An Interview with

ARNOLD A. COHEN

OH 138

Conducted by Arthur L. Norberg

2 July 1987

Minneapolis, MN

Charles Babbage Institute
Center for the History of Information Processing
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Abstract

The interview focuses on Engineering Research Associates (ERA). Cohen begins the interview by briefly describing most of the early ERA personnel. He discusses his own work and that of C. B. Tompkins on various ERA projects including the Goldberg Project and the design of memory systems.

ARNOLD A. COHEN INTERVIEW

DATE: 2 July 1987 INTERVIEWER: Arthur L. Norberg

LOCATION: Charles Babbage Institute (Minneapolis, MN)

NORBERG: Arnie, I have in front of me, and I'll pass it over to you, a document that was prepared by U.S. Navy Personnel at the request of Admiral Wenger, in August of 1946, which describes from their point of view the conditions surrounding the founding of ERA and the purposes of the contracts between Northwestern Aeronautical at first, and now in this month of August 1946 with ERA. In this document there is a list of names, which says that these persons were all Naval Reserve personnel, formerly engaged in communications intelligence work under CNO, with the ranks indicated, and they were all released from active duty in the Navy before entering the employment of this company. Several of the names are obvious. We all know William Norris and we know about C.B. Tompkins. We know a little about John Howard. And I've interviewed Hugh Duncan and Joe Walsh, so I know something about them. But many of the names, I don't know where they came from, whether they were young men who had just entered the Navy at the time, and had become involved with communications intelligence, and if not, where they were before. I have no idea what their backgrounds are, whether they are engineering graduates, or some other graduate, or not a graduate at all. I wonder if we could run down the list, and you can tell me what you know about each of them. We can skip the ones we know about, of course. Why don't we go to Steinhardt? What did you know about him?

COHEN: Lawrence R. Steinhardt. He was part of the group at MIT before World War II. He went into the Navy right out of MIT.

NORBERG: Now, when he was at MIT, he was working on the Bush project of the rapid arithmetical machine.

COHEN: Probably as a graduate student. All right. Why don't I tie his name to a few others? John Coombs was at MIT, along with John Howard and Larry Steinhardt. Perhaps that's all of them. This was in the Graduate School there, because I do know that John Coombs got his bachelor's degree at the University of Maine. Whether they all worked on the Bush machine, I don't know.

NORBERG: Coombs is the unknown here. The other two did.

COHEN: But Coombs knew... He probably worked with Bush also.

NORBERG: And you said that...

COHEN: Another man who was around there at the time was... information theory.

NORBERG: Claude Shannon.

COHEN: Claude Shannon, yes. As a matter of fact, it was Coombs who introduced me to Shannon. They'd known each other at MIT.

NORBERG: Do you whether they had any interaction with Perry Crawford?

COHEN: That was the other name that I was going to mention. Yes, they all knew... Well, they must have know Perry Crawford very well. Coombs certainly did, and I am sure the others did. I'm quite sure that John Howard did. Another contemporary of that MIT group was Sam Alexander, who went to NBS.

NORBERG: Now, you mentioned that Coombs came from Maine?

COHEN: Right.

NORBERG: Do you know where Steinhardt came from?

COHEN: No.

NORBERG: And how about Howard?

COHEN: I couldn't say for sure. I might think of it... There might be some connecting thing that will trigger my memory on it.

NORBERG: Now when you came to ERA, what was Steinhardt doing then?

COHEN: The fellows in this... not all of these down here, but certain ones. I think you've got them. This is interesting, because down to Joe Walsh on this listing... This is sort of a... must have been taken off of an organization chart (of ERA), because here you find Norris... Well, they're not stacked quite in that way. Norris was a vice president at the time I came. Steinhardt, Olofson, Duncan, Noble. Those people seemed to occupy a level known as engineering supervisors, and Howard... No, not necessarily. Howard, I think, had an initial title (you may have other documentation on this) Director of Development, perhaps. And Tompkins was the Director of Research. And I think somebody created a third title, I think, Director of the Laboratory. I don't recall how that went. Okay. Your particular question?

NORBERG: ... was about Steinhardt. What exactly was he doing when you came? Do you remember?

COHEN: Steinhardt. He was probably an Engineering Supervisor and probably was responsible for one or two of the tasks and projects. You see, by the time I got there, that Northwestern Aeronautical contract... Incidentally, I don't think you spoke it right.

NORBERG: I thought of that as I was saying it.

COHEN: Yes. It is the Northwestern Aeronautical thing that must have dated from about August 1 of 1946 and then the ERA turnover...

NORBERG: ... turnover was the next year of 1947. Yes. That's why I hesitated.

COHEN: Right. So, at the time that I showed up, they had the ONR contract.

NORBERG: Yes. 240.

COHEN: The N6onr-240 and that had just been activated. It couldn't have been long before that.

NORBERG: That's correct.

COHEN: Yes. I came early in December. But the other thing had started out in August and quite a number of these tasks were already underway. The reason that some of them were well underway is that they were continuations of the work that had begun at NCR.

NORBERG: Okay, then, continuing down the list. Boenning - what was Boenning doing? That is, where did he come from? W. E. Boenning, is that right?

COHEN: Well, there is Bill Boenning, but there is also a George Roning in here somewhere.

NORBERG: Well, Roning is the one I'm looking at. The third name on the list. W. R. Roning.

COHEN: This is probably intended to be...

NORBERG: George?

COHEN: Let's see. If it is a "W", it's Bill. Bill Boenning, who showed up at our reunion.

NORBERG: Okay, you think it is a "B" rather than...?

COHEN: Yes, and that would be William R. Boenning.

NORBERG: Okay. Now do you know anything about him?

COHEN: More about his subsequent...

NORBERG: No, I'm interested...

COHEN: Yes, because he went very early in the game, perhaps 1947, late 1947, to join NSA, or whatever it was known as at that time. And he was there until retirement. Rose into middle management of that agency.

NORBERG: Okay, but do you don't know anything before he came here?

COHEN: An engineer. Yes. Definitely.

NORBERG: Electrical?

COHEN: Probably.

NORBERG: How about Chaloux?

COHEN: Lou Chaloux... Let me try to dredge that one out. He had previous experience. He was from New York State somewhere, I guess, and had been with one of the business machine companies. Recordak. Was that a subsidiary of

Eastman? Yes. He had been with them. If he had an engineering degree, it was probably in mechanical. I remember some of his stories about his calling on various customer companies and things like that. Then he must have... Let's see, did he...? He also had some IBM experience, but I'm not sure of that.

NORBERG: Did he come from Washington or from Dayton?

COHEN: From Washington; yes. And then he... You probably know the subsequent history. I believe he was one of the group that was hired away by Remington Rand. And he was always in kind of an upper-level "assistant to" type of position. And at the time that he died (at his desk, incidentally), he was the manager, or engineering manager... He was the top technical management guy.

NORBERG: At Norwalk.

COHEN: No. At Philadelphia. He had moved there and was a pretty pleasant, popular guy, and knew his way around. I don't think he stayed in St. Paul very long, because he became the principal contact man at the agency. And whenever we, those of us from St Paul, were out on a visit, he would be our escort. He was always very good about Navy protocol. Before he brought us around to people we were really supposed to see, he would take us to see a few of the top brass, have a cup of coffee, talk a little bit, get acquainted. He always wanted to make sure that the brass knew the technical people from St. Paul. A very smooth, very nice guy.

NORBERG: Okay. Olofson - What's the "E. C." in Olofson's name?

COHEN: Cliff Olofson.

NORBERG: Okay, so it's E. Clifford?

COHEN: E. Clifford Olofson. I don't know what the "E" was. Cliff Olofson - mechanical engineer. At the time I came, he was sort of at the engineering supervisor level. He had in his purview several electro-mechanical projects - printers, counters, electromechanical. And putting together the survey for ONR, Tommy leaned on him for state-of-the-art information in that area - relays, punches, readers.

NORBERG: You don't know where he came from before the Navy?

COHEN: No. New Jersey, perhaps, someplace. I don't know what school he went to.

NORBERG: How about Ammerman?

COHEN: Don Ammerman is now living in Scottsdale, or Phoenix. Electrical engineer. He was one of the boys who went to Remington Rand in that exodus operation, and stayed with the company for some time. I don't know if he stayed through retirement, or whether he was at some other place afterward. But he and Patterson see each other frequently, since he's in the same area.

NORBERG: Is that Bob Patterson you're talking about?

COHEN: Yes. When I came, incidentally, Ammerman was working with Sid Rubens. It was one of the Goldberg subtasks. They were pulsing various metal rods, alloys, and things like that for possible use as solid acoustic delay lines.

NORBERG: Okay, Dave Noble.

COHEN: Dave Noble. He apparently retired out of IBM, San Jose area. Went to IBM very early. I don't know if he went for a time with the rest of the gang to Remington Rand and then went to IBM. That's a possibility. But where did he come from originally? I don't know. It could be from the east somewhere - New England, New York State... an electrical engineer.

NORBERG: Are there any family connections here that you know of?

COHEN: No. I'm quite certain that there are not. He was a very serious kind of a guy, and very hard to loosen up; very polite. I noticed that when he wrote me a letter, it was Dear Mr. Cohen, that sort of thing.

NORBERG: Moe. W. J. Moe.

COHEN: Walter J. Moe. Yes, he's around town. He's originally from here. University of Minnesota graduate; electrical engineering background and communications. When I came, he was on one of the communications oriented projects.

NORBERG: Communications oriented...?

COHEN: Projects. Communications, as opposed to something else, whatever it might be. But...

NORBERG: Would these be on intercept problems, or communication between machines?

COHEN: No. Frequency shift keying, and all of that sort of thing. I don't know how many different things. But Walter Moe is in the local area, and the last few times I've seen him in recent years, he's having fun on the commodities market. I don't know how successful, but apparently successful enough, because he got to be on somebody's prospect list.

NORBERG: Okay, H. G. Nilles.

COHEN: Yes, Herb Nilles. I don't know too much about him. I remember him; I used to chat with him. A mechanical engineer or a mechanical technician. Let's see. The way the Navy operated, I guess if you were an Ensign it means that you probably had a college degree, so he probably had a mechanical engineering background.

NORBERG: That would suggest, then, the first two-thirds of this list are likely to be college graduates.

COHEN: Oh, yes; very definitely.

NORBERG: What did he do for ERA?

COHEN: I don't recall specifically. Incidentally, going down this list with Patterson would be very productive, because Pat knew these people both before and during the ERA period.

NORBERG: Okay, that's a good possibility. Okay, who's the next guy? L. W. Goss?

COHEN: L. W. Goss.

NORBERG: He doesn't even ring a bell with me.

COHEN: No. I don't remember that name. Now you're getting into enlisted grades that are shown here.

NORBERG: Yes.

COHEN: I couldn't be sure of Goss as somebody who came out here. The next one is G. Ward Lund, a little older than the rest of us. He came in as a technician. What he did. Let's see, you're primarily interested in where he came from. Well, geographically, I guess, he must have come from the Seattle area, because he went back there on retirement. He stayed with Univac through about 1968, 1969, or so.

NORBERG: Was he possibly an acquaintance of Rubens?

COHEN: Yes, in the same way that we all were, as a matter of fact. I worked with a few of them.

NORBERG: Well, when you say, coming from Seattle, the first thing that comes to mind is a possible acquaintanceship with Rubens at the...

COHEN: Oh, you mean in that geographical area they knew each other?

NORBERG: ... University of Washington.

COHEN: No, because I don't recall that Ward Lund had any connection with the University of Washington. If you look over my other interview you'll see him. I think we put his name in there. When we took on that... What was it? A tape-to-card converter for General Groves. He was the project engineer assigned to that one. Einfelt, I guess you'd call him a mechanical technician, primarily. Machinist's Mate, First Class. Oh, let's see, if you go back to these. Chief Radio Technician A (whatever A is). And Nilles, what would that be? Chief radio mechanic?

NORBERG: Mechanical; maybe Chief Radio Mate.

COHEN: Yes. Something like that. Bob Einfelt was more on the practical side of things. Those fellows probably moved around a lot, probably out in the field.

NORBERG: Yes. Well, so did Joe Walsh, for that matter. He may have had a number of these people with him. But do you know whether they came from Dayton, or whether they came from Washington?

COHEN: As far as I know, Art Kotz came from Dayton. He jokes around that he came with the equipment that was shipped out here.

NORBERG: Yes.

COHEN: Have you talked with him?

NORBERG: Incidentally. I haven't interviewed him.

COHEN: He was a technician, and then got into other kinds of work in later years. But if he retired, it's only very recently.

NORBERG: Yes, he told me that at one of the Unihog meetings. He definitely came from Dayton, but I don't recognize any of the others as having come from Dayton.

COHEN: He might be the only one. You know that list that somebody gave us, Nichols or somebody, of people who were thought to be the first, early ones there. I talked with Art Kotz before we put that list into the book.

NORBERG: Right.

COHEN: And he gave me a few names to add on. But I don't think any of them came from Dayton. You know, people who might be overlooked at the official levels. So some of those might be on this list here.

NORBERG: Okay. Who was Grogan?

COHEN: W. L. Grogan. If I'm thinking of the right guy... One of the first projects I was on, recording on a drum for altering selectively,and NRZ, and all that stuff. He came through from somewhere. He just had gotten out of the service. And somebody brought him in. Joe Walsh used to bring people in. There was kind of an underground railroad through the company in those days. It was one of the strangest things. It took me awhile to figure out what the heck was going on. I never did. But I think he was the guy that they brought in one day and said, "Here, can you use a technician? Put him to work." I don't know that he knew an awful lot about anything. He stayed around there a few days, and then he went on to points unknown.

NORBERG: But this document is dated in August of 1946, which is before you came.

COHEN: Oh, then maybe it's a different guy. It may not be Grogan. I don't remember the name of that guy, but I remember the incident.

NORBERG: Yes. Well, did that sort of thing happen frequently? You are suggesting it happened frequently.

COHEN: Well, in various ways. I've told you the story a couple of times where Joe Walsh would bring in a guy and introduce him as a consultant. Well, he was just somebody's pal. One of them was Donald Menzel. Another one was the canasta guy. Jacoby. Oswald Jacoby. But man, this network of people that were in this operation during the war... And apparently... I think there was a stockholder's list. I think some of these fellows had original stock, too.

NORBERG: Menzel did. I don't know about Jacoby.

COHEN: Yes. I suppose by way of keeping them loosely coupled, just in case they could be of some use, why they'd throw a little so-called "consulting" in their direction from time to time, until the company got to flying right side up, you know, the first year.

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NORBERG: Okay, who is next on the list?

COHEN: Art Kotz, we talked about him. John Stallard. John Stallard, and the next man, Vandal... And there's another man... another name that I don't see here that goes with Vandal. Bud Kilham, doesn't show up here. Vandal and

Kilham were always passed as a pair, together. But they, and then later on, Stollard were in charge of instruments, the maintenance of them and so forth. They were the experts on instrumentation. So they may have done that kind of thing in the service. Harry Zimmerman. Zimmerman and Ammerman were kind of cast together. Zimmerman, I think, was... Was he primarily mechanically-oriented? He also went to Remington Rand. What was it you were looking for, in particular, about...?

NORBERG: Where did he come from? What do you know about his past?

COHEN: I don't know anything about his past. W. P. Horton. I believe he was working for Sid Rubens when I showed up, probably as a technician, probably classified as an engineer. The next one is Robbie, Tom Robinson, who, I guess, died in recent years. He was an electrical technician, a very good one. I guess he was out in the field in installation. That's the end of the list.

NORBERG: Yes, that's the end of the list.

COHEN: Which is not very complete.

NORBERG: These are people known to be with the company in August of 1946. I mean, if there were other Navy people...

COHEN: Engstrom was mentioned above here.

NORBERG: Yes, but I know about Engstrom. That's why I didn't ask about him.

COHEN: "Engstrom expects to be released." He came later. I see.

NORBERG: That's a little hazy, actually, as to just when he was working for the company, and when not. It's very difficult to sort all that out, because it appears as though people like Engstrom and Norris and Meader had not been relieved from duty when the company was running in the early part of 1946, and yet they were doing things for the company. So it's a hazy period. I don't think anything can be made of that. So we need not worry about that sort of thing now. Arnie, can I switch to a different topic?

COHEN: If you want to pause for just a moment before we get away from this. Let me see if any of these things [in the document] inspire any further comment. [pause]

NORBERG: I looked at your interview with Jim Ross. Can you review for me again what you were doing at RCA during the war?

COHEN: I was in tube development, in a section that dealt with gaseous discharge tubes, thyratrons, rectifiers. One of the more classified things was a hydrogen thyratron modulator. Strictly development. Well, there were some things that were a little researchy on cathodes, things of that nature. If what you're getting at is to see what connection there was to computers, zero.

NORBERG: Well, it wasn't so much what connection there might have been with computers, but how did that work carry over into what you did for ERA in early 1947.

COHEN: One of the things that interested Tommy was the tube development experience, because he had this idea - probably his own - that you should be able to store information on a beam of - I think he was thinking in terms of electrons, but if you generalize it, charged particles. So his thought was that one of the facilities that ERA ought to have is a tube development lab. And I was prepared to jump in several different ways. If we went that way, be prepared to set something like that up. So that was one of the thoughts.

NORBERG: Do you remember ever having any conversations with Tompkins (and I presume it would be Tompkins, but it could have been Engstrom too) about the standard means known at the time for storing information? Did you people ever discuss the range of physical possibilities? Now, I don't mean a drum, particularly, but what sort of theoretical background there is to decide where you should put your energies in research.

COHEN: I have a classic document. I'm sure I've told you this story. I don't know if I put it in my interview, but... Yes, to answer your question, yes, we discussed these things in a very wide-ranging way - what the properties of various things would be. Of course, the interest in acoustic delay lines going beyond mercury. The work on moving target indicators that was done toward the end of the war. And Ehrenberg's work under Lindsay at Brown was of interest. That was sponsored by ONR. Quartz multiple reflection delay line. Sid Rubens was trying out various alloys. Of course...

NORBERG: The thing you just mentioned in a connection with Ammerman?

COHEN: Yes, propagation... You need real long lines, you know. But apparently things like that were in use. Sid might be able to help us on that, experimental delay lines, again for this moving target indicator. For non-alterable storage, of course, starting with punched paper tape; there was photographic film, 70 millimeter film, I guess even punched 70 millimeter film. And that was primarily for applications where you do a lot of things to one long stream of data, comparing two such streams by offset. A particular story that I have reference to: Joe Walsh brought in Donald Menzel and introduced him one morning. George Hardenbergh and I were sitting in the lab. It was during the time we were working on selective alteration non-return-to-zero. "What are you interested in?" "Well, storage of information." "Okay, how are things like that done, and what might be done?" You know, the question you're asking. So I told him there was obviously a range of physical phenomena that might have possibilities. And we talked about that awhile. The following week Tompkins came into the office. Do you recall the story? Did I tell you this one?

NORBERG: Keep going.

COHEN: Tompkins came into the office with a typed-up sheet of paper. He said, "I ran into Donald Menzel at some meeting, Denver or someplace." And he said, "Here's something that Cohen wanted." One piece of paper. And here this very poetic thing about the wine tasters of old and all of this sort of thing, all of the various physical phenomena that represent different states that might be applicable to storage of information. He wrote this little essay. Apparently he was doing consulting for me and I didn't know it.

NORBERG: But was there anything in that document that you hadn't said to him, beside the wine tasters of old, perhaps?

COHEN: No, not much. But his recommendation, of course, as a consultant, was that we carefully investigate all the possibilities.

NORBERG: Oh, of course, a consultant would do that. But in your conversations with Tompkins and Engstrom, how did these ideas emerge? Was it the sort of thing where - let me just speculate here, I don't know - where Tompkins would come in and say, "Well look, if we examine all the phenomena we find the following. There are certain things that can happen in electromagnetics. There are certain things that can happen in acoustics. There are certain things that can happen in whatever." Was that the tenor of the conversation, or did he come in and say, "Well, others are doing this, and so and so is doing that, and why don't we do this?"

COHEN: I would guess the conversations of the former type probably were well under way before I arrived on the scene, because they had this Goldberg project. The most promising thing appeared to be to put tapes on a drum, to take care of that precession-of-data problem. But they were continuing as side investigations the matter of acoustic properties of alloys. Rubens was working on that stuff in several places. I think this business of 70 millimeter film may have had its origin on that same project, the Goldberg thing. I'm not sure. That kind of thing applied to a lot of different problems that they were concerned with. But Tommy was certainly aware of the general interest in the field. He had knowledge of a great deal of what was going on, particularly under Navy sponsorship here and there, ONR, or the predecessor to ONR, ORI. And one of his thoughts was that you should be able to, in effect, double the density of information if you used the telegraphic technique of what he called "non-return-to-zero", which is sometimes called pulse envelope and various other names, because technically, you should be able to get twice the information in the same band width by doing it that way. Then there is a trade-off there, you see. You have to supply other means of timing, supplying the clock for that information, because if there are just a series of ones, for example, you just get one long envelope, so the timing has to be preserved some other way.

NORBERG: Yes. Now, does that mean a separate circuit altogether?

COHEN: Well, in the case of a drum application where if you have a number of tracks carrying information, one, that later got to be called a sprocket track, by I think our patent attorney, would synchronize a whole series of information tracks. So that was no problem. As things became more sophisticated, later in tape technology, many other kinds of coding of magnetic information on, say, magnetic tapes [were available]... IBM had one or more of them, NRZI, change on ones, many variations on that. As long as your circuitry can decode what it was that was intended. Suppose you have a string of ones, but change on ones, then it will all be changes. Another one had to do with, oh, I guess, if you have a parity track, somehow a combination of the information tracks and a parity, if you do it right, will always have some timing information on it, an implicit timing track. At that point, nothing was that sophisticated. But Tommy wanted to pursue the idea of non-return-to-zero, so he got a task assigned under the ONR contract to investigate that, and I was assigned to that. Since we were looking toward ultimate application to computer storage, we had the problem of, it was first called selective erasure, but I as a purist preferred selective alteration, because there is a symmetry between the one and zero situation. So we included that. So the selective alteration and the testing of the proposition about non-return-to-zero were made the subject of the ONR study.

NORBERG: Now, non-return-to-zero, return-to-zero and so on show up in the Harvard documents on the Mark II. In the 1947 conference, January of 1947...

COHEN: Yes, the first conference.

NORBERG: ... I'm trying to remember... Moore.

COHEN: Benjamin Moore.

NORBERG: Benjamin Moore gave a talk about magnetic techniques and discusses the possibilities of non-return-to-zero and return-to-zero, and so on.

COHEN: And Tommy was there.

NORBERG: And Tommy was there. Yes, now see, there's my puzzle. How widely known and discussed are things like non-return-to-zero before they show up in the Harvard publication?

COHEN: I guess the reason most of us weren't too worried about the priority on that particular property was that this was a real old telegraphic technique under various names.

NORBERG: So any decent magnetically informed person, then, might have known something about that. Might have, I'm not saying they did.

COHEN: Yes, might have, right. But Tommy wanted to pursue it further, more seriously.

NORBERG: Yes, well the Harvard people came up with a drum, as you know.

COHEN: But they didn't use that technique.

NORBERG: Yes. They did investigate it, because the early reports include long discussions of magnetic techniques.

COHEN: Harvard?

NORBERG: Yes, the progress reports on Mark II.

COHEN: Because when they got around to producing Mark III, they took an ordinary discrete pulse, very conservative, maybe about ten pulses to the inch.

NORBERG: Yes. Well, so was Binac very conservative in terms of pulses and pulse techniques, was it not?

COHEN: Binac. Mercury tank.

NORBERG: I'm sorry, Univac.

COHEN: Mercury tank also.

NORBERG: There was some argument between Isaac Auerbach and Eckert on this question of pulse techniques, timing, principally.

COHEN: On the tape, maybe?

NORBERG: It could be.

COHEN: Because the internal memory of both of those was mercury delay line.

NORBERG: But you still have a timing problem, don't you, in keeping track of the information?

COHEN: Yes. One of the jokes, incidentally... You know, there's a lot of lore that developed. There was very careful attention paid to the temperature control, which is necessary for mercury delay lines. And I believe you let... I'd have to review which controlled which, but you can either vary the frequency of the oscillator... Well, that's probably the way it's done, depending on the delay time, and perhaps even a dedicated channel. But the story was that the British, I don't know if it was Wilkes or some of these other guys, were always kidding the Americans that we always went to very elaborate control schemes. What the British would do was to go over and turn the knob a little bit every once in awhile. (laugh)

NORBERG: Maybe that explains why they got a machine going first. One last question that is concerning me at the moment. You shifted soon after you came to the company from a consideration of storing information on charged particle beams and so on to begin to design logic circuitry for new ideas like the NBS machine and...

COHEN: Well, you jumped a year there, at least.

NORBERG: Yes, that's 1948. But, regardless of how long it took to make that transition, you did make the transition to essentially logic design.

COHEN: One thing you mentioned brings to mind a funny story.

NORBERG: Well, that's all right. Tell me the funny stories. They may bring something else to mind, too. And I'm interested to know how difficult that transition was, in terms of understanding the nature of the problems that had to be investigated.

COHEN: That's kind of an abstract question to answer, how difficult was it. It was a learning experience. It was fun. And...

NORBERG: Well, what sorts of things in your background prepared you for this new requirement, so to speak...? Maybe an easier way to get at this question...

COHEN: Well, maybe an oversimplified answer is nothing, and therefore I shouldn't be offered the job. (laugh)

NORBERG: One uses the personnel one has. What did you do after the charged particle study?

COHEN: Well, the charged particle thing was an exercise - that's probably the best word for it. I went through a paper analysis. Obviously, the heavier the particle, the more reasonable the dimensions of the apparatus. So, I postulated a hydrogen ion beam, or something like that. If you wanted to store so many pulses, let's say a thousand pulses, on an ion beam, then you have problems of diffusion, the way the little packets move along, and other things of that nature. We carried it down to a point where I think it would have discouraged anybody from investing a buck in it, and worked that into the progress reports.

NORBERG: But it seemed to me that didn't take very long.

COHEN: No, about a month.

NORBERG: So that means by February 1, you needed a new task.

COHEN: Oh, and one characteristic of a lot of stuff around ERA, and certainly something that I felt, was that you didn't carry something down to a certain point and then go on with something else. There was a great deal of overlap, and uncomfortable overlap, because you'd be on something else while you were under pressure to get a final report out, or something. As August of 1947 approached and things started to get into gear for a couple of additional projects... Something that Worthy mentions in his book about Norris that I find hard to believe is that it was Norris...

NORBERG: A lot of that...

COHEN: Yes. It was Norris that suggested to the Navy that they really ought to have a machine that could be programmed so you didn't have to build separate machines... We had to explain that stuff to Norris. (laugh)

NORBERG: No. That's very clear that that came from the Navy, for god sakes.

COHEN: Sure. Well, they were interested... Let's see, by that time by the time we got in toward the summer, I had been reporting to Tommy, who by that time, also, was not in St. Paul, but somewhere else - Washington... He was rather insistent that we do something this way and that way. And I go into that a little bit in... Have you seen the final version of my interview?

NORBERG: Yes, I did.

COHEN: Yes, he wanted us to essentially repeat Aiken's durability of data, stability of data tests. And I maintained that that had been established. And we weren't really establishing anything new if we did that, so why don't we proceed with whatever the next step ought to be. So, the right thing happened and Coombs came in as a local supervisor in charge of that work and other projects that were going on.

NORBERG: But now, what was happening in the intervening months, between, say, February 1 and August 1?

COHEN: Oh, well, we carried that work, the ONR, whatever the task number was... Did we call that B3001, or something like that, an internal accounting number. I believe the report on that... I seem to recall June 30 of 1947. But we might have carried the stuff further in getting it all finished.

NORBERG: Selective alteration?

COHEN: Yes. Selective alteration; stuff like that. And the final report may have had a later date.

NORBERG: Yes, November.

COHEN: Okay. Yes, so, there are overlaps in the paper recording all of that.

NORBERG: In the case of selective alteration, was it necessary to consider the circuitry problems associated with the logical manipulation of this data? There are circuits mentioned in that report.

COHEN: Oh, yes. Well, sure, flip-flops and pentode gates. Diode circuitry wasn't too reliable at that time.

TAPE 2/SIDE 1

COHEN: During the time that we were working on the selective alteration work - I was using the term "selective alteration" to describe that particular project to distinguish it from other things going on - I was asked to read some reports that were classified top-secret, as I recall, which were retained in the safe in the USNCML office, Captain Creasor's office. There was a receipt-signing protocol. And I would read this stuff in their office, on the premises of the Navy office.

NORBERG: Do you remember what those things were that you were reading?

COHEN: Yes, I'm getting to that. There may have been more than two reports, but in particular, there were two reports, both authored by Jim Pendergrass, Lieutenant Commander James Pendergrass. Jim had apparently spent time at Moore School and perhaps at IAS. I'm quite sure at IAS, because he picked up a great deal of what was going on there, and probably at MIT - WHIRLWIND. And he proposed what, in retrospect, I would regard as a variation on the von Neumann proposal for a machine at IAS; a variation on that with an instruction repertoire, an order code, that included some features that he thought would be useful in the crypto business. I believe one of those volumes was primarily a description of that kind of an architecture. And at least one of the other volumes took some standard problems of theirs and programmed these problems on this hypothetical machine. He drew some conclusions about the speed and also flexibility for changes, and this sort of thing. In other words, the whole thing was a pretty good sales talk for the utility of a general purpose machine, compared with all of the special hardwired mechanical things that they had been using at that time. That was a good source of education for me. I don't know, I presume Coombs and Howard may have decided I'm the guy that I should get into this, and although I didn't realize it at the time, apparently I was being groomed to follow through at such time as we got the task assigned. And that's what came out first. So you asked what I was doing in the interim. There was a lot of this sort of work, studying, going on. We had access, probably due to Tommy's efforts more than anybody else's, to Moore School reports, gobs and gobs of them. Some of the Moore School bound volumes just consisted of disclosures, many patent disclosures. And those would be wonderful historical things to have now, because they were authored by various people who went various ways later.

NORBERG: Now were these from the ENIAC project, or ENIAC and EDVAC?

COHEN: ENIAC and EDVAC, right. We also had access to the WHIRLWIND reports. WHIRLWIND had several series of reports. Some were engineering memos. Some were engineering reports. In those days there wasn't any Xerox, these came dittoed and stuff like that. So I guess, in answer to your question, that whole body of information was part of our education. You asked about the circuitry and circuit techniques. A good source of education on that was the MIT Radiation Lab series, which was at that time just coming out volume by volume. And I believe, Britton Chance's volume on...

NORBERG: Volume 127.

COHEN: Yes, all of these things, of course a lot more than you wanted to know. But these were the bibles that we used.

NORBERG: Okay, were you participating in the survey that was going on at the time, as well?

COHEN: Oh, yes.

NORBERG: Did you make site visits during this period?

COHEN: Yes, that's right; that was during the same period. Yes, sure, WHIRLWIND. Not too many site visits.

NORBERG: You went to Brown around the same time.

COHEN: I went to Brown, yes, in 1947. And for some of the information, you didn't have to make any particular site visits. You'd go to the IRE meeting, the big one in March 1947, and the 1948 meeting. You could talk to a lot of people, you would hear papers, visit exhibits. A big educational process going on in those years.

NORBERG: Yes. Can we go back a little to the Pendergrass volume, reports that he developed? What do you remember about those? I'm interested in two things. First of all, I'm interested in what sorts of specifications for the general, for what we would now call the general purpose machine may have appeared in his first report, because I haven't been able to come up with the report. And the second thing is, I'd be interested in the kinds of applications which are not so classified that they are really a problem for us now. You know, if you know, for example, that one or more of the applications has appeared in public print, like in Kahn's *Codebreakers* or so on, if you could point that out to me, that would be a great help.

COHEN: Well, I'd be glad to help find something like that, but without knowing the basics of their applications... This was all completely new to me at the time. But in reading over Pendergrass' reports and getting the general gist of some to the projects that we had going on, although they were highly compartmentalized, you didn't pick up an awful lot. But, you know, over a period of time you could get the general gist as to what these things are doing. You could break it down into some fairly basic operations. The logical operations obviously involved comparisons, either small numbers of bits, like one character's worth, or long streams, if you are looking for a particular group or word. So comparisons like that are in a lot of techniques. The exclusive OR, for example, on a single bit, which in the crypto trade they were calling false add, and I believe we used the term vector add in the ATLAS I.

NORBERG: All to stand for this exclusive OR.

COHEN: Yes. Exclusive OR, but for a group of bits. Vector add, implying that if you had a six bit thing, you're dealing with this six dimensional space, so a vector. You know, all sorts of thinly disguised names for... The basic Goldberg thing, which also was a property of many other things, where you run comparisons of two strings of data, and then you precess the data in a different relative offset. You run the comparison again. You do this over and over and over again. These are the kinds of basic things that they did. Although I don't recall which particular things Pendergrass showed as examples, he used the code name or cover name designation for the type of problem or type of machine, maybe a German machine or something like that, and showed how that could be programmed in a general purpose stored machine program, a computer of the kind that he had postulated.

NORBERG: Yes. Now, does that suggest that the problems themselves are fairly simple in terms of the way in which they would be programmed?

COHEN: Yes.

NORBERG: I mean, sorting and comparing, it seems to me, is a fairly simple operation for a computing machine to do.

COHEN: Yes, I think that, in general, is true. It was a good thing, too, because, you know, you didn't have higher level languages to deal with. Let me add to what I said a minute ago. One of the reports that was available and was a very good educational source was the soft cover Burks, von Neumann...

NORBERG: Goldstine coding paper?

COHEN: No, before the coding. There were three volumes. The first volume was the proposal or the architecture paper, you might call it. I forgot the title on the first one. I remember number three.

NORBERG: Can I get the volume... Here are the von Neumann papers and this is the first one. It's labeled three, because of our [Charles Babbage Institute's] Reprint Series, but it seems to me this is the first one on logical design.

COHEN: Yes, that's it. That's volume one. This terminology, "memory organ" and so forth, yes.

NORBERG: So you remember reading this sometime in early 1947?

COHEN: Oh, yes, early 1947, very definitely.

NORBERG: So you saw the first edition then, if that's the case.

COHEN: Yes, I believe Tommy had it very early in the game. And then, Tommy was very good at a lot of the stuff, like the applied number theory that goes into these things. I think Tommy presented... There were these casual seminars, you know, you'd call a bunch of people together around the blackboard, and Tommy would indicate the different ways you could go at it. The difference between a one's complement and a two's complement, and the difference in what happens if you do these things. He had some very good ways of setting these forth that you didn't find in the number theory texts, which hadn't gotten around to thinking about these kinds of applications. Also, some of the basic ways of sorting with cards, and I hadn't had any punched card experience at that point. So, just going through how one would sort, the fact that you could go from low order first up to higher order, which is backwards from the way you might normally think of it. Things like that.

NORBERG: So when you say "going from low order up to high order", what do you mean?

COHEN: You sort on a low order digit first.

NORBERG: Meaning the tens digit, say.

COHEN: Yes, the ones digit, and then move on up.

NORBERG: I see, you would think of sorting the other way.

COHEN: Yes, so, you know, you take these things and you lay them out on a big cross-hatch paper. You put some examples in and show what happens from step to step. So there was a lot of this self education that I certainly engaged in, and I'm sure a lot of other people did too. It was playing around with basic operations that we got to know about.

NORBERG: Let's go back to what you were saying about Pendergrass. In that case, he had the two reports that he wrote: one on the machine...

COHEN: At least two; maybe more.

NORBERG: Yes, he talks about two in the interview with Bill. There was the one on the machine and then there was the one on applications. You've mentioned that you also saw the Burks, Goldstine, von Neumann paper on logical design.

COHEN: And then a little later on their programming concepts paper.

NORBERG: You saw the programming concepts paper as well. Okay, fine, now, let me bridge the gap to what I was going to say. You also mentioned having seen MIT, WHIRLWIND, reports. Do you remember the nature of those?

COHEN: Well, one series that had covers on it - you know, a soft cover, but blue with an ONR imprint and a few things on the front, you could spot them across the room. I think those may have been called the ER series, the Engineering Report Series. I would hope that somebody has a complete file of these somewhere out there, because they were very good. They went through the proposed architecture of a parallel binary computer. Presumably, after they recognized that they could go a lot further, they did a selling job on their sponsor. This flight simulator would be better served if they went digital, rather than analog. And some of the early reports showed the simulated cockpit and all that sort of thing, as I recall. Then they got down to business and they were investigating storage techniques. By the time I visited Jay Forrester and those guys in the spring of 1947, they had their own version of an electrostatic storage tube, of which there were about three or four that were contenders for the way to go. Haeff - did he go from NRL to Raytheon or the other way around? It's hazy in my memory. Their particular technique at MIT. Of course, Rajchman at RCA had the selectron idea. So they were planning on a small memory of electrostatic type.

NORBERG: I have uncovered some reports of theirs on block diagrams of circuit distribution.

COHEN: Yes, they went into a lot of detail. We learned a lot from that, but we did not lift certain... For example, their basic...

NORBERG: Complete that sentence. You did not lift anything from their report?

COHEN: No, we did not. We departed from what they were planning to do in many respects. I guess our gating system, you know, heavy use of pentode gates, didn't necessarily originate with them, but we thought they were probably on the right track. They had a very tricky flip-flop that was AC coupled, not DC coupled, which, as I recall - and here I'm just trusting my memory. I'd have to check this out... As I recall, if the flip-flop sat there longer than a certain amount, obviously you couldn't trust it. So I think there was a restorative pulse of some sort that came along at intervals that reset the thing to wherever it was at the time. It essentially recharged a coupling capacitor, I guess, is the way to put it.

NORBERG: Now this is their design, or your design?

COHEN: Theirs. No, it was theirs. No, we went back to what was more conventional and pretty well established. You know, flip-flops are pretty old stuff. As a matter of fact, my first contact with that stuff was in counting circuits, back when I was working here at the University. I didn't know it as a binary counter. We used to call them scale of 8, scale of 64.

NORBERG: Yes. I've been going back to pull out some of the publications of the people here in the physics department on those counters

COHEN: As a matter of fact, Shepherd and Haxby...

NORBERG: That's the paper.

COHEN: Yes, theirs was based on gas discharge tubes. There's a whole literature on flip-flops and counters. The ERA book probably lists them all.

NORBERG: That's where I got the reference.

COHEN: Yes.

NORBERG: Okay, so there is this learning process going on. It's being paid for by ONR, obviously. And it's being paid for in other areas for the learning process to go on as well, it seems - MIT, IAS, Eckert-Mauchly.

COHEN: Yes, and apparently this was sort of a wonderful opportunity, a golden age. That post-war period was very great.

NORBERG: Yes, now, to what extent, Arnie, do you think that projects were encouraged to go in a particular direction only, rather than cover a broad spectrum. For example, once you people had seemed to take on the magnetic area, were you encouraged to pursue that further at the expense of doing any more with electrostatic storage possibilities, or acoustic storage possibilities and therefore let IAS do the electrostatic, and let Eckert and

Mauchly do the acoustic, and so on. Do you think there was any of that going on to your knowledge?

COHEN: Okay, let me attempt to give the ambience of the time. The preliminary thought, but subject to another further look or two, but the thinking as of August 1947 was that our sponsors at the agency would like to have us proceed with magnetic drum storage, which would give the machine a pretty healthy size storage capacity for main working storage. It was the idea that, whether the architecture would be the same or slightly different, eventually a faster kind of storage would come along, presumably electrostatic, and that would then be the basis for either an add-on or a new variation. But they were interested. I think that they were sufficiently convinced by Pendergrass' report, and I suppose, their own conversations among themselves, that even half-way measures of this kind - so you don't have the fastest memory in the world - would be far superior for their purposes than building a whole new machine every time you wanted to address a particular problem. They needed no further selling on that. So, I think that was the reason for saying, okay, let's proceed with, first of all, I guess it amounted to a proposal stage, a paper description of the machine, to be followed thereafter by...

NORBERG: Does that suggest that people had the assumption that the access circuitry was sort of irrelevant in this problem? That is, if you build an internal memory system, whether it's a drum, or electrostatic, or whatever, it doesn't really matter. And then, around that, you have an access system to this. Is the assumption here that the access system will be independent anyway, and therefore, getting in and out will not be affected by changing the internal memory to some other system at a later date?

COHEN: Well, I don't know if anybody really believed it quite that literally.

NORBERG: Did they even see this as a problem?

COHEN: They might have, yes.

NORBERG: Because it seemed to me that the Eckert and Mauchly people got into trouble over that matter, and that some of their later attempts to develop the UNIVAC ran into just this sort of problem. They had designed circuits for use in EDVAC; they left them alone; and they built a new internal memory system. When they came back they found they couldn't use those same circuits they had designed. I think that's the...

COHEN: I don't know which particular thing you're referring to, but that would certainly be true. A good example of that would be going from UNIVAC I to UNIVAC II, which had core memory.

NORBERG: Indeed.

COHEN: Yes. Completely serial logic in the UNIVAC I working into a core memory meant that you had to have all sorts of extra registers, lining things up, shift registers. Let's get back to the other thing. The first time that I went down to Washington on this particular work, August of 1947, John Coombs and I went to Washington up to the Nebraska Avenue location, since this was my first visit there, why, that was my first meeting with Howard Campaigne, Joe Eachus, and Jim Pendergrass. So we talked over the general ideas. And they urged us (we hadn't planned this in advance)... They hurried us to, preferably right on this same trip, to go up and see Rajchman's work at RCA Labs. Somebody must have communicated ahead, because we were welcomed. Rajchman's work was not unknown. He had presented a paper or two, quite a few... Oh, at least on one occasion at IRE convention in New York, he talked about the selectron. And it had been described by Burks, Goldstine and von Neumann, I mean, the properties of that kind of thing. So we spent some time with Rajchman going over the status of the development; how things were going, which, incidentally, wasn't as good as he had originally thought, you know. He had originally thought it was going to be 4096, but he backed down to 1024. I guess eventually, when they used it out at Rand, wasn't it down to 256, something like that? It was on that trip - I probably told you this story - that John Coombs wanted to look up his old friend, Professor Warren Bliss from the University of Maine, who was by that time working at RCA Labs. So after we got done with Rajchman, we looked up Bliss, and spent some time chatting with him. And it was on that occasion... We had, just a few weeks earlier, placed an order for a new triggered-sweep scope that RCA had advertised, with a big power supply. It rolled around on wheels, a heavy, big thing. With the encouragement of the Navy we had ordered that. We asked Warren Bliss what he thought of that, and told him that we had placed the order. "Well, yes, that's a pretty good product. But do you want to see a real scope?" He said, "Come on with me." He took us into the adjoining lab and he showed us the first of the Tektronix, whatever the number was. And he gave us a real pitch on the Tektronix. We got back and canceled the RCA one, and ordered our first Tektronix.

NORBERG: What had they been using before that? Dumonts?

COHEN: Yes, there were the Dumonts. Before that time, you will recall, there was something that Sylvania put out for radar use called the synchroscope. There were many variations on... not many variations, but there were a few variations on that kind of thing.

NORBERG: So you went to Washington, you met these fellows, you talked about these designs.

COHEN: Where this relates to your question, we were considering other things, you see, and they wanted us to take a look at, evaluate what we thought the status was of that thing. We did order from Technitrol... Technitrol sold us a free-standing mercury delay line memory for experimental use. And that was bought under one of the Navy tasks to evaluate it.

NORBERG: That would have to be near the end of 1947 though, wouldn't it?

COHEN: Yes.

NORBERG: Because that company didn't get started until early 1947.

COHEN: Yes, it was probably in there. So, all these bets were being covered. And then later on, when we established Task 29, or maybe before that, before the Atlas II thing, somewhere...

TAPE 2/SIDE 2

COHEN: We had a subtask to evaluate the RCA...

NORBERG: Selectron?

COHEN: Selectron. By that time, incidentally, they were no longer calling it Selectron, because somebody else had that name first for another purpose. Kelar was on that particular thing. To answer your other question, were we told to concentrate on the magnetic? Well, that was preferred for that particular application, but we never did burn the bridges of the Selectron. The Selectron work was going on in parallel with our developing circuits for a Williams tube approach, the Williams CRT approach. Those were in parallel, just to make sure that we didn't make any premature choices.

NORBERG: Do you remember the choice then for magnetic, when you finally decided: "That's what we're going to go with; we're not going to go with the other."?

COHEN: Well, for ATLAS I, I guess that sort of fell into place so that when we wrote up the proposed architecture of ATLAS I, beginning whenever it was - August of 1947.

NORBERG: Was it August of 1947?

COHEN: Well, it didn't get off to a rapid start.

NORBERG: That's when the contract was let, wasn't it? August of 1947? For ATLAS?

COHEN: Well, for the preliminary study part. The task was assigned, but we had no go-ahead on other than laboratory work. We did have laboratory experiments on many of these things. We put a number of people on preliminary work with regard to arithmetic circuitry, input-output stuff. We had a photoelectric tape reader that...

NORBERG: Now this is before... I'm a little confused about what date you're talking about.

COHEN: I'm talking about under...

NORBERG: Task 13?

COHEN: ... Task 13. At the same time as the architecture was being worked out on paper, we were also looking into other things.

NORBERG: Okay, but still starting in August of 1947.

COHEN: Right. But then the go-ahead and serious work would have come along in the middle of 1948.

NORBERG: The middle of 1948; okay, that's what I'm trying to get clear.

COHEN: Yes, specifically, to build something that was architecturally described in the proposal report.

NORBERG: Now, would the architecture description... In fact, many of these different problem aspects that were being investigated, including circuits and applications and so on, would these all have come together, then, in the report called *The ATLAS I Characteristics, Volumes I and II*? Do you know the year of that?

COHEN: That was probably spring of 1948.

NORBERG: Spring of 1948.

COHEN: Would that be about right, or was it earlier?

NORBERG: That sounds right. It wasn't earlier.

COHEN: Yes.

NORBERG: And that would be the result of these preliminary investigations, that you would just...

COHEN: As I recall, there was a little overlap there. I think by the time we were ready to formally submit that report, we got the informal word that, okay, the Navy was going to authorize it.

NORBERG: Okay. Then...

COHEN: Before they even got the report.

NORBERG: Therefore, between August of 1947 and when that report was submitted - let's assume it was the spring of 1948 - there had to be a decision made about magnetic drum, that that was the way you were going to go. Now, as I recall the research on the magnetic drum that was going on, by that time, Rubens and his various people already had a working model of a drum that would do what it is you wanted it to do in terms... No? Not by spring of 1948?

COHEN: No, no - down different paths. The Rubens work... Now, I have to be very careful because we were all spread over into each other's territory. But the Rubens work specifically sponsored under task 9, Goldberg, was for that project. And that went, you know, the large drum with the mechanical step-by-step load and then free-running for precession, recirculating channels, that was the Goldberg thing. And then some of those same things were also picked up and used in Demon.

NORBERG: But was that also carried over into ATLAS?

COHEN: No. Well, it depends on what you're talking about. Rubens was working very closely with us on, you know, the head design and what you'd have to do. And our very first design for the writing circuits, we used the miniature thyratrons. You see, that was my orientation background; my previous work. That was a carryover, to answer an earlier question. But then you couldn't repeatedly write, there would be a recovery time before you could pulse the thyratron again. It was almost like a miniature thyratron modulator circuit. You would get a current pulse of half a sine wave. A pulse through a winding of a relatively small number of turns on the head, but then the reading winding would be of more turns. Well, Sid Rubens, of course, was very instrumental in figuring, you know, the ampere-turns, and so forth, the best designs for that kind of head. That evolved into a better head design anyway. And ultimately, what we called the sparkplug head, which was so far superior to Aiken's approach where you couldn't adjust those things, mechanically positioned, anyway. This evolved through the design phase. How do I take it apart, because in my mind it all flows as one continuous evolutionary process...

NORBERG: Okay, but you have to take it apart, because if you were telling me a little earlier that you were looking at all of these various possibilities for internal memory, then somewhere along the line you had to decide: "All right, this is the one we're going to go with for this particular machine."

COHEN: I would say that decision was made even before August of 1947.

NORBERG: Even before August of 1947?

COHEN: Yes. And the only way I can explain this... It isn't necessarily a rational step-by-step process. After awhile, you have a good seat-of-the-pants feel for what you would have confidence in pursuing, and what you wouldn't have so much confidence in pursuing. And I guess there wasn't much question in the minds of most of us.

NORBERG: Keep going. That's exactly what I'm looking for, because it seems to me... Now I didn't want to say this before, but now that you've raised this question of confidence, it seems to me that the group at ERA would be more confident about magnetic techniques than they would about, say, acoustic - which would be way at the other end of the scale in terms of their past history, than people at Harvard, at Eckert and Mauchly, at Pennsylvania, and at IAS. Those groups were dealing with an entirely different spectrum of electromagnetic activity. Pardon the unintended pun. They were dealing with a whole different range of phenomena in their previous experience, whereas many of the people at ERA were dealing in magnetic techniques.

COHEN: Well, that's part of it. Perhaps a more important deciding factor would be the interest in going to a parallel architecture. Pendergrass liked the idea of following the von Neumann architecture. And MIT was going that way. And we found that a reasonable approach, whereas the strictly serial EDVAC, BINAC, etc., approach presented other problems.

NORBERG: Such as?

COHEN: Limitations of flexibility. Well, some problems we didn't fully appreciate until a later time. These guys were using completely direct coupled logic and things like that.

NORBERG: How does that become a problem, Arnie?

COHEN: Well, if you look at the UNIVAC circuitry, which we got to look at in much detail when the UNIVAC II thing came along, you have many, many supply voltages all stacked up. When you go from circuit to circuit to circuit, you know, passing information along, why, on a direct coupled basis, you are stacking up supply voltages as you go along. That's one problem, the problem of stability; if something drifts, everything drifts. But if you're dealing with capacitive-coupled circuitry, each flip-flop is direct coupled within itself. And then you can deal with relatively few supply voltages and keep them pretty well under control. We also did marginal checking, although we varied the heater voltages. MIT varied some of their other voltages as part of the testing, as they ran test problems. You could see what limits could be tolerated. Also, you could keep things designed very conservatively. You could design for large tolerances of voltage swings, and large tolerances of component values, where you might not be able to do that quite as well with the direct coupled approach.

NORBERG: I see.

COHEN: From a logical standpoint, there's nothing that says you cannot go the serial way for crypto applications. The best evidence for that is the Abner machine, which the Army people worked up on their own very quietly without too many people knowing about it. And it was essentially, you know, a binary EDVAC type of approach. Apparently that worked out very well.

NORBERG: Was the ATLAS machine a floating point machine?

COHEN: The ATLAS I? The ATLAS I or II, no. It didn't have built-in floating points, no.

NORBERG: Okay, that was just an incidental question.

COHEN: It had some oddball... Because of the customers interest in modular arithmetic of various kinds, the way it did division, for example, was unconventional. And that carried through the 1103. You divided so that you always had a positive remainder. In modular operations in general, modular logical operations, you're interested in the remainder, less interested in the divisor, or the dividend. Quotient, I guess, is what you'd call it.

NORBERG: Yes. All right, just to bring our discussion to a close, as I understand what you were telling me then, is that the period from, let's say, the beginning of 1947 through to August of 1947 when the ATLAS contract was finally let, that there was a substantial learning process going on; there was consideration of Navy reports as well as all these other project reports that happened to come to you through the courtesy of ONR; and that you, as a group, were looking at various techniques for building a general purpose machine. Is that a fair statement.

COHEN: Yes. Incidentally, when you say courtesy of ONR, it was their active interest. Primarily Mina Rees was carrying out what she told me just a couple years ago was the policy of the Navy to disseminate this kind of technical information as widely as possible, which is the reason that the book came out.

NORBERG: Yes. But now, doesn't this have a couple of ramifications? First of all, with those projects such as the Eckert and Mauchly NBS contract, aren't there problems with proprietary information? Why should Eckert and Mauchly, as a company, want to tell you people what they're doing, particularly before they patented the device that they're dealing with?

COHEN: Well, the answer is they don't and they didn't.

NORBERG: All right, you didn't see anything from Eckert and Mauchly.

COHEN: No.

NORBERG: All right. Then it's only those things which are development contracts that you saw reports for, rather than supply contracts?

COHEN: Right, they were... Yes, these were reports of the office... Let's see, in their case, it was Ordnance, wasn't it?

NORBERG: In whose case?

COHEN: Eckert and Mauchly; or, no - Moore School. Skip it. No, we didn't get any Eckert and Mauchly reports. This is strictly Moore School.

NORBERG: Yes, and therefore, by 1947, when the contract was let, you already had a basic, shall I say, concept for the design of this new machine.

COHEN: Yes... Well...

NORBERG: I didn't want to say design, but a concept for a design. You were moving in the direction...

COHEN: Rough concept, and to be refined, because...

NORBERG: Yes, of course.

COHEN: By the time we had our first meeting in August - and I couldn't be absolutely sure of this - we were handed some preliminary sketching out of what the properties of such a machine would be. And from there on, there was give and take. We had a whole series of meetings.

NORBERG: Not unlike the meetings a year later with the National Bureau of Standards.

COHEN: Yes, in which there was give and take. We'd make counterproposals. And they made counterproposals to our counterproposals. And I would say, we very quickly arrived at some meeting of the minds. But the arithmetic properties and the logic, and that stuff was what they wanted. "They" being Eachus...

NORBERG: The Navy in this case.

COHEN: ... Pendergrass, Campaigne. They came out to see us. Just very informal meetings as we went along.

NORBERG: Okay, very good. Thank you, Arnie.

END OF INTERVIEW