Computer Oral History Collection, 1969-1973, 1977

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TROPP:

I think probably a good place to begin in terms of where we left off last time would be when Von Neumann first became aware of the work that was going on at the Moore School and your first contact with him, and recollections that you have in that general area.

MAUCHLY:

Well, nobody apparently is able to straighten this out to an exact calendar date. The best that we can do in general would be to say that Dr. Goldstine met Von Neumann in Aberdeen apparently in August of l944, and that sometime around or after the first of September Von Neumann visited the Moore School and met Eckert and myself and some of the other people there. The exact dates on which some of these things happened don't seem to be well recorded. Goldstine, as I read his book, attempted to place the date from what he knows about his own travels and sicknesses and things about that time, August, and I've attempted to place the date, by knowing that some of my notes show that I could not arrange to be at the Naval Ordnance Laboratory for an appointment an early date in September because I found out at the last moment that a visit from Von Neumann had been arranged. This was somewhere like September 7.Perhaps it's not important that we be too precise about this date because there are records which show that some of the things claimed for Von Neumann were already known and recorded by us months before this. But the...it would seem nowadays from reading what Goldstine writes that he is trying to make the date as early as possible and claim as much as possible for Von Neumann.

TROPP:

Let's get in to that conflict because I think it's an interesting one.

MAUCHLY:

A peculiar thing. There is a manuscript or document in existence which has a date of January l944 in which Eckert wrote in several pages of single spaced typing, I believe, a disclosure for a magnetic calculator which had all the pertinent for its operation recorded on a magnetic drum. This was described as being a kind of electronic calculator. The intention that was in Eckert's mind when he was describing this was as far as I can tell, to

try to make it a fairly complete but not detailed description of what could be done in building a desk calculator even though at the time our main attention was of course the development of plans for an EDVAC which was much more than a desk calculator. So this document speaks in terms of a keyboard operated device which would record on the drum the operations to be done as well as the data to be operated upon, and it was so far as we can tell, the first case in which a description of a calculating device which stored all of its program information rather than just a part of it as did the ENIAC. Of course the statement I just made already implies something which is contradictory to the statements you find all over about the ENIAC. I maintain that a lot of the program information of the ENIAC was stored in electronic form where it could be operated upon with the speed necessary for an electronic computer to have a program available so that it would not wait upon human instructions as to what to do next. And part of that program information of the ENIAC was stored in the master programmer. Part was stored in certain repeat counters and other things which at first glance could be altered only by mechanical switches or _____ but actually it doesn't take very much to see that all of these things could be altered by the progress of the calculations. We don't reset the mechanical switches by calculations, but you can reset the electronic counters, and that's really the important thing, trying to manage a calculation at electronic speeds.

TROPP:

Later on after ENIAC was in operation wasn't it essentially used in the way you described it? I'm not talking about the first period.

MAUCHLY:

You're probably alluding to what is sometimes called the Von Neumann method of operating the ENIAC.

TROPP:

Or the Clippinger depending on who you talk to.

MAUCHLY:

I've talked to Clippinger and Clippinger has talked to many people, and I have just talked to Nick Metropolis to find out what the discussion in Japan was about. I don't know whether you've gotten a tape for that.

TROPP:

Not yet, no.

MAUCHLY:

I just the other day was discussing this question with some people at UNIVAC, and they said, let's call Nick and find out. So I called him, and he described what he considered an embarrassing situation for himself that here he was presenting a paper and he was asked by the program chairman, Louis Fine, to also be present on a panel since Louis Fine had found out Henry Tropp was not going to be there, and he wanted something more than Herman Goldstine on the panel. So one of the things which he used to try to persuade Nick Metropolis to be on the panel was that he told him there were many people who wanted to be on the panel so he would only be one among many persons. Apparently it did not turn out that way, and so after Nick had consented to be on this discussing panel he was very embarrassed to find that as soon as the panel discussion began Herman Goldstine got up with a prepared paper which of course he had had the opportunity to prepare because he had received the prior documents other people were going to present. And the prepared paper was to the effect that, at least in part, to try to correct what he considered Metropolis' error. The paper Metropolis gave was actually a joint paper and the other author presented it at the meeting...In it apparently Metropolis had alluded to the fact that Clippinger had come up with the method of using the function tables to control the program of the ENIAC, and he had suggested this to Von Neumann, and this was really an important genesis of whatever Von Neuman had done later with respect to developing a particular and specific method for instructing the ENIAC. And Goldstine's effort apparently was to discount heavily the contribution of Clippinger and say that this really was not a great contribution. The great contribution was from Dr. Von Neumann. After listening to this account to me as given over the phone from Los Alamos, I told him that I thought it was a great shame that the documents which had been developed in the recent court trial had not yet been made available to him or to others because in that testimony in the Minneapolis court Clippinger had made the claim that he was the discoverer of this method of instructing the ENIAC, and on cross examination he was shown the ENIAC drawings prepared about two years before Clippinger's date in which the terminal connections for using the function table in exactly that way were detailed in the ENIAC drawings. This use of the ENIAC of course was also described in the patent application which presumably Clippinger had not seen, and so it was perfectly appropriate for him to say that he had rediscovered this, but once having seen the original plans which Eckert and I had produced and made drawings for and so on, he had to admit and he did so in the court room, he admitted that apparently he was not the first, that this had been anticipated by the people who designed the ENIAC.

TROPP:

In his discussion of the same topic at the meeting we attended in Boston and in other places, with me in particular, he indicated that his route through familiarization with the machine when he first saw it, and I guess from a description of how to use it that Adele Goldstine had given him. And as he was thinking about the particular problem he was working on then he thought of this way of using ENIAC. So I'm not sure that he had even seen the drawings that you are referring to.

MAUCHLY:

I was trying to indicate this, that he had rediscovered this, in other words independently he had come upon this idea. Now it may be that Von Neumann also was unaware of this capability in the plans that we had made in this respect. And so the first time that he had heard of this or thought of it may have been when Clippinger called his attention to it. But that is neither here nor there from the point of view of the historical record because the drawings exist with a much prior date that show that Eckert and I had shown how to do this, and not only shown how but had provided the actual connectors so that they could be used any time anybody wished to use it this way. There were actually in physical existence the connectors to do this, and the whole thing is sensitive, you might say, to what the pertinent problems were at the time, at the time the ENIAC was being tested and presented for test and use by Aberdeen. The Aberdeen people still considered that the filing tables and trajectories and things of that sort were important to them because that's what they originally had sold the ENIAC to the Pentagon by. Although the machine was being tried out in other ways, Hartreet was using it in one way, and Metropolis and Granco were using it in another way, and other people using it in still different ways, there was a lot of need in the eyes of the Aberdeen people apparently to see that it could compute the trajectories and meet the originally called for uses. Regardless of whether it would do all these other things or not, this was sort of following out the original intentions of why they had named it the Electronic Numerical Integrator and Computer.

TROPP:

You raised a couple of interesting questions that I haven't even thought about discussing, and that is the different ways in which ENIAC was being used. You mentioned two or three, and we know of the use of the computer from Los Alamos for problems connected with atomic energy. What was Hartrees interest and when did he first become cognizant of ENIAC?

MAUCHLY:

I couldn't say when he first became cognizant of it, to its plans or what state of existence it was in because one of the things that Goldstine was doing, as and as far as I know fairly effectively was to make known to others the fact that the ENIAC was coming into existence, would be available for a general purpose calculator. So it was he, so far as I know, that saw to it what Hartree was engaged as a consultant to Ordnance with the mission of coming over to the United States and trying any problems he wished on it. And the visit of Hartree was in the summer of l945, but that doesn't set the date at which he first became aware of the ENIAC. He was certainly aware of it before he came because the reason he came was because he was engaged as a consultant by Ordnance to come and put on the problem of his choosing. He chose to put one on that had to do with laminar boundary layer flow.

TROPP:

This was essentially then the same kind of problem that Clippinger was the body according to his discussion. Were they related problems or were they independent?

MAUCHLY:

Well they were certainly related but I don't know how closely related because Hartree had a very specific set of tests to make which he did attempt to carry out, and I don't know how far he got, whereas I was not sufficiently connected with Clippinger who worked most of the time at Aberdeen in the wind tunnel, to know just whether Clippinger had fixed upon any particular tests. So far as I know he had not. Clippinger, while I did cross his path occasionally seemed to be talking about the ways in which the ENIAC could be used, but anything I heard from his was in respect to shock waves rather than laminar boundry layer flow.

TROPP:

I discovered, and again my memory may be off, that he had trouble getting time on ENIAC because of its use for other purposes, classified problems and ballistic...

MAUCHLY:

That may be so. My impression is that while he was working in the Aberdeen wind tunnels that his main connection with the computational laboratory at Aberdeen was probably because of a need for data reduction from the wind tunnel, but he was hoping to see some of the more fundamental calculations done which would essentially bypass the dependence upon the wind tunnel except as perhaps a calibrating device to see that your theoretical calculations bore some reasonable correspondence to what you would derive when you put the actual model to be tested in the wind tunnel.

TROPP:

I think really what we're doing is enunciating some things that people close to ENIAC realized and is not in the literature, and that's the fact that ENIAC was used by a variety of people in a variety of ways and was much more than just a device to calculate ballistic tables. Everyone knows about the famous, you know, Von Neumann's work, and... but only people close to it seem to be..

MAUCHLY:

You might say each person from looking at this from some historical point picks on what may be of most interest to him, and I've seen some accounts which had to be abbreviated for the purpose for which they were written _____ was nothing except the fact that at one time the ENIAC was used to do some meteorological calculations. Just as a kind of exploratory calculation in meteorology to justify going further with for instance the

Institute for Advanced Study plan to set up a more extended research into what could be done with computers in meteorology. You might liken this to the tests which Metropolis and Franco were making for Los Alamos. They were making exploratory calculations on the ENIAC with the hope of seeing whether future computers if they were developed, designed and built could be used in the way they hoped. And they weren't primarily trying to get practical useful results out of the first calculations on the ENIAC.

TROPP:

That's an interesting way to look at the early usage because again I think the literature is clouded over and tends to be distorted. And in terms of Hartree's particular problem, although it may not have been designed that way, it's closely related to the work that was going to come later in meteorology.

MAUCHLY:

This is something like to my mind what goes on in all kinds of scientific and experimental work. You might say that the ENIAC was the prototype for a new and extended capacity of calculating device so people were being encouraged, and Goldstine was encouraging them to try out as many ways as possible just so one could begin to grasp what might be done if more calculators with more extended capacities became available.

TROPP:

We might take up that last statement in terms of informal discussions between yourself and people like Eckert and Goldstine and Von Neumann and Clippinger in terms of the extended use of high speed calculating devices while you were still doing trial runs on ENIAC.

MAUCHLY:

I'm not sure I know how to answer that one. Maybe you'd better make it more specific.

TROPP:

If we follow the chain of the literature after ENIAC, the next major, one of the next major milestones of course is the proposal for EDVAC. And it's clear then that there were two things happening. There are people coming in from a variety of environments looking at the first electronic computing device and trying problems on it and seeing whether or not this calculating instrument can be adapted to much more exotic uses in the future. And then there's the thinking about if we're going to have future devices what should they be like? And at some point these two things come very close together, because once people see that they can get assistance then the next thing to do is what should we be doing,

which direction should we be going? I guess it's your recollection of the informal discussions that somehow fall in between the time when you first had a machine and you can say to people like Hartree come over, and the time when you're writing a document and saying here is our next stage.

MAUCHLY:

A curious division of labor went on here, and it's a little hard at the present time to describe to other people how this division of labor operated, how it came about or how it operated as it went along. The division of labor which Goldstine refers to in his history makes it much simpler than it could have been, but has some relevance to what actually happened, too, and that is Goldstine regarded us and speaks of us as technicians as quite distinct from mathematicians and scientists and logicians and those people.
We ourselves at the time were you might say content to fit into that kind of a role because we felt the most important thing for us to do at the time while we were not only finishing ENIAC, see that it was made to pass the proper tests satisfactorily, become a useful computing instrument. We at the same time were trying to plan a little more comprehensive device which we called EDVAC. And it seemed to both of us, really only to myself, but I think it was true of both of us, we thought that the time had come when the ...everybody's cause would be best served by having the more advanced thing the EDVAC available. And so we bent all our energies and spent all our time trying to bring these better computers into existence, and it was one of Goldstine's functions which we perfectly well accepted, thought he was the best to perform, namely to make it known to others that here was a class of computing device which presented new opportunities and new frontiers for the user, scientist, engineer or what. This was such a big step forward over what was previously available that it would take a lot of education, a lot of time of other people to become aware of what could be done. And Goldstine was inviting various visitors from England and other places, to take a look and consider these things and spread the word as to what was going to be the future of numerical mathematics and everything it implied. And in this connection we welcomed the addition of Von Neumann to the group of people who were doing exactly that, and he was a great expositor of what this new kind of computing equipment could do. We had no quarrel with that at the time whatsoever. This educational process, this dissemination of a new class of machines. It was very important. There is no use in having a new kind of beast available if nobody knows how to use it. And so as we saw it at the time every activity which Goldstine, Von Neumann and others might engage in which helped to spread the word and show what could be done was a very important step in the total progress. And it seemed as if all that would be for naught unless we actually got down to day to day drudge work if you want to call it of bringing this equipment into being. And so we were just as anxious to see the EDVAC plans advanced as we were to see the ENIAC being fully tested and experimented with from every possible user and all kinds of different uses. And it's hard to say which deserved more attention really. A lot of experimentation with the ENIAC would have to be done just to bring possible users up to an awareness not only what the ENIAC could do but how much further they could go if these more extended things like the EDVAC became available. So we at the time had no

quarrel and did not resist the idea that it was our job to build machines and it was somebody else's job to figure out how best they could use them.

TROPP:

It's interesting because Maurice Wilkes describes a phrase for this whole thing that you have discussed, what he calls the tension between the mathematicians and the engineers. He alludes to not only in the early period, but it is still in existence today.

MAUCHLY:

Well, our tension didn't appear to us as tension at that particular time. It just seemed like a natural division of labor. It became a tension as time went on, when we realized that there was not only a kind of explicit division of labor but it became an even more explicit condition of not only who was responsible for what but who was capable of what. So that as of today's version when you read Goldstine's book you have an extraordinary version of this tension he is now very explicit about saying we were technologists incapable of doing logic essentially. I submit that we did logic all the time. And not only that we did logic but we did logic with a reasonable amount of clarity. The fact that our exposition of this logic maybe left something to be desired compared to the exposition that Von Neumann and his manuscript on plans for the EDVAC, that his exposition was better than ours is not too surprising, I suppose, especially as I believe that he spent practically all his waking hours on doing exactly that, whereas we were spending our waking hours in trying to get the next machine realized and get the first machine ENIAC cleaned up and in as __ condition as possible. We had different tasks to do at that point.

TROPP:

I wonder if it's at all possible at this point in time to reconstruct chronologically with only approximate dates, and not worrying about the litigation, the development or the chain of events that ultimately led up to Von Neumann's exposition on EDVAC. You mentioned earlier this paper in January of 44 this paper by Eckert.

MAUCHLY:

Well, the paper in January of 44, as I say, was just a kind of a piece of milestone writing that we just filed away and didn't think much more about it at the time. In a sense, I guess I could say that that piece of paper was somewhat analagous to the August l942 memo which I wrote which led to the ENIAC. This piece of paper was saying in the future you can build a stored program computer, and you may be using magnetic recording which was not very well developed at that point, have a sufficient storage capacity so that you can afford to store all the instructions in the computer. Having written that it got filed away and less attention was paid to it than the August 42 memorandum which became activated in 43 with the ENIAC thing. Instead hereafter this paper was filed away and more or less forgotten about for a long time, the next thing

which occurred as we were building the ENIAC was the realization by Eckert and myself that a rather fast and low cost storage device was possible using the acoustic delay line which Eckert had already had some experience with in connection with a project in radar for MIT. And once we had the general conception of a way to vastly reduce the cost of storage over that which existed in the ENIAC then what could be done with this appeared so obvious to us that we didn't labor over trying to write a long discussion of how we would store the program in this. Obviously to us you would store this program in a sequence of primary digits, but you would presumably do the same thing with the instruction storage that you do the data storage. You would start out with some exterior input device which would allow you to put all this data whether it be instructions or data into the computer that was going to be built with this mercury type delay line. And so we were so on with this idea, it seemed so obvious to us, that it as of today it seems very strange to read some of the passages in Goldstine's history in which he tries to study from his own remembrance what happened in a very few weeks of history in the summer of l944. He makes a point of the fact that at one time he was discussing with Von Neumann or others the idea of storing programs in relays while he was thinking about the delay line device apparently as holding data. And as I read him it tells that neither he nor Von Neumann thought at that point of the fact that the instructions could be stored in exactly the same medium in exactly the same mechanisms or physical storage devices as could the data. And then his further point is you know that two or three weeks later something had happened, that suddenly the world had turned over, so to speak, and the thought of putting the instructions into the storage, too, well, how in the world they ever had this dichotomy of the storage was good for one thing but not for the other. We will have to devise some kind of relay things ala Stibitz or Bell Labs to automatically program all of this, why they ever got off on that tack at all I cannot imagine because as far as Eckert and I were concerned it was plain as day. It had been plain from the time we were talking about this little magnetic drum calculator. And any time we were thinking about storage we were not differentiating the storage and saying one part should be this and one part should be that. Whereas, I think I've commented many times before that Aiken continued to have this dichotomy for a long time, and after he was building the Mark III or something he deliberately saw to it that there were different drums in which to store programs than there were to store data, and he thought it would be almost a disaster if the program material were to be mixed up with the adat, that something might happen, that you had to keep these distinct. This was...

TROPP:

If you look at his origins, and this is my own conjecture, if you look at his origins in terms of his early writings in the Thirties, and you see that the idea of a mechanical sequential computational device follows the way in which a person would do it, you can see why this separation stayed with him so long. Somehow if you were doing these on paper you would keep these just separate. That's my own conjecture. And it comes from his 37 document in which he describes what such a sequential calculator should do and what it should be capable of. He seems to have carried that through. As you suggested the first machines at Harvard.

MAUCHLY:

Well, he certainly carried it through at least three machines. As far as I can see did not visualize the computer as being capable of doing what we now call compiling. So they did in fact physically build a separate device for punching program tapes for the Mark I. Why this lack of generality forced him to do this I don't know.

TROPP:

I think if you go back to his early work in the Thirties up through his exposition through about l940 I think you can see the origins of that, and its continuation through the first three machines. And that leads me to another possible extraneous question in terms of Aiken while we are on the subject. It seems the early machines on the European continent for the most part with a few exceptions seem to have gone down much that same road. If you looked at some of the machines in the Scandinavian countries, the early machines that were built--are you familiar with them?

MAUCHLY:

You must know a lot more about them than I do, I am almost ignorant.

TROPP:

I seem to find that ___ influence permeated, and I wondered then as to the impact that Aiken had in terms of people like yourself or Wilkes who were going a different direction.

MAUCHLY:

Perhaps you can see reasons for that in that Aiken himself went to traveled considerably in Europe and spoke at a good many places, so his influence could have been felt there just because he was a well-known person who appeared on the scene and having accomplished something they listened to him very carefully and got his thoughts.

TROPP:

Again Wilkes puts this very well, we were discussing this point in that the English environment was kind of picking up the state of the art where it was in computer development. For the most part, again there are one or two exceptions, whereas on the continent of Europe you seemed to find a repetition of what had early occurred in the U.S. before the, going through essentially the same stages, through the relay and the electromechanical devices, and finally electronic. Back to EDVAC, do you remember at all what was the impetus that led to Von Neumann's paper?

MAUCHLY:

I guess it's been described many times, but I'll try to go through it again. Somewhere around the first of September of l944 we began to have more or less regular meetings which were always run by Goldstine to suit the convenience and the travels of Von Neumann. And we would be told that Johnny will be here on a certain day and you should be prepared then to meet with him at whatever time he was there, and the people that met with him always included Goldstine and Eckert and I think they always included me, I may have missed some.

As far as I know they always included Mrs. Goldstine. Sometimes they included Arthur Burks and occasionally other engineers such as Robert Shoff. At these small meetings of six or so we made free use of the blackboard and the eraser in discussions which centered around how we planned to build the EDVAC. In other words there was not much discussion in these meetings as to how the ENIAC was put together or how it was designed. Most of that was already known to Goldstine and could be readily picked up by Von Neumann from discussions with them which I'm sure they had, to whatever extent von Neumann was interested in the characteristics of the ENIAC, what could be done with it, I think he got that information directly from the Goldstines. Which might of course account for why he didn't know immediately about the plans that we had for using the function table as a programming device. It may be that Goldstines never appreciated this point, do you see, and didn't have it in their own minds they couldn't relate it to him.

The discussions that we had in these conferences with Von Neumann, the ones that I participated in had to do how one might best design the EDVAC for which we had a considerable amount of planning already done. We had not planned every possible thing about it. It wasn't a detailed plan at all, and the question of exactly registers you might have, how many delay lines, what length they would be and what access you would have to this memory which being a serial memory presented some problems that were different from the problems of the ENIAC, all those questions would to our minds be problems which we would resolve as we went along. But Von Neumann's appetite for trying to get ahead on this was such that he wanted to immediately design an order code, specify a set of instructions, and even specify a typewriter keyboard through which all the input would go, and get it down to some very specific elements for study. Well, there is nothing wrong with this if you wish to do it, but from our point of view, for instance trying to specify a keyboard was a little premature. But this was one of the things he did, thought about it and worked on it.

We also had the problem which we discussed in meetings with him as to what extent the proposed EDVAC device would be able to take over some manipulations of data which were not usually considered within the field of numerical analysis, such as sorting. The fact that it might be desirable and efficient to do sorting with the EDVAC rather than to do it with exterior devices such as the card sorter which was in common use was something which we considered and were it was a problem we were very aware of, but Von Neumann decided that he would like to devise a sorting routine using this order code

to compare the possible efficiencies, we call it cost effectiveness now perhaps if you could put dollars on the equipment which we didn't, this seemed to us a perfectly worthwhile thing for him to do, and even at that moment there was no value judgment on our part as to whether he should or he shouldn't do it, nor would it have made any difference if we had had such a value judgment. He would have done it anyway. And so in the midst of these fairly regular meetings that we had with him he was called to Los Alamos as a consultant on the Manhattan project. There were reasons why he should be there. There were reasons he couldn't explain to us nor did we question. He had to go to Los Alamos. Sometime after he had absented himself from the Moore school to go to Los Alamos we learned from Dr. Goldstine that he was receiving correspondence at various times from Von Neumann who was actively considering all these problems and as far as we knew that was good. When we learned that he was still in Los Alamos but he was working on a rather extended write up of the logic we had all discussed and how it would be...what were the design considerations for the kind of computer which we were calling the EDVAC.

Now bear in mind that prior to this Goldstine had already at least by July which was before Von Neumann entered the scene, had already written to Government people either at Aberdeen or at the Pentagon or both telling them that an extension of the contract with the MOORE school was highly desirable in order to develop the delay line device and this would result in a much more efficient computing device if they would go ahead with extending the contract. I can't say at this moment how clearly he made known to them what they might expect from extending the contract. He did certainly hold out promises of getting much more capable computers if this was done. At any rate while Von Neumann was in Los Alamos we had no contact with him except to receive from Goldstine occasional word that Johnny him except to receive from Goldstine occasional word that Johnny was doing something on writing up the results of our conferences. And then one day Goldstine announced to us that a part of the manuscript which Von Neumann had been working on had been sent to him and that he was going to have that longhand manuscript typewritten, transcribed to mimeograph stencils, and that we on the Moore School project would all receive copies of this and that he was sure it would be a help to us all to have such an accomplished amanuensis do all this secretarial work for us, you might say.

The gist of the idea was we understood at that point was that Von Neumann was essentially acting as the man who wrote down what had been commonly discussed among all of us and this would be a great boon for everyone to have a document which put on paper in an ___ way the total result of all of these conferences. There was no suggestion at moment that as to who had contributed what. And at later times of course there having been disclaimers you might call them that Von Neumann contributed all of it. But in somewhat contradictory fashion there have been claims that he did. So, at any rate, after a period of time, I don't know whether it was a week or two weeks or what, this transcription was finished and we received mimeographed copies of what Von Neumann had written. It appeared to be purely a characteristic of Goldstine and no one else that in producing this mimeographed version Goldstine put no one's name on it except Von

Neumann's. After all, Von Neumann had written the manuscript, so the title of the thing as it read in its mimeographed form something about the design of the EDVAC and put Von Neumann's name under it. We'd really feel an insecure position. We thought that our work was our work and we shouldn't saw no reason to particularly complain about this. It was so far as we knew an internal manuscript. No one was receiving copies of this except the people that were on the project. It was only later that we learned that this had a much wider distribution than we knew of. Copies were being sent to other people, and indeed later we discovered they had been sent outside the United States. How this was justified or to what extent it was done I don't know to this day, except this apparently was not stopped by anyone. It was considered appropriate by Ordnance apparently to have this wider dissemination which Goldstine so far as I know was the instigator of.

TROPP:

That's interesting because wasn't ENIAC at that time a classified project?

MAUCHLY:

Not only ENIAC of course but anything subsequent to it on the Moore School project. There was classification upon the ENIAC and there was classification upon this EDVAC proposal, and everything that we wrote was subject to some kind of classification.

TROPP:

And yet it was disseminated all over. I know a copy did end up in England because that's where Wilkes saw it.

MAUCHLY:

Yes, and probably more than one copy. I think there are records somewhere that give a somewhat wider distribution list. Professor S. Reed ___ who became the project administrator at the Moore School for the EDVAC project finally did assemble a list of persons who were known to have received it, but nobody knows if even that list is complete because the copies were not numbered. There was no real authentic record as to who got them or when.

TROPP:

Considering the level of classification during wartime, and this was during wartime, this is very curious.

MAUCHLY:

There is some evidence, I don't know to what extent and where right now, which seems to indicate that Von Neumann chose to use neurons as the elements of his computer

rather than vacuum tubes just to make it possible for someone to say that they saw no need for classifying this. We were not consulted. We were not advised even. We didn't know these things were happening. So it was only some years later that we began to realize the full impact of this, some statements that I've seen indicate that Von Neumann believed, and probably Goldstine with him, that the greater good for science and possibly the United States was to be achieved by having the widest dissemination as possible of this report. So it was not just accidental but contrived that the report avoided any reference to electronic circuits as much as possible, and he phrased, presented in terms of neuron networks, hypothetical neurons. And of course it later turned out that this had bearing on what the Ordnance patent people and the patent office thought about the content of that report. It came about in l947 was it, that this EDVAC report was presented to Army Ordnance by the Advanced Study people, Goldstine and others, to be reviewed as to whether it had patentable information in it.

The idea was that whatever information was in it which contained patentable devices, patentable ideas, might be a bar to getting patents on electronic devices which Eckert and I had applied for to satisfy the requirements of the Ordnance contract that patents be obtained on any subsequent ideas. The conclusion of the Army Ordnance patent branch was that the Von Neumann EDVAC report was indeed a publication. That conclusion was not necessarily binding on the Patent Office. They were the ultimate arbiters of this. As far as the Patent Branch of Army Ordnance, there were patent attorneys assigned to apply for, prosecute patents through the regular patent office. It was their belief that this EDVAC report of Von Neumann's was a publication for whatever it disclosed, but that it didn't disclose anything. Now the conclusion that it was a publication was based on how they viewed the amount of dissemination which this report had received. And it was their conclusion that it didn't disclose anything because of the fact that it did not to their mind do what a patent has to do, namely tell someone who is skilled in the art how to build a computer. You can really argue honestly of course because the phrase someone who is skilled in the art was never well defined.

TROPP:

And also there is the point that you made earlier and that is that in order to have wide dissemination it's important that certain things not be disclosed because of wartime security. That had to be taken into account in the writing of this if wide dissemination was the plan. And it was to have a desired impact it did eventually get.

MAUCHLY:

It had sufficiently wide impact in l949. I guess several computers were near the finishing testing and use point in England. The publicity which arrived in the United States papers from the publicity releases in England acknowledged their indebtedness to Von Neumann but to no one else.

TROPP:

Well, it was again recalling one of these machines, the EDSAC that Wilkes was involved in and eventually and eventually ran in l949. One of the things that was happening was in war environment. These people had been working in radar since the time of the Thirties, so the whole idea of the delay line technology was part and parcel of their own experience. It was a very natural extension for them. And it was this particular report that was their first ___ which is how they first knew about it. As Wilkes has told me over and over again, he saw the report and then came the invitation to the l946 Moore School summer program which you lectured at along with other members of the staff, and at that time these were the two inputs for EDSAC. That was his first visit to the United States, so his only contact with what had been going on was that particular EDVAC document.

MAUCHLY:

I believe also the publicity for the ACE machine which the National Laboratory job where Turing was I believe. I think that also contained some reference to the Von Neumann report as being something to which they owed a debt, you might say.

TROPP:

Of course Harry Huskey was there but he had also been at the Moore School.

MAUCHLY:

He had been at the Moore School but he had only been connected with that computing devices for a short period. He came in rather late. He worked with me on reader card punch straightening out a big confusion which had been perpetrated by IBM delivering the wrong equipment wired the wrong way. We got that straightened out. He helped in writing the manuals, and he helped me actually in the editing of the EDVAC report which we made to Aberdeen. But at some point, I don't know exactly when, he took off for England.

TROPP:

But the EDVAC material was part and parcel of his life at the time he went to England. He knew of the thinking along that direction. And I think he is one of the people who is responsible for taking Turing's project and in a sense scaling it down so that in a sense they did the pilot machine first before they went on to larger scale device.

MAUCHLY:

Scaling down is a very practical thing to do.

TROPP:

Which is essentially what Wilkes did in Cambridge. This is most interesting because it is a description that I have not heard before because I have not been through all the litigation that you have been involved in, and the years of discussions. I wish I had the tape of the discussion of the earlier point between Nick Metropolis and Goldstine. Does Nick have any other comments?

MAUCHLY:

After I told him about Clippinger's being anticipated by our own drawings and things Nick expressed surprise. I was trying at that time to soften the blow to him by indicating that it really wasn't his fault because he would have been unable, even though he was one of the witnesses at this trial, he was not party to any of the other documents in evidence at that trial, and no one was supplying him with Clippinger's testimony, for instance, so how could he know that this had occurred. But I think that this is a point now that he appreciates and he says he is going to send me a copy of the paper which he Worlton...

TROPP:

The paper has been published in the proceedings but I just got a letter that the proceedings are still held up in transit and we expect them sometime at the end of the month. I have an earlier version, so a lot of Nick's difficulties are due to what I was able to give him, which included the same gaps that you just alluded to, and Clippinger didn't do anything to illuminate although I talked to him after the testimony of the trial.

MAUCHLY:

It's clear from what went on at the Boston ACM meeting that Clippinger isn't about to illuminate anybody on this because as you may remember he again claimed that he was the first to point out this use of the function table in spite of the fact that he'd had to admit in cross examination at the trial, that he had been anticipated by the people who designed the ENIAC.

TROPP:

I think a more important point is that Von Neumann had been anticipated also. The point I think of Worlton's paper is still, and Metropolis' paper is still well made in that the direction of the flow in information was not from your earlier material but from Clippinger to Von Neumann, as opposed to coming from your blueprints and descriptions to Clippinger to Von Neumann.

MAUCHLY:

As far as I know, I don't remember ever seeing Von Neumann examine at least at all carefully any of the blueprints for the ENIAC.

TROPP:

See Arthur Burks because Burks was very knowledgeable.

MAUCHLY:

Of course Burks left the Moore School and worked at the Institute.

TROPP:

He was there during that time.

MAUCHLY:

But again each person seemed to have what you might call a fragmentary knowledge and a fragmentary view as to what the ENIAC was all about. I think I mentioned to you a conversation with my own brother in law in writing a book completing some history of computers. Left the master programmer completely out of his description, as being unimportant compared with the arithmetic elements. And as far as I was concerned, and I would say that the press was with me on this, it's the control of these things which is the important thing. This is not an after thought we felt so at the time. Why would we build a master programmer otherwise? Why would we do any of the things we did do? This brings us to another thing which I don't know whether it was in Metropolis' paper in Japan or not, but it apparently came up in the discussion that Goldstine and he had over there, and that was the paper that was written by Goldstine and Von Neumann and Burks at the Institute on flowcharting. And apparently at the Japan meeting when this subject was brought up Goldstine claimed that the real architect of that paper was Goldstine, that Von Neumann and Burks had little to do with it, that he was the man that did it. This is in somewhat contrast to all the other writings and talks of Goldstine where practically everything else which he has ever done was attributed to Von Neumann. But in this particular case Goldstine is saying I did it. I, Goldstine. So I suggested to Nick Metropolis that I haven't spent much time in trying to reconstruct the early days of flow charting, but that I felt sure that most of what was done in the way of analysis of problems for putting them on the ENIAC or even for solving problems with more primitive methods with desk calculators that the idea of a flow chart was being used. It may not have been stated in exactly the mathematical symbols and environment that the Goldstine, Von Neumann and Burks report put it, but that this business of repeating certain things and recognizing sub routines and so forth is implicit in almost everything that was done in the way of large calculations particularly for instance that Franco and Metropolis were doing with respect to their own use of the ENIAC.

TROPP:

Essentially the project that you described to me before ENIAC came into existence where

you had the gals at the desk calculators had to be laid out in much the same manner so that they could just go through the tables and each one would know sequentially how to handle the data that they received from one of the other operators. I'm trying to remember one of the earliest flow charts that I've run into in print, actually used flow chart, was one that I think used Betty Overton's name and initials on UNIVAC.

MAUCHLY:

I could point to a little different application and that is the block diagrams which describe how the divider and square rooter of the ENIAC was supposed to operate is in essence kind of a flow chart. It was a block diagram which did not detail the specific electronic components but which said here is a counter and it's supposed to count this, and there is a line which goes from one block to another which says that whenever a control pulse comes here the counter is supposed to count, and things of that sort. All of these ways of diagramming what we wanted to build so that it would functionally perform the job it was supposed to do were essentially methods of putting on paper in block diagram form something which was quite analogous to the flow chart which Goldstine was describing when he was writing that paper.

TROPP:

That paper was dated l947.

MAUCHLY:

It's one of the first things he did when he went to the Institute.

TROPP:

We might take a look at this early skim off and separation. At one point wasn't there a proposal to do something jointly between the University of Pennsylvania and Princeton? Or am I wrong in that?

MAUCHLY:

Well, I am dependent to some extent on information which is contained in Goldstine's history as to just what was happening. The reason for this is again this kind of dichotomy where Eckert and I were technicians while the other people were logicians and supermen of one sort or another. And so we were not really in on all of the upper level politics and strategy. But I know of course from my personal experience what Dr. ____ of the RCA Laboratory came to Philadelphia and asked me about taking a job with RCA. And I know from ____ that he was being approached by Von Neumann and by RCA, to what extent RCA approached him I don't really know, but ____ and I would discuss what our futures might be like if we were to accept some of the offers that were being talked about. I can't say that the offers were made. At least in my case when I talked with Zworykin why it

sounded like he was making an offer, but we never progressed to the point where he could tell me how much per year I would get.

We stopped more or less at the point where when I asked what about patent relations, he said of course every person working for RCA assigns his patents to the company. And I said I would consider it but I wasn't giving any answer. So in the meantime I know that Press attended some meetings in Princeton in the RCA offices. I believe a man named Vance at RCA was one of those connected with the proposed joint project between the Institute of Advanced Study and RCA for the design and building of new computers. So Pres was present at some conference of that sort in Princeton and he was being asked as I understand it by Von Neumann to come and work for the Institute. It was really a confusing situation because we couldn't easily separate out who was boss so to speak. It seemed as if Von Neumann was saying we are going to have a computer project at the Institute and we are lucky to have the cooperation of RCA's research laboratories. It was never spelled out what you would pay for that cooperation in the way of Von Neumann related it as if it were gratis, that RCA was just going to be a nice big brother and make available to the Institute anything we wanted. It sounded a little Utopian to me, and I think it did to Pres.

On the other hand as I said when I talked to Svorkin why it was pretty clear that if the computer project were set up under RCA we would be employees of RCA and subject to their patent provisions. And we would jointly have the ungrudging free cooperation of Dr. Von Neumann and people at the Institute to act as consultants and help us in work for RCA.

TROPP:

So it was really an RCA computer project rather than the Institute. I guess the one I was alluding to then may never have existed, and that was a joint venture between the University of Pennsylvania.

MAUCHLY:

This I did not hear about until later. In other words, I gather from reading these later documents and histories that there was some kind of a proposal or at least an approach, intimation made that Von Neumann would be happy to act as a consultant to the University of Pennsylvania at the Moore School if they would make a sufficient effort to have a computer project carried on at the University. Eckert and I were already trying to get the University to find suitable funding for carrying on such work.

We saw our futures as continuing at the University of Pennsylvania. After all we were both residents of Philadelphia, and we'd gotten this far with the University of Pennsylvania and saw no reason why if the University of Pennsylvania would get funding why this couldn't be a big thing for the University. But there wasn't anyone who saw it the same way we did, apparently, from the University. Dr. Pender then Dean of the

Moore School would talk occasionally about how he might get some funds from the American Philosophical Society, but in my experience I've had little of it, I thought the grants from the American Philosophical Society were always small. Five hundreds to this man that might help him to get a part time secretary to help him with his research. A thousand dollars to this man to buy some laboratory mice or something. And so I had never heard of any large funding coming through that source. But that was the only source that I heard Dr. Pender talk about. And the only one that we knew of that would have adequate funds was of course the Government. And from our point of view it seemed the most natural to try to get the government to continue to fund the research as they had done in the past. But no one seemed to think that was an avenue that could be relied on for continued funding.

So as the early months of l946 went by for instance, we could see that even with the public announcement by Army Ordnance and the University that the ENIAC computer existed there was nothing following that to support more project work at the University. Actually when Dr. Travers came back from his active duty in the Navy and became Director of Research in the Moore School he apparently had bigger plans and more in view than the other people had, and must have thought that he could get such funding. At the same time he did not want to go out and get this without making sure that Eckert and Mauchly were firmly cemented to the University in a way that would keep us there for two years under a contract where we could do nothing else and have no share in anything that was patented either.

TROPP:

That's interesting because you communicated earlier on the early work on ENIAC that you at the same time had other duties. You were teaching a full course load and you were even involved in other projects for wartime work. And at what point did ENIAC computer become a sole occupation while you were at the Moore School?

MAUCHLY:

Never. It would have been if we had accepted the proposal made by Travis. Our sole duties would have been in the computer project. Mine as well as Eckert's. And apparently from what happened later I gather that Travis must have felt very confident that he could get the funding which he did get from the Burroughs company that he did get after he left. The project at the Moore School became supported by Burroughs after the Army Ordnance funding had been pulled out and the EDVAC sent to Aberdeen for completion. It was still incomplete when it left the Moore School.

TROPP:

At what point did you and Eckert decide to leave the Moore School and off on your own?

MAUCHLY:

Well, exactly that point, actually, the end of March '46. In other words, during the February publicity with respect to the ENIAC we were still hoping that some way would be found so that we would stay at the Moore School. And one of the things arrived on was to keep an open mind you might say with the friends at the Census Bureau believing that there was an opportunity where something could be secured if you went about it right. The Census Bureau people were much interested and they had some funds. And we thought that a contract with the Moore School was an appropriate way of doing this.

But asthings worked out somewhere in the period of March 46. In other words, during the February publicity with respect to the ENIAC we were still hoping that some way would be found so that we would stay at the Moore School. And one of the things arrived on was to keep an open mind you might way with the friends at the Census Bureau believing that there was an opportunity where something could be secured if you went about it right. The Census Bureau people were much interested and they had some funds And we thought that a contract with the Moore School was an appropriate way of doing this. But as things worked out somewhere in the period of March '46 Travis appeared on the scene as the new Director of Research and said that all of this work now was going to be under him, and that he would not tolerate anyone on his Moore School projects unless they signed some agreement which he presented to us but which we thought were quite unacceptable.

END OF TAPE