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Interviewee: Harry D. Huskey and Mrs. Huskey
Interviewer: Henry S. Tropp
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TROPP:

This discussion is being held in Professor Harry Huskey's office, University of California, Santa Cruz, March 9, 1972. Mrs. Huskey is also present. We are going to talk about Tibor Rado at present. Am I pronouncing it right?

HUSKEY:

Yes, I think so.

TROPP:

Could you tell me something about him, as you remember him as a person and the kinds of things he was interested in and his involvement? You knew him during what period?

HUSKEY:

From l939 to l943. I was a graduate student at Ohio University for one year and then came up to Ohio State and worked on my Ph.D. there. I guess the thing that one remembers about him is that he was a very strict taskmaster. She used to kid me, because I always felt that I had to wear my new shoes whenever I went to see him. He took a great deal of interest in his students, and he had us up to his house on many occasions. In fact, we used to go out about once a week.

MRS. HUSKEY:

He would often have his graduate students, with their wives, out to his house.

TROPP:

This idea of him being a taskmaster is brought out in one letter that I got from a former student. She remembers taking a course from him and having to do a demonstration in front of the class, and being absolutely devastated by his comments when she forgot to say: "as x tends to 0", when it was just sort of implied. She said he was a real taskmaster, and she appreciated his desire for everything to be precise and carefully stated.

HUSKEY:

You know, there's a certain threshold. For the first couple of years, this was what life with him was like. Then, when the day comes that he decides that you're careful, thereafter, it's all casual, and you know that there are no problems whatsoever.

TROPP:

When once you've demonstrated to him that if he says: " do this in a careful, precise way," and he doesn't have to worry about your ability to do it, then you can wave your hands a little bit.

HUSKEY:

Then, you can wave your hands.

MRS. HUSKEY:

I remember that before you gave a talk for the first time and so on, he made you do it in front of him.

HUSKEY:

Yes, we always had a rehearsal when we went to a math meeting .with a paper to present, we had rehearsals in front of him.

MRS. HUSKEY:

You've always said that you owed your conciseness to him. He didn't want any verbal nonsense.

TROPP:

What did you do your dissertation on, under him?

HUSKEY:

It was in the area of surfaces. TROPP:

In what year?

HUSKEY:

I got my degree in 1943. It was in Lebesgue surfaces. I took the non parametric case, and tried to find under what conditions the inscribed polyhedra converged to the area of surfaces.

TROPP:

How would you describe the areas in which Rado made most of his major contributions, and the impact he had on mathematics subsequently?

HUSKEY:

Well, I think most of it was in the Lebesgue measure: theory of surfaces, in the non parametric case. In more recent years, he worked on computer topics.

TROPP:

I noticed in looking at his papers, that there are a whole series of papers near the end, in this area. Do you have any idea what triggered his interest, and what his involvement was?

HUSKEY:

I don't know. I guess the person to talk to about it is Roger House. He used to be at Corvallis, and I suppose he's still there. He wrote some joint papers with Rado in this area, and he would have the best answer to that question.

TROPP:

His earlier papers, when he did collaborate, were mostly with Richeldorfer.

HUSKEY:

Yes. In the area of surfaces. On one occasion, he described his experiences in prison, when he started to study mathematics and so on.

TROPP:

Did he begin his study of mathematics while he was in prison in Siberia?

HUSKEY:

Yes. He was a civil engineer before that. Apparently, in prison, he managed to get hold of a math book. He said: "because there wasn't anything else."

TROPP:

Did he ever mention what it was?

HUSKEY:

He may have, but I forgot.

TROPP:

And that's when he finally worked his way back to Hungary, and he began studying mathematics. Did he talk much about the period of working his way back from Siberia to Hungary after the revolution?

HUSKEY:

Oh, he talked about his train incident, And it sounded pretty incredible to me, then.

TROPP:

I haven't read about the train incident. The incident that sticks out in my mind is the one :d'ere he traded uniforms with an enlisted man, and went to digging ditches after the revolution, so that he wouldn't be an officer, and therefore, suspect. I often wonder what happened to the poor soldier with whom he traded uniforms. I don't remember the train incident.

HUSKEY:

Well, I don't know that I have it down real pat, but I guess he and some other people, right at the end of the war when there was a11 a lot of confusion, had commandeered this train, and went all the way across Siberia to Vladivostok. It seemed like an incredible story to me then.

TROPP:

They literally hijacked it.

HUSKEY:

Yes, they hijacked it. But, having been in the East, I could see how it could happen, you know. Things fall apart at the end of a war, and there's no structure. I could see it happening in India, for example.

TROPP:

Right. Well, it was pretty anarchistic at that point in time. The government was in a constant state of flux, and it ultimately changed.

HUSKEY:

And in Siberia, there wasn't any real control.

TROPP:

How did he get from Vladivostok back to Hungary?

HUSKEY:

I don't remember, but I guess it was by boat or something.

TROPP:

That was a roundabout way to get home.

HUSKEY:

Yes, it certainly was. It's too bad that he didn't tape record this story for us.

TROPP:

Well, he did recount some of his experiences for the student newspaper there, but they're again recorded in a secondhand case, so it's hard to tell. I guess that his wife could be the best single source. Probab1y the best thing to do is to talk to her if I'm ever in the area, instead of trying to do it by letter.

MRS. HUSKEY:

Well, they have two children too.

TROPP:

Where are they living?

MRS. HUSKEY:

I'm not sure. The last we heard, the boy was going to UCLA, or was thinking about going to UCLA. He was interested in snakes and things. The girl married an artist or somebody in the East.

TROPP:

Well, he just sounds like a fascinating person. The comment that you made about his involvement with students seems to come forth in the letters and responses that I've gotten from former students, although they did indicate that in class, it was a very formal

atmosphere. It was outside of class that this interest and involvement occurred.

MRS. HUSKEY:

Well, these evening meetings were also, part of it, no nonsense When you arrived, the wives were separated from the husbands for a while, until they had done their talking.

TROPP:

Until they had done their mathematics.

MRS. HUSKEY:

And then we'd all get together. You had this feeling that he would really go all out for you.

TROPP:

This is, of course, the way a lot of mathematics is done. It's a corridor kind of mathematics, rather than n classroom.

MRS. HUSKEY:

Mrs. Rado was a very interesting person too. He always used to joke that he was going to open a dairy farm with his daughter, when his daughter got over twenty. I enjoyed them.

TROPP:

In doing a fairly short, and obviously succinct and pretty concisely written biographv of him, can you think of any aspects that you would think ought to be emphasized?

HUSKEY:

Oh, I would guess that it would seem to me that the highlights were his military prison experience and how he started in mathematics, and the business at the end of the war. Then, I guess I don't really know much about when he started in Texas someplace, before he came to Ohio State. I don't know anything about that.

TROPP:

Well, you were there about the time he became +d' at the documents call, Ohio State's first research professor.

HUSKEY:

No, that happened after I left. While I was there, he was just a member of the Department. He became Chairman a little later on. In l943 Kuhn was still Chairman of the Department, and Rado was just the outstanding professor in the Department.

TROPP:

Do you have any idea what kind of distinguishing characteristic there was to the title: Research Professor? Did this entitle him to work with graduate students and on his own research primarily? What were his teaching duties?

HUSKEY:

I would guess, but I don't really know, because that all came after. They didn't have such a thing when I was there.

TROPP:

That's right. He was the first one, as I remember.

MRS. HUSKEY:

What was the name of the fellow who came in as Chairman, after Rado? He was a fellow who was in your group. He would know a lot about that.

HUSKEY:

Mickle.

MRS. HUSKEY:

No, not Mickle.

HUSKEY:

Helsel.

MRS. HUSKEY:

Helsel, yes. Helsel should know a lot about that. He's at Ohio University now.

HUSKEY:

My guess is that this professorship was more of an honorary title, and it didn't really have much difference in what Rado did.

TROPP:

I see. What kinds of courses was he teaching when you were there?

MRS. HUSKEY:

Real variables, and things related to the area of surface sort of questions. Point set topology. He was always interested in what he called the practical things. You know, most people wouldn't think that the area of Lebesoue surface was really all that practical. I can remember when he poohpoohed Eilenberg's work in topology, this fiber bundle stuff and so on: "These guys are just out after academic problems. They're not being realistic."

TROPP:

So it was really this whole area that was triggered by his earlier work on minimal surfaces.

HUSKEY:

Yes. He always thought in terms of working on some practical problem in this area of surfaces. That was the real motivation.

TROPP:

It's hard to tell, in looking back at the papers, to pick out this emphasis on practicality. The papers don't look like practical papers.

HUSKEY:

He had a real running battle with Besicovich on this Lebesgue area sort of problem.

TROPP:

Can you think of anything else that you might add?

MRS. HUSKEY:

I don't know anything about his work.

TROPP:

This is really sort of a positive thing that I hadn' t figured on. You know, I've looked through the backgrounds of different people that I've ta1ked to, and nowhere did I draw this connection, although I should have known by the dates, that you were there

simultaneously. I guess I still think of Ohio State in contemporary terms, being such a massive place, that unless you were very closely connected with a particular individual, the contact would have been pretty minimal

MRS. HUSKEY:

Rado was really small in stature, but he was really an overpowering individual. I think that he sort of had extraordinary control over his students. They would be glad to do almost everything he wanted them to do. We really owed him a debt, because one time, when we were at the University of Pennsy1vania, I remember Harry called him to ask whether he thought he should go to Oklahoma University to take a job there that he had been offered. Rado, in this conversation, talked him into coming to Ohio that summer, and teaching in the program at Wright Field. He had no intention at all of going to Ohio, before he had his conversation with Rado.

HUSKEY:

I was almost ordered to go, you might say.

MRS. HUSKEY:

Another time, he had a couple of fellows come out to build an outside fireplace. He decided he wanted a fireplace to use as a barbeque. His wife was away at that time, so he asked the wives to come along, and get some food for him.

TROPP:

Did he have the kind of general ebullient personality that I associate with most of the Hungarian mathematicians that I've met?

HUSKEY:

No. He was very aristocratic and stern, it seemed to me.

MRS. HUSKEY:

You could imagine him taking over a train. He would ask somebody and you couldn't dare refuse him.

HUSKEY:

And you would expect it to happen.

TROPP:

I think that characteristic and his ability to get people to respond as if he were really in charge, adds to the credulity of the train story.

MRS. HUSKEY:

Well, this time (that Harry took the job that he really didn't intend to take) without even consulting me or anything, is an example of that.

TROPP:

Well, let's move from Rado and your information on him, on to you. You finished your degree in 1943. How did you go from Ohio State and an interest in Lebesgue measure and surfaces, to the computer? Well, I went to teach in the Math Department at the University of Pennsylvania, under J.R. Kline. While I was there, I was interested in earning some extra money, so I applied for some extra work over at the Moore School. After I got in the door, I discovered that here was a computer.

TROPP:

How far were they along by the time you got there?

HUSKEY:

Well, they were busy fabricating it. I suppose half the racks were set up or something, and they were busy making the works for it, and things of that sort. All the design work was pretty well done, and it was just a matter of fabricating it and testing it and so on.

TROPP:

What areas did you work in, primarily?

HUSKEY: I got involved in the report writing of it, describing how it worked and all.

TROPP:

So you had to go back and learn all about how they put this thing together.

HUSKEY:

And see then just what it was.

TROPP:

What were some of your feelings and attitudes at that time?

HUSKEY:

I guess that's the thing that one always regrets. In 1938, when I was at Ohio University, I thought that the graduate program there wasn't particularly difficult, so I was doing some other things, and I tried to invent n fluid transmission, you see, that they have in automobiles now. I even drew them up, and went to the mechanical engineering department, and they said that it wouldn't work, and I believed them. I also worked out a relay stepping arrangement, n decade relay arrangement. I said that it would be nice to build a computer with that, but by the time I added up how many relays my computer had, and it looked like it might be several hundred, I decided it wasn't practical.

TROPP:

Had you heard about any of the work that was going on?

HUSKEY:

I didn't know that Bell Labs was doing this.

TROPP:

Right. So you didn't know about any of the work that was going on at Bell Labs, because that was just about the same time.

HUSKEY:

So the thing that I regret is that I really didn't document that effort in some way, because although I talked to some of the other people at Ohio University, they couldn't remember it at all. So, you know, I just forgot all about them, and went on through graduate school, and then when I walked in and found this big machine at the University of Pennsylvania, I realized how it all fitted together, so to speak. It's really interesting.

TROPP:

In terms of your background then, coming in to the machine, you really hit that project pretty cold until you walked in through the door. So the only input in terms of ideas, were the kinds of things you had thought about back in your earlier years, in terms of step relay kind of computer.

HUSKEY:

Yes. Of course, I was interested in applied mathematics and integration of differential equations, and taught courses like that. The whole mathematical side of it was no problem at all.

TROPP:

Had you ever had any involvement with differential analyzers?

HUSKEY:

I had no involvement with differential analyzers, but I knew some electronics, primarily because I had friends who were ham radio types. It wasn't too hard to kind of get caught up things then.

TROPP:

As you look at the latter period then, the getting ENIAC to the stage you saw it, where only the fabrication remained until it finally got operative, do you have any comments to make on some of the people there and the roles they played in getting ENIAC going? I'm thinking, of course, particularly of von Neumann and Goldstine.

HUSKEY:

No, I think the main contributions were certainly Eckert and Mauchly. I'd put Eckert in terms of the engineering. He was a first class man in electronic circuits, and in terms of anybody being responsible for its being reliable enough to really do some computing, then I'm sure that's Eckert. I think the basic ideas were Mauchly's. Now, all these ideas were developed in a preliminary form by these people, and I'm sure in working out the details, that many other various engineers on the project contributed. But I think the basic ideas go back to those two people.

Now, the actual contributions of von Neumann to that activity were really quite sma11 in parts. 1ooking at it from the point of view of the computer in the development of the circuits, his contribution at that level was almost zero.

TROPP:

What role did Arthur Burks play in that period?

HUSKEY:

Well, I reported to him, and we were responsible, under him, for doing this report on how the ENIAC worked. He was involved in putting together some of the units, like the multiplier and things of that sort. I'm sure there are things that he contributed, but I'm not sure what.

TROPP:

I'm going to have an opportunity to see him, I think on Saturday, so I'll get a chance to

hear, from him, how he viewed that period. Then after ENIAC was completed, did you stay at the Moore School for any length of time?

HUSKEY:

Well, it was dedicated in February, and I left in June of 1946.

TROPP:

Where did you go?

HUSKEY:

Well, when I left the ENIAC, we thought we might end up out at the University of Oklahoma, but we had a prospect of a job at NPL in England. I went to Ohio to teach the course for Rado, and while we were there, in about July or so, we got word of the offer, so we decided to go to England then. The trouble is, it almost took six months then, to manage to get a boat reservation so that one could go. It was pretty tight at that time.

MRS. HUSKEY:

Working on the ENIAC actually changed his whole career then, because he met Professor Hartree there, and Professor Hartree was interested that he come to England. Of course, Harry was interested in going to England because of the opportunity of working there on computing machines, and also to be able to do a lot of traveling. It would be very appealing to a young man. It was through Dr. Hartree then, and meeting him there, that he had the opportunity to go to the National Physical Laboratories, attending it for a year and working there on the ACE, with Turing and Wilkinson, etc. TROPP:

Hartree's visit to the Moore School was when?

HUSKEY:

It was in the Spring of 1946.

TROPP:

Was it primarily to see what it was that ENIAC did, in terms of going back to the National Physical Laboratory?

HUSKEY:

I guess. I don't know the background as to why he was there. I'm sure he was interested in computing, since he had been interested in computing for a long time. So I'm sure that when any such opportunity occurred, he would jump at it, of course, in terms of trying to

get similar activity going back to England.

MRS. HUSKEY:

I think for at least one trip, there was also a family reason for him coming too, because they had their children in Canada during the War, and I've heard many times that they'd evacuated their children. So Mrs. Hartree came with him, and I know they spent some time in Canada with their children.

TROPP:

In talking about the year that you spent at NPL with the ACE Project. in terms of the input of ideas that went into the development and the conceptual aspects of ACE. what would you consider the most significant aspects of that?

HUSKEY:

I guess it's hard to say what was most significant. That operation sort of went on at two levels, you know. Turing was up there with his head in the clouds or something, and the rest of us guys were down there trying to make something he thought would really work, you see, and everybody was contributing one way or another. When I arrived, it was pretty much a software sort of project, as we would characterize it now. We could see that there was little expectation that we could build the hardware there. We thought that we could get somebody else to do it. During the year, I encouraged them to build some hardware. In fact, we got the pilot model project underway. We actually made some delay lines and things of that sort.

TROPP:

Would you characterize the design aspects of the machine as being an outgrowth of things that Hartree and others had seen here, from contacts here, or was the input from other directions?

HUSKEY:

I think the basic input was from Turing. He had certainly been exposed to some other ideas, I guess. He was the kind of person who would not study what somebody else had done. If somebody had done something, he would figure out his way of doing it.

TROPP:

How would you describe the ACE in terms of the primary characteristics that would identify it? Can you describe its capacities and the ways in which it functioned?

HUSKEY:

The distinction between it and the von Neumann type machine, is the fact that the logic and so on is much more intimately related with the memory functions and so on. As you transferred information around, you would modify it at the memory locations in contrast to having this box over here where all the arithmetic stuff was going. So typical operations were memory to memory transfers with modification going on, with a logical here and addition there, and things of that sort. I really characterize it, I think, by saying that history will show that there were to philosophies of design back in those days: there was what's come to be known as the von Neumann design, and what you call the Turing design.

TROPP:

In the work that you are doing now, do you plan to go into this second characterization which you call the Turing design, in much detail?

HUSKEY:

Well, I don't know. This is sort of going on at two levels. We're getting ready a paper for presentation at Japan if it's accepted, and I don't think I'll get that far in that one. It'll go over the subject matter in a lighter form. But we want to write this up in more detail, and this is certainly one of the points that I would like to make.

TROPP:

I'm hoping to get to that meeting in Japan, but if I don't, I hope at some date, you'll get me a copy of your paper, just so that I can see it and use the information input. I talked to Dr. Fine on Tuesday, and I was supposed to have a paper, which still isn't ready. I just heard about it a month or so ago, and their deadlines are a little tight. You plan the paper, and then subsequently, you plan to write this up in more detail form.

HUSKEY:

Yes.

TROPP:

In your historical archives, can you think of areas that you've run into in this time period that you're talking about, that are not generally known in the literature, that you care to mention?

HUSKEY:

I guess I'm not quite sure what sorts of things you're interested in. There were some meetings in Washington, for example, where people had made statements about what had

happened, relative to the MADDIDA development at Northrop, for example. I remember at that time, I found some correspondence saying that as of a certain date, their equipment didn't work, but at a meeting, you know, they quoted a date a year prior to that, and things of that sort.

TROPP:

I've been trying to pin down the date on which they brought the machine to the East Coast, and set it up in a hotel room. Von Neumann joined them one evening after they had gotten it going, and actually programmed a problem on it.

HUSKEY:

I don't have the dates for that one.

TROPP:

I know the incident, and I'm sure that I can pin down the date, because they wanted to get additional funds, and somebody suggested that they ought to have somebody of very high stature evaluate MADDIDA, and they decided that von Neumann was it. But he was too busy to come to the West Coast, so he said: "You claim that this machine is portable. If it is, bring it east, and I'll find time." They did, and set it up, and I think he programmed a Bessel function. They said: "We'll have to get some tables." "Oh, you don't have to get any tables", von Neumann said, "I know the solution."

HUSKEY:

That's believable. I imagine they sweated to make it work, too.

TROPP:

Apparently, he got there about 11:00 in the evening, and by 1:30 or so, he re wired it and had it programmed. He wouldn't trust their program, because he said that he needed to be convinced that they hadn't already put the solutions in. So the only way he could be sure of it doing something which wasn't already programmed in the machine, was to do it himself. He taught himself how to do it, and he did it all, I gather, within about an hour and a half. It's absolutely amazing.

HUSKEY:

I wouldn't be surprised.

TROPP:

That's a psychologically plausible story.

HUSKEY:

I guess the interesting question is: How much did those people that did that get out of the BINAC, you see, because BINAC was delivered prior to that activity.

TROPP:

Their motivation for building the MADDIDA was because they just weren't getting any time, any calculations out of the BINAC. It just wasn't working enough. They just weren't able to get anything out of it, so they wanted something that would give them some computing time. That was the motivation. In talking to some of the people who were involved in MADDIDA, I think that question is hard to answer because you get the feeling that they had their own concepts, and they were going to put something together that worked, and they weren't even thinking about where their ideas came from. They were going to engineer this thing and get it going, and start doing some calculations. The BINAC input is very difficult to ascertain in talking to them. HUSKEY:

Probably it didn't supply much in the way of technical information, but it was the real motivation, at least.

TROPP:

That type of machine was the same kind that so many people were successful with, even though that particular one was of almost no success. Would you care to make any comments at this point, because I have so little information about Turing and the contacts that you had with him, during the year that you were at the National Physical Laboratory?

HUSKEY:

I guess he was easy to characterize as one characterizes prodigies or genius types. They're impatient with the rest of us and a little difficult to talk to. They didn't respect him all that much. They thought some of his ideas were pretty crazy. I can remember having arguments; he was delivering lectures to the Ministry of Supply, so we'd go from Teddington up to London on the same train. This was Fox, Wilkinson, Turing, and myself, and maybe Woodbury or somebody. I remember on occasions arguing about how to do floating point arithmetic. Do you normalize or not, and what do you do about cumulative error that you know all about, and questions like this. Turing would get mad about this. He would get so emotionally upset that he could hardly deliver his lecture.

TROPP:

I take it he wasn't too worried about the cumulated error. If it was a problem, it just wasn't going to bother him.

HUSKEY:

No, he seemed to have some idea of how to get around it, I think. TROPP:

Did he ever communicate what the idea was?

HUSKEY:

No, he never explained it to us very well. At least, we couldn't understand it, anyway. You know, his mother talks about him running, for example. You've read her book, I guess.

TROPP:

No, I haven't.

HUSKEY:

You should read it some time. It's quite an interesting book. He was a contender for the Olympic Games in long distance running, a sixteen mile sort of distance, so he frequently ran. One time, we were to go out and visit the post office at Hollis Hill, and this was just about sixteen miles, so he decided that he would run. He and I were to go up this morning, so he would run and I'd go up on the train and carry this little satchel with his clothes in it, you see, and see who'd get there first. I was handicapped somewhat because I had to take three trains, you see. I went into London part way, and took a circular train around, and then took another train out, and sure enough, he was there when I got there.

TROPP:

I have visions in my mind of this man running alongside of the train and beating it. That's the visual image that comes up. (Laughter) When did you first read the1936 paper?

HUSKEY:

Oh, gosh, I don't know. I don't think while I was in England.

TROPP:

Had you read it before you went to England? Were you familiar with his work?

HUSKEY:

I don't know.

MRS. HUSKEY:

You didn't know anything about Turing before. You didn't even know his name, did you?

HUSKEY:

No, I didn't know anything about him before I went to England. I didn't even know his name.

MRS. HUSKEY:

I mean, Hartree hadn't even talked about him in computing. You see, he wasn't very much part of it at that time. You had trouble getting money and difficulty getting the thing built.

TROPP:

What were some of the different roles that some of the people played, whom you mentioned, like Wilkes and Wilkinson?

HUSKEY:

Well, Wilkes was doing his own independent stuff at Cambridge, and had cut back on proponents requirements. He was trying to make his delay lines run at 500 kc., instead of the megacycle that the Moore School was shooting for. So, he got his stuff running much more quickly, and fully deserves credit for running the first stored program example.

TROPP:

I guess this whole stored program concept is one that is another one of those controversial areas.

HUSKEY:

I guess my feeling is that when you're given all the environment, then it's automatic, you see. You start with something like the ENIAC, and obviously that's not a good way to go because it costs so much to store decimal digits. So you cast around for other means of storing information. I think probably somebody like Eckert has the greatest probability of having supplied the idea there. He was working in radar related activities before he moved over onto the ENIAC thing. The use of delay lines for timing measurements, I suppose, was the way that was done. Instead of splitting a single pulse down this line, all you had to do was split several, and have a way of feeding them, and you had your memory system. This was clearly going to be much cheaper than the flip flop sort of arrangement.

Then, once you go that far, you look up this plug in jumper cadence on the front of the

ENIAC, and you say: "Well, I'll have to manage to have the control information handled in very much the same way that the data is." You've already designed an arithmetic count, so you're thinking about processing data, and then switching the stuff in and out is just something analogous to the arithmetic functions, you see. So, I think that given the environment, that that development is automatic.

TROPP:

The name usually associated with the concept is Von Neumann.

HUSKEY:

Yes, but I think the other people there should deserve just as much credit, you see. In fact, I don't remember, in Von Neumann's 1945 report, that he makes any great to do about storing of information in anything like a delay line. I think he talks about storing information of flip flops, which was being done prior to this contact with this sort of thing, but I think the actual storage of information along delay lines probably more properly goes to somebody like Eckert than to Von Neumann.

TROPP:

Lou Fine talked about this same kind of thing in RAYDAC. Of its contemporaries in that period, 1951, I think, I believe it was the only one using delay line.

HUSKEY:

No. You see, the delay lines were developed at the Moore School. The first work in delay lines was done at the Moore School by a fellow named Brad Shepard, as part of this ENIAC project. The EDVAC was premised on the use of delay lines, and this was all well underway by the spring of 1946, and at their summer program when they brought in a lot of people and lectured to them that summer. That's where most of the dispersion of ideas came from. Wilkes attended that summer, of course, and Von Neumann built his machine, you see.

TROPP:

Do you have any notes from that summer course?

HUSKEY:

Oh, there are published lecture notes on it, you know. There are two volumes on it. I saw the second volume out the other day, and I think I have it at home. These were published.

TROPP:

This was the prime input then, for dispersing the information on not only the work of ENIAC, but the work that was going on at that time in terms of ideas that later...

HUSKEY:

Yes. There were ideas about binary number systems, recirculating memories, arithmetic units, binary arithmetic, and all the rest of it. But I'm sure that Raytheon came into it after the period.

TROPP:

Oh, this was much later. This was about four years later.

HUSKEY:

Raytheon was proposing to build a delay line machine in 1948, for example, when I was at the Bureau. We were busy trying to contract with them about these things and so on.

TROPP:

Well, the story, as I'm able to piece it together, is that they not only proposed to build it, but apparently, they said that they could build two for some relatively small amount of money.

HUSKEY:

I think it was something like $50,000.

TROPP:

It was something on that order of magnitude. After a year, they weren't very far along, and finally, in a third, or possibly even a fourth year, they became realistic and said: "Well, it's going to cost something like a million dollars, and that will get one machine." That, ultimately, resulted in the Raytheon machine. But I think the proposal, and the statement that they could do this in a year, and do two machines for a small amount of money, was about 1948.

MRS. HUSKEY:

Well, you see, it was in 1948 that we came back from England, and Harry started at the Bureau of Standards, in Washington. Part of his job was to check out Raytheon, and ...

HUSKEY:

And Eckert and Mauchly and so on, you see, because I was responsible for the Census

Bureau requiring a computer. Then, Curtiss wanted to get two computers for his mathematical laboratories, and he had some Air Force money to use on this. So we were looking at what Raytheon could do and what Eckert and Mauchly could do and we even talked to Engineering Research Associates out in Minneapolis, and so on.

TROPP:

How would you characterize the state of the art in 1948?

MRS. HUSKEY:

All they were worried about is if they were going to work.

HUSKEY:

All the effort was with these two or three groups, and all the other people worried about whether it would work or not. We were worried about whether they would stay alive financially long enough to make something.

MRS. HUSKEY:

And that's the time that report came in that he was talking about.

HUSKEY:

Yes, and that's when this Academy of Sciences thing came in, saying: "it won't work."

MRS. HUSKEY:

John Curtiss gave a cocktail party to celebrate this report, and of course, they all felt very bad about it at the Bureau of Standards.

HUSKEY:

Curtiss' cocktail party was like having fifty people in these two rooms, you see.

TROPP:

Wall to wall.

HUSKEY:

Wall to wall, yes. And so in the middle of the party, he picked up this report, you know.

MRS. HUSKEY:

You don't dare say it with the tape recorder.

TROPP:

That's all right. He can delete it later.

HUSKEY:

He said: "I know what I'm going to do with this." So he goes in the bathroom, and you hear the toilet flush.

TROPP: (Laughter) That may be where most of the copies have gone. That may be why it is so hard to locate a copy of it, in this point in time. Well then, you came from the National Physical Laboratory to the National Bureau of Standards. How long were you there?

HUSKEY:

Well, it was supposed to have been six weeks, because I was to go out to Los Angeles to this field station. That's where we had originally accepted the job to come, but I ended up being there almost a year. We came out about the first of December of that year.

TROPP:

And you spent most of this year, then, surveying what these various groups were doing, in order to try to see what you could get for the Bureau.

HUSKEY:

Right. The last summer, I managed to get the Bureau in the mood of building a computer. That's what got the SEAC started that summer.

TROPP:

Was that essentially your recommendation after you'd surveyed what was going on that the best way they were going to get something was to do it themselves?

HUSKEY:

The Bureau was in this position of having to pass on equipment and so on, in almost an academic kind of environment. They had not really ever made any of this hardware and didn't know how hard it was to make it work, and things of this sort. So, I recommended that they get busy and put one together, and that way they would know more about what they were talking about, and it might be that they just could get it together before many of

the vendors could, anyway.

TROPP:

Which turned out to be true. That's essentially what happened. Then, you stayed at the Bureau for a year, and then where?

HUSKEY:

To the Institute for Numerical Analysis, at UCLA. That was the Bureau's field station. We built this SWAC there.

TROPP:

Numerical Analysis was almost a new word at that point in time. Who were some of the people that you worked with there?

HUSKEY:

Oh, the mathematicians there were people like Hartree. He was there for a while, and Mellon, from Oregon State. There was Beckenbach, and let's see...who else?

END OF SIDE 1

TROPP:

I think, as we left things before lunch, we were talking about the period at UCLA, at the Institute, and you were going to mention some of the work there, and some of the mathematicians who were involved.

HUSKEY:

Yes. People who come to mind are people like Berkley Rossiter, for example. I guess I mentioned that Mellon and Hartree were out there, and let's see... who else was there? Of course, Beckenbock from UCLA participated, and I still can't think of the name of this fellow at Zurich. Rudishauser was there. Well, anyway, a lot of people like that stopped by for long of short visits, and it was a very interesting place to be. It's too bad that we don't have something like that still going on today.

TROPP:

In building the SWAC, who were some of the people that worked with you and under you, that were responsible for various aspects of it?

HUSKEY:

Well, the people that worked for me on that were, let's see. Oh, there was Bill Gunning, who came over from Rand Corporation and helped out, and went back to build the JOHNNIAC for them.

TROPP:

Bill Gunning. And what area was he involved in?

HUSKEY:

He worked on things like the memory detection circuits for the Williams tube memory. There was a fellow named Rutland, who worked on the control logic. There was Harry Larson, who worked in various parts of it. He's at Calcomp. Let's see... who else was there? There was a fellow named Ambrosio, who worked on arithmetic circuits.

TROPP:

What were some of the problems that you had with the Williams memory?

HUSKEY:

Oh, we had all kinds of problems. The biggest problem was the fact that these had flaws on the face of the tube that wouldn't store information, and after we were well committed, we discovered that DuMont made these tubes in an ex mattress factory, which is the reason they had some lint on them. It doesn't cause any trouble under normal use, you see, but when they evacuate the tube and heat it up, the lint turns to carbon, and that makes a lower conducting path then, so you can't store a charge where you want it to be.

TROPP:

How did you solve that particular problem?

HUSKEY:

We ended up by selecting tubes in such a way that you could adjust the pattern so that no one of the bits was on one of these spots.

TROPP:

But in checking the tubes, you had to identify where these spots were.

HUSKEY:

There was no way to tell except by trying to store information, you see, because they

weren't visible on the inside of the face of the [?], so you couldn't see them. We tried it, and if it worked, okay, and if it didn't, we got another one.

TROPP:

You would get a fairly high rejection, I would gather.

HUSKEY:

We would be quite selective, and mainly, we adjusted patterns so we could miss the spots. But you were never quite sure how far from such spots you were, and maybe with changes of voltages, you could get back on, you see.

TROPP:

Right. I say the accuracy with which you were able to, in certain regions of the tube, was probably limited with the ability to regulate the voltage and to keep it at a constant level.

HUSKEY:

This problem caused us to really go all out in the matter of regulations. From that point of view, we had a pretty good system. Short range regulation was about 1 part in 100,000, and just because we had all this trouble.

TROPP:

How long did it take from the time you got to UCLA to get the SWAC into operation?

HUSKEY:

Well, I arrived in December of 1948, and it was dedicated in August of 1950.

TROPP:

And it remained operative, you said, until very recently.

HUSKEY:

It operated until about 7 or 8 years ago.

TROPP:

I think it would be neat for you to tell the Robinson story about the Mersenne primes, so that it's on tape.

HUSKEY:

Okay. Fairly soon after we got the machine operating, Professor Robinson, of the Math Department at Berkeley, wrote a program to compute Mercenne primes. He did this without ever coming down to the Institute. I guess we'd send him up some information on how to program for it, and he may have known Dick Lehmer, who had been down. At any rate, he wrote a program and punched it into cards, and sent it down. So, we decided to try it, with no confidence that it would run because, typically, programs don't run the first time. Anyway we read it in and it appeared to run, and so, not having anything better to do that evening, we let it run awhile. I guess we started around 8:00 p.m. or so, and just after 10, we got a first prime out of it. It said it was a prime, but we couldn't be sure. It just happened to, so we let it run on, and another half hour later, we got a second prime. This produced the two pairs that were just above 500. We ran it quite a bit more, and I guess we picked up the next two or so, and that was about the limit with what we could do with that program.

TROPP:

Were you familiar at all with the efforts at Berkeley and I guess that Lehmer was one of the individuals involved to design and build a machine? I don't know that anything was ever completed, but were you familiar at all with their efforts?

HUSKEY:

Yes. He built what he called a number sieve, and this is to find numbers whose residues are the same, or something of this sort. He was interested in factoring type problems and so on.

TROPP:

Right. This is primarily to help build a table of primes and prime factorizations?

HUSKEY:

No, I think he was really interested in other kinds of number theoretic problems that you could solve by searching through numbers with this technique. I guess he built a mathematical sieve very early in the game. I think it was at the Chicago World's Fair where he was supposed to have exhibited a mechanical one.

TROPP:

Are you speaking of the 1933 Fair?

HUSKEY:

It was about that time, or perhaps it was 1935.

TROPP:

Because the Fair in Chicago that I remember, as a child, was 1933. It was called the Century of Progress. He had a mechanical sieve?

HUSKEY:

It was a bunch of disks, you see, and each one had a prime number on it, and ran so that whenever you got the product of certain combinations, then the holes would all line up and your light beams would shine through, and your photo cell would turn the motor off. Then you'd have to run back slowly to find out what it was, because you were close by. But he had a mechanical one like this, and he's built several electronic versions of this now, and he's still working on things of this sort.

TROPP:

When was the first electronic one that you're familiar with?

HUSKEY:

I suppose it was in the late fifties, but I just don't know for sure.

TROPP:

I think he's a person that I do need to see. I was about four doors from his office yesterday, and didn't have the chance to stop in to see if he was on campus. I hope to see him on my next trip.

HUSKEY:

He was involved for many years on the ENIAC activity, because he worked at Aberdeen Proving Ground.

TROPP:

He's been involved, I think, in a number of the activities because of his interest in doing some of these number theoretic problems. But I think there was some effort to build a digital computer, and I've only heard about it vaguely.

HUSKEY:

At Berkeley? Well, there was a project that was built by Professor Morton. He had a project very early, and it was pretty well completed in 1954 when he came up to

Berkeley.

TROPP:

And this was what kind of a machine?

HUSKEY:

It was an electronic, magnetic drum type machine.

TROPP:

What kind of an input did you have on the SWAC?

HUSKEY:

Punch paper tape to start with, and we added punch cards fairly soon, because we didn't care for the punch paper tape.

TROPP:

Were you familiar with some of the work that was going on at that time on magnetic tape?

HUSKEY:

We bought a tape drive from the Raytheon people, but we never really got around to getting that operative. We considered it a marginal device.

TROPP:

That was essentially then, the same one that was used on the RAYDAC.

HUSKEY:

Yes. It was an interesting tape unit. It was a slack mechanism that rode back and forth on the tracks. It was nicely designed.

TROPP:

There were some problems, apparently, with getting a uniform coding, and identifying the areas where there were flaws, so that you could use this as sort of a stop block.

HUSKEY:

That's right. The tape wasn't nearly as good a quality as what they have now. It was a serious problem.

TROPP:

I've heard descriptions of some of the difficulties they had. In working with audio, we aren't concerned with the kinds of things they were concerned with.

HUSKEY:

There'd be little lumps of magnetic material, you know, that were higher than the rest of the tape, and if that went over the head, then it would cause them to lose some information

TROPP:

So they had to make sure that when they isolated those, there was no information in that area. That caused some difficulties and took a good deal of time to locate. You were at UCLA then, until SWAC was completed.

HUSKEY:

Yes. I was there until 1954, really, except for a year's leave at Wayne University.

TROPP:

Now, Wayne had some computing equipment during that period, because Lou Fine told me that somewhere in the 1952 period, he was commuting and teaching a course there.

HUSKEY:

He taught a course. I went there before he started this course teaching. When I was there, we were involved in getting this computer from Burroughs, in Philadelphia. In fact, we got a teletype link running to the Burroughs computer, so we had a remote terminal then.

TROPP:

I guess the first remote terminal, though, was really the Bell Relay.

HUSKEY:

The Bell System demonstrated that.

TROPP:

It was at the 1940 Mathematics Meeting. It was one of their demonstrations that everybody got a chance to see.

HUSKEY:

I don't think it's worth putting any effort on, but I've wondered if we didn't have the first remote terminal in electronic computers. You know, Detroit to Philadelphia is several thousand miles away.

TROPP:

I'm not sure about the time, but when the University of Toronto had their Ferranti, they had hookup with the Medical School at Saskatoon, I think, but I'm not sure. And, again, the time is going to be about the same. When I asked Kelly Gotlieb about how this operated and what came of it, he said: "Well, one of the things that came out of it was that their operator married our operator." But I gather that it was not too efficient.

HUSKEY:

Well, I think there were problems.

TROPP:

They had real problems, and often people from Saskatoon would have actually come to Toronto and just worked [?] there.

HUSKEY:

They didn't have much information about it at that point.

TROPP:

They did have a teletype hookup, and I'm just vague about the dates. It would be in that 1953 1954 time period, I would guess in approximately the same period you're talking about and I don't know that that was even the first. It's hard to identify these things because there were so many little things going on in different places. But it surely was one of the early ones. Now, how effective was your teletype terminal to the Burroughs?

HUSKEY:

We didn't do any substantial computing. We demonstrated it, you see. It was not the way to do computing because it was just much too slow. It took too long to transmit data.

TROPP:

Was this course at Wayne one of the first university courses of its type, or were there many others that preceded it?

HUSKEY:

No, I think there were some. You see, we gave short term courses at Wayne. I even gave a short term course at the Bureau of Standards in 1948, so I think there were earlier ones.

TROPP:

So there were courses going on earlier.

HUSKEY:

The Moore School gave this one in the summer of 1946, you see. I think that may be the first one, if you want to give that priority.

TROPP:

That was the summer of 1946. I've got a set of notes from a graduate course that Howard Aiken gave at Harvard, that the individual who gave them to me took in 1953. Now, I don't know how long that course had been going on.

HUSKEY:

Well, I think the real landmark was this 1946 course, because that seemed to disperse information right at people.

TROPP:

In terms of its impact, probably that is a landmark course, in which so many things grew out because of the people who were there, where they went and what they did afterwards. At the time, I wonder if they realized what they were starting in that 1946 class.

HUSKEY:

None of us did at that time.

TROPP:

What were some of the other things in this area, during the period at UCLA, that you were involved with?

HUSKEY:

My primary job was just mainly helping run the research computing activities, as well as building a computer there.

TROPP:

What was the main bulk of kinds of problems that SWAC was handling during that period?

HUSKEY:

Oh, we were handling just anything that people were interested in. The kinds of things that people were working on were methods of solving linear equations, for example, worrying about conditioning of the ill conditioned situations, and so on. Some people were interested in partial differential equations.

TROPP:

So it was primarily for scientific kinds of purposes?

HUSKEY:

Yes, it was almost all scientific. There was a little bit of work for the aircraft industry, because it was an Air Force supported computer, you see. But mostly, it was set up for research and the people who used it.

TROPP:

By then, the aircraft industry was also beginning to get their own.

HUSKEY:

No, this was before they got their 701's.

TROPP:

That's right. Was the 701 delivered in 1955 or 1956 or 1957?

HUSKEY:

No. I think it was as early as 1953, wasn't it?

TROPP:

Oh, that's right. I'm thinking of the 704, which came later. So by 1953, most of the aircraft companies on the West Coast had their 701's. The work that you were doing then

was primarily through the Air Force, in connection with projects that were involved in the aircraft industry.

HUSKEY:

As far as outside the Institute. Most of the work was computing for the Institute, you see.

TROPP:

Well, in the aircraft area, were you doing primarily structural analysis, or were there other things?

HUSKEY:

I think it was mainly data analysis that they brought in. They collected measurements of some sort.

TROPP:

Then when you left UCLA, you went to Berkeley, and you were at Berkeley from then until ...

HUSKEY:

1967.

TROPP:

1967. And then 1967 is when you came here.

HUSKEY:

That's right.

TROPP:

Have you been associated in this general field as heavily during that period at Berkeley as you were in the earlier part?

HUSKEY:

Yes. It was the same. I taught in the EE Department, and I helped out and even ran their computer center for a while. I did quite a lot of consulting for industry. The reason I have the G 15, you see, is that I did the logical design on that.

TROPP:

That was when?

HUSKEY:

1954.

TROPP:

That was just about the time that you were going to Berkeley.

HUSKEY:

Yes, it was just prior to that.

TROPP:

When was that machine completed?

HUSKEY:

Oh, I guess they had their first ones out about 1955 or so.

TROPP:

And the one that you showed me today, that you had in your office, you acquired when?

HUSKEY:

Actually, that's the second one that I had. I had one on loan for a while, that they took back. Then I got this one, and I suppose I must have gotten this in 1962, or sometime thereabouts.

TROPP:

But the one that you had on loan, you had from the very beginning?

HUSKEY:

Very early, because in the beginning, I had to develop some software for it, you see. That was the reason I had it.

TROPP:

Looking back through this whole period, would you care to comment on what some of the milestones were, from your point of view, or perhaps some of the most significant ideas that ultimately got us where we are today.

HUSKEY:

Well, let me think. I think there are some very definite hardware landmarks. I think there is the development of magnetic cores which brought a reliability into memory that wasn't present with either the delay lines or the Williams tube.

TROPP:

By this, you're referring to the work of Forrester and his group.

HUSKEY:

Yes. Then, after that, there's the transistor circuitry where we got away from vacuum tubes. On the step up onto integrated circuits, and so on, that's just a refinement of the technology. I don't think there's a step there that is significant, although economically it's significant in that the price for logic now is really the price per board rather than price per logical element. That means that you can do a lot more complicated things than you used to be able to do. In fact, with this development of integrated circuits, you can get students to put together very complicated logical devices that they just didn't have time to do in the vacuum tube technology. So, I think the integrated circuits are important for that reason. On the logical end of it, there has not been any real significant improvement in logic. The Burroughs series of machines are probably the more interesting ones, in that they are more oriented toward a computer language. There have been some relatives, like the KDF 9 in England, along this same line. Japan has described a machine of this sort that will have an impact on the world market if it sells. There's not been many real strides in the logical organization of the computer.

TROPP:

Going back to the early period, I guess if you were talking about generations, we might go back to a negative numbered generation, if you think of ENIAC as being Generation One. Looking at the time period, what would you consider some of the most significant concepts that either led up to the early electronic machines, preceded them, were necessary for their evolution, or occurred in the general evolution of electronics?

HUSKEY:

Well, certainly in leading up to ENIAC, the important thing was the existence of flip flops, the Eccles Jordan patent sort of thing. In the development of gates in which you had dual grid control, you could get a logical element. Then I think from there on, it was the means of storing information mercury delay line memories or Williams tube

memories that was the next significant step. In the software area, I think that one of the early outfits that doesn't receive much attention in the literature is this PACT development. Some of these 701 types should have been talking about that.

TROPP:

Right. There was PACT, and then SPEEDCO and the predecessors of FORTRAN.

HUSKEY:

Right. PACT was the more widely used one of these.

TROPP:

Well, there was a good deal of argument.

HUSKEY:

I think it was a West Coast thing, and the Easterners didn't know how to cope with it.

TROPP:

That's right; and there was a good deal of argument last night about: really, if you look at it in a broad sense, PACT and FORTRAN do not differ significantly. There was a good deal of controversy about a statement of this sort.

HUSKEY:

Oh, they are different.

TROPP:

The majority opinion would agree with what you just said, but there were still a couple of individuals who felt that when you get right down to the nitty gritty, where do they really differ? I guess it all comes down to how you define what a compiler is.

HUSKEY:

Right. That's true.

TROPP:

In terms of even earlier ideas and the contributions of other individuals, we think in terms of Boolean algebra design, and the idea of the binary arithmetic, and the work of Shannon and Stibitz's work almost simultaneously.

HUSKEY:

Then there's Edward Condon's patent on the binary number system.

TROPP:

I've not heard of it. A patent on the binary?

HUSKEY:

There is a patent on the binary system.

TROPP:

How could one patent a number system?

HUSKEY:

I don't know, but Westinghouse holds a patent anyway.

TROPP:

They do? I guess you could find binary evidence in the seventeenth century, as far as tables and things go, and I guess they were used in a lot of the early codes.

HUSKEY:

Yes.

TROPP:

But the idea of this concept in terms of logical design and logical analysis is a recent concept in the sense of the thirties.

HUSKEY:

Again, I think that it's a natural development when you take other things along with it; and clearly, when you begin to have devices that will remember one bit, you see a yes no combination. Then you look at how to represent a decimal digit using four of these, and how, when six out of those sixteen aren't doing you any good, it's pretty automatic to try to get a little more out of it, particularly when circuits are so hard to make work as they were back in those days. I think anyone versed in the state of the art would have been able to do it, you see, in patent language.

TROPP:

In terms of significant ideas, which is a different realm, would you classify ENIAC as one of those things, even though you can't isolate it as you can a concept like Boolean algebra.? I would have a tendency to do so, in the sense that ENIAC is a total entity, it is a significant idea.

HUSKEY:

Yes. Well, in that sense, I think it is a significant landmark, and there's no doubt about it.

TROPP:

Well as a landmark, I don't think there's any disagreement.

HUSKEY:

In terms of its logical structure, it was just a whole bunch of adding machines. They happened to be electronic and fast, but logically not really different from a desk calculator, you see. Except that there was a bunch of them and you could inter connect them and have them run a program automatically instead of manually. So the aggregate, I think, is a significant landmark; but in terms of logical principles and so on, I don't think much.

TROPP:

It's still amazing today that anybody would even attempt to build something that would depend on so many tubes.

HUSKEY:

There were 18,000, yes. And it was at a time when the average life of a tube was like 2,000 hours. No wonder Von Neumann said it wouldn't work.

TROPP:

Right. When you look back, you wonder that anybody would have attempted it. There are people who thought about it earlier, and just the idea of using even a much smaller number of tubes staggered them.

HUSKEY:

That's right. I think again of somebody like Eckert, who made that possible. He knew what sort of links you had to do in terms of designing tubes from twice the low cutoff, which is the normal figure in ENIAC design, to well above grid potentials so you're in

grid current, which made them insensitive to noise. So you overdrove at each stage through the system. That meant that any noise tended to be drowned out in this process. I think it's that kind of thing, and the fact that they burned, in those tubes, a hundred hours before they used the machine, so they had to survive this hundred hour burning test. That got rid of most of those that were mechanically weak. It was just knowing how much care to take in this direction that made it possible to do some computing in the ENIAC.

TROPP:

Right. And, of course, along with that there was the fact that the longer the tube runs, the more likely it is to keep running.

HUSKEY:

It has to be just a little old. We used to have big arguments about that. Should you systematically go through and replace tubes of a certain age, or should you have routine maintenance on chasis, pull them out periodically, and do something about them? We came to the conclusion that if it worked, leave it alone.

TROPP:

You know, in today's context we're so used to the standard notion of tests that are applied to various machines before acceptance, before they're delivered and paid for and when you look back at the early machines, you wonder if they had been forced to, if even anybody could have devised such a test.

HUSKEY:

Well, there were some interesting tests that were devised, I guess, in the early fifties.

TROPP:

Right. But before that, it would have been almost impossible.

HUSKEY:

That's right. In fact, there was a thing called a leap frog test. Your structure particularly the Williams tube memory had certain locations in memory that tended not to store, and so on. A good way of testing it was to put a program in and move the program around the memory and see if it would still operate. Then you would have a whole cycle that it went through, and this became known as the leap frog test.

TROPP:

At various times during this early period, you were associated with and came into contact

with almost everyone who was involved in the early development. In terms of your own ability to solve problems as they came up, and to handle situations in terms of characteristics that you wanted to build into the machine, can you recount where some of the ideas came from, and some of the people who were able to give you the leads or help in this development?

HUSKEY:

Well, I guess by the time I went out to UCLA, and we decided that it was to be a Williams tube machine, for example, then it was just a technical development and we really didn't need to go outside for ideas. We had a lot to do, and the problem was to make each of these things work. We usually tried something, and if it didn't work you were just more careful. Prior to that, the concept of the Williams tube storage versus mercury delay line, of course, was a significant idea about how to store information.

TROPP:

When you were at the Bureau, for example, or when you were convincing them to do their own machine, you were beginning to deal then with conceptual ideas in terms of: if they were going to build it, what kinds of alternatives and what choice they had.

HUSKEY:

Well, I think the idea then was not so much: "Well, let's do anything new [?]. Let's just do what everybody is trying to do, and we'll get the experience, and maybe we'll make it work."

TROPP:

So again, you weren't trying to do any really creative new work?

HUSKEY:

Most of the Bureau work was all premised on mercury delay lines, and the experimental work had been done at the Moore School. I think we had a pretty good idea of what was required. There were some interesting questions about the life of a mercury delay line, because impurities tended to deposit out on the crystal and ruin the interface between the crystal and the mercury, and so on. But other than questions like this, there were no amount of anticipated problems, and people expected to be able to make them work. They just didn't know how much effort it would be.

TROPP:

When you look back, I guess it is amazing that some of these things actually did happen.

HUSKEY:

If we'd had the hindsight, we probably wouldn't have started it.

TROPP:

Your wife described the excitement of the period: the long hours that people used to come to your house to work way into the wee hours of the morning, and starting in the very early hours of the morning. I guess it took that kind of effort in order to come up with something in the relatively short time period that you did.

HUSKEY:

We certainly worked long hours, and mainly because we were just interested and excited about getting to where this thing worked.

TROPP:

I see we're nearing the end of the tape, and I might just throw in a few sort of irrelevant or maybe not irrelevant, but totally different kinds of areas. The whole concept of computer science as an independent academic activity is also relatively recent. I would gather that you've been involved almost from the beginning. Maybe you can help tell me how it happened.

HUSKEY:

Well, I'm not sure that there was any definitive way that it really happened. I think the other work either started in EE departments because of the people being interested in hardware or, in mathematics because of people who were interested in computation. So usually, computer courses were added on to existing programs there. The development of so-called independent computer science departments, and so on, has come more recently, and, I think, partly because in the typical American university the structure isn't flexible enough that a new discipline or group can easily develop its own identity in the system. You see, if you take a place like Berkeley, I think their structure is frozen enough so that it's hard for this to happen. So the tendency has been to try to come into existence as a separate unit, and I think the reason that this has happened this way is primarily because mathematics departments have not been able to service the need. Certainly, engineering departments haven't, because they've kind of closed out a whole segment of the L&S type student. So I think that's the reason for it. Whether it should have happened, you know, depends on the individual cases, I think. I think most computer science departments could have been in applied mathematics departments, really, because they're interested in practical problems. They're interested in algorithms to solve problems, maybe a bit more theoretical than the typical applied math department, but it could have been that way. I think the fact that applied mathematics has not succeeded well in the American university is primarily because industry has siphoned off the best people and paid them high

salaries. That's been a contributing factor. So I think that computer science has come into being for all these reasons, servicing a need that math departments have not serviced.

TROPP:

Also, it's not too common to find applied mathematics departments being separate from the mathematics department, and when they are part of it, then you run into the same structural differences that you mentioned earlier. I guess one of the reasons that I asked the question is because there's a possibility that we're seeing a trend in a sort of a reverse direction now, somewhat analogous to statistics. First it was within mathematics, and then out, and then back again. I think there are a couple of institutions now that are moving computer science back into one of the other areas. I don't know how general that is.

HUSKEY:

I don't really think it's too general yet, and I guess I don't really see that happening in a general sort of way. I think that what will happen is that math departments by and large will become more theoretical, more ivory tower oriented, and there will be an applied development that will either come back under computer science, or, in some places under somewhat different names: maybe applied science, or things of that sort.

TROPP:

It's rather interesting, in listening to the development of your career today, to see how you began in mathematics with these Lebesque surface measure problems. I guess I'm interested in whether you ever continued to do any work in that field after you got involved with computers.

HUSKEY:

Since I left the University of Pennsylvania in 1946, I've not really done any serious purely mathematical work.

TROPP:

I'm very much interested in your historical interest at the moment, and you touched on it earlier. I was just wondering if you'd like to describe some of your efforts at the moment, because once I have it on tape, it may remind me of other things later on.

HUSKEY:

Well, it's mainly just trying to sort out the historical development in this initial electronic area at the Moore School, and in England, since I worked with Turing there, you see. I guess part of the reason that I have been interested in this is that I've been involved in a

lot of this ENIAC litigation over the years, because this report that I wrote on the ENIAC which described how it worked, was the basis of the patent that was filed on it. This was in interference of IBM and Bell Labs early in the game; and in the court trial with Honeywell at the moment. So I've sort of had contact with that, where there was an effort to establish the historical priority of events, and so on.

TROPP:

This is the same litigation that Atanasoff is also involved in. HUSKEY:

Yes, that's right. So, because of that contact and so on, I had access to some materials that I wouldn't have otherwise. I've been interested in trying to follow through on it. In fact, we've got this Japan conference coming, so it made it easy to promise a deadline, you see, when you have to get something done.

TROPP:

I've been in some contact since I got word of the Japan conference with the Japanese co chairman, who has sent me some of his publications, which span a broad period. The first one that he sent me is a description of what they call the Mark II relay computer, which is dated 1955, and discusses in the preface its prototype model, which they call the Mark I of 1952. It's an interesting looking machine. It's got some of the characteristics that seem to be common to Bell Lab's relay machines as well as to Aiken's Harvard machine. But it was the time period that interested me.

HUSKEY:

Yes. It came long afterwards. But the other, more recent, interesting thing they've done is this thing called Nicchi Mark VI, which is a KDF 9 kind of computer.

TROPP:

This is the one that you described earlier.

HUSKEY:

Yes. As far as I can see, it was a paper presentation of the thing. There were no signs that they would ever make the computer, or they haven't.

TROPP:

What were some of the characteristics of it that you found so interesting?

HUSKEY:

Well, it had a stack almost exactly like the KDF 9, with some of the Burroughs 5500 6500 series. I think there was paging in it, too. It worked in a time sharing environment. But there is no evidence that they really made such a thing. I think the most interesting thing in the Japan development is their efforts at education. They are giving a very systematic training of FORTRAN on their T.V. system, and they sell a little book to go with this program, and I think the sales from the book is something like ten million, or some such incredible figure.

TROPP:

That is incredible. I didn't know about that. My only knowledge of some of the aspects of Japanese education came during a discussion with Professor Hazen, who went to Japan as head of a committee for the engineering group.

HUSKEY:

Is he at Princeton?

TROPP:

MIT. Harold Hazen. I think he was the Chairman of the Electrical Engineering Department there before Gordon Brown. I think it was in 1951 that this committee on engineering education went to Japan to help them reorganize, or at least talk about, education in Japan. He described his experience to me, essentially involving changing their educational model from one that had been largely oriented in the German tradition to one that was more closely related to the American concept. That's the only knowledge that I have of Japanese education. This other is really quite interesting: that FORTRAN is part of public broadcasting. Ten million books. That's absolutely incredible.

HUSKEY:

It's really something.

TROPP:

Well, I hope that I get a chance to go to that conference, and I'm sure that it'll be interesting there. I've seen the list of areas of invited papers in Japan, and three or four of them are quite interesting in terms of development. They are doing things in the history of computers, but on a relatively small scale: meeting periodically and hearing talks, which they record. I think that will be an interesting session. I see we are nearing the end of the tape, and I think this is a good time to quit. I'd like to thank you very much for your time.

HUSKEY:

You're very welcome.

[END OF INTERVIEW]