

An Interview with
ARTHUR W. and ALICE R. BURKS
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Conducted by Nancy Stern

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Abstract

Arthur Burks describes his work on the ENIAC and Institute for Advanced Study computers. He reviews his upbringing, education, and work experiences (mainly teaching) before joining the University of Pennsylvania Moore School of Electrical Engineering in 1941. He then discusses his associations with J. Presper Eckert, John Mauchly, John Brainerd, Herman Goldstine, and others and their work at the Moore School. Various aspects of the ENIAC project are discussed in detail: interactions of project members, division of tasks, decision making processes, patenting issues, initial operation, and von Neumann's association with the Moore School and the ENIAC and EDVAC projects. There is a general discussion concerning the classification of general versus special purpose computers and computers versus calculators. Patenting issues concerning the ENIAC project are given particular attention. The Burkses discuss the dispersion of ENIAC and EDVAC personnel at the end of World War II. Burks recounts his move to the Institute for Advanced Study in Princeton, his experiences there, and his consulting work with Burroughs prior to accepting a faculty position at the University of Michigan.

ARTHUR W. AND ALICE R. BURKS INTERVIEW

DATE: 20 JUNE 1980

INTERVIEWER: Nancy Stern

LOCATION: Ann Arbor, MI

STERN: Art, I know you were born in 1915 in Duluth, Michigan.

A. W. BURKS: Duluth, Minnesota.

STERN: Minnesota.

A. W. BURKS: On Lake Superior.

STERN: Lake Superior you said?

A. W. BURKS: Yes, isn't it the end of Lake Superior.

STERN: But I don't know much about your background in terms of your parents, how many sisters and brothers you have. Could you just give me a little bit of background information?

A. W. BURKS: Yes. My father and mother have both been teachers. At the time of my birth my father was a teacher in the school system of Duluth, Minnesota and my mother sometimes taught as a substitute. The first child is my older brother Richard, two years older, who now teaches history in Wayne State University in Detroit. The third child was my sister Sara Elizabeth, who died a few years ago, and the fourth child was my brother David who teaches history at Hunter College in New York City.

STERN: Your parents - did they teach a particular subject?

A. W. BURKS: My father taught mathematics but he was really more interested in history in the sense that he was an avid reader of history. Read books and knew a great deal about the Civil War, for example, and about Lincoln as well as a lot about European history. My mother had been an historian, though when she had children she became interested in what you might call "educational activities" through the P.T.A. and the like. I can remember a book sitting at the main table in the living room; *The Child; It's Development and Growth*, something like this, and this was before very many people were interested in developmental psychology.

STERN: It's not hard to see where you got your interest in math and history from.

A. W. BURKS: Yes. That's right. Yes.

STERN: You went to the school system in Duluth, I assume?

A. W. BURKS: I went to grade school in Duluth and in 1924, when my youngest brother was just a baby, we moved from Duluth, Minnesota to the Chicago area. My father had gotten a position in the Chicago school system and he remained there until he retired, around 1950 I think. We lived first in a little village called West Chicago, for just one year and then we moved to Batavia, Illinois, which is now well known because of the accelerator, but then it was a small, industrial town of about five thousand people with lots of first and second generation immigrants mainly on the west side and Swedish people and a lot of Lithuanians in our neighborhood on the south-east side and a lot of Germans also in that neighborhood.

STERN: And you went to college in DePauw?

A. W. BURKS: I went to college at DePauw University in Greencastle, Indiana from 1932-1936. And at that time was unable to get a teaching job though I had qualified as a teacher in both Illinois and Indiana. So I came here and studied philosophy from 1936 to '37.

STERN: This was after you had your bachelor's degree?

A. W. BURKS: After I had my bachelor's degree. In that one year I got a master's degree in philosophy here and at the end of that year was offered the first job I ever had which was teaching high school and grade school at a consolidated school north of Flint, Michigan. I spent a year there and then came back as a teaching fellow in philosophy and studied philosophy from 1938 until '41, supporting myself as a teaching fellow or with a fellowship (study fellowship). And in '41 I got my Ph.D. in philosophy.

ALICE BURKS: Do you want to say what your undergraduate work was?

A. W. BURKS: Yes, it was in mathematics as a major and physics as a minor, though I became very interested in philosophy and took what was the equal of a minor in philosophy.

STERN: I assume that you were always interested in going into mathematics as a young boy?

A. W. BURKS: Yes, I was pretty good at mathematics in grade school and high school and my dad is in mathematics, of course, and so I planned to study mathematics. And indeed I think I planned as a high school student that I'd eventually get a Ph.D. and teaching in college, or at least that was my ambition. I remember we were very active in the congregational church and I remember when I was a senior in college, one of the--it wasn't the minister but one of the leaders in the church, a young man, wanted to know what I was going to do when I graduated and I said go study mathematics and get a Ph.D.. And he said, "Well what could you write a thesis on, are there any new unsolved problems in mathematics?" And of course the kind of mathematics you got in high school those days, or even in college, didn't show you that there were lots of unsolved problems.

STERN: That's right. As a math major in college myself, I had that same feeling, you know, that everything that could be solved has been solved. When did you change your mind and decide to go into philosophy?

A. W. BURKS: Yes, I became interested in it as a junior, when I took my first philosophy and then as a senior I began to think seriously about it. I can remember taking or walking out to our garden, we lived in town and half a mile away my father had 4 acres where he raised vegetables, had a cow, provided milk for us, and he had a horse to plow and he raised raspberries. Indeed a lot of the money that went to send my brother and me to college came from our selling these raspberries. And I can remember walking out there, we had to go out there quite regularly to work, and thinking whether I wanted to go in math or philosophy. Actually at the end I received no offers of a fellowship in math except one in statistics from Iowa State College. I don't know whether you know but, then and still, Iowa State College (it's now a university) had a very excellent statistics department. But by that time I decided I wanted to do philosophy so I came here to Michigan. Now in philosophy I worked a lot with Harold Langford who was a logician. I took a course from Ray Wilder in the foundations of mathematics -- he later published that course in a book. So I concentrated on philosophy of Science and philosophy of mathematics at that time.

STERN: You taught at the high school level?

A. W. BURKS: It was actually sixth grade, eighth grade, ninth grade, tenth grade, and eleventh grade for one year.

STERN: And in the subject of philosophy or mathematics?

A. W. BURKS: No, it was math. I think the sixth grade was English, because you did whatever they told you to do, but mostly I was a mathematics teacher.

STERN: I see. I understood during that period that getting jobs in the math field were very difficult.

A. W. BURKS: It was very difficult to get a teaching--well it was very difficult to get any job in 1936 but this one--well I already had enough practice teaching from DePauw to be certified in Michigan. My advisor in education had pointed out that if I took twice the usual amount I could then be certified in Michigan and other states and at the

end of a master's degree in Michigan I was given a life certificate to teach in Michigan, which I still have. And so unless they revoke that, I have the right to go in and teach, if I can get the job, teach any subject in any part of the public school system of Michigan. Indeed this particular place had about thirty teachers and only half a dozen of them were men and so they needed a man because they had to have the men to drive school buses and I drove the school bus in addition to teaching.

STERN: Now you got your Ph.D. from Michigan in 1941?

A. W. BURKS: In philosophy.

STERN: And then you went to the Moore School at that point?

A. W. BURKS: Yes.

STERN: What precipitated that?

A. W. BURKS: Well, I wasn't able to get a job and I heard about the summer course that they offered. So I applied and was admitted to that course, and went there and took that course, and that's where I first met John Mauchly; he was also a student in that course. As I remember there were about 30 students in that course.

STERN: 30 students. This is the engineering management science war training course at the Moore School?

A. W. BURKS: Right. Yes.

STERN: So that your main reason for going there was because of the difficulty in getting a job in your own field.

A. W. BURKS: Well, the difficulty in getting the job, and when I went there we weren't at war, but it was clear that we

very probably would be at war. The war, of course, was raging in Europe and so I thought that I would be better able to contribute to the war effort by getting this training in engineering. The idea of that course was that it would take a person who had a bachelor's degree in physics and math and make that person into somewhat of an engineer.

STERN: Now your dissertation at Michigan was on what?

A. W. BURKS: On the philosophy of Charles Peirce - his philosophy of science, but that also included his philosophy of mathematics.

STERN: Yes. Carolyn Eisele has been doing work in that field.

A. W. BURKS: Well I later went to Harvard and edited two more volumes of Peirce's collected papers, volumes 7 and 8, and in recent years Carolyn edited a lot of Peirce's mathematical papers; 4 volumes, 5 books. It came out just a few years ago.

STERN: So that when you went to the Moore School in '41 it was essentially to get a kind of re-training in an engineering area.

A. W. BURKS: Or a converging, you might say, to an engineering area for the purposes of assisting the war [effort].

STERN: What was the Moore School like in '41?

A. W. BURKS: Well it had the one building which I can define for you. I think you went in the front door and you went to the left into the shop and if you went to the right there was a room that went the full length of the building of those days. It was one big room, which was the laboratory for machinery. It was where they had motors and generators and transformers. And then on the other side, besides the shop room, there was a laboratory room and then a rear room which wasn't used at that time but that's where the ENIAC was built. And that gave you the extent

of that building at that time. On that floor there was down in the basement a room that held the differential analyzer and a room with lockers for the students -- the gym lockers.

STERN: They're still there incidentally - the gym lockers.

A. W. BURKS: Okay. But that basement part did not go all the way back, it was just along the front. And then upstairs there was a corresponding amount of offices and classrooms or around the wall and the library and the classrooms and other rooms in the center. Since then, of course, they've added to the south and to the west; where they added to the west was a tennis court in those days and they've added a third floor too. Actually in one of the projects that I worked on (John and I did most of the work on this) we took radiation patterns for antennas by mounting the antennas on the roof of the Moore School and directing them down to a receiver in that tennis court below, and then we would rotate the antenna to take a pattern of the intensity of the radiation as a function of angle around the front of the antenna.

STERN: Now before you did this work you were essentially enrolled in a course?

A. W. BURKS: For the summer of '41 I was enrolled in this course and at the end of that course I was offered an instructorship at the Moore School, as was John Mauchly.

STERN: Now at the summer course - who were some of the instructors in that course?

A. W. BURKS: Brainerd was an instructor, Chambers was an instructor, Weygandt, and others.

STERN: Faucett?

A. W. BURKS: No, Faucett was not an instructor.

STERN: Did Pender teach anything?

A. W. BURKS: No, Pender didn't teach in that course. Reid Warren did, I think. I'm not positive on this, Nancy, because I later got to know these people very well. I definitely remember Chambers and Brainerd. Yes, I know Reid Warren was, because he taught us electricity and magnetism. That was one of the courses, and who else was there that I haven't mentioned? Weygandt was, I'm pretty sure. In that course you had two options; an electronics option and a machine option, and I elected the machine option. Eckert was the graduate student laboratory assistant.

STERN: Was Showers involved?

A. W. BURKS: No, Showers was not involved in the summer course. And then they brought in visitors. There was a man from Lehigh and there was a man from Swarthmore.

STERN: It must have been a pretty intensive program, I would assume.

A. W. BURKS: Yes, we started fairly early; eight maybe - nine certainly, had classes all day Monday -- classes and laboratories -- Monday through Friday, and Saturday morning. And then in addition we had homework. But of course they were trying to give us a survey of all of engineering in one summer. I don't remember the length - 10 weeks maybe.

STERN: There were about 30 students in this?

A. W. BURKS: That's the way I remember it.

STERN: And you had to apply and then be accepted for this program, is that how it worked?

A. W. BURKS: Yes.

STERN: I see.

A. W. BURKS: Chambers was in charge of it.

STERN: Based on your recollection, did the Moore School have a reputation in electronics at this junction?

A. W. BURKS: I just wouldn't have known.

STERN: How did you find out about the course to begin with?

A. W. BURKS: I heard through a friend here on the campus . . .

STERN: Michigan?

A. W. BURKS: . . . in Michigan that this course was available, and he gave me the address and I wrote and applied. I guess I wanted to know in order to make plans, so I called up the Moore School and talked with Chambers and he said, "Yes, you're admitted." So I went. So it was just kind of by accident that I heard about the course.

STERN: And essentially that summer course provided you with enough background to do the kind of work you did on the ENIAC?

A. W. BURKS: Well, no there was more than that. You see, I started teaching in September of 1941 and in a way I was really what we would now think of as a teaching assistant. That is, my teaching assignment consisted of two parts: one was to supervise the machinery laboratory, since I had learned something about machines, and the other was to teach quiz sections in a course, a survey course, of electrical engineering for chemical engineers and mechanical engineers. The Moore School then was separate from what was called the Towne School. The Moore

School did only electrical engineering. But it trained the Towne engineers in electrical engineering. Faucett gave the lectures in this course, using a book by a man named Dawes from MIT, and I was Faucett's teaching assistant, as we would call it nowadays. So that the level of teaching at which I started was not terribly advanced. At the same time I became a master's degree student and took evening courses. Well, most of their graduate courses were taught in the evening because there weren't very many graduate students, and by teaching them in the evening they could get people from the Philadelphia area. Or sometimes they had them in the day and I was, of course, free to take them. In the end I took a number of courses equivalent to the master's degree and that would have been--well certainly by the time of the ENIAC I had taken enough courses to be equivalent to the master's degree. In addition, in December of '43 they got a research contract having to do with an airplane that was to be used to sweep lines, and they assigned me to work on that. And I worked under Mauchly and Weygandt.

STERN: December of '43 - would be after the ENIAC then?

A. W. BURKS: I'm sorry, December of '41.

STERN: Right.

A. W. BURKS: Thank you. I worked on that, and then when that terminated they got this contract out of the Signal Corps in New Jersey to work on antennas, and I worked on them. So by the time I started the ENIAC, I had had a year and a half of research experience, two years of teaching, and by then I was teaching courses in which I was giving the lectures myself, and I also had the equivalent of a master's degree in electrical engineering.

STERN: You mentioned that you started working in the machine laboratory. What sorts of machines? Differential analyzer or other machines?

A. W. BURKS: No, engineering in those days was divided into two parts; the British called it low frequency and high frequency. The machinery part meant generators, motors producing 60 cycles or the like, transformers, a little bit of relay control equipment for the system. And every student of the Moore School had to study machinery and had to

take a laboratory which would involve hooking up motors and generators and running them, and measuring their functional characteristics and their temperatures after they had run a while, similarly with transformers, and so forth. The other part was electronics, where you did radio and video and a little stuff that was associated with radar. The Moore School kept up to date, pretty well to date on that in this laboratory.

STERN: So essentially you didn't have very much to do with the differential analyzer at that time?

A. W. BURKS: Well I knew that it existed and I helped set up problems some, so I had a general idea of how it worked. And it was, of course, the most interesting thing, in a way, that was going on at the Moore School.

STERN: Who was responsible for asking you to stay on at the Moore School, do you remember?

A. W. BURKS: Well, it would be the Dean who made the decision, but he wouldn't have known me because he didn't participate in the course. They had lost faculty, either to industry or the war, like Irv Travis. You must know who he is. He had been a faculty member there, and he was called up into the naval reserve. And other people had gone into industry, so they needed people and I guess they looked at the people in the course and saw that Mauchly and I both had Ph.D.'s, and they hired us both. I believe we were both hired as instructors.

STERN: And were you the only two people hired from this course?

A. W. BURKS: We were the only two people hired from this course, yes.

STERN: So I gather you got to know Mauchly fairly well during the course and then subsequently to it.

A. W. BURKS: Yes, and then after we accepted the job, John was unable to find a house in Philadelphia our first year, that would be the academic year of '41 - '42, and so we roomed together.

STERN: Oh really?

A. W. BURKS: And he would stay in during the week and then go out on weekends to visit his family in Collegeville.
Is it Collegeville where Ursinus is?

STERN: Yes. Did he tell you about his idea for an electronic digital computer back in those days. When was the first time you learned about that, can you recall?

A. W. BURKS: Well, no I couldn't give exact dates on that. I remember his taking a course in cryptanalysis when we roomed together because I can remember his working at the desk and telling me . . .

STERN: Where did he take that course at?

A. W. BURKS: Well, it was a correspondence course he took out of Washington, I guess. And we talked freely about things and he told me about his visit to Atanasoff. I couldn't date when he told me about these things.

STERN: Did he talk about the machines that he was experimenting with, the harmonic analyzer and neon tube?

A. W. BURKS: Yes, he told me about these machines. Again, I couldn't date when he told me.

STERN: But this was prior to his memo to Brainerd?

A. W. BURKS: Yes, his memo to Brainerd was '42 and I certainly knew of his interest in machines before '42. How much detail I knew then, I couldn't remember.

STERN: Can you recall your first impressions of working with Mauchly, what it was like?

A. W. BURKS: Well, I can remember when we were students together it soon became clear that he was the most

knowledgeable member of the class. Again, it was a small class and we typically went out to eat together and so it was not long before I got to know John pretty well, and I can remember studying for an exam once and wanting some help and going over to his room, he lived not too far from me, and asking him for help, and he gave me help on the problems that we were supposed to solve in preparation for this exam. And of course I got to know him well enough that we made that arrangement to room together.

STERN: Yes.

A. W. BURKS: He was always very friendly and helpful and easy to talk to. And when the students went to lunch, we generally talked of typical things.

ALICE BURKS: He had a very droll sense of humor and was a great punster, I recall?

A. W. BURKS: Yes.

ALICE BURKS: He seized on every opportunity to throw a pun into the works.

STERN: Really? You both worked on the mine-sweeping project?

A. W. BURKS: We both worked on it. The mine-sweeping project had two parts; one was a calculation part. The Navy was building an airplane that would have a large coil from nose to wing-tip to tail-tip to wing-tip to nose, and it had a large gasoline motor and direct current generator inside and this would produce current through the coil, and that direct current would produce a magnetic field. The idea is that the plane would fly along above the water and the magnetic field would detonate the mines and get them out of the way so a ship could then go through. The Navy had already decided on the design of this plane. What they wanted to know was information about how to use it, and they provided us with data about the characteristics of the mine itself, as they had gotten some German mines and taken the mechanisms out of them. The mechanism is essentially a galvanometer, in other words a needle that is

moved by the magnetic field, and after it moves far enough it makes a contact and that electrical contact is then used to detonate the mine. How fast that needle moves depends on how strong the field is, and the galvanometer has certain characteristics: natural period and all things like that. So we had to calculate the field for various positions as the plane went by. Well, to begin with, the plane would be remote from the mine and then it might not go over, it might go to the side of it and so forth. So there were many cases. We calculated the field, and then we also made a model setup down in that room where the ENIAC was later built. With this model setup we produced a source of magnetic field by actually using an alternating current magnetic field (because it is easier to make measurements with it) and then we moved a probe around down below to measure the field. Mauchly and I ran a mechanical calculator to calculate the field using spherical harmonics, and I worked with Weygandt in this experimental setup to measure the field. And then after those tables were gathered, that information went into the differential analyzer, and the differential analyzer was set up to make runs which would simulate the plane in respect to the mine and calculate when the mine would go off. That data was then provided to the Navy Yard down in Philadelphia. I don't think I actually set that problem up or participated in the setup of that problem on the differential analyzer but I knew exactly [how it was done].

STERN: Who supervised that project?

A. W. BURKS: Well, Brainerd was the director of research. Chambers was the director of War Education, you might call it, that's why he was in charge of the course. Brainerd was the director of research. He's the one who made the arrangement with the Navy. He's the one who told Mauchly and me and Weygandt what he wanted done. I'd say the calculations were supervised by Mauchly. He knew the appropriate mathematics to get spherical harmonics and so forth. And Weygandt supervised the construction of the model. I can remember this was my second contact with Pres. Pres had been the laboratory assistant in the summer course. Pres made a sensitive amplifier to pick up the field in this experimental model, I can remember that.

STERN: So he was also involved in this particular project?

A. W. BURKS: To the extent of making that amplifier.

STERN: Were you involved in Eckert and Mauchly's initial discussions about the proposed electronic device at that juncture?

A. W. BURKS: No; well, I knew they were going on because, as I say, it was a small school and we typically went out to eat together. So I went out to eat with Mauchly and Eckert a lot, but I was not involved in the detailed discussions.

STERN: Mauchly sent [gave] that proposal to Brainerd, the memorandum concerning a vacuum tube device in '42. Did you know he did this?

A. W. BURKS: Yes.

STERN: He told you about that?

A. W. BURKS: Yes, well I knew it was going on. Yes, I knew that he was doing that.

STERN: I'm trying to pinpoint your initial reaction to the concept.

A. W. BURKS: Oh. Well, I knew that John wanted to build an electronic computer and exactly when I knew it, I'm not sure. But I feel pretty sure that by the summer of '42 I would have known this. I mean this idea intrigued me, I was not in any position to evaluate it. I can remember a lot of talk about counters. Of course counters are the bases that he was going to use for this, and I can remember once his reading a review written by a professor of physics (I think it was Harnwell, the chairman of the physics department) in which the reviewer said you could build counters so fast. And this was faster than John thought they could be built, and I can remember his going over and asking or trying to ask this professor how this speed was accomplished. John later said, "Well, the professor couldn't really justify

this higher speed." So that would be typical of the sort of information I had.

STERN: What were your initial impressions of Eckert at that junction, the '42 period?

A. W. BURKS: Well, he was very bright and capable. He--how do I want to put this, maybe you could help me, dear -- it was not easy to get him to help you. He was supposed to be helping...

TAPE 1/SIDE 2

STERN: But he was still a graduate assistant at this time, was he not?

A. W. BURKS: Yes, now I think he may have had his master's then, I just don't remember. But he was functioning as a laboratory assistant for that summer course. But he had other projects he was doing, and so when he helped you he was very helpful, but on occasion it might be hard to get him to help you. So then we would go ahead and do it by ourselves.

STERN: Goldstine's book relates all of the events leading to the proposal that was sent to the Army Ordnance Department on behalf of the Ballistics Research Laboratory. Can you tell me how you became involved in that?

A. W. BURKS: I knew it was going on, and I can remember talking or being involved in the conversations between John Mauchly and Goldstine.

STERN: When did you meet Goldstine? When did you first meet him?

A. W. BURKS: Well, about as soon as he came, and that was '42, I assume.

STERN: Right, it was '42.

A. W. BURKS: And I can remember meeting him in the shop, the shop was a place where people often congregated, and his telling me that he had taught at Michigan -- I had not known him at Michigan -- telling me he had taught at Michigan, and he knew one of my philosophy teachers - Paul Henle. I can remember his saying, "Well, I know Henle." And that was fairly soon after he came and, of course, initially his job was to supervise. He was the military supervisor of young women like Alice in a contingent that had been sent up from Aberdeen. But then again, whenever he was there. Now he wasn't posted in Philadelphia at that time. As I remember, it was only later that he was posted permanently at Philadelphia. So he would be in and he would be out. But we got to know one another and again he and Adele would go out with us to eat, so I got to know him very well.

STERN: You had already met Alice at this time?

A. W. BURKS: Yes.

ALICE BURKS: We met in July of '42 when I came up from Aberdeen in the first group of girls.

A. W. BURKS: Yes, there were 4 or 6 sent up.

ALICE BURKS: Very small number.

A. W. BURKS: Very small number, and I remember they were working in that middle room on the south side of the ground floor, between the shop and, let me call it the ENIAC room, where the ENIAC was built, operating this calculator then.

STERN: What was your background that led you to that kind of work?

ALICE BURKS: Well, I had attended Oberlin College as a math major and I had had two and a half years and because I had no more money I asked my math professor there to help me get a job. He had a friend at Aberdeen and so he

arranged for me to go there and work there. That was in June. I arrived there in June of '42 so I was only there about a month. But my goal was to get back to college and when I heard of this project in Philadelphia, University of Pennsylvania, I immediately applied to go up there and I was sent up there.

STERN: Who else went with you at that junction?

ALICE BURKS: I can't remember. Having been there only less than a month and working in a room with one other person who didn't go, I really can't remember now.

A. W. BURKS: Didn't go? You mean at Aberdeen you were working?

ALICE BURKS: Yes.

A. W. BURKS: Yes, I don't even remember two girls in the room when I first met you, but there may have been.

ALICE BURKS: The day I arrived I certainly arrived alone.

A. W. BURKS: Yes, but I think when I went into that room and I was introduced to you, that there was another girl working there too. That's the way I remember it.

ALICE BURKS: I'm not even sure she came from Aberdeen.

A. W. BURKS: No, she may not have.

ALICE BURKS: I'm not sure anybody else came up from Aberdeen. I don't really remember.

A. W. BURKS: So I would assume . . .

STERN: Didn't Betty Snyder come from there? Didn't Betty Snyder come up from Aberdeen?

ALICE BURKS: If she did it would have been much later.

A. W. BURKS: Oh, I don't think so. I wouldn't have thought so.

ALICE BURKS: No I don't think anybody else came.

A. W. BURKS: I just don't remember that. She was a Philadelphia girl so I think she was one of those--I would guess that she was one of those . . .

ALICE BURKS: I think it was more an opportunity for anybody in Aberdeen, who cared to, to come up. And I can't recall anyone else that came.

A. W. BURKS: But of the group of young women who eventuated there, I guess it was about a hundred or so, most of those were recruited out of college. Kay McNulty, for example, was recruited after she graduated from her college in Philadelphia.

ALICE BURKS: And weren't they set up in the Towne School and actually given a training course of some sort?

A. W. BURKS: They were given--yes, there was a training course and at a certain stage Adele was put in charge of that training course.

ALICE BURKS: Yes, that's right.

A. W. BURKS: So often these were young women who had some knowledge of mathematics, but we trained them

further in how to integrate trajectories, whereas you never went through any training course.

ALICE BURKS: No, I didn't.

STERN: And Adele clearly came from Aberdeen?

A. W. BURKS: Well, because she was there with her husband.

STERN: She was there with her husband, right.

ALICE BURKS: She hadn't been working there though, I don't believe.

STERN: You first came to know her up in Philadelphia?

ALICE BURKS: That's right.

A. W. BURKS: Yes, you didn't know Herman in Aberdeen did you?

ALICE BURKS: No.

STERN: I see. So you essentially met each other at the same time that Herman came up on a regular basis?

ALICE BURKS: Well, no, Herman came later.

A. W. BURKS: I think Herman probably first came later.

ALICE BURKS: Because there was no group.

A. W. BURKS: Yes, they sent the girls up first.

ALICE BURKS: John Holberton was in charge of our program.

A. W. BURKS: But John came later than you did.

ALICE BURKS: He came later too, right.

STERN: I know that one of your papers talks about the fact that the women who did this work were doing it out of war necessity; that is, it made sense to have women do this because men were so scarce.

A. W. BURKS: Right.

STERN: But it strikes me as particularly unusual that they were all women, that there weren't any men associated with this. Did that strike you as odd at the time?

ALICE BURKS: Well, not at the time. There were very few men around and at that time, of course, in our culture the men did have the supervisory jobs.

STERN: Was this regarded as a kind of clerical position? Because it seems to me it required some technical expertise.

A. W. BURKS: No. We might throw a little light on it by looking at, say, the wiring people, who were technical people. There was one young man. But most of them were women, because the men were subject to the draft unless they were needed for technical work. And essentially what this enterprise consisted in was sending people like Adele out to the colleges, where she recruited young women who had a major in math, telling them: "come and we'll teach you how to become a computer."

STERN: Well, it's interesting because there is a letter on file at the Moore School from a man asking to be considered for this position even though he knows that it's a job mostly for women, he feels that he is interested in detail and he has some mathematical experience and would they please consider him for this job, which I found particularly interesting that it really was viewed as a female occupation.

ALICE BURKS: And he was turned down, was he?

STERN: Well there's no correspondence . . .

A. W. BURKS: I can well imagine the attitude would be: "Well, it's going to be a problem to have one man among all of these women. Let's not complicate things."

STERN: He probably wouldn't have thought so.

A. W. BURKS: No.

STERN: It seemed to be a natural from there that female computers would become programmers, which happened in many instances.

A. W. BURKS: Yes.

STERN: But then programming gradually became a male-dominated field and I was just curious about that kind of transition. I wanted to shed light on that. To get back to the discussion we were having about Goldstine getting the proposal from Mauchly.

A. W. BURKS: Yes, okay. Now I can remember a conversation, more than one, in which John was saying, "Well you

know, Herman, the way to calculate firing tables is to do it electronically, not on the differential analyzer and not with young women operating desk machines." He probably said computers operating desk machines because we talked with--we called these people computers. And Herman saying, "Well give me a proposal, John." And John saying, "Well, I wrote such a proposal quite a while ago and gave it to Grist." And they went off to see Grist to get this proposal but Grist was not able to produce it, at least immediately, and the impression I had at that time was that Grist had either misfiled it or thrown it away. I mean, in other words, he didn't have it anymore, and it actually turned out at the trial that the proposal was found in Mauchly's file with a note from Grist. So I think Grist had given it back to John, and John had forgotten that he got it back. That must have been what actually happened, but at the time it was my definite impression that Brainerd couldn't find it and John was sure he had not returned it. That I remember very definitely. But John's memory was not totally good on some of those matters.

ALICE BURKS: What did the note say?

A. W. BURKS: The note said this is a good idea, that sometime it will be appropriate to do it. Those weren't the words, but it said it's a good idea, but it's not time yet.

STERN: Now, but Goldstine was immediately interested in the argument, is that correct?

A. W. BURKS: He was immediately interested so he (to my knowledge they didn't find that proposal, the '42 proposal at that time) and so he said to write a new one. I can remember John and Pres going off and working on this. I can remember the room. It was in the southwest corner of the building that they worked on this, and at one stage I think they worked most of the night to finish it up, because in the morning Goldstine picked them up and they went out to Brainerd's house -- Brainerd lived out on the mainline out near Paoli. Herman picked them up in his car and they went out to Brainerd's house and picked up Brainerd and then drove down to Aberdeen to make the presentation. And I can remember Mauchly telling me the next day that he and Pres had continued to write on this proposal while Brainerd and Goldstine were making the presentation of the part that they had already written.

STERN: Now in your paper *From ENIAC to the Stored Program Computer* you make a statement in which you said that Goldstine persuaded the Ballistics Research Lab to fund a proposal.

A. W. BURKS: That's my judgement.

STERN: That's your judgement. Now you don't mention Brainerd in that, so you regard Goldstine as the prime impetus behind this?

A. W. BURKS: Well, that's a good question. Why did I say that? Brainerd and Goldstine made the presentation. I guess I left Brainerd out of that attribution of responsibility because he had not pushed that proposal in the first place. Also I can remember an idea that Pres had, it may have been connected with that sensitive amplifier he made to measure the field in our first research project of how to make a submarine detector, to detect it magnetically, by sending a magnetic field down from a plane. No, he was going to use a natural magnetic field but he thought he could make a detector that would be sensitive enough, if flown with an airplane above the water, to detect a submarine. I can remember his having John Pedley in the shop make some models, and running experiments probably with that same apparatus that we had used for that first research project. I was involved with a discussion of this proposal with Mauchly, who at that time had the office that Brainerd later came to occupy (the first office to the west of the main office, on the south side near the top of the stairs.) And I can remember John getting down a book, a German book that told something about magnetics in this connection. And so Pres had this idea, and Pres and John and I were involved, at least in the discussion. I think a proposal must have been written. I'm not sure about that, but at least Eckert and Mauchly explained the idea to Brainerd and suggested that he ask the government to support it, but we never felt that he did. Now maybe he did, but I can't recall his saying, "Well, you know I went down and I..." Well maybe he did ask them -- I'm hedging because I don't remember all the details. But it was certainly our impression that he had not pushed that idea, and so we had kind of formed the tentative conclusion, let's say Mauchly and myself, that Brainerd didn't push these ideas, and since he hadn't pushed the computer in the first instance, I guess it was my natural reaction to think that he hadn't pushed it at all. Now that's not to say that he didn't do his best to sell it when he got down there. But I always felt that it was the confidence that Gillon and the

mathematician who was the chairman of the advisory board (Veblen, of the Institute for Advanced Study) had in Goldstine -- that was the important factor. But that was just my own subjective judgment.

STERN: Well, there is this talk that Brainerd's job was on the line relative to the ENIAC. That is, he went out on a limb and if the ENIAC project didn't succeed he may well have lost his job. Do you think that such a thing was possible at the time?

A. W. BURKS: I just don't know. I never had that impression at the time, but then of course I would not be privy to any discussions between Brainerd and Dean Pender but when I heard that story a few weeks ago (when I was talking with Herman Goldstine), it came as a surprise to me.

STERN: Now, as a result of this proposal, which was approved in the Spring of '43, Mauchly became principal consultant, Eckert became chief engineer. Brainerd was project supervisor. At what point did you get involved?

A. W. BURKS: Well I was hired as one of the first workers. That is, I knew about this thing and I became interested. John was very encouraging, and because of the relevance of this to my background, that sort of structure appealed to me. And I had talked with John, and John was very knowledgeable about mechanical calculators as to how they worked and how they multiplied and divided and things like that. I naturally wanted to work on it and so I asked to work it. I was hired almost as soon as anyone.

STERN: Anyone else hired around the same period?

A. W. BURKS: Yes. When we started around June 1, I don't remember the exact date, there were 3 or 4 of us in the laboratory besides Eckert and Mauchly.

STERN: And you can't recall the names, I assume.

A. W. BURKS: No.

STERN: What was it like initially working on this project?

A. W. BURKS: Well, we were in that large back room and the first task was to develop reliable counters. We had certain information: Crawford's master's thesis from MIT and a National Cash Register report. I found out later, I believe, that the National Cash Register activity was aligned with the MIT activity in electronic digital computing. I didn't know that at the time. But here was this report and it gave the design of a thyratron counter, that's a gas tube counter. Pres's idea was to take the counting circuits that were known and build them and see if we could make them reliable at a hundred thousand pulses per second. This one hundred thousand had been set up as a goal, and it was not the case that there were reliable counters that operated at a hundred thousand. There were counters that operated at a hundred thousand, but they were used by physicists to count cosmic rays and if they missed a count now and then it was only a small percentage error. So Pres gave me the task of building and testing this thyratron counter, which I did, and then he would suggest changes to make in the resistors or the capacitor values or modify the circuit in this way or other. And the idea was to see whether we could get this circuit to work at a hundred thousand. Now the problem with the thyratron or gas tube is that while it fires easily because the ionization helps it fire, it's difficult to quench, to turn it off, because the gas is still flowing. We modified the circuit in order to try to make the turn-off work faster, and so forth. So I would work with circuit and get it working and put in some parameters and make measurements and diddle with it and then repeat this process. In the end we were never able to get that circuit to work reliably at a hundred thousand. I could make it work at a hundred thousand or more but it just wasn't reliable, and so that circuit was abandoned. In the meantime Pres was working with Frank Mural -- who had come on fairly early, I don't remember just when -- on the counter which was based on a circuit of Grosdorf, of RCA, and I can remember Pres and John visiting RCA and talking with John Rajchman. Generally John would tell me what went on at such places. It's not that Pres didn't want to or anything like that, but since I knew John well and I saw him a lot, we would have these conversations. I can remember for example his telling me about these circuits and the earlier computer project that RCA had. I can also remember getting the impression, probably directly from Pres, that Pres was not impressed with Rajchman as an engineer.

STERN: Well RCA was given the opportunity to work with Moore School on this project and turned it down.

A. W. BURKS: That I didn't know at the time. I remember, if I may interrupt, another fact was that of course it was very important to know how well the tubes hold up, and I can remember Pres saying, "We went up there and we asked these people if we have 20 thousand tubes, how long will they last?" And they couldn't tell us because they were interested only in tubes for radio, and from their point of view the radio was going to be obsolete soon anyhow. They couldn't give us those figures, but the telephone people in contrast could tell you that a relay would fail once every--and Pres would give the figure as 100,000 or a million operations - I don't remember what the figure was, but it was a very large figure and I remember that interesting contrast that Pres made.

STERN: All of this information that you got from NCR on the thyatron and from RCA came through the filtering agent of the National Defense Research Committee. Now there are documents that indicate that NDRC did not look favorably upon the ENIAC project; that they regarded it as naive. Were you aware of this sort of feeling on the parts of the so-called scientific elite who sat on this committee?

A. W. BURKS: I don't remember either way, Nancy. I mean I don't remember being aware of it.

STERN: You don't remember having difficulty getting papers on the thyatron that you were working on?

A. W. BURKS: No. I wouldn't have been involved in that level. I assume that that was Brainerd and Goldstine and all I remember is John or Pres saying, "Well, we have this thesis from MIT" and I was actually given a copy of the NCR report so that I could read it, and I read it and I looked at the diagram.

STERN: Because in your Rackham [Graduate School at UM] talk you said something about the fact that the older engineers said the ENIAC wouldn't work.

A. W. BURKS: I was referring to the Moore School.

STERN: You were referring to the Moore School?

A. W. BURKS: Yes.

STERN: Like who? Can you give me some names of people who felt that way?

A. W. BURKS: Faucett, the man I taught under, was certainly skeptical, and I had the feeling that Pender was somewhat skeptical, though I don't recall his specifically expressing skepticism.

STERN: How about Chambers? How did he react to this?

A. W. BURKS: I can remember the first meeting of the engineers. We engineers had relatively frequent meetings. Chambers was at the first meeting. He made suggestions and seemed sympathetic, so I don't think he was skeptical.

STERN: No. I don't think he was. In fact, in many ways I'm surprised that he was not put in charge of that project.

A. W. BURKS: Oh, well now you see the setup was that at the beginning of the war, or when they saw the war was coming to the United States, I don't know the exact date, Pender picked two people for the two functions; one was the function of supervising the auxiliary educational activities, such as the course I took, and a course for Navy officers and later the ASTP.

STERN: ASTP?

A. W. BURKS: The Army Specialized Training Program. There were also evening courses. I remember going out to Philco and teaching an evening course, and all of that was managed by Chambers. So Chambers was put in charge of

the auxiliary educational activities associated with the war and Brainerd was the designated director of research. So the hierarchy was already established.

STERN: I'm interested in knowing what these meetings were like that you mentioned. Eckert was clearly in charge, you said. Now were these open discussions or was it the kind of thing where Eckert said, "You do this and you do that," and it worked in that fashion?

A. W. BURKS: Well, they were always open and in the end assignments would be made, maybe not at the meeting but, sometimes in the meeting. In fact, I have a record in my notebook that at a certain stage when we were near finishing--at this meeting it was said that A would do this and B would do this and so forth. But they were mostly meetings to discuss what we had accomplished and what to do next. And anyone could speak and give ideas. Eckert would speak the most and Mauchly would speak the next most. Typically Brainerd was not there and after the first two or three meetings Chambers was not there. So Eckert and Mauchly and the rest of the engineers, including me, were there through most of them.

STERN: But you would say it was a free and open exchange of ideas?

A. W. BURKS: Yes.

STERN: And people contributed and Eckert didn't necessarily dominate the conversation?

A. W. BURKS: Well, I would say he did most of the talking and he was clearly technically in charge.

STERN: Yes.

A. W. BURKS: But I want to make it clear that anybody could express any idea and this would be discussed reasonably.

STERN: It would be reasonably discussed?

A. W. BURKS: Oh, sure. There would never be any suppression of any idea or putting anybody down because they didn't like the idea.

STERN: Initially you did the research on the thyratron counters and decided that it was not feasible for your use. What happened then? What did you do after that?

A. W. BURKS: Well, then I got involved in the accumulator some and in the high-speed multiplier. Sharpless had done some preliminary work on the high-speed multiplier, and I don't remember the sequence of whether I first worked on the multiplier or first on the accumulator.

STERN: Who did most of the work on the ring counter?

A. W. BURKS: The ring counter that we actually adopted was designed by Pres starting with the Grosdorf circuit from RCA. For a decade counter, you would have ten flip-flops. And Frank Mural built the circuit and did the testing under quite close supervision by Pres, and I can remember Pres spending hours with Frank. Mauchly was involved mostly in discussions with Pres. The key concept that made that counter reliable was an idea that Pres had, which was to hook all of these flip-flops to a common cathode circuit in such a way that it would be impossible for more than one flip-flop to be in the representative state. I'm sure that was Pres's idea.

STERN: When was that idea in place? The ring counter concept?

A. W. BURKS: Well, the counter was, as I recall, working reliably up to 150,000, maybe 200,000 pulses per second. Well, "up to" means that at 150,000 it began to fail but it was working reliably at 100,000. And it would work with a wide variation of voltages, because a pulse standardizer had been put on the front end of it, and it would work with a

significant variation of parameters such as resistors and capacitors. As I recall, Pres was satisfied with it late in the fall of '43 and, of course, it was the key element. If the counter didn't work, we didn't have the basic arithmetic and storage device of the system.

STERN: And then at that point you'd worked on the accumulators you said.

A. W. BURKS: Well, no, you see these things started gradually in a way, but they would start early and you would work on them a while and then be shifted. So I don't--by then I would expect I was working on both the accumulator and the multiplier. I was certainly working on the multiplier by late fall of '43. I must have been working on the accumulator some by then. I'm not positive.

STERN: I have some difficulty determining what Mauchly's relationship to the group was at that time. Did he actually make engineering contributions? Was he more concerned with roundoff errors? What actually did he do at this point?

TAPE 2/SIDE 1

A. W. BURKS: It is my impression when we started out that in the laboratory the hierarchy was that John was in charge in some overall sense and Pres was the chief engineer. I didn't expect Brainerd to be in the laboratory or even to be directing this, because I had worked on research projects for a year and a half and in general Brainerd would get the project and give us initial instructions and then leave us to work alone. So when there were technical questions to be answered I would go to John or Pres or some of the other engineers around there. I might ask Chambers, for example. In other words I'm making the point that I had never, in my work, gotten any substantial amount of technical supervision from Brainerd, and so when we started on the ENIAC I didn't expect Grist to give technical supervision. And I'm not saying that's inappropriate. He was a director of research for all projects, and it was my perspective that John was in charge and Pres was the chief engineer, and then the rest of the engineers were on a par underneath. I might have gotten some of that impression just from my attitude toward John and his attitude toward me.

STERN: But John was clearly on top of all of the engineering developments that were going on at that time?

A. W. BURKS: Yes. But it also soon became clear that John did not accept the kind of administrative responsibility of scheduling things and seeing that things were done and so forth that was needed, and that Pres did. I can remember some change of announcement of titles of these people and I understood Brainerd to be saying in effect, "Now I'm putting Eckert in charge and Mauchly is the consultant." Now, how this was reflected in their titles or what Brainerd told them, I don't know. This may have been just my perspective. But that's at least the way I viewed it and not just as what was going on, but what Brainerd wanted to go on.

STERN: So that at some point after the project began it was at least Brainerd's perception that Mauchly wasn't doing what he had intended him to do?

A. W. BURKS: That was my perception of Brainerd's view.

STERN: I see.

A. W. BURKS: But that didn't mean that John was not working hard, it didn't mean that he wasn't making technical contributions. It meant that he wasn't doing the administrative things of making sure that an engineer knew what he was doing; of working out the schedules of deciding what is it we are going to do next. In other words it was administrative decisions John was not making and Pres was making. But John was still very much there on the technical side. I would more typically ask John technical questions about what to do, and get more information from John than I would from Pres, just because I knew him better and because in a way John was more accessible. You could always go up to him and get an answer, and Pres might be deeply involved in something and you didn't want to interrupt him.

STERN: What about what I would call the logical design features of the ENIAC? You would seem to be the most

appropriate person to make some of those decisions. Did you?

A. W. BURKS: Well what kinds of decisions are you talking about?

STERN: I'm talking about whether to have a square rooter, for example.

A. W. BURKS: No, I would not have been the person to make that decision. That was more Mauchly and Eckert, though I would have been involved in the discussions.

STERN: You were involved in those discussions.

A. W. BURKS: In general I was involved in these discussions, yes.

STERN: What about the techniques for round off that were decided upon. Who did most of that kind of work?

A. W. BURKS: Well, I think the best way is just to kind of tell the sequence as I saw it. Mauchly was the one who initially thought about the round-off problem because he had done computations and he knew it was a problem. I can remember his getting papers on it, and these papers were by astronomers, because the astronomers had done the most sophisticated calculations. We knew -- I guess everybody knows -- you can round off by throwing a five in, and then the question is - how would the round off error grow? And that was a question that had to do with how many decimal digits we were going to store and so we would be discussing this question. Grist hired Professor Rademacher of the Mathematics Department as a consultant. And so here was Rademacher and here was Grist saying, "Now what can you do?" And it was John's idea, as I recall, "Well, let's get Rademacher to calculate what happened to the round off error." There was a session in which we explained to Rademacher what the problem was. I was involved in that explanation. Rademacher then went off and did some research and came back.

There were two factors here, to give the details: one factor is the round off error, and the more steps you take the

more round off error there is. But of course it doesn't grow linearly, it grows by some complicated law which is a function of the equations, and he used the calculus of variations to work on that aspect of it. On the other hand, the smaller interval you take the less the truncation error. Intuitively it's clear that there is going to be a compromise somewhere in between. That is, with very small increments the truncation error will go to zero but you can have lots and lots of steps and the round off error will eat you up, but with very large intervals of integration--considering the fact that the ENIAC is not terribly intelligent and cannot be as sophisticated as say the young women computers were--the truncation error's going to destroy your accuracy. It was Rademacher's task to find out where the minimum ever occurred when the machine would be calculating trajectories. I can remember his saying that you could make the interval as small as you like and the round-off error would not increase. Cunningham of Aberdeen was extensively involved with planning the ENIAC. Sometimes he came to the Moore School, sometimes we went down and talked with him. He came to the Moore School and we had a long session in the inner classroom, and we couldn't figure out where Rademacher had made a mistake. In the end Cunningham and I found out what mistake he had made. As I recall, it was essentially that he was treating the ENIAC as if it were floating point machine, and of course it wasn't a floating point machine. He was also assuming that you could keep as big an exponent as you needed but in a floating-point machine you can't do that. We pointed out to him, these things, and he realized that he was wrong and he went back and worked on the problem again. But we had to choose the number of digits right away. So the decision to make it 10 decimal digits, starting from the accuracy of firing tables, was made, I'd say primarily by Cunningham and Mauchly.

STERN: Goldstine was involved?

A. W. BURKS: Goldstine would have been involved if he was around. As I remember it from the time he came and was put in charge which would have been the fall of '42 until sometime in '43 he was not primarily assigned to the Moore School, so he would be in and out. Then I can remember later on and it must have been sometime in '43 John telling me that Herman is now going to be assigned full time to the ENIAC and that Adele would be there and she would start working full time. That may have been when she started to recruit in '43. Do you know when she started?

STERN: No, I do not.

A. W. BURKS: So it could be that her role in that group came a year later and I can remember, honey, about the summer of '43 it must have been, you thought you'd go back to work and you attended a few sessions. You weren't a student in the course, you were helping Adele in the course, and after a few sessions you decided that you'd rather go back to school and finish up your schooling. And so you dropped out of that job and then went continuously on to school until you finished. So it must have been in the summer of '43 that Adele became very active in the business of training these people. Now she may have done things before, I just don't remember.

STERN: What was the spirit of the group? Was it dominated by the wartime cooperation that you frequently read about.

A. W. BURKS: Oh yes. I mean, everybody felt this was terribly important to the war and that we get it done. So nobody hesitated to work all day and then all evening or come in on the weekends if necessary. Typically we worked six days a week and didn't come in on Sundays, I recall, until things later became very busy and then we would come in on Sundays.

STERN: How did this compare, for example, with the Signal Corps contract? Was it a different kind of atmosphere, a different kind of feeling about the war?

A. W. BURKS: Well, I don't know that it was different except that, of course, this was a much larger group. In the case of the Signal Corps contract on the antennas it was Mauchly and myself, and Brad Shepherd built some of the circuits. We built a framework on the roof that would hold an antenna and then in the shop they built a big wooden mold. Pedley stretched wires across this mold to make wire paraboloid antennas. Some factory in Philadelphia made the solid aluminum reflectors. John and I had one or two junior people, technicians you might call them, that would help us mount these things and help us take readings.

STERN: What I'm trying to get at . . .

A. W. BURKS: So anyway my point, Nancy, if I may interrupt, is that all the projects before ENIAC were typically fairly small with not more than half a dozen people. With the case of the ENIAC it got to be a dozen engineers or near-engineers, and I don't know, twenty wire people and so forth, and it was obviously much bigger and better organized and so it was a bigger thing.

STERN: Was there any sense of here are a group of relatively young men setting out to do something revolutionary. Was that part of the atmosphere?

A. W. BURKS: Yes, there was certainly the idea that this was novel and we were ahead of everyone else.

STERN: So you had that compounded with the wartime thing, it must have been very exciting.

A. W. BURKS: Yes. It was the first general-purpose electronic computer.

STERN: So there was no question that people realized the importance of this at the time -- the people working on it anyway?

A. W. BURKS: Yes, we engineers definitely did.

STERN: Now, you did work on the multiplier and the divider/square rooter, too, is that correct?

A. W. BURKS: Yes.

STERN: Did you work on your own? Were you given direction by Eckert? How did that work?

A. W. BURKS: Well, some of both. In the case of the high-speed multiplier, Kite Sharpless had done some preliminary work. He had made a cross section of the multiplication table to test its characteristics. He had drawn up the resistor matrix connection appropriate to the multiplication table. And then Pres pulled him off of that and I can remember Pres saying, "Well, now I want Kite to work on something else and I'd like you to take over the multiplier and Kite will tell you what it is." So I sat down with Kite and he told me what he had done and what the general nature of it was that I didn't already know -- I'd already known a lot about it from general discussions with John. After that it was then my responsibility and I would ask questions of John or Pres, more typically John, or ask them to look at what I did, though it was not easy to get people to look at detailed matters, and so I would just rely on my own decision or ask, say, Kite to come and check it to see if that's the way it was. In the case of the divider/square rooter I was put in charge of it, but Chuan Chu had come on board and he was assigned to me, and so I outlined how it was to work to him -- what the algorithm was, in effect. It was to be the same basic algorithm of a repeated subtraction to overdraft and then instead of restoration, going back in the other direction - adding instead of subtracting. Alternately subtracting in one position and adding in the next position, subtracting in the next decimal position, adding and so forth. So I explained all this to Chuan and gave him general guidance of how to do the circuits for this; we'd have to have flip-flops to control whether you're adding or subtracting and so forth and so on. And then he would work a while and he'd show me what he had done and ask me about it, and I'd make suggestions. In the end he was completely knowledgeable about the divider/square rooter and did the testing pretty much by himself.

STERN: But essentially the multiplier design was yours?

A. W. BURKS: Well, I'm not sure what you mean by "mine." The decision to make a high-speed multiplier, to use the method of multiplying one decimal digit of one number by the whole number in one addition time, was a decision made by Pres and John primarily. This method comes from desk calculators and Mauchly was the person who knew the technology of desk calculators and IBM machines. So that decision they would have made, which would involve having selectors and shifters. We call those two numbers the multiplier and multiplicand, the two numbers you were multiplying -- for short, "ier" and "icand." The selector for the "ier" driving vacuum tubes in the resistor table matrix

and the output going to a left-hand selector and a right-hand selector, then to a left-hand shifter and a right-hand shifter. That general design was fixed at the time I started.

STERN: I see.

A. W. BURKS: -- And is the electronic way of doing what was done in some mechanical calculators and in the IBM 601.

STERN: It was at this time, and I'm talking the 1944 period, that Brainerd prepared a report for the Applied Mathematics Panel. Were you familiar with that report?

A. W. BURKS: Well, I remember the various incidents involved in connection with that.

STERN: What interests me is how would Brainerd know enough about the design of this, considering the fact that he did not participate in the discussions, to prepare such a report?

A. W. BURKS: Well, first of all we did prepare reports of the ENIAC project every six months.

STERN: Who prepared those reports?

A. W. BURKS: The first one--various people were assigned to write chapters or pieces of it -- and I think I was assigned to coordinate them. Then after that I was in charge of the reports, so to speak. Whether I had a formal assignment from Brainerd, I don't remember, but it was clear that it was my job to be responsible for them. I discussed with him what was to go in, of course, but then I would carry it out and did most of the writing. What was written went to Brainerd and he looked at it. He had some information about that and I don't want to give the impression that he never came down or anything like that. But he was not close enough to it to make the decisions and give technical supervision, though he had considerable general knowledge about what was going on. Still it

was my judgement and John's judgement and Pres's judgement that he really didn't know enough to write the Applied Mathematics Panel report, and I can remember John complaining rather bitterly that Brainerd had been asked by the panel to write this report and that John and Pres didn't think it was fair that Brainerd alone should be writing the report because of the credit implications -- and that they were skeptical that he even knew enough to write it and I shared that skepticism.

STERN: I see. Now, it did have to be rewritten according to what Eckert has told me.

A. W. BURKS: Well, what happened was that there was a big brouhaha about that and, as I recall, John and Pres talked with Herman about it. And I think they went to Pender and complained, and in the end it was worked out that it would be a joint report. Was Herman an author then?

STERN: Yes.

A. W. BURKS: Yes, in fact it seems to me they went out to Pender's house one Sunday to talk about that, but I'm not positive. But as a result of their complaints it was in the end decided that a report would be written by all four. Now, who wrote each part, I don't know.

STERN: Mauchly during this period did some work down at the Naval Ordinance Laboratory.

A. W. BURKS: He was a consultant and he went down there occasionally, I don't remember how often but I remember his telling me that he was a consultant, and I remember his being gone.

STERN: Did he speak to Atanasoff at that time?

A. W. BURKS: As I recall, he told me that this man Atanasoff, whose computer he had visited, was in charge of this laboratory and was making him a consultant. That's the way I remember it.

STERN: So that at least one of his motives for going down there was to find out more about Atanasoff's work?

Would you say that's correct?

A. W. BURKS: No. I don't remember whether he said that Atanasoff asked him or whether he said that he asked Atanasoff. I guess all I remember is that he said that he could consult for the government through Atanasoff's operation and was going to do so - one day a week was the original plan maybe, but whether he held to that I don't remember.

STERN: But Atanasoff was not working on computers at that time when he was working for the government?

A. W. BURKS: No. No, I didn't understand that this was a computer consultancy at all. I don't remember what John told me he'd be working on.

STERN: What I'm curious about at this point is Mauchly's perception of whether he would be getting more information about electronic computing devices from Atanasoff.

A. W. BURKS: I never got the impression that when John was going to do this consulting job he expected to get more knowledge about computers. Indeed, John's attitude was that he and Pres already knew much more about computers than Atanasoff did. So it was not that he was to be consulting on computers. Now at some stage Pres and John did talk with Atanasoff about mercury delay lines and he might have said that this had something to do with mercury-delay lines, but I don't remember that.

STERN: At the time do you think that if someone were to have asked Mauchly in 1943, 1944 whether his ideas were based to any extent on Atanasoff's work, do you think he would have said yes at that time? It's a hard question.

A. W. BURKS: I don't know. He had told us about the calculator and I knew roughly how it worked. That is, it had

the drum and it had the electronics for calculation and it was a machine for solving simultaneous algebraic equations. That he mentioned on several occasions. The question of what ideas he got was never raised.

STERN: It just simply was not raised?

A. W. BURKS: No.

STERN: Getting back to the 1944 period, in September of '44 Eckert sent around a note asking all engineers working on a project to sign a patent release form.

A. W. BURKS: Yes.

STERN: Was it your perception that it was an official request from the Moore School or that it was an Eckert request?

A. W. BURKS: My perception was that Pres was acting in his role as chief engineer as a representative of the Moore School.

STERN: And that therefore . . .

A. W. BURKS: It was on the Moore School stationery, I believe.

STERN: It was on the Moore School stationery. Now Eckert was really going to apply for the patent on his own behalf and not assign that patent to the Moore School. Were you aware of that at the time? At least that's what happened eventually.

A. W. BURKS: Well, you don't mean to be talking about the patent as a whole but the private rights. Because it was

always clear that the government had a royalty-free license. It was my understanding -- and I certainly got some of this from Mauchly and I may have got some from Herman -- that the private patent rights were to go to Eckert and Mauchly and this had somehow been settled early. That was my impression at that time.

STERN: So that your signing of this, was it based on the belief that you were doing something that the Moore School wanted you to do? Because there was some controversy about this during the trial.

A. W. BURKS: Yes. Well, now the letter specifically asked what patentable ideas we had and I had--I was naive certainly on this and had no idea of what a patent really was. I knew that they existed and it didn't occur to me that any of my ideas were of a patentable nature. I thought that Eckert and Mauchly were entitled to the patent -- [that it] should be in their name because it was their idea to build the ENIAC and they had the general overall design and they were in charge of the developing.

STERN: Yes.

A. W. BURKS: In particular, at that time it never occurred to me that the logical design would have been a patentable item, and the idea that Seymour Yuter later put forward -- that the whole group could do something and be inventive - - that idea would never have occurred to me.

STERN: Brainerd was upset about the form coming from Eckert.

A. W. BURKS: That's the way I heard it later. He never told me that he was upset.

STERN: So that at the time you were never aware of any difficulty between . . .

A. W. BURKS: No, I was not aware that there was any question at all about this or that it was Pres's own decision then and had not been approved by the Moore School. I would not have know that at all. In fact, I would have

assumed just the opposite because Brainerd certainly knew that the letter went out at some stage. So I assumed that it must have been all right with him.

STERN: Shortly thereafter Brainerd resigned, at least from the EDVAC which came about right after that, the patent release form was in September, the EDVAC was a supplement to the ENIAC contract in October of that same year. And shortly thereafter the best I can pinpoint was by January of '45 Reid Warren was now project supervisor.

A. W. BURKS: Not for the ENIAC, but for the EDVAC.

STERN: Yes. I don't know the precise date for that change over, but it was for somewhere in that period.

Now, let me give you the two sides and then you can tell me your view in this. Brainerd claims he resigned--he said at the trial he resigned in protest -- because of this patent release matter, because the Moore School did not pursue the right to have the patent assigned to them. That's why he resigned is what he claimed. Eckert on the other hand says that he threatened to resign if Brainerd wasn't removed from the EDVAC project. Can you shed any light on what happened during that period?

A. W. BURKS: I did not know those things. The last fact about Pres I had not heard before you mentioned it just now. I remember very distinctly that Reid Warren was put in charge of EDVAC. That was perceived by us, and it was presented to us that Brainerd had too much to do. Certainly I was not told that Brainerd had resigned on any principles. Now there are different principles involved here in the patent, too. One was the question of whether Eckert and Mauchly should have the private rights or whether the University of Pennsylvania should have the private rights. The other was the question as to whether that letter should have been sent out by Pres and in particular whether, for example, all engineers should have been called together with Brainerd saying, "Well, here's the patent situation and this is how patents work." In other words they could have given us all kinds of explanations before such a letter came and they did not. I did not know that Brainerd had resigned until I heard that recently.

STERN: But based on what you're telling me there was some controversy over this patent matter in late '44?

A. W. BURKS: Well, there was definitely a controversy. I understood the controversy to be a controversy between Eckert and Mauchly and the management of the University. I can remember Eckert and Mauchly going with Goldstine to McClelland's office over this controversy.

TAPE 2/SIDE 2

STERN: McClelland being the president of the university at that time?

A. W. BURKS: I forget his title, but he was the chief officer. That was an issue not as to whose names the patent should be in. That was never an issue to my knowledge at that time, but a question of whether the university or Eckert and Mauchly should own the patent rights. That's what my understanding of that controversy was. I knew that Brainerd was involved in these negotiations and that he would have gone with him probably. I would have expected him to be involved in these negotiations. I did not know that he had resigned on any principle concerning the outcome of these negotiations.

STERN: Well, that's his statement on the Honeywell trial that that's what happened.

A. W. BURKS: Sure.

STERN: But I wasn't clear from my reading of the transcript whether this was an issue that arose as a result of the trial or whether actually in '44 there was a patent controversy.

A. W. BURKS: Oh, there was definitely the controversy that I described between Eckert and Mauchly on the one hand and the university on the other as to the private rights of this patent.

STERN: And also you mentioned earlier that there was some concern as to whether it was proper for Eckert to send this notice around without first consulting the school itself.

A. W. BURKS: I didn't know that there was any concern in the Moore School administration that this was a problem. That never came to me. The first time I ever knew that was after I got to working with Yuter on the claims that Shaw and Sharpless and I had. He showed me documents: a report that Warshaw wrote after going down to Washington, and a report of the meeting between Eckert and Mauchly, Goldstine and von Neumann and Dean Pender, etc., at the Moore School in the spring of '47.

STERN: That was a different issue. That was over the stored program concept which is different from this one.

A. W. BURKS: Well, but at that time, as I recall, Nancy, in the minutes of that meeting the issue of this letter that Eckert had sent to the engineers came up at that time.

STERN: I see.

A. W. BURKS: That was the first time that I knew. So this would have been 1964 or later that I knew that Pender ever objected to that. I certainly didn't know it at the time.

STERN: Okay. Now we are talking about '44. This was the period in which von Neumann came to the Moore School.

A. W. BURKS: Yes, he probably came in August of '44, right.

STERN: There seems to be some difficulty with the date. The clearance letter reads September of '44.

A. W. BURKS: Well, that's not very relevant.

STERN: Would I be correct in assuming that von Neumann could have come to the Moore School before he had clearance to the Moore School?

A. W. BURKS: Oh, definitely. I think the clearance issue was this: von Neumann had the highest possible clearance because he was a consultant to Los Alamos and he was one of the few people who worked on the A-bomb who could leave Los Alamos. I mean most of the scientists of Los Alamos had to stay there for security reasons as they didn't want them going elsewhere and visiting. He was a chief consultant, a very high consultant to Aberdeen. Our security was a low grade security. It was the lowest as I recall, restricted maybe was the term. We were not supposed to talk about it in general but if we really needed to tell somebody about it in order to get some useful information, we as individuals had the right to do that. That's the way I remember the security so it was really quite a low grade of security. So there would never have been a question of von Neumann ever needing security to consult on this.

STERN: But your recollection is that he did come in August?

A. W. BURKS: Well, now the dating of August, I may be relying somewhat on what Herman said. But on the security I can remember something that is, I think, quite relevant to this. He came once at least and maybe twice, and then Herman told me, "Well, von Neumann is coming down and he's coming down with another mathematician from the Institute," and he gave me the name. I remember the name--the guy that came--there was clearance in that letter for two people, not just von Neumann. It was von Neumann and Alexander and Herman saying, "Well, you know Alexander is a communist and he marches in the Mayday parade. He's a nice guy and he's a bright guy and he's produced a lot of good mathematics, that's why he's a member of the Institute. But he's a communist," and kind of laughing about this and Herman's presenting it as a contrast between this man who was clearly a member of the establishment to be a member of the Institute for Advanced Study and a full professor and so forth. And yet he would go march with the workers in the Mayday parade. Now I didn't know any more, but it would be my immediate guess that they wanted the clearance because of Alexander's communist affiliations. And as I recall that letter it mentions Alexander's as well as with von Neumann and clearance for these people. And I remember very well the

meeting and I remember having this session in a room where we discussed these things and Alexander was trying to explain to me why a counter wouldn't work. Well let's start with a flip-flop. In the case of a flip-flop it has two states - it has two inputs and two states, the set state and the unset state. And if you push the set input when it's already set nothing happens. If you push the unset, similarly. So there's no real conflict in it's operation. A counter consists of taking those two inputs and connecting them to make one input. You want a reversal each time so if it's set it will unset and vice versa. Well, this does make a static conflict in the sense that's it like a teeter-totter. At a certain stage it's half on and half off, and what Alexander couldn't understand is why when it got half on and half off it didn't sit there and then go back. I tried to explain to him the dynamics of it, that the condensers are charged in such a way that it will go over. And he never understood, and he never seemed to accept this. So I remember that visit very vividly, and I remember very vividly that this was not the first visit by von Neumann because on the first visit, Alexander wasn't there.

STERN: What was your impression of von Neumann coming to the Moore School at this junction?

A. W. BURKS: What kind of impression do you mean?

STERN: Well, we talked about the fact that this was a young group of people working on the ENIAC and really going to prove that you've got something very exciting going on, and here's one of the most eminent mathematicians in the world coming to look at this.

A. W. BURKS: Yes, it was a great thing.

STERN: Because people have told me they didn't know much about von Neumann. His name didn't mean much to them, you know they're engineers, and you were a mathematician so you clearly knew about von Neumann.

A. W. BURKS: Well, Herman is the one who made the arrangements, and Herman certainly told us who von Neumann was if we didn't already know. I'm not sure I knew von Neumann's name before, Nancy. Von Neumann had

done work in mathematical logic and set-theory and I knew a little of set theory but whether I remembered von Neumann's name, I don't know. So Herman certainly let us know who von Neumann was and it was immediately clear that he was a distinguished person. So at least among the three of us, John and Pres and I, we were greatly impressed by this.

STERN: And when he came what were your impressions of him?

A. W. BURKS: That he was a very amiable and friendly person, cheerful, obviously very intelligent, and obviously interested in really understanding things down to details. I can remember one occasion when the five of us -- it was Goldstine, Mauchly, Eckert, Burks and von Neumann -- tried to explain how one aspect of the ENIAC worked, and I thought the explanation was clear. Von Neumann clearly couldn't understand, and in fact there was something missing. I mean we were assuming a lot that we hadn't yet told him.

STERN: But once . . .

A. W. BURKS: But he was not the kind of guy who would say, "Oh yes." That is, if there was something missing he told you "I don't understand it," he said.

STERN: But he understood enough about engineering to be able to pick up the concepts once you explained all of them?

A. W. BURKS: Yes. Now he would not have understood some of the details of the tubes, of course, and why we would be using this tube rather than another tube. So he was not current on engineering details, but he understood the concepts very well.

STERN: Now he came in the August-September period of '44 and in October the EDVAC supplement to the ENIAC contract was let by the government.

A. W. BURKS: Yes.

STERN: Would you say that it's a fair statement to say that his involvement played some part in the acceptance--in the Army's acceptance of the EDVAC contract. Do you think it would have succeeded without him being there at all?

A. W. BURKS: Well, I don't really know anything about the negotiations that went on. I assumed at the time and I guess I would still assume that the Army was impressed enough by what we were doing that they had confidence in us and that they would have given it, but I don't really know. I guess I had a lot of confidence that they had confidence in Goldstine, and they certainly showed confidence in what we were doing. And we had given them these reports, and we had observable results in the ENIAC project.

STERN: Yes.

A. W. BURKS: Again I'll put it this way; I wouldn't have thought at the time it would have been necessary to get von Neumann's approval in order to get that support, but I don't really know.

STERN: It would just seem to me that chronologically his involvement would have at least bolstered it.

A. W. BURKS: Oh it certainly wouldn't do any harm, but that it was needed I guess I would be skeptical. But I'm really guessing on that.

STERN: Can you tell me something about these so-called meetings with von Neumann over the EDVAC concept?

A. W. BURKS: Well, he would come and I guess the first couple meetings were mostly explaining how the ENIAC worked, but he would have been told about the EDVAC concept since it was under way at the time. And then he came back in March of '45 and he was there a couple of days or he came two days with a gap, I can't remember which,

two or three days. We went up into the classroom which was on the west side of the second floor -- the big, big classroom -- and talked at length about the plans for the EDVAC.

STERN: How did those meetings compare with the initial meetings on the ENIAC?

A. W. BURKS: Well, in the initial meetings on ENIAC, of course, it was more our telling von Neumann how the ENIAC worked. There were other discussions involved. I can remember, for example, his pointing out--this was the first time I remember hearing it--well maybe you don't really need a divider because by the iterative method it wouldn't take too many accumulators and the multiplier to do it.

STERN: Yes.

A. W. BURKS: And I think he may have made that point before we made the final decision for the divider, to make the divider for example. But in the end the divider/square rooter took only one panel anyhow and so it seemed a reasonable thing to do rather than make another accumulator.

STERN: You were talking about the free exchange of ideas over the ENIAC when you were first involved in the '43 period and I wondered how von Neumann's presence affected similar meetings relating to the EDVAC in '45.

A. W. BURKS: Well, there was a perfectly free exchange. Now the meetings were different in the sense that the engineer meetings involved all of the engineers and some of the technical people. For example, there was an insurance salesman who was a radio man and who was half way between an engineer and a wiring person. The von Neumann consulting meetings had von Neumann, Goldstine, Mauchly, Eckert, and Burks, and maybe some others like Sharpless sometimes or Adele some of the time. These were not meetings of the whole engineering staff. They were smallish meetings but the discussion was equally free. We didn't put any constraints on the discussion.

STERN: Well, we said that Eckert really dominated . . .

A. W. BURKS: The engineer meetings were, you might say, more directed towards specific tasks and so forth and so Pres would have been in charge of the meeting. In the case of consulting it was much more equal, with von Neumann talking a lot, Goldstine talking a lot, Pres and Mauchly talking a lot, and I talking some.

STERN: Yes.

A. W. BURKS: So I wouldn't say that Eckert was dominating the conversation while von Neumann was there -- but I don't think you'd say von Neumann was, either. I'd say it was pretty equal and Mauchly would speak a lot and Herman would speak a lot.

STERN: From the time that the EDVAC contract was let until the following year . . .

A. W. BURKS: If I may interrupt on this, Nancy, we were of course working--they were thinking about the EDVAC before the contract was let, and it was a matter of getting more money so we'd have more time to devote to it.

STERN: Did this take away from the work being done on the ENIAC? Could the ENIAC have been completed earlier if the EDVAC contract wasn't being done?

A. W. BURKS: No. By the time the EDVAC contract was let most all of the design of the ENIAC was fixed. Now the divider/square rooter probably wasn't done, and certain aspects of the second unit may not have been done and so forth. The constraints at this stage were more a matter of wiring it up and testing and things like that, and people were working terribly hard. People didn't take that much time away from the ENIAC to work on the EDVAC.

STERN: Did you take away that much time?

A. W. BURKS: No.

STERN: Your primary responsibility was still on the ENIAC?

A. W. BURKS: I was working on the ENIAC, yes.

STERN: And aside from these meetings with von Neumann did you spend very much additional time working on the EDVAC?

A. W. BURKS: No. Now, we've got to be careful about the role of John, you see, because in this period, roughly the academic year '44 to '45, as I understood it at the time John was only to have consulting responsibilities for the ENIAC, because he was to be working on the ENIAC patents.

STERN: John Mauchly?

A. W. BURKS: John Mauchly. And while working on the ENIAC patents, as I say, he was working on other ideas as well, including EDVAC ideas. He was thinking some about it. There wasn't that much done on the EDVAC before the meetings of March '45. There wasn't so much done that it subtracted from the ENIAC.

STERN: The ENIAC's first problem was in the fall/winter of 1945. The Los Alamos problem.

A. W. BURKS: Yes, I date it as having started on the machine in December, but that may not be quite right.

STERN: December of '45.

A. W. BURKS: Yes.

STERN: Were you involved on that test?

A. W. BURKS: Well, in various ways. Not centrally. I can remember Metropolis and Frankel coming and a session in which Holberton was there and Mauchly and Eckert and Goldstine and they were explaining to us their task. Now, they made it very clear that they couldn't tell us what the equations were. And then we were to explain something about the ENIAC. So we explained things about the ENIAC and then we gave them reports. So I was involved in that initial, you might say, "education" of them.

STERN: Excuse me, if I may interrupt a minute. Was von Neumann responsible for Metropolis and Frankel's coming to the Moore School to work on this project?

A. W. BURKS: Yes. My knowledge of that would have been what Goldstine said: something like, "Well, von Neumann knows these people that have this problem, and they're going to put it on the machine."

STERN: One of the articles I worked on was critiqued and someone -- and I don't know who -- said that Dana Mitchell was responsible for Metropolis and Frankel coming to the Moore School. Do you know anything about that.

A. W. BURKS: Who is Dana Mitchell?

STERN: I don't know who Dana Mitchell is. I just had this [comment].

A. W. BURKS: Well, responsibility, of course, is a complicated matter. It was my understanding at the time and it's still my understanding that Herman consulted with von Neumann and von Neumann was a consultant [and] he knew they had these problems. Herman didn't know anybody at Los Alamos other than von Neumann. I can remember Herman saying, "Well, von Neumann goes out to this place," without telling me what the place did, and how much Herman knew what the place did I don't know. But it was certainly my impression that it was through von Neumann that Metropolis and Frankel were coming to put this problem on, and that Goldstine had arranged it with von Neumann. Now, this other person may well have played a role at Los Alamos or played another role that I don't know

about.

STERN: By December of '45 the atomic bomb had already been dropped.

A. W. BURKS: Right. Okay, so we knew what Los Alamos did but now when was the plan made that they should come, do you know?

STERN: It was the fall.

A. W. BURKS: Yes, September. Okay. So we knew that there was the atom bomb, I had forgotten the timing there. So then we knew what von Neumann had been doing. Before we knew he was doing something mysterious. By then we did know what he was doing, right.

STERN: I was just curious. Was your perception that the ENIAC was complete at that point?

A. W. BURKS: No.

STERN: Were you testing the machine with this particular problem or was the problem going to be given a solution.

A. W. BURKS: We felt that the machine would work. We felt that it ought to be proved that it worked and this is a good way to prove that it worked.

STERN: Yes.

A. W. BURKS: So it was testing it in the sense that we wanted to test it and show that it worked too. But testing in the sense that it would show the skeptics that it worked but it would also be a better test than little piece tests on the thing because here would be the whole problem and we'd see if the machine worked on the whole problem. So it was

both being tested and we hoped it would solve a useful problem.

STERN: One would have thought that a ballistics problem would have been put on to the ENIAC as the first problem since that's why the machine was built in the first place.

A. W. BURKS: Well, but you see the war was over so the rush for firing tables was no longer there. Remember the idea of the ENIAC was that once you make a new shell or gun or something--this was the way I understood it--you fire the shell through coils and determine a resistance function for that shell out of that gun. But now to make a firing table you have to take that resistance function and recompute all of the trajectories. So there was a time lag between having the empirical resistance function and preparing the firing tables and it was the time lag that the ENIAC was to whip. Well, the time lag was clearly only important because of the war. You weren't going to develop new guns and shells very fast so that the original motivation for ENIAC ceased to exist when the war ended. That is, the original financing motivation of the firing table problem. Obviously, the machine would be used to make firing tables but there wasn't any great [need]. So I just understood, well, there's no great need for a firing table now and here's an interesting problem. And there is a need for this problem.

STERN: There has been some discussion in various articles about whether the ENIAC was a general purpose or special purpose computer. I notice in your article you call it a general purpose computer. Do you think there is any credence to calling it a special purpose machine?

A. W. BURKS: No, there's no credence to calling it that. I think the concept of general purpose needs to be defined. If you think of a stored program computer of the kind we have now it obviously has a lot more capabilities than the ENIAC had. We did think of the ENIAC as a machine that might be used to rearrange trajectories to make firing tables. But in general you didn't think of the ENIAC as a machine that would be good for data processing because it had too limited a store. But we certainly thought of it as a machine that was capable of solving a wide variety of problems.

STERN: As opposed to, for example, the Atanasoff machine which was clearly a special purpose machine.

A. W. BURKS: Yes. Right.

STERN: I just want to sum up this tape.

A. W. BURKS: Or the idea of the IBM 601 which was clearly a limited device.

STERN: There are those that claim that is not proper to call the ENIAC a computer at all.

A. W. BURKS: Because?

STERN: Because in some connotation, in some context, computer means stored program machine and the ENIAC was not a stored program machine. Do you think there's any credence to that? What would make the ENIAC a computer in your terms?

A. W. BURKS: Without having the capacity to store its instructions in read/write form, it stored its instructions in read-only form by setting switches and plugging cables, and it computed the solution to all kinds of problems. I don't know why it shouldn't be [called a computer]. Well, maybe your question is, Nancy, is it a general purpose computer if it's not a stored program computer? And the answer that I would give is that a general purpose computer is a machine which is general purpose in the sense of being able to solve a wide variety of problems. And Atanasoff's machine could solve only one particular type of problem: finding the roots to simultaneous equation. Now, there are many variants to the problem depending on what the equations are but I would think of that as one problem. The ENIAC could solve all kinds of problems.

STERN: So that you would categorize Atanasoff's machine as a special purpose computer. . .

A. W. BURKS: Oh, no problem with that.

STERN: And the ENIAC as a general purpose computer, and now what I want to know is what would--what makes both these machines computers as opposed to calculators?

A. W. BURKS: Well, I'm not sure that whether you're asking an historical question about words or not. When you think of it this way, I would say in a sense that Babbage's difference engines, built by Schputz, were computers in the sense that they had a whole bunch of registers and that they could transfer information around in these registers in a much more sophisticated way than a desk calculator. A desk calculator has an accumulator to store the answer in a couple of registers. And it could do a few limited arithmetic operations. So it's a special purpose computer, but it's a very limited variety. As soon as you go to the difference engine, for example, you have something more sophisticated. Now you could say, "Well, isn't there a continuous line?" Sure there's a continuous line.

STERN: What about speed, does that enter into the definition of computer as opposed to calculator?

A. W. BURKS: Well, I would call some of these the little machines that Alice has on her desk a calculator even though it's electronic. So I don't see that speed is a relevant feature. Speed, of course, depends more on the technology than the logical structure or architecture.

STERN: Even talking in the 1940s you would not have said that speed would make something a computer as opposed to a calculator?

A. W. BURKS: No, because I would have said that Harvard Mark I was as much of a computer as ENIAC, only it was slow because it was electromechanical.

STERN: But its versatility was responsible for it being considered [a computer].

A. W. BURKS: Yes, it was comparable in versatility as I remember. It had more registers and more accumulators.

STERN: Okay. I'd like to sum up this phase just by asking you one thing that we had discussed in correspondence and that is whether you perceived of any science versus engineering dichotomy while at the Moore School at that time?

A. W. BURKS: Science and engineering?

STERN: Yes. You, Goldstine, von Neumann were mathematicians, essentially scientists, pure scientists if you will - at least by training. You were working with many men that were engineers. Was there any sense of controversy of a dichotomy in attitudes that existed at a time based on your backgrounds?

ALICE BURKS: Mauchly was a scientist also.

A. W. BURKS: He was a physicist. No, there was no perception of that. And, of course, Pres was trained as an engineer and my training in engineering was more recent and more minimal than that. Mauchly was trained as a physicist and then he had roughly the same training as an engineer that I did, but obviously he was a physicist. He had done all kinds of engineering related tasks. Goldstine we would have thought of as more than a mathematician because he hadn't had the amount of engineering training that the rest of us had had.

ALICE BURKS: You mean as more *of* a mathematician.

A. W. BURKS: More *of* a mathematician, right. And then indeed he had gotten involved in the technical aspects of it later because originally he was there only infrequently, and then after he came he was more administrative, but he gradually got into the technical aspects and we always thought of him as someone who understood the technical aspects very well. Von Neumann was presented as a physicist/mathematician. His book on the theory of games had come out not too long before, but it soon became clear that he knew enough engineering that we could talk with him

about engineering.

STERN: Well, but it seemed to me . . .

TAPE 3/SIDE 1

A. W. BURKS: You've got to remember that logical design was not separate from engineering design at that time.

STERN: It wasn't in anyone's mind?

A. W. BURKS: Well, it wasn't in actuality. That is, you couldn't do a logical design of an accumulator circuit without knowing enough electronics to do it right. So it was partly not a separation in mind, but it was also a matter of the technology.

STERN: Would you say that von Neumann's main interest was, however, in logical design and not in hardware? If I can make a distinction.

A. W. BURKS: No, I wouldn't say that. I would say his EDVAC report was more a contribution of logical design than hardware, but he was certainly interested in the hardware, and after all he suggested the use of a cathode ray tube as a storage device and that's a hardware concept.

STERN: So that I'm making a distinction that really did not exist at that point?

A. W. BURKS: We would have thought of von Neumann as less of an engineer than Mauchly, of course, and von Neumann would have thought of himself as less than an engineer than Mauchly, just to give a comparison.

ALICE BURKS: Some of this distinction did exist outside of the project. I would think that people inside the project

knew. But I certainly heard when I was starting to date Art that he was just a philosopher. He's a Ph.D., but just a philosopher I was told.

A. W. BURKS: Yes. Okay. Fine.

ALICE BURKS: This distinction was made but I think anyone that worked with Art directly knew that he could make a contribution to the engineers.

A. W. BURKS: Well, I suppose in the same way that people would have thought von Neumann's a mathematician.

ALICE BURKS: I think there may have been some of that there.

STERN: My perception is that wasn't the case. I think Art's position was a very unique one in that I think you were accepted as both an engineer and as a mathematician. But my sense of it is that von Neumann was not. He was accepted as a mathematician, primarily.

A. W. BURKS: That was not my perspective.

STERN: But Alice is saying that to some extent that kind of thing did exist.

ALICE BURKS: That thinking was there and, well I wouldn't want to sound prejudiced about engineers, but I think there may be something of a cultism there, I don't know.

A. W. BURKS: Well, are you suggesting, Nancy, that Mauchly and Eckert regarded von Neumann as more a mathematician and less an engineer?

STERN: Yes, I would say that.

A. W. BURKS: That maybe. It would never have been reflected in our conversations or anything like that.

STERN: And I would say they had the same feeling about Goldstine.

A. W. BURKS: Well, I think it's a fair feeling about Goldstine because as I say Goldstine never had any electronic training, and by the end of the ENIAC I had been an electronics engineer, you might say, from '41 to '46 - for five years. And, of course--but von Neumann, you know, had a degree in chemical engineering and we knew that.

STERN: But the cultism that you mentioned, I think it's a good word, about how engineers would feel about non-engineers.

ALICE BURKS: Yes, I think there's still some of that.

STERN: And I think there is also, by the same token, a feeling amongst pure scientists that an engineer is somewhat narrow.

ALICE BURKS: Yes.

A. W. BURKS: Yes.

STERN: And I wondered if you sensed any of that at that time.

A. W. BURKS: I wouldn't have sensed it at the Moore School. I remember sensing it at the Institute. That some of the people at the Institute didn't feel that was quite appropriate for the Institute to have a computer, for example, because that was engineering and the Institute was mathematics and theoretical physics.

STERN: Okay. We had gotten up to the 1945 period. Were you aware in 1945 that Eckert and Mauchly would become interested in commercial computers?

A. W. BURKS: Yes, I knew at a certain stage that Pres and John wanted these private patent rights so that they could found a business. I knew it some from them and when it was actually done, they invited me to participate in the business. To work for them. And when I first knew this I don't know, but I would have known it by '44, I would guess. And certainly I knew it at the time that von Neumann was setting up his project. There were discussions, and I heard of these through Herman mostly. There were discussions between von Neumann and the Moore School about the possibility of von Neumann participating with the Moore School design of a stored program computer. But those didn't work out, so at one stage von Neumann decided to start a project. Herman told me about this and told me that he was inviting Pres and he invited me. At some stage he told me also that he didn't know whether Pres would come because Pres was thinking seriously of setting up a business. So I knew it both from what Herman said in his negotiations with him and directly from John and Pres. I'd gotten it mostly through John but Pres would have let me know it too.

STERN: You said that there was a possibility of a project that von Neumann would work on with the Moore School. Why didn't that work out?

A. W. BURKS: I was not involved in the negotiations, so I can only give you the impression I got from Herman which was that the Moore School would not really move or make any commitments. I know that Herman thought that the Moore School ought to offer Eckert and Mauchly professorships, since they had now [completed] this great accomplishment. This seemed to me to be a reasonable thing that they should do, but it was clear that they weren't going to do that. So, in other words, Herman thought -- and this would fit with my own perception of the Moore

School -- that the Moore School was not willing to make the really necessary commitments to have a viable project there at the Moore School. That is, von Neumann wanted a computer that he could use and if the Moore School had moved and had a good organization that was committed to making a computer and von Neumann could have some role and use it, I think von Neumann would have been happy with that arrangement. And Herman would have been happy with that arrangement.

STERN: Well, Herman has said that he felt he wouldn't have been happy with the Moore School arrangement because the mathematical capability of the Moore School was not what it was at the Institute or at Princeton.

A. W. BURKS: Well, that would have been a consideration. At the time, I didn't get the impression that that was the dominate consideration.

STERN: Because after all the Moore School was involved with the EDVAC project, they had made some sort of commitment.

A. W. BURKS: Well, but now wait a minute. At the time that this was under consideration there was a good chance that Eckert and Mauchly would leave. I would guess that Herman had in mind something like this, though he never spelled it out to me. Well, now since the Moore School wants to be serious about this they will go to the government right away and ask for more money than they have, the EDVAC money. They will make Eckert a professor, and Mauchly a professor, and me a professor and von Neumann a professor associate, and if they make all of these commitments then maybe these people will stay and we'll have a good project. It would be my perception now as to how he had it in mind though he never said that. I'm just making a construction.

STERN: With the war's end in the 1945 period, you were still an instructor.

A. W. BURKS: Yes.

STERN: And was there any discussion about your leaving as far as the Moore School was concerned or were they prepared to give you a professorship?

A. W. BURKS: Yes, they wanted to keep me. They offered me, as soon as these issues became [known]. Well let me go back a minute. Cunningham wanted some more knowledgeable senior engineer to go to Aberdeen and take a permanent position there and be in charge of the ENIAC. He thought of me and expressed interest in this and I was not interested, but Herman, who was always trying to mediate and make arrangements, asked me to go down and be interviewed. So I agreed to go down and be interviewed. When the Dean heard about this, he immediately said, "Well, of course, we'd like you to stay on, we'd make you an assistant professor." And I told him--I told Eckert that I was not really interested in going to Aberdeen. I was going down for the visit because Herman thought I ought to and while it was a possibility, I certainly wouldn't do anything without telling him. And I said that I would probably go back into philosophy. I hadn't made up my mind, but Alice and I talked about it and probably thought that's what we would do. And he said "Well, I'll see if I can't make some kind of arrangement here." So he talked with your Plato teacher . . .

ALICE BURKS: . . . Morrow . . .

A. W. BURKS: Glen Morrow. Alice had taken philosophy from Glen Morrow and I knew Glen Morrow because he was a well-known philosopher. As soon as I went -- to Penn -- I made contact with him. He used to eat in the Horn and Hardart, too, and so we sometimes ate together, and we talked about Plato and so on. And Glen was then the dean of the college and had been brought to Penn as chairman of philosophy. Morrow said, "Yes, that's fine. I'd be glad to make a joint appointment." So Pender and Morrow had it worked out that they would like to offer me an assistant professorship; half in philosophy, half in engineering. And I was very interested in this and said, " Well, go ahead and see if you can work it out." Well, it turned out that the philosophy department at Penn vetoed that half of it so that offer never went through. But I was still welcome to stay as an assistant professor of Electrical Engineering.

STERN: But you really felt you wanted to go back to philosophy?

A. W. BURKS: I really wanted at least some philosophy. I think that there is a good chance that if they had made that offer we would have stayed at Penn, I don't know. At the time I was a visitor at Swarthmore. We still had it in our minds that philosophy jobs were hard to get. In some sense it was still true because I remember Alice and my going to the Swarthmore library and looking up 50 department addresses and she typed 50 letters for me to send out. So if I got an offer from Swarthmore, as I later did, or from Michigan, I might have changed my mind. But actually the Penn offer didn't go through, and I got an offer from Michigan about the time of the dedication. In fact I think I came to Ann Arbor between the first public demonstration to the press and the later one, and I was interviewed and offered the job, and then I got an offer from Swarthmore, so I had to choose from Swarthmore and Michigan. But I was that much interested in philosophy that I didn't want to give up the opportunity to come to Michigan or go to Swarthmore.

STERN: We'll get back to that in a minute but I wanted to ask you something more about the '45 period. Eckert and Mauchly claimed that they would have been prepared to stay at the Moore School. That although they were interested in doing some part time kind of commercial pursuits they would have stayed there and that really the Moore School forced them out. Is that your sense? You're really telling me that you really thought that they were going to go and that everyone seemed to think that they were going to go, inevitably at some point?

A. W. BURKS: No. No, I didn't mean to imply that at all. But there were some fundamental issues. Now, I've mentioned one. I'm sure the Moore School would have kept Mauchly and Eckert on in some role. But Goldstine's perception was, it seems to me correct, that they were entitled on the basis of this success to be elevated pretty high in the hierarchy, at least an associate professor if not a professor. They would get tenure, in other words, and a high stature and Herman felt that this would encourage them to stay. But it was very clear that a fundamental issue was the patent rights. I think it's true to say that if the Moore School had continued to give the private patent rights to the inventors, if that had been the policy at the Moore School, there was a good chance that Eckert and Mauchly would stay. How good, I don't know. Even if one has good options, one has a hard time judging. But it was equally

clear to me that the Moore School wouldn't do this, and that if the Moore School didn't do this that Eckert and Mauchly wouldn't stay. Because in a private business, or if Eckert went to the Institute, it was my understanding that at the Institute the inventors would have the private patent rights. [That's] what precipitated their leaving, as I understood it, and I was around then. It's true that after the dedication I did not spend much time at the Moore School because I soon terminated. Maybe I had a month vacation that I worked at home, and I remember working at home on that ENIAC article -- Super Electronic Computer. Alice helped me do that I remember. But I didn't spend much time at the Moore School, say, after March, because I had shifted my job to the Institute. I was on my way to the Institute while still teaching at Swarthmore in the evenings.

Irv Travis came back from the Navy. He was made the Director of Research and then he initiated what he thought was a fair policy. It was actually the policy that the university thought it had; namely the policy that the private rights to an invention went to the university. I say the university thought it had that policy because it did say in the catalog, for example, that that was the policy. The university did not enforce the policy in the sense of asking the engineers to sign a patent release when they were employed. So Travis inaugurated that policy, and he said that hereafter all engineers would have to sign a patent agreement. It wasn't an issue with me because I had already decided to go to the Institute, but it was clearly an issue for Pres and John. I always understood that it was because they didn't want to sign that policy that they left.

STERN: We talked before about the commercial interest of Eckert and Mauchly prior to that period. They said that their designs on forming a company did not come about until they resigned from the Moore School.

A. W. BURKS: Well, of course, they know a lot more about their own beliefs and goals and so forth than I did. They were certainly talking about a company before they resigned. Now, I don't know. What dates are you talking about here?

STERN: Well, the initial request that they sign this patent release began in March.

A. W. BURKS: That's right.

STERN: And their resignation was effective March 31.

A. W. BURKS: Yes.

STERN: So that their saying that it was March '46 when they began to think seriously about forming a company.

A. W. BURKS: Well, I don't know how the word "serious" is to be taken, and I don't want to contradict what they were saying, but I understood them to be considering the possibility of a business before that. Now how seriously, I don't know. And I can remember at a certain stage Herman saying he thought that Pres would go to the Institute and his later saying, "Well, it looks now like Pres is very serious about forming a company and that he doesn't want to leave Philadelphia because his mother and father are there, and his mother and father don't want him to go to Princeton, that's too far away. And his father is encouraging him to go into business." So the idea that they might form a business certainly started earlier than that. How serious their motivation was, I don't know. And I guess I would have thought that this could have been the precipitating factor. That when they finally saw that they would not be allowed to keep the private patent rights at the Moore School, that was enough to switch the decision.

ALICE BURKS: And they're calling that being forced out, possibly, or viewing it that way?

STERN: Well, I used that expression. That's right I should have been more clear.

A. W. BURKS: Yes, I think that's too extreme to say that they were forced out. They were, and it's not only they, every engineer was told "if you want to stay in this role you will have to agree to abide by this long-standing, but previously unenforced, university policy on patents." And I understood that this was going on at the same time.

STERN: It seems to me that the computer projects gave the Moore School a kind of stature that it wouldn't have

otherwise had. Travis comes back in '46, and really is not tied to this computer development.

A.W. BURKS: Well, that's not true, because Travis was the preeminent computer man before he went off to war. He had taught computers, and Mauchly had taken a course, an evening course at the Moore School, from Travis. Mauchly had learned, as I understand it, Mauchly had learned a lot about the differential analyzer and the MIT differential analyzer from Travis. Well, it's referenced in some of the correspondence we have seen. During the war Travis worked mostly on fire control computers. So he was a computer man; it's true that his technology was more analogue and mechanical than it was electronic, but he was a good engineer. So I'm sure Irv thought of himself as returning as Director of Research, but very much interested in continuing in the computer business. But I think he also thought, and maybe the dean had told him, this is the way it has to be. I don't know how much . . .

STERN: Well, that's what I was going to ask you because it seemed to me that they would have to know that losing Eckert and Mauchly was going to put a drain on their capability to complete the EDVAC project.

A. W. BURKS: Yes.

STERN: And I'm kind of surprised that they were willing to take that risk.

A. W. BURKS: Well, okay. I don't know, dear, but I think it's fair to say that the Moore School never fully appreciated the contributions of Eckert and Mauchly and perhaps myself and Goldstine and von Neumann, as kind of outsiders.

ALICE BURKS: I mean they were the establishment really throughout and here comes this project of young people. They may even have felt a little threatened to try to carry on and allow another group to be the stars of the school.

STERN: Even Pender you think felt that way?

ALICE BURKS: He must have been very close to retirement.

A. W. BURKS: Well, yes, he had a while to go, but in general Harold Pender was not very active in administration as he delegated to these people. He delegated to Chambers running the war courses, and he delegated to Brainerd running the war research; then he gave Travis the patent research position and pretty much left it to him. And I think you want to remember that the university patent policy seemed to many people, and I would be included, as a reasonable policy: that if the government is paying engineers to do research, then why shouldn't the university have the same policy towards those engineers that a company would have. If you go to work for a company, of course, you don't keep the private rights, and I certainly felt during the war that since we were all being exempt from the draft because of this, it was overly generous of the university to let any individuals have private right in addition to what we were getting.

STERN: Well, if you look at it in contrast to a place like the Institute, of all places you would expect the Institute to make such a request of its employees and it didn't. And there were people on the Moore School staff who thought that Travis was wrong. For example, Chambers.

A. W. BURKS: Yes, well I think Chambers would have been more pragmatic, and seen that if you do this you're going to lose the best engineers. Sharpless left later with Stu Eichert to form a company. However it was the best decision for the university to let the ENIAC patent pass to Eckert and Mauchly in order to protect the government rights.

STERN: So that I--it's been my sense that the Moore School really was not entirely fair to Eckert and Mauchly and you're saying that you . . .

A. W. BURKS: Now wait a minute. You introduced the word "fair". If you could say that they were unfair to Eckert and Mauchly in fighting the initial allocation of private rights, because the patent law says that the inventor gets the patent unless he releases, then it becomes a question of, for example, that Eckert and Mauchly knew that the

university did not enforce its policy and so forth, and they certainly had not agreed at any stage to give the patent up. So you could say that the university was unfair on that point. I don't think it's unfair for the university--maybe unwise but it's fair for the university to say, "Well, hereafter we're going to have this patent policy and if you want to stay you agree, otherwise you can't stay." That doesn't seem to me unfair.

STERN: No, I meant more in terms of not offering them the professorship that we talked about before.

A. W. BURKS: Well, again I don't think that's a matter of fairness. I think the smart thing for the university to have done was to offer the deal. That is, I think it is fair and it is correct to say (Alice I'd like your reaction) that the university didn't give these outside engineers -- and by outside I'm including John and myself as having come in because of the war and Pres as having come, and Goldstine -- as much credit as they deserved for what they had done.

STERN: I don't want to belabor the point but it would seem to me Eckert was not an outside person. I mean he came through the Moore School.

A. W. BURKS: Well, he was a student. He had never been a faculty member. And he had this research position because of the war, you see.

ALICE BURKS: I think the university possibly should have gotten into the matter, the patent issue, much earlier.

A. W. BURKS: They should have clarified it at the very beginning. And this would have been fundamentally Brainerd's responsibility, I would think, because he was the director of research. This is a clear policy issue. And he knew--the Moore School knew when it had signed a contract with the government that the government was going to have its right to the patent. So the Moore School should have realized the patent normally belongs to the individual, and therefore we had better straighten out with the individual what his rights are in the patents.

ALICE BURKS: Yes, I think that if you're going to use the word "fair", too, it seemed to a lot of people and people looking back on it feel that, I think, that there's some unfairness in Eckert and Mauchly having been able to enrich themselves and their careers and monetarily through a project during the war funded by the government for war purposes.

A. W. BURKS: For which they were exempt from the draft.

ALICE BURKS: I mean there may have been many people who could have done the same thing. I would say there is where they should have been [restricted] and perhaps not later as a policy. But the university paid no attention before it was too late.

A. W. BURKS: It would make more sense for the university not to give them the rights during the war because it was a government project and they were exempt from the draft and then give them after the war rather than vice versa. But I think it has to be said, and Alice can elaborate on this, the University of Pennsylvania when we were there was not an institution that operated in terms of picking out policies in advance. It operated on traditions and rules determined by secretaries and various people who played roles rather than determined by somebody sitting down and saying, "Well, what are issues now and what should our policy be?" And she can tell you plenty of instances.

ALICE BURKS: Well, as a student, a transfer student and all, I ran into some of that.

STERN: Government by crisis or something?

ALICE BURKS: Yes.

A. W. BURKS: Government by secretary.

ALICE BURKS: Yes, there was a certain feeling of decadence. Penn has been revitalized completely since then but

that was certainly part of the atmosphere. In the philosophy department, I'm sure the reason that they didn't want Art was that he didn't fit in with their particular very narrow view of philosophy. All of their people, or most of them, had been Penn-trained, and there was a lot of friction between the people that were not Penn-trained, not Singerites as they called them, and those who were. This is a certain narrow-mindedness, in my opinion, but it pervaded much of the university.

STERN: Art, you wrote two articles on the ENIAC, as I recall, one for *Electronic Industries*?

A. W. BURKS: Yes.

STERN: I believe the other for *The Electrical Engineering Journal*.

A. W. BURKS: IRE it was called.

STERN: Institute for Radio Engineers?

A. W. BURKS: Right.

STERN: Did it strike you as odd that very few articles were published about the ENIAC?

A. W. BURKS: Well, I don't know I thought of it in that way. I can tell you more specifically about mine and how they came about and say something about the security restrictions that had something to do with this. We first had this press demonstration and that took a whole day as I remember.

STERN: It was in February of '46.

A. W. BURKS: That involved some briefing and I think they had lunch with the press and there was a demonstration

and I was in charge of the demonstration, making the ENIAC work. Sometime in the afternoon towards the end of the affair--well maybe it was at lunch--I was approached by the editor of *Electronics Industries* and asked if I wouldn't write an article. And he approached me, I think, because he knew that I knew enough to write an article and maybe heard me give a demonstration. I don't remember the time sequence. So I said I'd be interested in this, if people agreed to it. And so I checked with Herman, regarded him as a chief security point here as he was a military representative, and I told him what the fellow wanted and that while he wanted some electronics, it would have to be kept very general. And Herman said, "Fine. I don't see any objections." So I agreed to do this and I cleared it with Pres, too. And so then I went ahead and I wrote that article. Then later on Herman arranged, I think through Jan Rajchman - at least I dealt with Jan Rajchman, that I should make a presentation to the local chapter of the IRE at Princeton. And this was a rather pivotal chapter because the RCA research laboratories were at Princeton so there were a lot of good engineers and I welcomed this opportunity. Herman said, "You could then write it up and submit it as an article."

TAPE 3/SIDE 2

A. W. BURKS: And Jan Rajchman said, "Yes, we'd be glad to have you give a talk." And he told me something about the audience and how long I would have and who the audience would be, and then I could write it up and submit it to the journal. And so I went and gave this talk. We had moved to Swarthmore in the fall of '45 because I was going to teach at Swarthmore in the evenings of '45-'46. So I went over from Swarthmore, I remember this, and gave the talk and then worked on this article and then submitted it. But before I submitted it, this particular article, by the time I had agreed to this talk and article, Travis was the director of research and I remember going in to Travis and telling him that I had this invitation to talk and what I would say and I was going to write up this article and was it all right to publish it, and he said, "Yes." And I don't remember whether he said show me the article first or not, but I cleared it with him and it was automatically cleared with Goldstine because he made the arrangements. So while I had never had anything in writing, I had cleared it with both the Moore School and with Herman.

Now, Eckert and Mauchly at this time were going around and making talks and I remember hearing--I never heard any

of these but I remember hearing complaints from people, well I guess Mauchly made most of the talks, and one complaint in particular about a talk he gave in Philadelphia before a knowledgeable audience of engineers, that Mauchly just rambled on and never told them very much. Maybe it was security, but I think it was also patents. So Eckert and Mauchly were not interested in the details coming out from the point of view of patents and commercial applications because if there were an IBM engineer, why then that IBM engineer might use these ideas. I didn't have that restriction and as long as I had cleared the security I felt it was all right to describe the ENIAC. So possibly part of the explanation as to why there weren't that many articles to begin with is that. That is, Eckert and Mauchly weren't interested in that kind of publicity. Actually Herman was invited by MTAC -- it's *Mathematical Tables and Aids to Computation*, and I think D. H. Lehmer was the editor at the time -- to write an article and he and Adele wrote that article, of which you know, and then, later on, Grist and Sharpless wrote an article. To talk about Grist's knowledge, I think it's true that Grist would not have been able easily to write that article by himself. He relied on Kite for a lot of technical information.

STERN: Well, you've answered the question I had as to why Pres, for example, did not publish very much.

A. W. BURKS: Yes. This is my projection. Pres never told me that.

STERN: I'd like to go on to discuss your work at the Institute. But before we do that could you summarize what you regarded as von Neumann's most important contributions to the Moore School work?

A. W. BURKS: Well, he made no contributions to the ENIAC, as it was worked on at the Moore School. Now, later, after it went to Aberdeen, there is the matter of the central programming to which he contributed. The substantial contribution that he made is reflected in his 1945 draft of the EDVAC report. Now that's not to say that's the only thing he did. He wrote this manuscript when he was at Los Alamos, and he sent it to Herman. I remember Herman saying he had it, and it was typed up and copies distributed among the few of us, and then later it was typed on the masters -- these masters which punch holes through the paper, whatever that's called.

ALICE BURKS: Stencil?

A. W. BURKS: No. Well, yes, it was a stencil of a particular multilith -- well, I guess multilith's the later name, but that sort of thing -- and reproduced. And then he wrote a long letter to Goldstine as a supplement to that in which he talked about having programmed up a sorting problem, and Don Knuth wrote a paper on that much later. So I think it's fair to say that von Neumann's contributions made at the Moore School are pretty much covered in that draft report and that supplementary letter.

STERN: It's your position the material in that draft report is primarily von Neumann's?

A. W. BURKS: I didn't mean to be implying anything on that question. He certainly wrote that thing after these discussions in March in which he and Goldstine and Eckert, Mauchly and myself, and perhaps Sharpless or Adele at some of the sessions, were there. I think it would have been a very different matter, historically and much less controversial, if more attention had been paid and that the report had been treated more officially. Von Neumann could have written, say, an introduction to it, in which he would have pointed out the contributions of these other people. I'm sure that von Neumann would have given credit to these other people, and say that what he did was based on these preliminary discussions.

STERN: So you're saying the report puts a limit on what he did?

A. W. BURKS: It certainly puts a limit and he certainly was influenced by what went on. It is not accurate to say, as is sometimes said, that that report is only a summary of what has been agreed on before he wrote the report.

STERN: Well can you isolate things that you would regard as von Neumann's own contributions?

A. W. BURKS: Yes, but I don't want to in this context, because Alice and I are working on a paper on the ENIAC. Subsequently I'll go into the origin of the stored program computer, and will then take the summaries that exist of

those meetings and read them over, and review what von Neumann did in the report, and on the basis of that data, try to come to an item by item answer to your question.

STERN: Well let me ask you this. Goldstine made a comment to me on this paper that I'm talking about that he thinks that von Neumann's most important contribution to the draft report and something that was solely his. Was the concept of representing numbers and instructions in main memory the same way. That that was his idea, and didn't come from anyone else. Do you have any recollection on that?

A. W. BURKS: Well let's be careful what the idea was. It was certainly agreed at the March meetings that instructions and numbers would be stored in the same way: the mercury delay lines, more possibly the electrostatic store. The emphasis was on the mercury delay lines because more had been done on that, and it would seem more reasonable that they would work. Nobody had ever built a mercury delay line store yet. As to the particular mode of representation, I'd have to go through those notes to see how much was in that discussion, so I'm not sure just what Herman is saying.

STERN: I'd love to see the paper when you've got a copy.

A. W. BURKS: In due course, you'll get a copy.

STERN: Now you said just a few minutes ago and in your paper, that..

A. W. BURKS: The Los Alamos paper you mean, or the early draft of the present paper? You remember I sent you the Los Alamos paper, then later I sent you the first two or three sections of the present paper that Alice and I are still working on.

STERN: The Los Alamos paper.

A. W. BURKS: Okay.

STERN: You said the same thing you just said now. That von Neumann would have given credit to others.

A. W. BURKS: Yes.

STERN: It raised an immediate question on the parts certainly of Eckert and Mauchly as soon as the draft report came out. Why is it that von Neumann, at that point, didn't make a statement, said that this is not my work entirely or not only my work.

A. W. BURKS: I don't remember Pres and John raising any question at the time on that. Maybe we should go back and talk about the attitudes and goals of these various people, and they were somewhat in conflict. Whether or not Eckert and Mauchly made up their mind to form a company at a certain stage, it was a possibility that they had it in their mind, for some time. There was the matter of the patent, and there is the problem of publicity in connection with a patent before it's applied for. There was the problem that John had not finished much patent work in '44, or '45, so clearly in their mind -- and we recognize -- this problem of getting that patent on the ENIAC. It was often brought up. There is record in my notebook, for example, of statements that Mauchly has been working on the patent, and here's the goal date, and things of that sort. I was very much aware of the possibility of a business and of the timing problem of the patent. I didn't know, for example, that it was at all conceivable that the main problem would be publicity. I'm not sure I knew how much publicity was a factor in this, but it would never have occurred to me that the public demonstration was a factor. I didn't know that much about patent law, but I know now that there was a problem getting the patent on the ENIAC. I knew enough about that that you get them out as soon as you can. John was not doing this. So they had this patent problem, as a reason for delay of publicity. Of course, there was the problem of classification status which was also a barrier. Herman's and von Neumann's motive was more to let the scientific community know about it and get this thing moving in science. So it was Herman's decision to take this draft report and have it duplicated and send it to, I don't know, you can probably tell me, fifty people or so, whatever it was. It was marked the draft report, and I didn't think anything about it at the time. I wasn't that much concerned

with credit, and I didn't think it was odd that he hasn't said who gets credit. I just regarded it as draft, and I knew Herman was sending it out. I didn't know who he was sending it to. How much John and Pres were concerned at that point, about the publicity, I don't know. You can probably tell me more than I know about their complaining to Herman about this.

STERN: I think their complaints were based less on the publicity issue than on priority issue. That they felt that there should have been some acknowledgement as to where these ideas came from. They objected to in the same way to Brainerd writing a report to the applied mathematics panel with only his name on it.

A. W. BURKS: No.

STERN: Even though this wasn't official, it was still being disseminated.

ALICE BURKS: But von Neumann didn't know it was going to be disseminated.

A. W. BURKS: Von Neumann didn't know; it was Herman's decision, he didn't even consult von Neumann on this. Of course the analogy with Brainerd is not very close because nobody doubted that von Neumann had written this report. People did doubt the ability of Grist to write the report on the grounds he didn't know enough. Furthermore the applied math report had a kind of status in the sense that here was the official organization asking for the report of this project, whereas the von Neumann report was sent out as a draft report. I didn't know how many people Herman sent it to at the time. Did Pres and John know how many people he sent it to?

STERN: I think the list is in the high thirties I believe.

A. W. BURKS: Did they see this?

STERN: At the time they did, and at the time they objected to that.

A. W. BURKS: You're telling me. They objected on what grounds at that time?

STERN: They were not acknowledged as having participated in this report. I thought you said a few minutes ago that you thought it was a little odd? That this went out with von Neumann's name on it and no one else's?

A. W. BURKS: No, I didn't mean to say that. I guess I wouldn't have thought, at that time I wouldn't have thought of these things that were done. That is, von Neumann had written and was capable. It went out as a draft report, and a draft report is a draft.

STERN: You said something to the effect that it seemed odd but then it was a draft.

A. W. BURKS: Yes. Well, in retrospect, certainly the better thing for Goldstine to have done would be to say, "Here (first page) is a draft prepared by von Neumann, it was after a consultation with such and such people."

STERN: I agree based on my reading material that von Neumann's approach was very much the scientific approach, and that you do disseminate information as quickly as you have it available so that other people could do work in this area and very much he followed the ideology of the scientist, but on the other hand the ideology of the scientist is to give credit to people involved in the work.

A. W. BURKS: Yes, sure, but it wasn't von Neumann who made the decision.

STERN: To go ahead...

A. W. BURKS: Yes, I think you also have to ask yourself the difference between how much were Eckert and Mauchly concerned with the patent implications of this going out, and how much were they concerned with the credit. These are two perfectly legitimate concerns on their part, but distinguished concerns.

STERN: Prior to 1946, March of '46 when you left, was there to your recollection any dissension between von Neumann and Eckert and Mauchly?

A. W. BURKS: I do remember. Let's see, I'm not sure I can give dates.

STERN: Before you left.

A. W. BURKS: Yes, I know. I remember an incident, and this is a significant incident. It was probably the fall of '45. It was after von Neumann was interested in having a computer, whether he got it into the Institute or worked with the Moore School. I don't know if he even got that far in his thinking, but he did consult with Zworykin on this, and Rajchman, about memories, for example. Von Neumann had the idea of an electrostatic memory, and RCA laboratories had worked on this computer project so there was an interest there. Then von Neumann and Zworykin were invited to a meeting in Washington which, as I understand it now, was supposed to be off the record, but there was a newspaper reporter there. At any rate, they talked about a future computer and Zworykin outlined a future electronic computer as if it was his idea, while von Neumann proposed to use it to predict the weather. At least that's how it appeared in the press. We were startled to read in the Philadelphia press a brief notice about this speed. Now John had worked on the weather, his original reason to build the computer had to do with weather calculations, as you know.

ALICE BURKS: John Mauchly?

A. W. BURKS: John Mauchly. Now he had an idea that there was a periodicity in the weather that was controlled by the periodicity of the sun spots. He was going to make a statistical study about that, and I can tell you about some conversations between Mauchly and von Neumann on weather calculations, but let me continue this main line. Von Neumann was due to come as a consultant that next day. Mauchly and Eckert walked in and were clearly upset by this publicity, because Zworykin and von Neumann were clearly talking about a computer that they (Mauchly and

Eckert) had something to do with, so why weren't their names mentioned? I can remember von Neumann coming, and he was also surprised by this publicity and tried to call someone and find out why this publicity. He explained, as I recall it well, there had been this meeting, and it was supposed to be off the record. That there wouldn't have been any problem except that somehow somebody got in there and publicized this. At the time it seemed that Eckert and Mauchly were satisfied by that explanation, but it was not the last time that Eckert and Mauchly and von Neumann were upset. Now maybe that continued or maybe they weren't as satisfied as they indicated, but John at least, with whom I was closer, seemed satisfied with that explanation, and did not blame von Neumann, but blamed Zworykin for claiming credit he shouldn't have.

STERN: So the way you perceived of that, it was a minor incident?

A. W. BURKS: Well, it was a big flash, but then it was resolved.

STERN: I wanted to get into von Neumann's work at the Institute.

ALICE BURKS: I wanted to ask why Herman took it upon himself to distribute that report.

A. W. BURKS: Well, I don't know. Maybe you can answer some of it. We can go into personal mores here.

ALICE BURKS: Are you interested in that question?

STERN: Yes.

A. W. BURKS: He had something to gain by this, clearly. He was the director of the project from the military point of view. He was playing an important role in making it go. We all had something to gain from the publicity. It just so happened that Eckert and Mauchly, really because Mauchly hadn't done his homework, had something to lose by premature publicity. So Herman wanted the publicity. And a way to get publicity was to take this paper which gave

a lot of information of the scientific sort, and *not* of an engineering sort -- and I'll talk about that in a moment since I've been reminded of it -- and send it to such people as Hartree. I think this was very legitimate. This is apart from the question of what the covering letter should have said, because the government was putting in a half a million dollars into this project. Was the war over when he sent this? He sent it in June...

ALICE BURKS: ... of '45.

A. W. BURKS: When did the war end?

ALICE BURKS: August.

A. W. BURKS: The German war was over...

ALICE BURKS: ...but the Japanese [war] was not until August.

A. W. BURKS: Okay, the German war was over, but we all felt the war was going to end. He sent it, for example, to Hartree, who showed it to Wilkes. This was Wilkes' first acquaintance with this. So what Herman was trying to do was to the advantage of the government as well as to all of our advantages, putting on the side for the moment the disadvantage to Pres and John because of the patent, the commercial aspects of it. Get the idea out and have it influence [computer development], so that the government is getting more for its \$500,000. That was a perfectly legitimate thing to do. Brainerd told me, for example, at Los Alamos in 1976, that the report should have been classified. As I said, our classification was the lowest, "restricted." It was really up to Herman to determine our classification. Maybe he should have consulted with Brainerd, I don't know. I would guess that Brainerd would have said "all right." Maybe he didn't consult with Brainerd when he should have, but it was certainly a reasonable classification not to classify it. A reasonable classification judgement, because after all it was the logical design. It did not give the engineering details. We had a clear understanding among ourselves that we could tell them how the thing worked as long as we didn't tell them the electronic details. That was the really classified thing. We didn't want

the enemy to be able to build one fast because they had learned the electronic circuits. So since the report didn't have electronics in it, it was very reasonable for Herman not to classify it. Apart from what covering letter or title, authorship and so forth it should have, it was reasonable for Herman to publicize it and send it out to these people and not to classify it.

ALICE BURKS: You're saying that some of that report, could have been or should been attributed to Mauchly and Eckert, or are you reserving...

A. W. BURKS: It would have been a complicated thing to explain what each one did, this report having dealt with the logical design. You asked about logical design and engineering design. This was the first time that logical design had been separated from engineering design. That bears on the classification question I just talked about. Von Neumann is clearly the person who separated logical design from the engineering design. I'm sure Herman thought, "Here's a logical design, and von Neumann separated it out and that's important."

ALICE BURKS: Then Herman didn't feel any need perhaps...

A. W. BURKS: I guess I'd be a bit critical of Herman that he was not always that careful about these questions involving relations among other people. I think you could also say the same about Pres and John. I don't think they were careful about my interests, for example. So I don't want to say that the fault is all on Herman's side. In general, I guess around the Moore School people didn't pay much attention to giving other people credit. At least that was kind of par for the course. At least that was my impression.

ALICE BURKS: Almost throughout life.

A. W. BURKS: I mean, clearly when Brainerd was going to write this AMP report on his own that seemed to be a clearcut case of somebody not giving adequate credit. Of course, he would have mentioned Pres and John in the preface, but still that wasn't adequate credit.

STERN: Well could there have been antagonism in Herman's feeling at the time he issued that could he have been wanting to come out on this side of it?

A. W. BURKS: To my knowledge, the antagonism that arose between the two sides of it, if we put Herman and von Neumann on one side and Pres and John on the other, first arose as a result of that report. So this wasn't a product of an antagonism, but a cause of subsequent antagonism. I guess, now that you mention it, I probably did know at the time that John and Pres were upset, but I would have put it more in terms of publicizing these secret ideas than in terms of credit. I certainly don't remember John complaining that this went out and didn't give credit to the others. But maybe he did and I forgot. I think it is fair to say that this was the start of a long controversy between the two sides.

ALICE BURKS: I wonder too, in the matter of publicity, whether someone like Eckert was a person who would write. Has he written articles?

A. W. BURKS: Eckert couldn't write in the sense he didn't like to write and didn't write much. Furthermore, on the matter of writing, engineers don't write, they get patents.

ALICE BURKS: It's much more an academic practice.

A. W. BURKS: Scientists and mathematicians and philosophers write and don't get patents. I think that was a lot of it. Pres didn't think that there was any glory in writing an article. I thought there was glory in an article, and Herman thought there was glory in writing an article.

ALICE BURKS: Pres might not have been able to write either. He hadn't that kind of a training at all and I don't think his information was that set. But Mauchly was supposed to write up the patent, and he didn't. I wonder about his capability to write.

A. W. BURKS: Did Mauchly ever write much? He wrote the '42 memorandum. Brian Randall published a brief note of John's on programming, and he may have written a few short things, but I don't think John Mauchly ever wrote much.

STERN: Well, he just seems to defy a description in terms of the typical scientist's concern with publications, because he did not seem to be that way.

ALICE BURKS: But at the same time a very fluent person, who organized things in a way that they could have been [published].

A. W. BURKS: Well, you can talk in retro about it. Well you can see the difference now in style in that von Neumann, after a relatively small number of days in consultation (total number of days), got the problem formulated in his mind, in the background, and then his next step was to write this long draft report. Well, that was just the opposite of the way Eckert and Mauchly wrote their [later] report, and of course it was part of von Neumann's style because von Neumann had six different things in the fire; that is, he was at Los Alamos telling them how to make an A bomb at the time he would be writing this up in the evening.

STERN: Did it strike you as odd, that von Neumann went to the Institute in an effort to get computer projects started? It was an ivory tower, quote "ivory tower," institution; it seemed odd that they would take on an engineering project like that.

A. W. BURKS: Well, I should say to begin with that I knew very little about the Institute for Advanced Study. I took a course from Ray Wilder, fall of '36, at Michigan, and I may have heard from him that he was at the Institute. He was going. I knew it existed. I didn't know much about it. I knew Einstein was there, but I wouldn't have known enough probably to know what its biases were.

TAPE 4/SIDE 1

A. W. BURKS: I was surprised when Herman told me that von Neumann -- well, I knew that von Neumann wanted a computer -- wanted to build his own. That surprised me a little, but not particularly because of the orientation of the Institute.

STERN: Why was that surprising?

A. W. BURKS: Well, computers were going to be built, so why didn't he latch on to one that's going to be built? Well, I say surprised, not terribly surprised, but he was a theoretical person.

STERN: Can you speculate on why he wanted to do that?

A. W. BURKS: Well, yes, he had a very wide range in interests. He had this basic design which is reflected in the paper that he, and Herman and I, wrote, which is different from the EDVAC design. I think he saw too what I didn't see at the time, that there would be a great demand for these computers. So if he was to have a computer to use he would probably have to build his own, because if he waited for the others it might be a long time -- not before computers would be finished, but before he would have sufficient access to them. I think Herman was encouraging him in this. Herman wanted to go ahead to build a computer and saw an opportunity for himself in working with von Neumann. On this von Neumann had a lot of confidence in himself to manage all these things at once. So anyhow, it became clear, it didn't take me long to realize that Herman and von Neumann were right that things wouldn't work out very well at the Moore School if von Neumann tried to work things that way. So if he really wanted a computer the thing to do was to build it. After I came to the Institute it soon became apparent that the other people at the Institute weren't enthusiastic about such an experimental thing. They were doing it because von Neumann wanted it, but he clearly had enough prestige that if Johnny wanted it he could have it.

STERN: You came to the Institute in March of '46. Is that correct?

A. W. BURKS: I came in the sense I started to work sometime in March, maybe it was March 1st, I don't remember the exact date. We still lived in Swarthmore. That semester I taught in Swarthmore on Tuesday, Thursday, and Saturday and commuted from Swarthmore to the Institute on Monday, Wednesday and Friday. Indeed when I first commuted there, Herman also lived in Philadelphia. We took the same train over and the same train back and Herman had obtained an Army car which sat at the Princeton railroad station so that when we got to the Princeton railroad station we get into this car and drive out to the Institute and then we went home the same way. Then sometime later Herman and Adele moved to Institute housing, and after the semester was over at Swarthmore [and] on through the summer I went five days a week, commuting the same way, but now by myself because Herman and Adele had moved to the Institute housing.

STERN: When you started working even on this part time basis at the Institute, you had already made the decision to go to Michigan in September?

A. W. BURKS: Essentially, yes. I'm hedging a little because I don't remember the exact timing; it all happened fairly fast in there. I know that von Neumann and Herman offered me a permanent position, but I told them I wanted to go to philosophy and would take a suitable position. At about that time, Michigan came through with the offer, and as soon as it came through I accepted it and told them that I would go to Michigan. So most of the time I was working there, it was known that I would leave at the end of the summer and go to Michigan.

STERN: There was no possibility of joint appointment with Princeton, you didn't pursue that?

A. W. BURKS: Nothing was pursued in that. Teaching philosophy at Princeton, for example?

STERN: Well, I would have thought that you saw it as a good idea at Penn, it would be an even a better idea at the Institute.

A. W. BURKS: Yes, but I would have thought of that as an idea that the administrators could arrange. I don't know

whether Herman and von Neumann would have thought of that. The connection between the Institute and Princeton University of course is not as close as the connection between one school of the University of Pennsylvania and another school, especially when the two deans of the those schools know each other.

STERN: Can you describe the work you did at the Institute?

[INTERRUPTION]

STERN: Eckert and Mauchly also asked you to work for them in their company. Is that right? Around the same period?

A. W. BURKS: Yes.

STERN: What made you decide to go to the Institute?

A. W. BURKS: Well there was a triple decision here. That is: go with Eckert and Mauchly; go to the Institute; [and] either permanently or temporarily return to philosophy. After I got the offers from Michigan and Swarthmore in philosophy, I knew we'd take one of those, probably the Michigan one. But even apart from that I was not interested in a commercial activity. I liked very much working with von Neumann. I was very much impressed by him. I thought the Institute a great place and a great opportunity. Also, I think it's fair to say, I didn't regard it as that easy to work for Pres. He wasn't the easiest person to work with. But even apart from that, I think our academic orientation, Alice was always involved in these decisions, was such that we didn't think of business as the place that we wanted to be.

STERN: What kind of work did you do at the Institute?

A. W. BURKS: That first summer, it was Herman and I, and von Neumann. The first part of it [was spent] writing that

report. Well I think that came out, what, the end of June? So as soon as I got there in March, we started on that.

Herman and I had an office next to Kurt Gödel's. You know how the offices are arranged there, with a big office for a professor, and then an anteroom and an office for a secretary?

STERN: No.

A. W. BURKS: Okay, well let's take von Neumann's as an example. It was a large room, at least as large as this, if not larger. You'd, say, enter it through a door here. That door went to an anteroom which then went to a hall over here. Then there was a quite sizable secretary's office, with room for two desks and a couple bookcases. Well von Neumann had a secretary who sat in his secretary's office, and the entrance room was a place where people could sit, and books and what not were kept. Kurt Gödel didn't have a secretary, didn't want one, I assume. So for that summer, when of course we didn't yet have a building for the computer, Herman and I occupied the secretary's office next to Gödel's office. It had a blackboard on the wall. We spent most of our time the first few months planning this new machine, working out the structure and the instructions, and we would consult periodically with von Neumann. After we'd done a certain amount of planning, we decided we'd better write it up now. Which was fine with me. So Herman and I wrote the first draft and I don't remember how we divided it but we both worked writing it. Then we'd show it to von Neumann and he would revise, or we'd discuss it, and so forth, and then that completed the draft and that was issued as a report at the end of June. The rest of the summer would have been July and August, two months more, and we continued to plan. Actually when I was commuting alone and not with Herman, I began working on the method of programming and working on the idea that we would have a library of subroutines. I began to write some of the subroutines and think about how the combining routine would work. Indeed I wrote a first draft report which, I don't know, was at least fifty handwritten pages long together with programs which when I left at the end of the summer I gave to Herman. Then since I had left when they made the next volume which had to do with programming, I was not included as co-author, which was quite appropriate. I did go back the following spring vacation and look at their draft, and in the preface of the draft they gave reference to having consulted with me, and part of that consultation was this report I made available to Herman.

STERN: During the period that you were there prior to going to Michigan, was any engineering work being undertaken yet?

A. W. BURKS: Yes, now let's see, Bigelow started coming down, and had he moved there?

STERN: He moved there from MIT.

A. W. BURKS: Yes. Well he moved there. I don't know the exact dates. I remember he also got involved one summer. I think it was that first summer. Adelotte was the director. He had been president of Swarthmore. I think Julian Bigelow got involved and spent a considerable amount of time helping the Institute arrange for moving some temporary houses down from New York, and they moved down, and the next summer when Alice and I went there we stayed in one and the Goldstines moved into one of these. I remember Herman being a bit unhappy about this because, in the first place, he didn't think that was the smart thing to do. In the end, the Institute had to build its own buildings anyhow, so he may have been right. Also he didn't like the idea of Adelotte taking Julian Bigelow off this project to spend time on that. But Julian was there and starting to plan, and I think Pomerene, too. I think Pomerene used to come down from New York. Pomerene had worked at Hazeltine, and he still lived up there but he commuted. In a sense it wasn't too effective to begin with, because all of us except for Herman (and then later Bigelow before he got involved in the other thing) had to commute long distances. Some people would come in at ten and leave at four, but the engineers were there and they were starting to work.

STERN: Willis Ware was there?

A. W. BURKS: I can't remember whether he was there that first summer.

STERN: Now the intention to have RCA to build the Selectron was already made by this period, early '46?

A. W. BURKS: Yes I can remember quite distinctly learning about the Selectron before I went there. I think I

probably heard about it in late '45. I can remember quite distinctly the time schedule that was anticipated because this turned out to be very wrong. It was that Rajchman was already working on the Selectron. The original Selectron was to have 4096 windows. Rajchman expected to have a model of the single tube by the summer of '46; by the summer of '47, they would be turning them out. They'd probably be made by hand because there weren't going to be very many at least in the beginning. So they might not come very fast, but they would start coming in the summer of '47. The relevance of that was, one had the choice between three storage media that were cheap enough, large enough, and fast enough for a stored program computer. One was the Eckert-Mauchly delay line, which being cyclic had a relatively high access time on the average. We typically thought of the pulses being spaced a microsecond apart and the lines being 1024 pulses long, so you'd be waiting five-hundred microseconds on the average for this. Then the other two forms were the cathode ray tube alone, or as Rajchman thought, the cathode ray tube with this framework inside to do the switching. That is, in a cathode ray tube you would select a point by moving a beam up and down, and of course moving beams was standard in cathode ray tubes. But I can remember hearing this idea of Rajchman's of putting this framework of crossed wires so there'd be a cylinder of wires and then vertical crossed wires. There'd be 4096 windows, and you would control these wires to select the window, and he thought that would be a better idea than just the tube. It was this 4096 thing that would be done, they decided. Well, let me go back a minute. Nobody had really built one [a mercury delay line] for cyclic memory since the mercury delay line had worked successfully for radar use. Since we had all this experience with circuits, it seemed that while you'd have to do some developmental work on a mercury delay line, one could feel confident that it would succeed with a reasonable amount of work. In the case of the cathode ray form of memory, you didn't know that much. But now we were assured by RCA Princeton laboratories, one of the best, that yes indeed they could have a model by the summer of '46 and start turning them out in '47. We knew that we wouldn't have the design of the rest of the machine any faster, so we felt secure on the memory. Now, as it turned out, that didn't work out. It wasn't until, I don't know '50, '51, before they got it working. Instead of having 4096 it had 512. In the meantime, Williams at Manchester had come up with the Williams tube. None of these were the best of memories, but all of them were far superior to using vacuum tube memories as in the ENIAC. So that's why it worked out for the Institute machine that it used the Williams tube.

STERN: Doesn't it strike you as a little odd that Eckert and Mauchly have been criticized for going into business in

which they made projections, we talked about this yesterday, completion dates for parts of their machines that were completely unrealistic, and here Rajchman who has had a lot more experience in R and D at a major industrial research center was doing something very similar?

A. W. BURKS: Well I guess it doesn't strike me as odd. I guess I don't want too try to take to much credit there. Yes, I guess I probably was over confident in what they would do, so let me look at it as historical matter. Yes, these were similar mistakes in judgement. And I would have assumed from my knowledge of electronics of the ENIAC and my knowledge of Eckert and Mauchly that they would make bad estimates. And in the case of Rajchman, since this was a different area really -- it was construction of tubes, where I had no expertise -- I was probably naively confident. On the other hand, you've got to remember that Eckert and Mauchly had the fate of a corporation resting on this, whereas we had the fate of one computer resting on this, which is quite a bit different. And there were alternatives. I mean, if worse come to worst we could always go back and use a mercury delay line. It would have made the machine far less attractive, but success or failure really did not hinge on that judgement of Rajchman, which was I think accepted by von Neumann and Zworykin and Goldstine, and then by myself with pretty much confidence. But the Institute project had a lot more ways out, whereas, in a sense, Eckert and Mauchly didn't have any way out.

STERN: I think that's a good point, I think that's a very good point. Did you work with Rajchman on this Selectron or did you focus exclusively on logical design?

A. W. BURKS: Well, I went over there sometimes before I even started to work on a regular basis. There was a period in there I can remember staying at home in Swarthmore. And incidently Alice and I lived in a house on the south side of the railroad tracks, on the other side of the campus, that the poet Auden had occupied before, an apartment on the first floor, yes. The previous year he had occupied that. I can remember being at home awhile; it was probably that I had accumulated vacation time from the Moore School. I wanted to write this paper. I wanted to work on these papers before I went to work, but nevertheless I went over there. I went over there once, for example, with my brother-in-law, my sister's husband who was a surgeon just out of the Army and he had come to Philadelphia to see where he might do his residency. I remember taking him over with Herman to see von Neumann, and von

Neumann took us to the Princeton Club for dinner. We had consultations that John Tukey was involved in; that's why he's referred to in that first report. And we also went to RCA and consulted with Rajchman and with a young man, who was a Ph.D. in math and statistics from Harvard. I know he took symbolic logic with Quine, and he later went, he left RCA, he was working at RCA under Rajchman at the time, and he later went to Rand, and then he left that and set up his own corporation. You probably have heard his name, his name doesn't come to me now, but he was a young engineer except he really wasn't an engineer working on this. I can remember talking with them about the Selectron. I can remember Jan Rajchman taking us into the room where they did the modeling of electron motion for tubes. Their method was to take a diaphragm and the height of the diaphragm would represent the intensity of the electrostatic field, and then pulling the diaphragm down and fixing it at various points to give what the field would be in the absence of electrons. Then rolling little iron balls, small shot balls down there . . . See, the problem in designing a tube is first you figure out what the field is if there aren't any electrons there, and then as soon as you put the electrons in they change the field. So they would do this by figuring out what the field would be without the electrons, at critical points, by pulling this diaphragm down. Then they would roll a bunch of balls and the effect of the weight of the balls was a gravitational effect which represented the electrostatic effect of these balls. And I can remember Jan showing how they did that in designing tubes because they had to know what this effect would be to know where to put the wires and things like that. And they also talked about an adding circuit which was later used in the IAS computer. I think the suggestion from that may have come from Tukey, at least part of it, in which the adding circuit was not strictly binary but used three different voltage levels. So I remember that much from the visit to RCA. After I got to Princeton, there were fewer consultations with RCA, though we went there maybe once or twice again. Tukey soon dropped out. He didn't play much of a role. The logical design that Herman and von Neumann and I did was not much influenced by either Tukey or RCA after those initial conversations. I think they're both mentioned, certainly Tukey's mentioned in the preface, and I don't remember whether it was appropriate to mention RCA in the preface or not. We certainly in the report said we expected to use the Selectron memory, and our design was based on Selectron memory.

STERN: What sort of engineering work was Bigelow doing at this time if RCA was working on the memory?

A. W. BURKS: Well, there was of course the whole rest of the machine: the arithmetic unit, and the control, and the input and output, although we thought less about the input and output because that was the less novel part and it would be geared as to how the central part was.

STERN: Excuse me, if I can interrupt a moment. Your report, as I recall, did not devote much attention to I/O.

A. W. BURKS: And that was true of the project at the beginning. We didn't devote much attention to it.

STERN: So it wasn't decided what kind of I/O? At that point?

A. W. BURKS: Well, there were certainly some conversations, but we didn't go deeply into it. So leaving I/O aside there was still the arithmetic unit and the control and that's a lot of electronics. RCA wasn't going to do that. I guess I heard at one point that RCA was willing to do the whole thing for the Institute, but it was decided by von Neumann, and Goldstine I guess, that that wasn't what they wanted to do. We were going to make the rest of the computer. Circuits had to be designed and tested. The adder, even though the adder circuit was influenced by RCA -- they had built a model -- but the ones that were to actually go into the machine had yet to be developed, and made reliable, and tested. There's the carry time. We were going to use -- well, there were various carries considered -- but one is the direct carry all the way down the line rather than successive carries, but you have to build the adder or a chunk of the adder and measure the carry time to see how long it will take. So there was a lot of work to do. Now how far they got on that the first summer I was there I just don't remember. But that's what Bigelow was doing. He was setting up the lab and hiring people, hiring draftspeople; for example, I remember participating in an interview with a draftsman that he considered. So he was setting the organization up and starting the laboratory, and this was done in the basement of the main building. There is one main building -- well, you've been in the Institute, but you wouldn't know that one building because they've attached wings and so forth -- but there was one central building and that's where the lab was down in the basement until the new computer building was made. I also participated in discussions about the new building, and helped pick the site with Herman and Oswald Veblen. And I remember the guy coming up with the bricks. So I was involved a little in those negotiations.

STERN: But essentially your contribution was in the area of logical design at this point which was separate?

A. W. BURKS: Well, it was my contribution to that report which was logical design, and verbal. There aren't even any diagrams as I remember, but based on electronic considerations, and I can remember drawing circuits on the board, drawing circuits on paper I suppose. Maybe Herman has these in his papers somewhere. So that while we did the logical design in that paper it was also based on a lot of electronic circuits that we thought about, and while we didn't build them at that stage, we planned them as feasible circuits.

STERN: During this period, in the summer of '46, the Moore School was having its lecture series on the theory and design of computers. Did you participate in that at all?

A. W. BURKS: Yes. I was invited to give one or two lectures. I guess it was one. And it was very interesting to me because there was this problem of communication, or lack of communication, between the Moore School and the Institute, because by this time Eckert and Mauchly had decided to set up their business and IAS was operating and the Moore School was still going to continue with the EDVAC. There were these three enterprises. And there was a certain amount of planned isolation in the sense that Eckert and Mauchly, on the one hand, and Goldstine and von Neumann, on the other hand, were now clearly at odds over various issues. And Goldstine had arranged this so that I was actually to give a lecture on numerical methods. I didn't talk about the IAS machine, and I wasn't asked to talk about the Moore School machine, either. So I did give a lecture (actually, two), but not central to anything that we were doing at the time. I assumed that it was because of this conflict that they didn't want too much information going back and forth.

STERN: The very fact that they had this series of lectures would seem to indicate that they were concerned about disseminating information on computers. So that it wasn't complete isolation.

A. W. BURKS: No. What I am saying, Nancy, is that there was a certain amount of planned isolation between IAS

and the Moore School and Eckert-Mauchly on this issue. In other words, I am suggesting, and this is just a guess on my part, that Goldstine and von Neumann didn't want to say too much about what they were doing, and I certainly have the impression that Eckert and Mauchly didn't want to say too much about what they were doing either -- the later stuff -- in those lectures.

STERN: But based on Goldstine and von Neumann's attitude towards disseminating information prior to this period, there seems to be a change. Before they were really very much interested in disseminating information.

A. W. BURKS: Well, I think there is a difference between disseminating the information before you prepared the report and your attitude toward getting out reports. There's no conflict there.

STERN: I think I see what you're saying. At that point you went to the Philosophy Department here at Michigan?

A. W. BURKS: In August we left and went to Michigan.

STERN: And the next two summers you went back to the Institute. The first summer you went back what sort of progress was made?

A. W. BURKS: Well, they had their building and they had laboratories, and Bigelow had laid down many of the fundamental principles. There was another man involved, Snyder. Do you remember Snyder?

STERN: I don't.

A. W. BURKS: I think maybe Snyder was the chief engineer. Snyder had worked with Rajchman.

STERN: What was Snyder's first name?

A. W. BURKS: Dick. Richard. He had worked with Rajchman at RCA, and then he had gone somewhere else. He was at one stage, and I can't remember the sequence, working there, and maybe it was the first summer he was working there. I just can't remember.

TAPE 4/SIDE 2

STERN: We were talking about the engineers at the Institute. My list indicates that Ware started in 1946. He came from Hazeltine.

A. W. BURKS: So maybe he and Jim started at the same time.

STERN: I have Jim down at the same time and Ralph Slutz as June of '46.

A. W. BURKS: Yes, I remember his coming.

STERN: He also came from NDRC, National Defense Research Council.

A. W. BURKS: I thought he came from Princeton.

STERN: That may be. I'll have to check that.

A. W. BURKS: But he may have been at an NDRC project at Princeton. At least I have the impression he had been working at Princeton University. I know he was a recent Princeton Ph.D. I remember that. [His Ph.D. was] in physics.

STERN: And he left early. He left in about '48, according to my records.

A. W. BURKS: Okay, but you see, I didn't go back after the summer of '48. I don't remember whether he was there my last summer or not. Rubinoff was there one of my summers, I remember.

STERN: Rubinoff?

A. W. BURKS: Yes.

STERN: And Hildebrand started when? Do you remember?

A. W. BURKS: Well, it would not have been that first summer.

STERN: Okay. And I have a man who I don't know anything about. Peter Penogaust?

A. W. BURKS: Don't remember him.

STERN: And Jim Simms, who was Pres Eckert's brother-in-law.

A. W. BURKS: Yes, I don't recall Simms being there very much. Were you telling me that he was the first business manager? And, and I know that while I was there, and I'm pretty sure the first year, they hired as a business manager a young man named Bliss, who was the son of the famous mathematician Bliss at Chicago, under whom Goldstine had studied and learned differential equations and firing tables.

STERN: It was Gilbert Bliss.

A. W. BURKS: Right, yes. It was his son who, I think had recently married a woman who had a couple children -- may have been a war widow, I don't remember that -- and he was hired as a business manager.

STERN: Why did they need a business manager?

A. W. BURKS: Well, to keep the cost records. The Institute wasn't going to do this. That was to be paid for in the project itself. To get parts, get equipment, it's a big job to get the tools you need. Laboratory tables and all that stuff had to be ordered, you see. They were setting up a new building from scratch. The building would have to be supervised from the Institute's point of view. Herman didn't want to do all of that, of course, because he wanted to work on the scientific engineering.

STERN: But these people I mentioned essentially did work on the engineering end of it?

A. W. BURKS: Well, not Bliss and not Simms.

STERN: Simms was an engineer I thought.

A. W. BURKS: Well, I guess I'm surprised to learn that Simms worked on the electronics of the IAS computer.

STERN: Simms or Bliss?

A. W. BURKS: Simms. I know Bliss didn't because he didn't have any training. Herman must have been telling me this, that he had hired Simms as business manager. Do you know who Chedaker is?

STERN: Yes.

A. W. BURKS: Chedaker was an engineer on the ENIAC project, but in the end he was really the guy who ordered things, who saw that they came, who checked them. He knew enough engineering to be able to do the job, and I would think of Simms in a similar category.

STERN: I see what you're saying.

A. W. BURKS: So this is a business manager/engineer. Could I see that list that you're working from?

STERN: Sure. What about Bliss then?

A. W. BURKS: Bliss was not an engineer. He was a pure business manager. I think Herman was telling me that he hired Simms because Pres wanted him to hire him. At that time Pres was thinking of going to the Institute. Simms was a nice guy, and a friendly guy. Jack Rosenberg I remember very well.

STERN: I remember him.

A. W. BURKS: We went over to his house near Newark to hear his hi-fi. Do you remember how loud it was, Alice? We didn't quite appreciate all the volume.

STERN: Yes. And Thompson was the last name on there we didn't discuss. Do you know him?

A. W. BURKS: I don't remember him. That's not to say these people weren't around working in a laboratory, I just didn't have enough contact with them that they would stick in my memory.

STERN: Now after '48, you decided to work as a consultant for Burroughs.

A. W. BURKS: Right. That was arranged by Travis. Indeed the summer of '48, when we were living at the Institute, Irv Travis invited me down to Philadelphia to see him, and I went down on a Saturday, I think, and he interviewed me and said he was a consultant for Burroughs, and they were looking for a consultant and would I be one. And I said I would consider it, and I went back and we talked about it, and I talked about it with Herman, and Alice and I decided we would do that, because Burroughs was then in Detroit. They are still in Detroit in a sense, but all of their activity

was then in Detroit; there was no activity in Philadelphia, that came later.

STERN: So that your decision was based primarily on the proximity?

A. W. BURKS: Yes, that was important. I also felt that just being at the Institute in the summer, and given Bigelow's way of operation, I couldn't be as effective as I would like to be. But also I preferred not to have to move every summer, you see. The Burroughs consulting also enabled me to work during the academic year. The contract with Burroughs was to consult one day a week during the academic year, and the University allows that, and you can earn money for that. They gave me a generous, quite generous, stipend, and then I could work full time in the summer for them.

STERN: What did you do for them?

A. W. BURKS: The first semester, roughly, I went to Detroit, took the bus to Detroit, spent the day there, and then took the bus home at night, learning about their business, talking with them about machines. I spent a lot of time with the vice president for research and his assistant, because they didn't have many people working on computers then. I said, after a preliminary study, "Well, I will sit down and try to write a plan or proposal, kind of a guide or an outline, of what an electronic computer for business would be like, what it would do, what its possibilities were, because they were not yet convinced they wanted to go into electronic computing. Of course, their orientation was business, and all the prior computers, except the UNIVAC that Eckert and Mauchly were working on, were science-oriented, so that you had to have a different orientation. So they said fine, so after that I didn't go in every week, but only as I needed to, and I wrote reports. In the summer of '49, I can remember the vice president for engineering and his assistant, and Irv Travis, came out to Ann Arbor because they were considering setting up a research laboratory in electronics for computing in Ann Arbor. They came to our house on Morton Avenue late in the afternoon. It was a Saturday, I think. They were very disappointed because the zoning in Ann Arbor at the time was such that the only place they would be allowed to put a research laboratory was in the industrial area, and of course they weren't going to do that, and it just showed that Ann Arbor was old fashioned. Now it's all changed. They have research

parks, and so there are good places to put laboratories. Burroughs was also thinking of putting their laboratory at Paoli or in Philadelphia, because that's where Travis was located. Well, that was not a reason in itself to put it there.

At this time they said that they would like me to hire some mathematicians and scientists, and get a group started on designing, doing the logical design of machines and doing programming. So I then founded my group which is now called The Logic of Computers Group. They let a contract to the University, I think the original contract was only 22,000 dollars, which could buy a lot in '49, and that didn't include my pay. They continued to pay me as a consultant. They gave the project money to the University, and with that I hired people to do research, and we did planning on computers for business and planning on programming languages, and wrote papers, monographs, [and] reports. Most of these were never published because they were proprietary with Burroughs, and by the time the proprietary interests had ceased it didn't seem that they were appropriate to publish. Other people had done a lot of the same things. That's what happened after I left the Institute.

STERN: Did you, during this period at all, have any contact with Eckert and Mauchly?

A. W. BURKS: Do you mean after the summer course of '46?

STERN: Right.

A. W. BURKS: How much contact did I have with Eckert and Mauchly? Okay, just occasional contact. The Association for Computing Machinery met in Ann Arbor, seems to me in the late '40s, early '50s, and I remember John coming here. I remember Grace Hopper, at this meeting. I remember Huskey came to this meeting, stayed in our house, I think. Indeed, would it be possible that the ACM met in Ann Arbor? When did we live in the carriage house? In '48?

ALICE BURKS: '47-48.

STERN: '47-48 was the year that the event was founded.

A. W. BURKS: Huskey was here for another reason then.

STERN: Huskey was in Detroit, at Wayne.

A. W. BURKS: Not this one time. He was still in California and he came here for some purpose. Maybe to consult with Project Michigan. Then later he came to Wayne for a year and they visited us, and we went over there. We had our Morton house then. So anyhow Mauchly, came out to some meeting here, I remember that, and I remember that at the same meeting the man at Argonne who hired me -- his brother was a senator -- was here.

ALICE BURKS: Flanders?

A. W. BURKS: Flanders, yes. Moll Flanders came to that same meeting at the ACM.

STERN: Were you involved in the founding of the ACM?

A. W. BURKS: No, no. I became a member, not a founding member, but a member afterwards. I never went to meetings much, but after they met in Ann Arbor, I went, I went to a few other meetings. I did go back to Philadelphia on occasion. I remember being invited to lecture by some group, and Ike Auerbach is the one who met me and took me around, and this was some local computer organization. Pres wasn't at the lecture, I can't remember whether John was, but some of the young women that I had worked with at the Moore School, who now worked for UNIVAC were at the lecture. And then I went back when Chuan Chu was the chief engineer for UNIVAC, and this was just about the time Mauchly was leaving, because I remember visiting their operation, in North Philadelphia, I think it was. Chuan Chu showed me around, and Grace Hopper was there, I remember seeing her. Mauchly was still there directing a small group, but Chuan told me that Mauchly had resigned and would be leaving at a certain date.

STERN: From 1946 on, were there any sources you used to keep informed about other activities in the computing

field? How did you find out what was going on or didn't you?

A. W. BURKS: In the work I did at Burroughs, I had contact with people in Burroughs and Paoli. After they decided it wouldn't be feasible to set up a research laboratory in the Ann Arbor area for the zoning reasons, Burroughs decided that Philadelphia was the place to manufacture electronic computers, so Burroughs established its laboratory in downtown Philadelphia. I went there periodically to report on what I was doing. Sometimes I would take people from my group and we would go there and present what we were doing to the Burroughs people, so I had contact that way. The year 1950-51, the University of Chicago and Argonne worked out a joint deal where I was made a research associate at the University of Chicago, and they paid me an amount that was maybe half of a salary to do my research in philosophy and participate in their seminars, and then Argonne hired me at a consultant one day a week, and that paid me enough to be the other half of a salary. Chu was in charge of the computers at Argonne. Flanders worked there on them too, and a man named Alexander. So I consulted with them on the design of the machine that eventually became the Oak Ridge machine. Indeed out of some of our work a patent came, taken out in the names of Alexander, Flanders, and Burks, I think, on the arithmetic design of this new way of handling of complements in multiplication: carrying an extra bit and thereby not having to make as many complement corrections at the end. So I had that contact. Arvid Jacobson, who was at Wayne State, organized a program there, where I gave a paper on programming. I remember this was the '40s. Chambers came out and talked at this program. Gradually my interests shifted from architecture or computer design and software design to more theoretical things, so that by the mid '50s the work my group did was more automata theory than it was computer design or software design.

STERN: At what period did you found the CCS department here?

A. W. BURKS: Well there was a man on the faculty named Gordon Peterson, who had been a fellow student of mine at DePauw, in physics. He went to the University of Illinois, and then to the University of Louisiana, took a Ph.D. in physics and went to Bell Labs. Then he came here in the speech department after the war, and we became reacquainted. He had students in the physics of speech, phonetics, and acoustics and so forth. Then I began to teach automata theory even in the late '40s in my philosophy of science and mathematical logic courses, and so I had

some students who were interested in writing theses, basically in what we now call computer science, though we didn't have that name. John Holland was the first of these students, and they clearly didn't fit in our departments. That is, Holland wasn't about to study two years of courses, to learn history of philosophy and other philosophy courses, in order to write a thesis on computing. And Gordon's students didn't fit in his speech department, which was oriented toward speech and drama. So as a consequence, Gordon and I organized or started to organize the joint program in Computer and Communication Sciences, bringing in other people. In 1957, we got permission from the graduate school to give masters degrees and Ph.D. degrees, even though we didn't have any budget other than our research project budgets.

ALICE BURKS: It was a program.

A. W. BURKS: It was a program, not a department. That continued to exist as a program until 1967 when it became a department.

STERN: I see. In your paper "From ENIAC to the Stored Program Computer," you have a figure which describes the initial stored program machines. BINAC isn't one of those mentioned. Is there a specific reason why you didn't mention that machine?

A. W. BURKS: Why should I mention it?

STERN: Well, you list all the initial stored program computers.

A. W. BURKS: Oh, I see. That is down at the bottom of this diagram where I list UNIVAC and IAS and EDSAC and so forth. I wasn't try to list them all; maybe I should have mentioned BINAC. At the time I wrote that paper I knew very little about BINAC. I heard that it existed, and I can remember John saying at one of the meetings, sometime in the '50s, that he had thought it would always be better for them to go ahead and develop the BINAC and manufacture it rather than go ahead with the UNIVAC. So I knew it existed but that was about all. It just never occurred to me

when I wrote that paper to put it in, but maybe I should because of the priority. I wasn't trying to list all of the machines, I don't know, I kept adding machines. Huskey thought I should mention SWAC, and I think I probably added it. Do you think I should mention BINAC?

STERN: Well it seems to me BINAC was the first stored program computer completed in this country.

A. W. BURKS: It depends what you mean by completed, of course. Yes it worked well enough for the demonstration. It never did any useful work. Well, that's not quite fair. Let me ask it to you as a question: It didn't do much useful work, did it?

STERN: It did not do the work it was intended to do but it did do some work out in the California, and the reasons why it was dismantled are somewhat controversial. The people out there said it was Eckert and Mauchly's flawed production problem and Eckert and Mauchly said that the people out there didn't know how to use the machine so that there is some controversy, but the machine did work in Philadelphia. So they did have an operational machine.

A. W. BURKS: Yes, it worked for the demonstration, and then I guess you can raise the question had it been left there maybe it would have done useful work. I got the impression, originally from what you told me or what you wrote, that it didn't do much of anything on the west coast. But after you published your article then there was another article I read that said it did some useful work.

STERN: Two articles indicated that it did.

A. W. BURKS: All right, well that's something for us to consider whether to put it in; as I say I was not trying to be complete but given that priority maybe it should go in. It was certainly not like UNIVAC or IAS or EDSAC or a machine that was built and continued to do useful work during a reasonable lifetime.

STERN: Okay. In the preliminary discussion of electrical design you talk about possibly including floating point,

and it's decided not to do that.

A. W. BURKS: Yes.

STERN: On what did you base your decision?

A. W. BURKS: Simplicity.

STERN: That's really seen as a quote "mistake."

[The last ten minutes of the interview are undecipherable because of a failure with the recording device and cannot be transcribed.]

END OF INTERVIEW