?

WARE: Yes. Well, especially the ones who were copying our work. They were in from time to time from all over.

STERN: I meant essentially other projects -- "competitive" projects?

WARE: No.

STERN: So, essentially the people at the Institute were working independently of any other...

WARE: If the things that you are asking about were occurring, they were occurring either at the Goldstine level or at the Bigelow level; the rest of us were kind of sheltered down there.

STERN: You had very little dealings, then, with Goldstine?

WARE: Oh, I talked to him. Would see him and Adele from time to time. But on a professional or a daily basis - very little.

STERN: As an administrator, he seemed competent enough.

WARE: I had no way of knowing because Julian was buffering us from all that. I'll tell you a funny story. Which maybe Pomerene told you because he was part of the story. But those were the days when everybody was trying to get a television set, and Jack Rosenberg had bought a kit to put one together, and on the strength of that, we all bought kits and put these darned things together, including Herman. Well, Princeton was halfway between Philadelphia and New York so you really had to have a good antenna to catch it. So one weekend, and I think Pom was part of it, they put the antenna up for Herman, so the joke Monday morning was - over the weekend, Herman had had an erection!!!

STERN: That's funny!!! How [did] it strike you -- von Neumann consulting for a commercial organization?

WARE: I wouldn't even have thought about it.

STERN: Isn't that an odd thing for a --

WARE: I wasn't alert to those things in those days.

STERN: Because it would seem to me that, you know, his stand on building a scientific computer as opposed to a commercial computer -- that this would be something unusual for a man --

WARE: Wait. It was a commercial organization, but it was still a scientifically oriented computer.

STERN: The 70l you're talking about.

WARE: Well, it had an earlier name. See, I got to Rand just about the time that was tooling up, but it was called the Defense Calculator at the beginning. No, I'm sorry. That was tooling up earlier than that. Before I came to Rand in spring of '52, I put in a short period of time -- six months; from the fall of '5l to the spring of '52 -- with North American over in Downey.

STERN: As an IAS representative?

WARE: No, as an employee.

STERN: So when you left the Institute?

WARE: When I left Princeton -- when I left the Institute, I went to work for North American Aviation -- it was called at the time. And they were just beginning to try to get into the digital business. They had built a digital differential analyzer. They were beginning to understand what diodes were all about. And they were thinking of them as mechanisms for guidance on missiles. And at that time the Defense Calculator was -- IBM was running around

talking about the defense calculator and letting it be known that if they had l3 orders they'd build them. And then I switched -- I switched from North American to Rand at about the time that it got renamed the 70l.

STERN: Would you say it's fair to call the 70l a copy, in some sense, or dependent, in some sense, on the IAS computer? Or was it a completely independent project?

WARE: Oh I think the logic and architecture was a precise copy. It was obviously a different engineering.

STERN: What about the ethics of a person like von Neumann doing this at the Institute and then having IBM "copy" it?

WARE: I don't think -- I think the motivation's wrong in the way you put it. IBM under T. J. Watson, Sr. was a very patriotic company, and he probably believed he was doing a good thing for the interval, that period of time, they did not see big electronic digital computers as a profitable venture for the future.

STERN: But they did have people like Cuthbert Hurd that was there who was really thinking in terms of making IBM a computer company -- don't you think?

WARE: The story is that George W. Brown who was at Rand when I came is responsible for persuading IBM that there was a future in digital computers. And the claim is that George has a watch from IBM as recognition of that good deed that he did!

STERN: I assume convincing Watson, Sr. would be no mean feat at that point.

WARE: I'm not privy to any of those details. I'm sure there are people at IBM who could fill you in.

STERN: Yes, I have plans to have a couple of discussions with people there. But you essentially think that von

Neumann --

WARE: Cuthbert Hurd's in San Francisco.

STERN: I'm seeing him tomorrow. I'm going back there. You would think then that von Neumann worked with IBM essentially for defense purposes?

WARE: No, I want to sort it out a little more carefully than that. I'm sure that -- I would feel certain that Johnny understood that he was consulting for a commercial company that might make commercial products. On the other hand, my intuition says that IBM, in thinking about building that 70l, was kind of patriotically oriented and they figured they'd build a few of these things that the country needed for defense business, and then go back to punched card stuff. The whole ethics question -- the whole ethics question just wasn't high on anybody's list, I'd guess, at that time. Remember, Pom and I were young engineers and we didn't think these thoughts!

STERN: Why did you leave the Institute?

WARE: I was finishing my degree. I was just looking around for someplace else to go.

STERN: No particular reason other than that to leave Princeton?

WARE: Well, in some sense, it's an accident. I had gone to the IRE Convention in New York City, and that must have been March of '5l. And somehow or other I got talking to Hughes Aircraft who had a big recruiting drive on there, and so I suddenly found out there was a thing -- I found myself negotiating by phone with North American for a job. And I was winding up the degree and it looked like kind of an adventure to come out here, and it's kind of that simple. Opportunistic.

STERN: You stayed with North American not all that long?

WARE: Six months - because all of the things they said were going to happen didn't happen, partly; and also the person here at Rand who was building the machine, Bill Gunning, who's now Xerox-PARC had gone skiing and broken a leg. And George Brown who ran the mathematics -- who was No. 2 in the math department here at the time, suddenly realized they had all their eggs in Bill Gunning's head, and if he got hit by a truck, Rand was in trouble. So I had a suggestion, as I remember it, from Bill Gunning, saying "Why don't you come over and talk. Maybe we have things of mutual interest." And that's that. It was a natural move for me.

STERN: Did George Brown ever have any dealings with RCA?

WARE: There are two George Brown's. This is George *W.* Brown, and the other one is nicknamed George "Antenna" Brown.

STERN: Now the other one -- did he go to UCLA, or is this the George W. Brown?

WARE: This is George W. - mathematician. A statistician who used to be at Ames, Iowa -- if you go back far enough.

STERN: Because there was a document that von Neumann had written to Goldstine saying that had George Brown remained at RCA, he thinks the Selectron would have been completed.

WARE: That's RCA's George Brown.

STERN: Okay.

WARE: And I don't remember his middle initial.

STERN: That clears up a lot because I've been having some problems with that.

WARE: The Selectron was a tough vacuum tube development job. It really was tough. They were doing things inside that vacuum that hadn't been done before.

STERN: A very ambitious project, too, from what I gather. When you left the Institute, was the machine completed?

WARE: No. The machine was not completed. But now you're going to ask me how far along it was.

STERN: Yes.

WARE: The memory was working and we had married the memory to the main frame. We'd stuck the memory up on top. The multiplier was working because Pom and I had tested that. And I suspect that what had yet to be done were some of the nitty-gritty details of what we called in those days the "logical" control. The instruction, fetch and execute, and all that kind of thing. The details -- the intellectual work had been done. It was clear how these things were going to get done, and what remained to be done from an engineering point of view was to implement all this stuff into nitty-gritty circuits and get them built and have it go. And I think that was the part that hadn't been done. I do not know the date on which the machine executed its first program. I just don't know. Pom probably does.

STERN: When you left, had Bigelow already gone for his Guggenheim, or was he still Chief Engineer?

WARE: He was still there and there was a little bit of stress between Julian and me at the time because he saw me as deserting a ship. And in looking back, in some sense a he had a right to feel that way because I sprung the whole thing on him on kind of short notice. That probably -- I probably left within -- I probably gave him only a couple of months, maybe -- an indication that I was thinking about leaving and he wanted to get that damned machine done; and I'd been there and I knew a lot about it, and obviously I'd been an asset to help finish it, and here I was going gallivanting out to the West Coast. So --he had reason to think nasty thoughts about me.

STERN: You had no desire to wait to see the machine -- see the machine to completion?

WARE: I don't remember any such desire.

STERN: Snyder worked on the accumulator. He left shortly thereafter. Was he a good engineer? Snyder? What was the problem there?

WARE: I would call Snyder -- I would call Snyder an inventor in kind of the classical sense. He would cobble up anything on a bench and he could make it work, and he was kind of innovative in doing that. But it was another matter to reduce that to a hard engineering design that would perform reliably.

STERN: He had some documents that were among von Neumann's papers in which he wanted to title a patent on Magnetic Core. This was after he left the Institute, so apparently he'd been doing work on that on his own as well.

WARE: Could be. I don't remember that.

STERN: Were you involved in the funding end of this at all?

WARE: No. I believe that was von Neumann purely and simply, probably aided somewhat by Herman.

STERN: But there was never any problem getting the appropriate funding?

WARE: I have no idea. I have no idea. I don't recall any discussion that ever suggested that there was a funding problem. The paychecks always came on time.

STERN: That's how you know there isn't one! You don't recall problems like that! Why is it that the copies to the

IAS machine were completed before [it was]?

WARE: Because it's always easier to go faster when somebody else has -- you know, the lead dog on a sled has got a hard job compared to the one in the back.

STERN: It's as simple as that?

WARE: They didn't have to do any of the intellectual work. There it was. The details were all worked out; to the extent that they wanted to make changes, all they had to do was make some engineering refinements, or package it a little bit better. We'd got all the test equipment and that stuff shaken out, so they could just grind it out.

STERN: Essentially they got their machines working before the Institute got its machine working.

WARE: I guess that's right. I guess one of them did come on line before the IAS machine. And that would have been after I left, and I don't know why. Why it didn't come roaring first. I really don't. I'm sure that people will point fingers at Julian and claim that he was trying to get the last vacuum tube squeezed out, or some such thing. But that's all conjecture on my part, because I wasn't there.

STERN: Can you give me some information on the input/output that was supposed to be used for the machine initially?

WARE: The original idea, the very original idea was to have been a teletypewriter, as I remember. It was called the "inscriber and outscriber." And they were to come up from the National Bureau of Standards -- and we did some playing around ourselves at Princeton. We built Julian's famous bicycle wheel magnetic wire thing. And I built a gadget that depended upon -- what used to be called teledeltos paper, which is a thermally sensitive paper that you'd run a stylus across and spark it. And it worked great --

STERN: How do you spell that? What kind of paper is that?

WARE: TELEDELTOS. It was a paper -- There was a mechanism that went with this paper that had been developed by Western Union, I think, for printing telegrams. So we got one of these things and decided to see it we could adapt it for a printer or an outscriber or something. And the National Bureau of Standards weren't there quite on time. It wasn't going to go anywhere. It was one of those little excursions - like the dinosaur (?).

STERN: Well, there was a good deal of time spent on the magnetic wire, was there not?

WARE: Yes.

STERN: And that -- what happened to that?

WARE: It was probably OBE (?). If magnetic tape had not come along, magnetic wire was the thing in those days, and it was an ingenious mechanism that Julian conceived, and it worked. Magnetic head development was not in a very high state of progress. We used to get heads out of brush (?) as I remember. But then IBM tooled up and along came magnetic tape, and Eckert & Mauchly tooled up and they had magnetic tape. So the wire was guaranteed to die.

STERN: And even their tape was not the plastic tape? It was a steel tape of some sort?

WARE: Initially that's right.

STERN: But - why "bicycle wheel"?

WARE: Convenient. He wanted this big diameter thing. Julian had a lot of the inventor in him, too. And he wanted some big...How do you get big things? You go buy a bicycle wheel and cut a groove in it. So you don't have to turn

very fast for a high circumferential speed. Engineering expedience is another way to say it.

STERN: There were some documents in the Library of Congress that referred to the use of photographic techniques for outer memory. Do you know anything about that? Eastman Kodak involvement?

WARE: At Princeton?

STERN: Yes.

WARE: No.   I don't remember that at all. No.

STERN: A man named A. W. Tyler? This was early on.

WARE: Tyler? That's all you're going to tell me?

STERN: That's all I know.

WARE: How early on?

STERN: '46.

WARE: At Eastman Kodak?

STERN: This Tyler came from Eastman Kodak to discuss the possibility of using this photographic technique.

WARE: Can't help you.

STERN: Okay. There was also some discussion about Princeton University constructing certain components in its laboratory. Do you know anything about that? I mean, Princeton had a lab. The Institute had a close relationship with Princeton. why not at least initially start to work out of that lab?

WARE: Well, what lab at Princeton?

STERN: The University's own Laboratory -- Physics Laboratory.

WARE: Well, Princeton Physics Laboratory at that time was oriented toward nuclear physics. In fact, Jack Rosenberg used to -- that's where Jack Rosenberg came from. He worked -- I forget the name of the gadget that they built. Jack Rosenberg lives up here in the Pacific Palisades, by the way.

STERN: Really -- I didn't know that.

WARE: I forget the name of the gadget they built, but it was in the basement of Palmer Physics Laboratory, and Jack was one of the engineers on that thing. And then it had a big fire (?).

STERN: Where does Jack work?

WARE: Now? I think he's a private consultant.

STERN: So I can reach him in the Pacific Palisades?

WARE: Unless he's moved from 606 Bienvenida, Pacific Palisades. He should be in the phone book.

STERN: I really would like to talk to him too.

WARE: Yes, he's got a piece of this history. Princeton's laboratory in the Physics thing was oriented toward nuclear physics, experimental physics, instrumentation, etc. I didn't know -- I was not aware of any discussion that they might build some gadgets up there.

STERN: There was initially some discussion but it never seemed to materialize. There was never any -- To your knowledge, did they ever use the Princeton facilities?

WARE: To my knowledge, no.

STERN: When you stop to consider, it took such a long time to get any laboratory facilities at all at the Institute, either Princeton or RCA might have been a good point for using things initially.

WARE: But, let me sketch Julian, though, as a person. When Pom and I first went down, we went down one day to talk to Julian. Pom and I went down together and I imagine we rode the train. We drove back to New York City with Julian in his little old green Austin. By the time Pom and I got back to new York City -- we were still at Hazeltine -- we were just absolutely enthralled with this guy because he just bubbles with ideas all the time. And he was a great guy to do things for himself, and I remember one day walking out the back door of that little brick building, and here's Julian lying under this little Austin, welding a hole in a gas tank. And he said "Nope! It won't explode!" And he had some perfectly reasonable explanation for why it wouldn't explode, based on the principles of physics. So Julian was a guy that kind of like to have it her. And I can see Julian wanting everything tightly under his administrative control as well as under his own intellectual control.

STERN: But what you sketched out for me -- as certain areas where that was a real positive -- but certain areas where that was not so positive.

WARE: But after the fact.

STERN: Of course after the fact. That's what historians do -- is after the fact. did Aydelotte come to see the project very much?

WARE: I never remember seeing Frank Aydelotte in the building.

STERN: Because he was very positive -- at least on paper -- of the entire Institute's staff, he was the one person who seemed to really push for this.

WARE: I never knew that.

STERN: It might have been just because he wanted to be sure that he kept John von Neumann on staff, but he --

WARE: Well, but Frank Aydelotte was a Quaker and a great peacemaker. He really was. And I think he would have smoothed over anything.

STERN: Were there any changes when Oppenheimer took over?

WARE: I wasn't there.

STERN: I thought he took over in '50?

WARE: Oh, I guess that's right. The only changes I know about were that more physicists used to come.

STERN: But Oppenheimer himself did not come all that often? At all?

WARE: I really don't remember seeing him ever there.

STERN: Were you aware of any -- I guess not, from what you're saying -- von Neumann's relationship with Oppenheimer?

WARE: No. Remember, we were a mile or so down the -- we were really but in the boonies, and sheltered down there -- really sheltered down there from the main activity in the main building. Never -- The only person that might have wandered up there very much -- you might ask these questions of Slutz because he might have been more alert to some of those things at the time.

STERN: Now, the meteorological group -- they came to sort out some of their problems on the machine even before it was operational. Is that correct?

WARE: They were there early on, yes. At least some of them were. Charney I think came very -- rather early.

STERN: How about Smagorinsky?

WARE: Joe Smagorinsky? I don't remember when he came.

STERN: And how about some of the other mathematicians like John Todd, Taub.

WARE: Abe Taub. Abe Taub was at Illinois, wasn't he? And he probably came in to spend the summer or something like that. I believe Abe Taub was at the University of Illinois.

STERN: So they were working on their model. But there was work, then, being done on the machine even while it was in its developmental stage?

WARE: I don't know what you mean by the phrase "work done on the machine." People were thinking about how to do the mathematics to get onto the machine when it materialized. But there was no programming being done.

STERN: No programming at that time. What about Metropolis? Did he come down to try to -- not so much in expectation of building the MANIAC, but my understanding is there were some Los Alamos problems that they were thinking of running on the IAS machine.

WARE: Could be. I don't know.

STERN: What about programmers? Who were --

WARE: We never had any when I was there.

STERN: Now, a lot of faculty members' wives did programming work on the machine. Selberg, Sonia Bargman (?)

WARE: These are all names that I've heard. I didn't know the people.

STERN: But they were not essentially working in the machine room when you were there?

WARE: They didn't run.

STERN: Right. Some estimates say that the IAS computer cost a million dollars. would you have any knowledge of that at all?

WARE: No. I have no idea.

STERN: Did Strauss ever come to see the machine -- Lewis Strauss.

WARE: Not to my knowledge.

STERN: Now, there was some discussion in some of the papers about two types of Selectrons. A sandwich type Selectron, versus a quadrant type. Do you know anything about that? A sandwich type would perate at 8 microseconds; the other one at 2 microseconds.

WARE: Those must have been some ideas that were being thrown around when that tube was under development. No, I don't remember that. The 8 microsecond one is the one that materialized. i have one but it's not here. It happens to be at home. there were a few left over here, and we passed them out to people who were interested.

STERN: That's interesting.

WARE: Originally it was supposed to have 4,096 bits per Selectron? It was much less. I don't think it was ever that high. It might have been 1,024.

STERN: And what did it --

WARE: 256. But there's a technical paper out describing the Selectron. I don't know what it's published in --

STERN: IRE, I think. I'll have to get hold of it. I don't have it yet. Pomerene made a statement I thought was interesting. I wonder if you can comment on it. He said: Had Rajchman not had the idea that the Selectron could be built, he thinks that von Neumann would not have chosen to build a parallel machine.

WARE: I don't know whether that's true or not. It's a valid statement, because there were not any parallel memories around at the time.

STERN: What essentially did Panagos do on the project?

WARE: He was a draftsman. Pure and simple.

STERN: And Rubinoff?

WARE: Oh, I forgot Morrie. Pete Panagos and his mother lived up in an apartment on Palmer Square -- made their own guilder (?) -- and purely and simply a draftsman. What did Rubi do?

STERN: While you're thinking I'm going to shut this off.

WARE: He's in Philadelphia. He's on the faculty of the Moore School.

STERN: Either he's retired or it's just a semi-appointment, but he's not usually there.

WARE: He's also running a company.

STERN: Yes.

WARE: Whose name I don't remember.

STERN: I don't know the name of his company, but --

WARE: For the life of me, I don't remember at all what Rubi did.

STERN: I have the dates on the Williams tube here. The work was begun on June l6, l948, and there was a working model by August l3, '48, that Pomerene had development. But it wasn't until July 28 of the following year -- that is, almost l3 months later, that they had a revised working model that worked sufficiently -- was sufficiently immune to noise. Until that point it was not usable, essentially.

WARE: That's '4_?

STERN: '49. July 28, '49. You were still there at that time. I have a few other names -- perhaps you can tell me something about them --

WARE: You're asking me things I haven't had occasion to think about --

STERN: I know. It's hard. It really is hard. Hildebrandt.

WARE: Oh! Theodore W. Hildebrandt, whose father was a famous mathematician at the University of Michigan.

STERN: Yes, there were a few people who had famous fathers that were working on this project.

WARE: Pom and I didn't have them.

STERN: I wonder --

WARE: Ted Hildebrandt -- yes. He came and was a member of the engineering group. But he came later. And I don't remember precisely what he did -- which part of it he did. But he came at a time when we were in the construction phase and he picked up some part of that, but I don't know what.

STERN: How about Bliss? Ames Bliss? Is that his first name? He was the son of a mathematician too. A ballistics person. Originally he was an engineer, and then he became head of procurement.

WARE: A strikeout on that one.

STERN: You don't remember that. Richard Melville -- Dick Melville we said was leader of the technicians, essentially.

WARE: He ran the shop. And I don't know where he came from. He may have come down from Palmer, but I'm not sure of that.

STERN: I have it in one of the things that either Bigelow or Pomerene told me -- one of these people just came out of the Navy and came in and said they were looking for a job because they'd done some work with radar, and Bigelow was taken with him. I think it might have been Melville. And he put him in the shop. I don't know. Leon Harmon.

WARE: Yes. Technician in our place. Then he subsequently went to Bell Labs, and then to Case Institute in Cleveland where, as far as I know, he's a faculty member.

STERN: Case Institute. Good to know.

WARE: That's not what it's called any longer. It's got a different name.

STERN: Western Reserve.

WARE: Case Western Reserve. Yes.

STERN: Hewitt Crane?

WARE: Hew Crane. He's now at SRI. But I think he was a technician with us. But I'm not sure of that. I don't know what else he would be. See, some of these people may have come after I left -- for short periods of time -- and Crane may have been one of those people who came after I left.

STERN: I think he was there before, but he might well have been a technician, because the list I'm trying to

accumulate are those people that were there prior to '5l. So we're talking about the same period. Phil Duncan worked

with Charney on a meteorological group -- No engineering relationship. Is that correct? And what did Rosenberg.

What was his -- what did he work on? Jack Rosenberg.

WARE: Jack came at a time when we were starting to assemble what we called. I guess we called it the mainframe. It

was the structure that held all of the ... electronic equipment; the ventilation, the distribution of power. He picked up

that part of it.

STERN: He also had a Ph.D. in physics, didn't he?

WARE: No.

STERN: Well, he was at ---

WARE: He was at Palmer. But as a technician or as an engineer building their gadget. As far as I know, Jack's a B.A.

in engineering.

STERN: There's been a lot of people writing about von Neumann's love of power. I don't know if you've read any of

the ... talks about that in his obituary, and there's a new book that's out, the Heims book on Wiener and von

Neumann. They talk about this. Is that your sense of the man?

WARE: No.

STERN: Not at all?

WARE: No. In fact, I would have put it just the other way around. I would have said he was a very self-effacing

person.

STERN: Did you have any dealings with von Neumann after you left the Institute?

WARE: Some, because Johnny was a consultant to Rand and I would see him out here. I saw him out here maybe once or twice or three times. But just casually, Hallway conversations. I did not see him at all after he went to Washington. Julian did. Julian did see him when Johnny was in Washington, and had been diagnosed with his cancer problem.

STERN: Julian said he went to visit him many times at the hospital. Another criticism of Bigelow that I saw in writing was that he was too eager to make dramatic changes when minor ones would do. Do you think that's a fair evaluation?

WARE: I don't know.

STERN: You don't have a sense of that?

WARE: No. It's not a remark I would have made of Julian.

STERN: Now, were there any journals at this time that had much in the way of computing information?

WARE: Well, I'll tell you the ones that got started -- whenever the dates were. Ed Berkeley's Edmund C. Berkeley's newsletter kind of thing.

STERN: He now has *Computers and Automation*.

WARE: That's the new title. I'm not sure -- the IRE got -- what was that group called? Electronic Computers, or something; that got started. Well --let me -- there was a bundle of things, but I don't remember the timing of it. The

AIEE and the IRE got these national joint computer conferences going, and then the ACM came along and ..... If I just look up on that top shelf -- that says '52. *Proceedings of the Eastern Joint Computer Conference* it says -- if I'm reading it right -- it says February '52.

STERN: ACM was in '47. Now, did you read ACM journals when you were at the Institute?

WARE: Probably didn't even know it existed.

STERN: I have the sense that there really was almost no contact.

WARE: Well, the ACM thing was started - that was kind of the academes and the mathematics departments on campuses.

STERN: Did you have much relationship with the National Bureau of Standards? Did they come down much?

WARE: I would see Sam Alexander from time to time on a visit at Princeton.

STERN: He would come to Princeton?

WARE: That's the way I remember it, yes. I don't recall ever visiting the National Bureau of Standards in Washington during this period.

STERN: What would you say von Neumann's most significant contributions in computing were?

WARE: Well, he had the insight to get the funding together to get a machine going. Whatever you want to call that -- leadership -- something. He was in the right place at the right time with the right connections with the right idea. I'm setting aside the hassle that will probably never be resolved as to who's ideas they really were. In other words,

Eckert or Mauchly, or Johnny, or Metropolis or all of them. I don't know that it's necessary to ever resolve that, and in fact I find myself distressed when I hear Pres Eckert making very strong statements from time to time in public. I think it's not necessary for him to do that.

STERN: Like the Los Alamos book you're talking about -- that kind of thing?

WARE: Yes.

STERN: What about Turing's influence on von Neumann? Do you know anything about that?

WARE: You know somebody asked me a while -- somebody asked me several months ago. Did von Neumann and Turing ever meet? And I attempted to find that out and to the best of my knowledge, they never met.

STERN: No, no. They *did* meet.

WARE: They did meet?

STERN: Yes. In fact, they met in '38 -- in England. And von Neumann asked Turing to come back as his assistant, and Turing said, "I'm nobody's assistant" or something like that. But, they did meet.

WARE: But before the war?

STERN: Yes.

WARE: I didn't know that.

STERN: I don't think they met after that again.

WARE: Nobody that I could ask knew.

STERN: It's just, you know -- compounding this problem of the stored program concept is the influence that Turing had on von Neumann, and I thought maybe he had spoken at some gathering about Turing.

WARE: Not to my knowledge.

STERN: There was a man named R. D. Huntoon.

WARE: Oh, Huntoon was -- I don't know what he was at the time, but he was probably Sam Alexander's boss, but I'm not sure of that.

STERN: He was at the Bureau, then?

WARE: Yes.

STERN: And did he come down, or did he have any relationship with the -- I thought he worked on the I/O, or had some responsibility for it.

WARE: Could be, but I don't know.

STERN: Now, Bigelow was a consultant to Rand at some point. Prior to the   ??? during the development of the JOHNNIAC.

WARE: That's possible, but I didn't know it.

STERN: You were not a consultant to any of these projects?

WARE: Correct. Oh, I had this little side arrangement to sell them pictures!

STERN: And are you - I read in some document, and nobody has any knowledge of this -- that IBM made a grant, or wanted to make a grant to the Institute.

WARE: Cuthbert might know that. But I don't know.

STERN: You left before there was any discussion about making the project people permanent members of the Institute?

WARE: It was very clear while I was there that we were employees of the Institute. In fact, I think -- well, there was a standing rule -- one of the reasons it was clear we wee never going to be anything but employees -- first, we were in the wrong rackets, but secondly we didn't have Ph.D.`s. And I believe it to be true, that Julian is the only one that's become a permanent member of the Institute without a Ph.D.

STERN: Isn't that surprising that they would appoint him permanently? How did that happen?

WARE: I have no idea. I have no idea. It must have been a deal that he cut somewhere. I have a vague recollection of having been told that he might have gotten an honorary degree someplace, but I'm not at all sure of that.

STERN: And even that, I'm sure, wouldn't have counted.

WARE: It probably would not have.

STERN: Goldstine also was made a permanent member. I think they were the only two.

WARE: I don't know. Could be, but I don't remember hearing it. Herman's back there. Sort of coasting out on his IBM Fellow. And I don't know where Julian is. I guess he's still in Princeton.

STERN: Yes. That's where I interviewed him. You told me Acrivoe was a secretary? Wasn't there something about her doing accounting work. Or was there a Luella Trintero (?).

WARE: I don't know that Luella person.

STERN: But Acrivoe was a secretary?

WARE: As far as I know, she was the project secretary.

STERN: Okay. And you mentioned -- she's local?

WARE: She lives with her husband whose name I can't remember. It's a big long Greek name -- in Long Beach.

STERN: You said Kondoppias?

WARE: Her name was K O N D R O P I A.

STERN: Because I find from experience -- secretaries are good people to speak to. They often remember a lot of details.

WARE: Boy - I don't know how in the world I'll ever get a reference for you so you can track her down. I might have it at home.

STERN: With a strange name like that, I may look it up in the --

WARE: But that's not her married name. Unfortunately.

STERN: What would you say your most important contributions to the project were?

WARE: I don't know. I haven't any idea.

STERN: Circuit design?

WARE: I don't know what to tell you. I really don't.

STERN: While you were working on the project there, did you feel that you really wanted to work on computers -- make that your career, or --?

WARE: No.

STERN: You didn't feel that way?

WARE: No. The awareness wasn't there at the time. It was another job.

STERN: Do you think there's anything I've left out, that I haven't discussed relative to the IAS period?

WARE: Nothing pops into mind, Nancy. It really doesn't.

STERN: Okay. Is there anything you can tell me about Rand's JOHNNIAC -- just in a general kind of sense?

WARE: Well, let me just make some remarks for perspective. At that time there was a pretty good schism between

East and West Coast, and there was a lot of action on the West Coast that the East Coast establishment either didn't

know about or didn't pay much attention to. And the whole business of describing machines by ..... algebra and

using that as the basis for doing logical design and implementing those in diode networks -- as far as I could tell --

pretty much sprang up here in Southern California. So there was a -- and then the MADDIDA - the digital differential

analyzer business sprung up here in Southern California. And some of the earliest work on what we would now call

an operating system got started here in Southern California. But that was going on here, and it wasn't very widely

known back there. There was the Princeton action, and then later there was the Whirlwind action centered around

MIT and there was NBS action. But this was kind of another piece of the business that just never got much

attention. In fact, it was a long time before anybody from the West Coast even attained prominence in the

professional societies. There were some early professional society activities out here that were as early as -- I think,

as early as anything that got started back there. And Rand was kind of part and parcel of all that. We started -- Rand

started business with the 604 -- CPCs, and then 604s, and at one time we had, I don't know, two dozen of them or

something, banging out cards in the basement of an old building that no longer exists. And Rand was fortunate in

having a fellow here by the name of John D. Williams who was an astronomer by training, but he was like Johnny in a

sense, because he was very imaginative and forward-looking. So when he got -- and I have no idea how he came to

know about the Princeton work, although John Williams during the war had been associated with - was it called the

National Research Council? -- NDRC -- National Defense Research Committee. John Williams had been associated

with it, so he was probably traveling in the right circles to have heard about the Princeton work. So he got Rand into

that action right away. So we built that machine, and then we got into the 70l, and we've progressed ever since.

STERN: Did Gunning have anything to do with this?

WARE: Bill Gunning -- Well, prior to our involvement in the digital business, Rand was deep into the analog

business and had bought commercially a machine called the REAC - Reeves Electronic Analogue Computer -- and

made an enormous number of significant improvements to it, which Reeves then took over and marketed

commercially without any credit. And Gunning had come here from UCLA as an engineer to work on the .... Then when we got into this digital business, Bill moved over and started the whole JOHNNIAC project. I guess there's some probability that he visited Princeton, but I certainly don't remember it.

STERN: Could it be this bifurcation you were talking about and this lack of attention to some of the developments -- could it have anything to do with the fact that the work being done in the east was being done by essentially academic institutions, and a lot of the work out here was being done by commercial organizations?

WARE: That's possible. But people weren't traveling a lot in those days either. It was a big thing to get on an airplane. Rand was one of the early -- as far as the West Coast was concerned -- was one of the early pioneers and Northrup would have been another one. North American Aviation was a laggard. They weren't in there early. Douglas was pretty early in the business.

STERN: When was the Rand computer completed? Do you recall?

WARE: Well, let's see -- I came in '52. Probably '53 or '54. Those dates -- I can get them for you if you want.

STERN: There are lots of charts that have dates and things like hat. Okay. Anything else that I did not mention that I should have?

WARE: No. I guess -- but you've asked a lot of questions which cause me to make the following remark. Remember the context it appeared in. People weren't turned on to computing and it wasn't a widely disseminated thing, and the words weren't hardly around; to the extent that there existed a mind set on computing. It was all very much oriented toward analogue. The old shaft-turning digital-differential analyzers, and there were only ever what? -- three of them. And one of them was at MIT, so there was a kind of a little hotbed of action. And I think that most of us that were down there doing that just were -- now maybe Bigelow and von Neumann -- I won't speak for them -- but certainly for myself, and I suspect Pom and most of the other engineering group -- we were doing a job, and it was an interesting

kind of a job. But we didn't have the big foresight and the big omniscience to see all the consequences.

STERN: I get a very different impression when I talk to Eckert and Mauchly people -- the engineers, who were in similar positions. I get a sense of dynamics and real loyalty and real excitement, and working all hours and Saturdays and Sundays. I don't get that sense in the Institute project.

WARE: I guess that's right, now that I think back on it. Oh, sure, we put in some overtime. We'd do an occasional Saturday and Sunday. But we weren't busting our butts to be in there and to make deadlines. But now, remember that Eckert and Mauchly had a contract to deliver a machine.

STERN: Well, but in a sense, so did the Institute. I mean you were still working on the government contracts here. I don't think it was as much of a --

WARE: Now wait, Nancy. I think there's a difference. Eckert and Mauchly had a contract to build a machine. Princeton had a contract to do R D which would eventuate the machine.

STERN: You think it's as simple as that.

WARE: The drive was just different.

STERN: You don't think it had to do -- my sense of it is it really had a lot to do with administrators -- the people running it.

WARE: I'm sure that either project could have been quite different with different administrators. I'm sure that's right. Now, if Eckert, for example, had been the Number l at Princeton, I think the project probably would have been a lot more dynamic. But he would have brought with him all of his experience out of ENIAC, and all of the insights that he had on how to make 20,000 vacuum tubes perform.

STERN: That's not to say that he was the easiest man to get along with by any stretch of the imagination.

WARE: Oh, no. Not at all. But it probably would have been a lot more dynamic. Eckert -- I didn't know Mauchly very well, but I knew Eckert very well. Eckert and Julian are just two very different people.

STERN: I understand what you're saying from your vantage point -- how really this was in analogue computers. But there was work going on. It's just the engineers were not all that familiar with the other work that was going on at that time.

WARE: Like the stuff at Bell Labs and Stiblitz?

STERN: And MIT and Raytheon, and Eckert and Mauchly, and places like that.

WARE: Well we knew some --

STERN: And ERA --

WARE: Well, the ERA scene wasn't very visible. We had some awareness of the Eckert and Mauchly stuff because it would get talked about, but not a great deal, and certainly we did not visit back and forth.

STERN: And in some sense, that not visiting, I think, was von Neumann's feeling that - really, commercial computer development was something very different from academic development.

WARE: It was a different world. And - it's hard to sit in today's world and appreciate its interaction and its dynamism and all that, and think back and realize that that was a very different world. The big drive -- first, it was right after the war, and so there was a lot of attention on trying to get the country back to other things. And the big

drive around RCA was AM/FM and TV. That was Jan Rajchman's goofy little idea! I suppose, thinking back now --

maybe von Neumann's big contribution -- and maybe this is true of Rajchman, too -- they were visionaries.

STERN: I think that's a good assessment.

WARE: I don't remember what Jan Rajchman did before he got mixed up in the Selectron business, so I really don't

know why he even got fooling around with the Selection. Oh! I'll bet I do. RCA had a contract. There were three

contracts, all named after storms, and RCA had one and it was called "Typhoon." Raytheon had one called

"Hurricane," as I remember. And somebody else had one --

STERN: Zephyr.

WARE: And they were all supposed to do 6^x (?) 6 degree of freedom simulation of flight for the Navy. And the

Raytheon thing was digital. The RCA thing was kind of a blend. It was kind of partly digital and partly analogue.

And I suppose that maybe as a result of that, Rajchman got interested in computers or computing kinds of things.

STERN: There was also a contract for the NDRC on electronic counters, that Rajchman had worked on. It never

materialized, but he had done some work on electronic digital counters. And a function table was something that the

ENIAC used from RCA's work. So I think he had demonstrated an interest in computing even before this point.

WARE: Either I never knew or I've forgotten how Jan Rajchman got into that action.

STERN: Well, I think calling them visionaries is just what they seem to have been.

WARE: Well, they had a little vector started that turned out subsequently to be very important vectors.

STERN: Terrific. Anything else?

WARE: I'll tell you one final story. Jan Rajchman's sidekick in the Selectron was a fellow named Milt Rosenberg, who came out here and went to work for International Telemeter and built the first 4,096 x 40 bit magnetic core store for us here at Rand. But Milt tells the story of all the trouble they had with the glass blowers back at RCA while building the Selectron. And one day the glass blower in a fit of pique said to Milt - he said, "I can blow any tube that you can design." And Milt said "How 'bout a Fallopian tube?"

STERN: No answer to that, huh? Terrific. Thank you very much.

WARE: You're welcome.

END OF INTERVIEW