



## **Computer Oral History Collection, 1969-1973, 1977**

**Interviewee:** John W. Mauchly (1907-1980)

**Interviewer:** Henry Tropp

**Date:** January 10, 1973

**Repository:** Archives Center, National Museum of American History

### **TROPP:**

This is a discussion with Dr. John Mauchly in his home in Ambler, Pennsylvania. The date is the 10<sup>th</sup> of January, 1973. Is that correct, Dr. Mauchly?

### **MAUCHLY:**

I think it is.

### **TROPP:**

Okay, let's just try this for sound. (Recorder off) As I mentioned before we started taping, I'm going to limit my questions on the beginning of this tape to roughly the Moore School and immediate post-Moore School period. And I think we'll start with the question that we discussed briefly; namely, really why did you leave a whole professoriate in physics at Ursinus to go to Penn as an instructor?

### **MAUCHLY:**

Well, the real brief answer is that there was more opportunity. I think it needs a little more explanation though.

### **TROPP:**

Right, right.

### **MAUCHLY:**

That is, the opportunity to do something which was in many ways an engineering project, such as building a large computer, was close to zero at a small liberal arts college; and I had already convinced myself by a few explorations that I couldn't just jump into an industrial job with better pay and get the opportunity I wanted there. So when the chance came to take an electronics course at the University of Pennsylvania at government expense, I took that. And then when, after the summer of '41, I was offered a position on the faculty, not on the faculty, but the staff of the Moore School as an instructor, why of course the same question occurred to me, you know, that an instructor is a pretty low rank

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when you've been a full professor. But it turned out that the pay was about the same, and the opportunities were much greater because, as was explained to me by Dr. Chambers who was the one who asked me to consider being on their staff, there would be many chances to get consulting jobs and outside income which would supplement the instructor's income. Of course, the war was going on in Europe and defense contracts were already making possible the course I'd just taken, and the possibility then was not just that you'd get outside consulting contracts but that you would work on defense projects of an urgent nature and if you couldn't get what you wanted done, at least you'd be doing something useful and interesting.

**TROPP:**

Arthur Burks was the other--you mentioned the other Ph.D. who was in that course with you.

**MAUCHLY:**

I believe he was the only other Ph.D.

**TROPP:**

Arthur also...

**MAUCHLY:**

He came all the way from the University of Michigan as a mathematical logician who had satisfied their requirements that he was capable of becoming trained in electronic engineering.

**TROPP:**

I've heard this story from the other viewpoint and I guess now I'd like to hear it from your viewpoint, and that's the now famous trip to Iowa State. That's another story I think that it's very important we get on the record.

**MAUCHLY:**

Well, that happened the same year, just prior to my enrolling and being accepted in the Moore School Defense Training Course in Electronics. Actually I'd met Dr. Atanasoff at a triple AS meeting in Philadelphia the preceding winter, and we had decided we had some common interests and he was loath to reveal anything but the most general description of what he was doing in the way of a computing machine except, as he stated it, If I were to visit Iowa and visit Ames. He said that if I came out there then he could reveal to me what he was actually doing and only under the condition that I came out there would he do this. I understood from him that the reason for this kind of behavior

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was that they hoped to get some patents on their machine and they felt that IBM or someone else might take advantage of this if they were not quite close-mouthed about what they were doing.

Of course I knew that there was a large statistical laboratory and a very well-known one at Iowa, and so I thought that this was kind of an interesting place to go to, if I could ever manage it. The problem of getting there was not an urgent one but it was a hard one to solve economically. So after an attempt to carry through plans for visiting him at Easter, I believe, I finally managed to get some neighbors of ours to pay for the gas and oil while I drove the car out to Ames. I also, since there was room in the car, took my son along--James, who was about six years old, I guess, then.

So we started off immediately after the close of the academic year at Ursinus. The last formality was the faculty meeting and graduation ceremonies, and we then started off what was to be a two-day trip, dropped off the paying passengers along the way, stopped at the University of Iowa where there was an American Physics Teachers' meeting, I believe, and listened to a few papers there, including one, I guess, on meteorological courses and teaching in the United States. We finally arrived at Ames, Iowa, rather late on Friday evening. I think it was one of those Friday the thirteenth. At any rate the thirteenth of June, I think, is about the time we arrived, and by arrangement with Atanasoff we were staying at his house. I'd been surprised to learn that his wife was surprised to find that I'd brought my son with me, but at any rate she accepted that. My son joined her children in playing during the next few days while Atanasoff and I spent a lot of time together, partly in, just between the two of us, talking about computers and other subjects which were of interest to the both of us. And part of the time spent there, of course, was in meeting other people, visiting other people including Dr. Bryant, for instance, with the Statistical Laboratory.

The thing that was on my mind all the while I was there, however, was that I had already made an application for taking this Defense Training Course in Electronics at Penn and I didn't know for sure whether I was going to get in it or when it began, and this was something hanging over me. If that worked out, I might have to rush back. If it didn't, I had some offers to teach courses at -courses which Penn State was setting up in the State of Pennsylvania.

At any rate, the time that I spent there was somewhat limited and as near as we can fix it now--many attempts have been made to fix this for other purposes--I must have left there about Wednesday morning, because I had to come back, pick up the riders, my neighbors, to bring them back to Collegeville. Then correspondence shows that by Friday evening I was in Massachusetts where I had an arrangement to meet somebody of the American Optical Company on Saturday morning and talk to them about a possible job in industry.

So from late Friday night to early Wednesday morning is the total scope of time, you might say, I had available. So on Saturday I know that we went over to the Physics Building where, although no classes were in session and the campus looked rather dead,

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Atanasoff and I were able to go in and he could take the covers off his partially built machine and start explaining to me what was the plan.

At that time there was a framework, steel angle-iron, which supported some axles and shafts and a drum memory, which was not a magnetic drum as we might expect a drum memory to be nowadays, but an insulating drum filled with capacitors and little metal studs sticking through the surface of the cylinder, each one connected to one terminal of one of these capacitors. This was essentially an electrostatic memory device which was able to function in spite of the leakage of a capacitor, because of the fact that he had devised a means of restoring the charges periodically as the drum turned and the studs on the drum were contacted by little brushes on the stationary part of the machine. A circuit would then restore a positive charge on the condenser, capacitor, if it had a positive charge or if the charge were negative it would replace this charge or, you might say, build the charge back up to the negative value which it would have as a nominal starting value then.

This storage device was *the* essential thing, so far as I was concerned, to explain what he had promised in Philadelphia when I first met him; a computing machine which could store many binary digits at a very low price. The price was exceptionally low. The first disappointment I had, of course, was that the speed was exceptionally low as well. The drum turned about one revolution per second. So it took all of one second to restore the sequence of binary digits which represented one number. Because these things were in parallel, on the drum you could have a number of such memory devices or storage devices being regenerated or restored simultaneously. So in his plan he had, I believe, 30 such registers of something like 30 or 40 binary digits, each of which every second then could be renewed in their ability to store whatever had been placed on them. Of course, through the same kinds of brush contacts he could alter what was stored on them, providing he had the right kind of arithmetic circuits to figure out what he wanted to put on the storage registers in the next revolution. I could go on and describe some of his plans –

**TROPP:**

I guess it's more. His plans are important as well as what you saw.

**MAUCHLY:**

Yes.

**TROPP:**

I guess my real question, because I'm not interested in the legal aspects of the current litigation, I'm interested in the intellectual input in terms of ideas.

**MAUCHLY:**

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Yes.

**TROPP:**

You know, it's clear that in both of your cases, your backgrounds in physics, the kinds of problems you were involved in, in teaching and with students, and intellectual interests--that you were both concerned with computation. You had computational problems. You had both gone through the differential analyzer period and computation was much on your mind, and clearly you had both thought about it and, you know, from the standpoint of intellectual input and ideas, which is a very different thing from what the legalistic approach is.

**MAUCHLY:**

No, I'm not trying to answer any legalistic problems here.

**TROPP:**

Right, right.

**MAUCHLY:**

The nearest I came to that was saying that there are limits to the time which I was out there, because some people tried to stretch those and make it appear longer. In my mind it really isn't important as to how long I was out there. If it had been two weeks or two months, the result would have been about the same, because the result so far as I was concerned was that, although what he had to show me was interesting, I did suffer a rather drastic disappointment, you might say, as soon as I found out what his plans were and what he was doing. And I suffered a series of further disappointments as I talked to him, and yet I felt that it was not an entire waste of time, because here at least, was a man who seemed to be interested in some of the same things I was interested in and we went on to compare notes and to see why we were doing --

**TROPP:**

What about the advanced circuitry, for example?

**MAUCHLY:**

Well...

**TROPP:**

Was there much of that up then in the main frame?

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### MAUCHLY:

He had a few, what we call modules, I believe, built and he was able to test them one way or another. He had a separate apparatus for testing, for instance, off of the main machine, which would show that they did indeed perform, bit by bit, binary addition, that 1 plus 1 was zero plus a carry and things of that sort. The fact that he was able to do this didn't astound me at all. I mean this to me was something that you could have known in advance. Anybody could do that. The laws of binary addition being already well-known, the laws of electricity and vacuum tube circuits and things already being well known. Given that you wanted to do binary addition, there was no problem as far as I was concerned. So one of my kinds of disappointments was to find out that he thought that he had solved a major problem in devising an electronic circuit to do this. There was no problem to it at all as long as you were doing it slowly and he was indeed doing it very slowly. The problem to me, the problems I was interested in seeing solutions to were not only cheap circuits for storage, which he indeed had, but making use of the electronic equipment to gain the full advantage of the speed which it was capable of. This he hadn't done. Secondly, he had not done anything to make this machine--or to plan for this machine to be anything but a single set purpose machine and to solve sets of linear equations, a limited number of them.

### TROPP:

Up to a given size, 28 or...

### MAUCHLY:

Size 29 or something...

### TROPP:

Something of that order.

### MAUCHLY:

Being able to store 30 constants and things of that sort made it possible for this machine to--you'd expect it to store up to enough numbers to complete the solution step by step.

### TROPP:

It wasn't in any way even intended as a sequential machine. You were going to have to, in a sense, do each elimination and each equation and then go back and reset the machine, as I understand it.

### MAUCHLY:

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That's right. The kind of thing that he had set his frame for and plans for was going to store just enough  $s$  so that far from being able to solve for all 29 variables in such a problem, it would only be one step of the elimination between two equations to eliminate one variable.

**TROPP:**

Right.

**MAUCHLY:**

Then the input and output had to be used as storage and there were big difficulties, because he was using paper as a medium to store the input information and to record output information. So for every elimination you had to feed in paper storage and get out paper storage, just for that one step of elimination. So to eliminate on 29 equations why you already had to--just to eliminate one variable you had to do this something like 28 times.

**TROPP:**

Right. So 28 times then would reduce it to a set of equations of  $n$  minus 1, and  $n$  minus 1 variables would then go through again until you finally got one equation and one unknown that you could backtrack.

**MAUCHLY:**

Then you'd have to do the so-called back solution to get all the others. All of this meant manual work and feeding these paper storage things. Now the paper storage was, to start out with was the Hollerith out of punch cards which you could produce on any old card punch, but thereafter he got his output by piercing the paper with a spark and then depending on input to be able to read where these little pinholes were in the paper, as they had been produced by the output spark. This was, as far as I could see, not very reliable. From all I've heard since, the reliability of that device was never satisfactory.

**TROPP:**

It never really got going. Did you meet Clifford Berry during that short visit?

**MAUCHLY:**

I did, yes.

**TROPP:**

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Because Clifford apparently was working on the input-output.

**MAUCHLY:**

I understand that that was a Master's thesis for him, to try to get something there that would stand up to a test of actual use.

**TROPP:**

Decimal input conversion to binary and output in binary and then back to decimal. That was going to be very difficult.

**MAUCHLY:**

Another thing which was somewhat disappointing was to find that Atanasoff considered the problem of converting from decimal to binary and binary to decimal as something rather difficult also, and he had a special part of the machine with commutator drums to do that and thought that that was, you might say in itself, a major achievement. The conversion to different number systems was something that to me didn't present any problem really, mechanizing it. Of course, it was quite a different problem than just setting down rules on a paper. Nevertheless, there was no theoretical difficulty about this and I didn't see any real difficulty about it.

**TROPP:**

I have a problem with time sequence. I'm going to throw in an extraneous question here. You mentioned that you were at Dartmouth when Stibitz demonstrated the complex calculator.

**MAUCHLY:**

Yes, I was.

**TROPP:**

Now did that meeting occur the year before your visit to Ames, or was it the end of the summer of the same year?

**MAUCHLY:**

It was the year before, 1940.

**TROPP:**



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On that machine you already had seen a conversion from decimal to binary, which was how that machine operated, and conversely.

**MAUCHLY:**

It was my impression that the actual calculating device at the West Laboratories of Bell Telephone worked on a two by five or biquinary system of relays.

**TROPP:**

Right. Okay.

**MAUCHLY:**

It was already a checking system, from that point of view, that you only used two contacts out of the possible 10. In other words, you had a binary and a quinary system coupled so as to become a decimal system.

**TROPP:**

Right. But your input as far as a person who knew nothing about the internal mechanism, when you put in your complex numbers, you put them in in decimal form.

**MAUCHLY:**

Yes. You went through –

**TROPP:**

You added 7 plus 31 to 12 minus –

**MAUCHLY:**

Yes. You went through some sort of a code which was acceptable to the teletype circuits and this was then suitably coded to relays of the computer machine so that it was able to do its storage and its computation with a biquinary system, and what it sent back again was teletype code which operated the output typewriting part of the thing at Dartmouth, the terminal at Dartmouth, so that you got decimal numbers printed on the –

**TROPP:**

That particular device meant the symbol--or I'm sorry, the problem of solving the input-output had been handled, as I gather, very simply and easily.

**MAUCHLY:**

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That's right. There was no conceptual problem that I knew of.

**TROPP:**

But back to the Atanasoff visit, do you remember any conversations with Berry and what areas you might have discussed?

**MAUCHLY:**

No, I don't really. I was, I remember, somewhat impressed by the fact that Atanasoff on that Saturday when he took me in there, was not going to demonstrate the machine at the time. He merely described it. He described it and we looked at it and I understood what he was trying to do. A demonstration of the operation of it, he wanted to postpone until a day when Berry would be available because Berry, the graduate student working on this machine, was apparently the man whose work might be upset if somebody else started monkeying with this. Atanasoff did not want to even turn the machine on. He preferred to leave that to Berry so that Berry would have a clear field in whatever it was he might be doing. This kind of attitude I respected, because I've seen many cases where somebody trying to demonstrate something has upset the work of somebody else who was really working on the thing.

**TROPP:**

Did Berry subsequently turn the machine on for you and demonstrate it?

**MAUCHLY:**

Oh yes, yes he did. They did many rather simple things with the machine, but I wasn't too interested in whether they could demonstrate that what they had already put together worked as it should, because it seemed clear to me that if it didn't, it was just a matter of a little more work. It ought to work. The principles were clear and the only part which looked like it was dubious, from the point of view of practical realization, was input-output. The rest of it, even though much of it hadn't been designed yet where you're getting the proper sequence of operations, the rest of it to me was just pushing it through. And I personally had no doubt that a good graduate student such as Berry and a man such as Atanasoff could eventually complete this machine. All they had to do was keep at it.

**TROPP:**

Let me really hypothesize now. This is kind of looking back. Had the machine been completed within a reasonable period of time, can you see it having any impact in terms of the computational environment of our then scientific world?

**MAUCHLY:**

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Well, it's hard to say what impact it would have had. It seems to me it should have had some impact, just as a demonstration of the fact that it was possible to use this kind of storage which was certainly a low-cost system, to implement an important part of a calculating device--namely storage which could be electrically manipulated, you might say. There wasn't anything awfully sophisticated about it other than that, and as I say, it was slow. It seemed to me that they could have done better with the speed, even given that they were going to mechanically compute these things. My interests were in--was getting more generality to what you could do, and also in improving the speed to cost ratio, you might say, a great deal. If no one had been able to take such a device and provide it with a better input and output, why, of course, it was doomed to be inefficient.

**TROPP:**

OK, let's leave that period. I gather you went back because you had these other commitments and also were concerned with the beginning of the summer course at the University of Pennsylvania, which began shortly thereafter.

**MAUCHLY:**

Yes. At this time I don't know when I first found out about my acceptance at the University of Pennsylvania, but I did have this commitment to get up to the American Optical factory on that following Friday. It turned out that the course at the University did start fairly soon thereafter, so I was tied up for about two more months that summer in that course without much time to do anything except keep track of what was going on in the course, and all the spare time I had was essentially used for talking with Eckert whom I met there.

**TROPP:**

You met Eckert for the first time?

**MAUCHLY:**

I met Eckert for the first time.

**TROPP:**

Was Eckert a graduate student at Penn?

**MAUCHLY:**

Yes, he completed--had gotten his degree for Bachelor's in Electrical Engineering and he was expecting to do graduate work. I don't know whether he was yet doing it, but at any rate he had been signed on as a lab instructor for this Defense Training Course. And so in

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addition to meeting the faculty in the school who were teaching the courses in the morning, in the afternoon we had laboratory sessions and Eckert was the person in charge of some of those sessions where the people who were not acquainted with electronic gear at all had to, of course, go through every experiment very carefully. Many of the experiments, especially things like just the characteristics of a triode being measured within a few millimeters and things, those experiments were ones that I had already designed and given to my students at Ursinus in an advanced course in physics. So –

**TROPP:**

I was going to say, in terms of technical input for you, I would have guessed that there wasn't much new in that summer course except possibly for its orientation or its emphasis.

**MAUCHLY:**

Well, they had a few pieces of apparatus around the Moore School, of course, which I couldn't possibly have found at Ursinus. They had for instance, a sinusoidal generator which generated harmonic signals of any frequency you chose up to some limit like ten thousand cycles per second. This was an old piece of Bell Laboratory's gear or something which they'd come across. It was already a little ancient but it was much more than I had at Ursinus, and so it was nice to have a few pieces of apparatus to try things on.

**TROPP:**

They had some differential analyzers there, didn't they?

**MAUCHLY:**

That was a more interesting phase, of course. Once you're inside the building, you might say, and you have some time to run around, you could get acquainted with the mechanical differential analyzer which they built during the thirties. So that was a lot of background information.

**TROPP:**

This was a three-stage –

**MAUCHLY:**

In a sense this differential analyzer was supposed to be a twin sister of another built at the same time for Aberdeen. They had done a joint project where while building one they built another, and Aberdeen got one and they got one. But the two were not really twins because the one at Moore School, I believe, had something like 14 integrators, whereas

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the one at Aberdeen only had, say, 10. There was something like 3 or 4 more integrators on the one that Moore School had.

**TROPP:**

It was really a very large one.

**MAUCHLY:**

Oh, yes. I have a picture of it around here somewhere.

**TROPP:**

I should mention on the tape that while visiting you in your home and going on a tour this morning I saw the differential analyzer you built at Ursinus. I just want to get that on the record.

**MAUCHLY:**

That was a harmonic analyzer.

**TROPP:**

I mean harmonic analyzer, excuse me.

**MAUCHLY:**

Yes. Quite a different breed of cat really.

**TROPP:**

Yeah. This was the one that you mentioned in the earlier tape that you had built

**MAUCHLY:**

Yes.

**TROPP:**

At Ursinus.

**MAUCHLY:**

Yes.

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**TROPP:**

The early proposal for ENIAC, the very first one, occurred sometime in 1942. Is that a correct date?

**MAUCHLY:**

That was the first written one.

**TROPP:**

The first written proposal. I guess what I would like to do is kind of lead up to that proposal in terms of the people you were talking to, who was interested in it,

**MAUCHLY:**

Yes.

**TROPP:**

and who got involved with you in that early proposal, which would then cover approximately a year or so time period?

**MAUCHLY:**

Yes.

**TROPP:**

From when you joined the Moore School.

**MAUCHLY:**

Well, my interest in calculation goes away back, as you know. My interest in working on devices for aiding calculation was, began to show up, you might say, in the mid-thirties. And by the time I got into the Moore School I was convinced (clears throat) that a lot of advantages would accrue if you could build your calculating devices out of electronic components instead of out of mechanical ones. That was one of the reasons why I was so anxious to have this course in electronics. I already knew that it was going to take money to build anything like this. With my own funds I had done very immediate work at Ursinus in the way of laboratory testing of counter circuits and ring counters and things of that sort. So I was hopeful that once in an engineering environment there with a lot more resources available than I had before that it would be fairly easy to, so to speak, start something going. I was also aware of the fact that I had to do work on projects which were brought into the school through Government contracts, because that's what

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was available and that was the only place to get some extra money, and I needed extra money.

So as soon as they told me that they would like me to work on a particular project which was already in-house, I was glad to do it. It turned out that some of these projects, although interesting, didn't involve a great deal of computation. But it also turned out that some for the Signal Corps involved a lot of computation. As a result, I became more and more anxious to persuade somebody, if could, that it was time to get some better computing systems built because these computations could be so much more readily handled, not only from the point of view of cheaper, but faster. So Dr. Brainard, for instance, was able to make negotiations with the Signal Corps in New Jersey to bring a contract in where we were studying the patterns of a parabolic reflector and a dipole radiator placed within the focus of this reflector. The object of the program (classified at that time, of course) was to see whether alterations in the shape of this parabolic dish could produce beneficial changes in the pattern of radiation so that one could get a sharper definition in a radar sense, of where the target lay, or something of that sort. Well, the only way to handle this that anyone knew of was through numerical integration of the electromagnetic equations over the circuits of the reflector. So you had to start out with the intensities of the dipole radiation at the focus and then consider for each little piece of reflector how that radiation would be affected thereafter, using essentially Huygen's (?) Law known in optics.

**TROPP:**

You see me shaking my head because I realize what tedious work

**MAUCHLY:**

Right. that kind of integration is.

**MAUCHLY:**

So Dr. Brainard in getting this thing, of course, had told the Signal Corps that, "oh yes, we can do that easily," you know. "We've got the differential analyzer. We've got all kinds of computational means at the University." He set some kind of a time period like 6 months. I don't know that I ever actually saw the contract and I can't recall now what the time period was. But he set a time limit on which he said they would do certain studies and get certain results. The contract was in part experimental; that is, some other people were put to the task of building such a parabolic reflector and testing out for real what they found out experimentally. These two parts of the project were going on simultaneously. Arthur Burks, whom I also met in this summer course, had also been taken on the faculty and he was assigned to this project. He, I think in part, worked with the people who were doing the physical part and he also kept in touch with what I was doing on the numerical integration.

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### MAUCHLY:

Well, the first thing I found out when we started this project was that there was no one who was going to tell me how to set up the numerical integration. Dr. Brainerd just handed me a book by Stratton of MIT and pointed to a vector integral and said "This is what you use." That was a general electromagnetic equation applied to all branches of electricity in magnitudes --any place where Maxwell's theory would apply, you know. It had no specialization to what we were trying to do. Then he handed me an emeritus professor of physics whom he felt ought to be useful on the project and I spent literally weeks--I don't know how long now--trying to tell this emeritus professor what vectors were. He did not understand vector terminology as applied to radiation or anything of that sort. He was already no longer teaching the first course in physics, which was the only physics course he ever taught, I think.

### TROPP:

Where the word vector never showed up.

### MAUCHLY:

So, yes. So I kept trying to explain to him the geometrical picture of what this was all about, what we were trying to do, and I finally had to go to Professor Brainard and say, "I'm spending more time trying to educate my helper, he'll never be any help to me. So please forget about that." I think at that time he then said, "Well, we'll get somebody else," and I think that's where I met Professor Wigner, Witmer, rather, of the Physics Department. He was then put in touch with me to try to help us. Well, I had many interesting discussion with Professor Witmer, but I can't remember that he was any great help to me at all in this. I see at the moment that I misstated something. The first time I met Professor Witmer was earlier when I discovered that he was trying to integrate, or he was trying to calculate the tables of the energy values of the asymmetric rotator, and he already had more conveniences than I had, because he had funds from a grant which paid for a person to sit at a calculator 8 hours a day to do what he told him to do. Both of us were outclassed, of course, by Dr. Gilbert King, who was then at Arthur D. Little doing this all with punch card equipment. So we both gave up the ghost before this

### TROPP:

(Laugh)

### MAUCHLY:

An asymmetric top problem was concluded. So I had known Professor Witmer before, but I believe that he was brought in to help me on this Signal Corps radiation pattern job. Well, time went on and, of course, it didn't take too long before I had set up a schema, you might say, for doing the calculations. Laid out tabular forms and said, we'll multiply



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this column by that column and row by row we do this and column by column we do this, and all that. But again, I couldn't personally carry through all these calculations for small elements of this big parabolic dish in the time that was required, so the obvious thing was to get more help. Well, they had students available for hiring on such jobs, part-time during the school year, come in and helps you with the calculations. But the real problem back there was that no one was going to do this calculation in his head; no one was going to do it with pencil and paper even with a log book. This is the kind of thing that at the very least you ought to do with desk calculating devices, something that's capable of multiplying. Adding machines weren't good enough.

### MAUCHLY:

So, at this school which prided itself on the knowledge of and use of computing machines, what did they have? It turned out that aside from the differential analyzer which was probably fully employed, sometimes for two shifts, the only other calculating device they really had was one Friden desk calculator which did not even have what is called automatic multiplication. It's just a plus and minus bar. It was electric.

It was electric. But it was multiplication by repeated addition and slowed down to the mechanical speed of a Friden desk calculator of the time. They had one such machine which was usually located in the analyzer room because it was used to check the first few steps in an integration with the analyzer to see whether they put in the proper gear ratios and things and had the thing set up so it was going to turn out what they expected. So they had no other use apparently for this desk calculator except for this checking, and no other projects had ever seemingly dealt with calculation. And so all the rest of their proficiency in calculating devices seemed to reside in the fact that there had in the past 10 years been graduate students who did theses on special calculators such as polynomial equation solvers with electric generators coupled on a shaft and operating in different phases, and things of that sort. Those things were stowed away under the benches in the analyzer room and nobody knew how to use them, if they had been useful for anything. As a matter of fact, in my own office there was another kind of calculating device called the sound prism; one of the first examples of analyzing sound which used a heterodyne principle to take the various waves, frequencies coming from a microphone input and transforming them to a frequency through a standard filter and then displaying it by a rotating mirror on a ground glass screen, so that you could see the sound spectrum, amplitude versus frequency. This had been used for some studies for trying to differentiate whether good violins and poor violins could be distinguished by the spectrum that you saw on such a sound prism. Well, the machine stood there and nobody really knew how to operate it any more, you know.

### TROPP:

(Laugh)

### MAUCHLY:

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The graduate students who had done this had gone and all that. The professor who was supposed to have known most about calculating devices and the theory and design was Dr. Travis, who was a Naval Reserve officer who had been called to active duty just before I joined the staff.

**TROPP:**

This is Irven Travis?

**MAUCHLY:**

Dr. Irven Travis, yes. So there was no one there who really understood much about the theory of calculation, especially digital calculation. I was it. And here I was supposed to do a Signal Corps contract and they only had one desk calculating machine. So we tried to remedy that by various means. The first thing I could do was to try to persuade Ursinus, which had one Monroe automatic multiplying desk calculator, try to persuade them that we needed it more than they did. This was not automatic. I had to fight for it.

**TROPP:**

But you did succeed?

**MAUCHLY:**

But I succeeded, and so we got that. You understand, of course, that this machine--the factories that were normally building such desk calculators, the Friden, the Marchant's, and the Monroe's--apparently were all diverted toward building parts for mechanical gun fire control devices or for fuses in large shells, or what not. There were all kinds of things to which the factories which had been accustomed to doing, making mechanical computers, were diverted for munitions work. So there was probably an extra demand, too, for desk calculators for other purposes other where in the Government. At any rate, you had to have a priority to obtain delivery on a desk calculator of any kind. Dr. Brainerd and others seemed to think that they couldn't get a priority, that it would be almost impossible for them to persuade the Signal Corps or somebody to give them the necessary papers so we could order another desk calculator and get it delivered to us. I think we finally did get one through that route. The Moore School wound up with one extra desk calculator in that case, obtained on the priority system, but altogether even then that would only be three, you see. We needed more.

The Wharton School of Business, however, taught courses in statistics and had what they called a Statistical Laboratory. So two blocks away on the Pennsylvania campus, the Wharton School had something like 10 or 12 desk calculators. We borrowed as many of those as we could so as not to deplete their teaching requirements, and during the holidays when there were no students supposedly needing them, why we borrowed all of

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them and hired as many students as we could to sit. The students, of course, were part-time most of the school year and so you did shifts. You'd have one student using the machine for a couple hours and then another student come in and use it for a couple hours, things of that sort.

**MAUCHLY:**

At the end of 6 or 8 months we at least had gotten some of the work done, knew what we were doing, had checked out that our integration method had been correctly laid out and planned, and so forth. One of the ways we knew this was by the fact that the first test experimental pattern and our calculations both agreed and further, that the curve looked awfully familiar to me.

**(Recorder off)**

**TROPP:**

You were talking about the fact that (before we were interrupted) at the end of 6 or 8 months you had verified the experimental-

**MAUCHLY:**

Oh, yes, I know now. Yes. Not only were these two curves in good correspondence, the experimental one and our calculated one, but the curve looked familiar. And I had as one of my nice possessions the German book, Junkejende(?), which portrays graphically many of the functions which it lists. So I leafed through that and sure enough, this was the first order Bessel function. Because our first experiments were with the complete paraboloid out to the--what do you call it, the plane of the or something-and it turns out, although we didn't know this in advance, you know, that we could now prove it, that if you had a perfect dipole and a perfect paraboloid of this shape, that the pattern you should get for the amplitude of the wave against angle at a distance was the first order of Bessel functions. Now beyond that, of course, Junkejende was no help to us, and all the Bessel functions that the Harvard Mark I ever computed would probably have been no help to us, because the next step in the experimental program was to take pieces out of this thing. Something like you do with (Fennell?) zone plates and things, you block out radiation or you make it missing here, hoping that it will reinforce at one place and diminish the radiation where you don't want it, alter the side lobes of the pattern, and what not. That was purely an experimental thing for which the mathematical solution would have been, I'm sure, very onerous. Then from that point on we went ahead trying to do this thing, but we far over-ran, of course, the promised time for doing this because, first of all; we didn't have the organization; we didn't have the equipment; and as far as I was concerned, we didn't have an electronic digital calculator of the kind that I kept talking to people about in the preceding time at the Moore School and kept saying, this is the way to do it. So that project as it stood, however, of course, didn't convince anyone either. The Signal Corps was only interested in getting its results and when they got the

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results that we gave them, they thought that was fine and the school got a follow-on to do more of the same, I believe. So we had more of this calculation to do by desk calculators and no chance at building a computing device which would answer those questions and lots of others more efficiently.

So as I began to see it then, after the first year or so of working at the Moore School, the only real hope seemed to be in understanding what the needs were of the Ballistics Research Laboratory, because they were more and more needing firing tables computed and were falling further and further behind in getting their output. It was literally true that they weren't keeping up to the schedule of the production of guns, or even the shipment. Guns could be shipped to France long before there were any tables telling them how to aim them. So after they had hired a few girls to keep the analyzer running in two shifts instead of one, the next thing they did was to try to hire and train girls for using Friden desk calculators for step-by-step integration. They took about two weeks to go through the process of computing one trajectory and there were many, many trajectories, of course, for a firing table for one gun. So they had a hundred girls or so around that place in 1943 to try to get the artillery tables computed and still they were falling behind. By that time then we had, you might say, the atmosphere which would lead to somebody being interested in a new method which would try to cure bottle necks, such as an electronic computer.

### **TROPP:**

When you were first thinking about writing a proposal, the very first proposal or draft proposal as you might call it, who were the people that you talked to in the process of writing this up.

### **MAUCHLY:**

Well, I had already done the talking before I wrote that up. I talked first, of course, to Eckert in 1941 in that summer course. I continued to talk to him from time to time on this.

I was impelled, you might say, to come back to that subject every time that I got fed up with the slowness of the hand calculations on the Signal Corps project, for instance. And I would talk to Dr. Chambers; I would talk to Reid Warren, Professor Warren; I'd talk to Professor Grist Brainerd. Dean Pender was not one that you would do a lot of exposition on this to. He wasn't as available; he had plenty of other things to do. I talked to other people. The man who had most to do with running the analyzer—Dr. Weygandt—I talked to him and some of the other people around, but occasionally there would be a chance to talk to somebody that came up from Aberdeen, because the Aberdeen use of the differential analyzer started very shortly after I joined the staff of the Moore School. Well, I talked to, for instance, Dr. Dederick, who was in charge of computations at the Ballistics Research Laboratory, and found that in 1942 he wasn't much inclined to get enthusiastic over what I was talking about. His answer was that, well, it would take at

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least a year to build this thing that I was talking about and by that time the war would be over. So what's the use? So he did not take fire or suddenly say, I agree with you; let's do something about it. It wasn't until later that the ordnance people actually took hold of this. In the meantime I had written this memorandum. Now, why did I write it? Because Dr. Chambers, who was a pretty practical fellow about getting work done and people interested, said, "All you're doing is making sounds in the air, so to speak, if you talk to people. You've got to put something more permanent on paper so somebody can consider it. So why don't you write up some kind of a memorandum or proposal and circulate it and see whether that is concrete enough that somebody will do something about it? This is what I did.

### **TROPP:**

I want to ask two questions. I think I'll ask them both together and you can respond at will. In terms of the approach to this problem, what areas were you and Pres Eckert discussing prior to that first memorandum? And the second question is, what response did you get, positive or negative or critical or otherwise, from the people that you circulated it among?

### **MAUCHLY:**

The first question is answered this way, I guess, and that is, with respect to the feasibility of any kind of an electronic device to do any kinds of calculation, why, Pres was a most helpful person for discussing this because he already had a very practical knowledge of electronics and the need for reliability. He immediately seized on the fact that if you're going to do calculations you've got to do them with a reliable instrument. You can't just depend on being right three quarters of the time, even ninety-nine per cent of the time isn't sufficiently good. You've got to be very reliable about it and, therefore, the first question of feasibility really is, can we make something that's reliable enough to be a component of an instrument which has to be almost faultless? He saw, the way he considered it, this should be possible. We didn't, of course, figure out, either of us, exactly how you would do that, until the real project later came along. But he had perfect confidence, you might say, in the fact that if you take the proper account of the variations which every component in a circuit is likely to have, if you sample the kinds of variations that occur in these various pieces--the resistors, capacitors, the vacuum tubes, and so on--if you took proper account of those then you could certainly design a circuit which would work reliably, taking all of these into account. It will certainly not work reliably if you don't do this, you see.

So he understood, in general, what the requirements were from the point of view of how much work would go into designing a, you might say, a complex of equipment which would produce something useful. We didn't put ourselves to the task of saying how big or how costly or how much would be involved in the way of man hours, but we did try to estimate at what rate these things would work. How fast could we get a counter to count; and given that, if you made an integrated all-in-one system computer with lots of

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counters in it, what could you expect of the speed of the thing. That's the kind of thing that I incorporated in my memorandum. Also, as one can see if you read the memorandum, I addressed myself to the problem of the computation of firing tables because I already knew that that was the item of interest to the Moore School and that was probably the most likely source of funds and there were the people if I could just get them interested.

Now as to the response I got, the people from Aberdeen were just occasional visitors and I didn't even know whether it was in my province to hand this memo out or send it to anybody outside the Moore School. What I understood the situation to be was that if I gave this memo to the various faculty members at the Moore School, that they could carry the ball from that point. And so my memory is that I gave a copy of this to three or four different people: Dr. Chambers who had suggested my writing it presumably; and certainly to Dr. Brainerd who was the one faculty member who did most of the outside contract work and contact with the Signal Corps and the Army Ordnance people and such. I expected then that I would get some criticism or response from them. I didn't.

**TROPP:**

Kind of like died. It was one of those memos that just vanished into the files.

**MAUCHLY:**

That's right. It's like dropping a stone into the well and never hearing it splash.

**TROPP:**

I wish I could remember the scribbled comment by Brainerd on the cover of the copy that I have. Do you have any recollection of that or ever having seen it until it was later resurrected?

**MAUCHLY:**

I can't recollect what the comment was but I didn't see that comment until the last few years, because apparently this copy of the memo I submitted to him could not be located at the time that I asked for it in 1943, and only appeared after the files of the Moore School were subpoenaed or otherwise acquired by the legal people in some trials.

**TROPP:**

I say I can't remember the exact phrase but it struck me as the kind of thing that a professor would write on it, having received it, intending to return it to you with that comment.

**MAUCHLY:**

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The comment certainly looked like it was intended to come back to me.

**TROPP:**

It was addressed to "John".

**MAUCHLY:**

Why would he put a salutation on it unless he were addressing it to me?

**TROPP:**

Apparently he intended to do this and for some reason it never got sent.

**MAUCHLY:**

One of the oddest things about it is that that comment was dated in December, I believe, of '42 or somewhere thereabouts, whereas it had been submitted much before that and the time that I asked for the document and it couldn't be found was in something like April '43 when I had an opportunity for the first time to get Lieutenant Goldstine interested. Goldstine's first reaction was, "Well, this ought to be written up." I said, "Well, I have written it up." He said, "Well, let's see the write-up." Unfortunately I found that I didn't have a copy myself, and when I found that no one else had a copy we resurrected that, re-transcribing the secretary's shorthand notes, which were in good Gregg.

**TROPP:**

Originally, when Goldstine came to the Moore School, what was his role supposed to have been, his original role?

**MAUCHLY:**

My understanding of his role was that he was there as a kind of expediter. In other words, he was in uniform, as a member of the Army. He was an emissary from Ordnance and he was spending considerable time at the Moore School; and the reason for this was that the calculations they had been doing, both on the analyzer and with the girls at the desk machines, was not yet producing as much as they hoped for, and it was his job to see what he could do to get more production out of what was going on there under the contracts with Ordnance.

**TROPP:**



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It was perfectly natural that he would become involved with a better method of computation, whatever it would be, and hence the idea of doing computations electronically which would be faster and cheaper.

**MAUCHLY:**

Yes. You might say that he had more of a directive in that direction maybe than Detrick or someone else that might have visited before. I remember there was a Lieutenant Tornheim, for instance, who was a mathematician who came up frequently and visited the analyzer room, but apparently he wasn't told to see what he could do to improve things. He was just there to see that things were more or less going as per schedule, or something. So with Lieutenant Tornheim; and there were others, a man named Zugg, I believe, who was involved in teaching the girls, seeing that the girls got their numerical integrations done by the paperwork system. All the various other representatives of Aberdeen who were either sent up occasionally or were on the premises as employees of Aberdeen, were merely administrative assistants of sorts. The first person that really was given the commission to say, "For goodness sakes, let's see what you can do to speed things up," seemed to be Lieutenant Goldstine.

**TROPP:**

I'll ask you another hard question.

**MAUCHLY:**

Yes.

**TROPP:**

and I guess this spans a number of years in terms of your involvement with Pres Eckert.

**TROPP:**

Yes. I guess it would be interesting from my point of view to discuss the relationship between the two of you during this period, the amount of dreaming—vision if you want to call it—that went on in your conversations and the kinds of things you were talking about in this period from the time you joined the Moore School in '41, until you got into the nitty-gritty of actually having to build a computer.

**MAUCHLY:**

Well, I'd say first of all that any dreaming we did was more technical than political.

**TROPP:**



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That's what I meant. I was thinking of the—

**MAUCHLY:**

In other words, I don't think we spent any time really on the question we maybe should have been spending our time on; namely, where could you get this interest, how are we going to get somebody interested, what can we do to excite somebody to do something about it? Rather, we were always thinking about could we make it cheaper or could we make it more reliable or could we do this or could we do that, from a technical point of view, and we didn't really get into anything that wasn't technical.

**TROPP:**

How would you describe your relationship during that period in terms of discussing the technical aspects? Clearly he was the, as you considered him, the authority on the electronic feasibility aspects.

**MAUCHLY:**

Yes. In terms of the major criteria that you were going to have to be concerned with at the outset.

**MAUCHLY:**

And I might say that on the opposite side, it appeared to me at the time and I will still say, that Pres Eckert was discussing this with me and looking to me as the man who knew what it was that a computer should do. In other words, he didn't consider himself as the guy who was going to design a computer, already knowing exactly what was wanted. Rather, I was the person who understood the role of computation, and whatever computation was all about, and I was the one who was going to guide him or anyone else in what should the specs be for a computer if you were going to build one.

**TROPP:**

The capsulated thing then would be for him to take your criteria or outlined specifications for what it should be and should do and make sure this would be done without violating any fundamental laws of physics.

**MAUCHLY:**

Well, it was more—

**TROPP:**

Was it feasible?

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**MAUCHLY:**

I was going to say it wasn't just violating the fundamental laws of physics, but it was satisfying these quality control characteristics, if you like, that the damn thing should work long enough and reliably enough so that you got something useful out of it before you had to fix it; the very thing that Fermi thought was not possible.

**TROPP:**

Backtracking a bit, in terms of your this intellectual aspect of it,

**MAUCHLY:**

Yes.

**TROPP:**

and not only your earlier work but in specific the visit to Atanasoff, from the standpoint of your visit to Atanasoff and say 1943 or so when you constructed a second proposal; what would you say the intellectual impact of that visit was, in that time frame, on your own thinking about computers?

**MAUCHLY:**

Well, the impact of that, I suppose you could say, was certainly different from the impact of visiting the World's Fair and seeing what IBM and Rem Rand had on exhibit in the way of operating calculating devices. There was a ciphering device also on exhibit by IBM which was very interesting. It was also a different experience than any other that I had had with looking into calculating devices because at least this was the first time that somebody had got close to doing something with electronics. The very first requirement, you might say, is that you're going to have your signals in an electrical form so that you can manipulate them that way. Well, IBM with its relays and plug boards had a kind of flexible device which could handle data in the form of electrical signals and do something with that data which was flexible through the plug board wiring, but it was still tied down to the mechanical relays.

**TROPP:**

As was the Stibitz machine that you also had seen earlier.

**MAUCHLY:**

That's right. So again, I regard the Stibitz machine as a nice prototype, you might say, for what can be done in keeping things electrical, except that relays weren't going to realize

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the speeds of the vacuum tube. I constantly was making this comparison that the relays which you would have to buy to do reliable computations were not the little cheap 20- or 30- or 40-cent things that you had in an automobile for a cut-out or something, you know. But in telephone service reliability you were paying 3, 4 or 5 dollars apiece for relays. Vacuum tubes were cheaper than that. It's true they had to have a socket to plug in to and they had to have other things to go with them before a vacuum tube made any sense in any circuit. By itself it did nothing, but after you put a few resistors and capacitors with it and all that, still the cost of a vacuum tube as a component of a circuit and its associated other things was to my mind about comparable with what you'd have to pay for a good relay, not a cheap one.

**TROPP:**

Well in a sense would you say in today's terminology it would make, you saw making me, using vacuum tubes opposed to relays making the cost of multiplication per multiplicand cheaper.

[Recorder off]

**MAUCHLY:**

The cost per operation which you're interested in. Sometimes you're interested in elapsed time for a calculation, the real time as they call it nowadays, and that was obviously of interest to people who were in fire control. I wasn't particularly acquainted with fire control applications. I was really interested in the economics of it, and it was a question of how much could you do for a dollar.

**TROPP:**

Another question that has interested me because I've only heard one person make one statement on it, and that is, after Goldstine encouraged you to go ahead and resubmit essentially the same proposal and it was going to be taken to the group at Aberdeen, the comment was made that it was Veblen who said, "Give them the money." Had you had any contact with Veblen during that particular presentation? Can you remember any reactions, comments, questions emanating from him?

**MAUCHLY:**

The answer is no for a very clear reason. That is that the proposal was a long tedious job which we were doing under very short notice, like two or three days, and we hadn't finished it the night before we were supposed to deliver it. Pres Eckert and I stayed up practically all that night. I don't think we got to bed; and we still hadn't finished it when we were to meet Goldstine to go to Aberdeen. So as a result we still had unfinished portions of this thing when we met Goldstine. Then we had to drive out to Paoli where Professor Bruce Brainerd lived, pick him up, and then drive on down to Aberdeen. All

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the while we were driving to Aberdeen, why Pres and I sat in the back seat trying to figure out what the remaining sections were going to say. When we got there, why we still didn't have, of course, a completed manuscript, we just had a little more outline as to what we were going to write. So we were put in a room which was not just around the corner, but some distance away from the room in which the conference was being held with Simon and Veblen. And we stayed there all day long, not even having lunch. Then when the meeting was over and they invited us in, I probably met Veblen but I don't remember him particularly, and we were told that it looks like everything's good, we've got a go ahead, so I hope you fellows have finished your work here, you know, and we can get this proposal wrapped up. Let's go back and catch the train, because Goldstine was staying in Aberdeen and we weren't driving back with him so we drove back to the Pennsylvania Railroad station and heard a nice conversation about what a good lunch they'd had at the Officers Mess, Officers Club, and there we were still hungry.

**TROPP:**

You went back, then, with Pres Eckert and Brainerd?

**MAUCHLY:**

So we went back to Philadelphia.

**TROPP:**

Do you remember any of the conversation? Did Brainerd describe any of the conversation to you that went on during the day, any of the discussion, any aspects of it that—

**MAUCHLY:**

No, as a matter of fact I can't really recall this at all, and I suspect that what happened was that railroad coaches being what they are, two people sit together and the third party sits somewhere else. So probably Pres Eckert and I sat together and didn't even have a conversation with Brainerd about much.

**TROPP:**

So all you knew really was that what you had done was OK and go ahead?

**MAUCHLY:**

Yes, and, of course, we got a caution from Brainerd somewhere along the line the next week or so that we shouldn't count too much on this, that Lieutenant Goldstine was sort of new to the Army, and young, and that he might be more enthusiastic than he should be

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and that it was not just a lead-pipe cinch, you know, to get one of these contracts, so don't be disappointed if it all falls through.

**TROPP:**

When did you, about how long afterward did you finally get word that it was official?

**MAUCHLY:**

I don't remember exactly, but the thing we do know, of course, is that there was assurance that it would be implemented by contract and that we actually started official, serious work on it before the official word came through, here's a contract. There was some back and forth between the Pentagon and the University in that Professor Brainerd and maybe Eckert had to go down to the Pentagon and visit with Colonel Gillon.

**TROPP:**

Colonel who--I'm sorry.

**MAUCHLY:**

Gillon, G-i-l-l-o-n,

**TROPP:**

Oh yes, I've heard that name.

**MAUCHLY:**

Who was in General Barnes's immediate vicinity and it was up to him at the Pentagon end, you see, to get this contract prepared and signed by General Barnes.

**TROPP:**

I'm going to turn the tape over shortly because I'm going to ask you a question that's going to take you a long time to answer, but at this point in time there are still essentially only two people who can define as being concerned with what is to become ENIAC. That's yourself and Pres Eckert, with the possibility of a third person being Herman Goldstine. What did you and Eckert think about in terms of your initial steps at this point, in terms of what you were going to do next?

**MAUCHLY:**

I see. You're talking about the period of time where we were commencing work.

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### TROPP:

You were commencing work and you were sure there was going to be a contract that was beneficial yet. Now you've got to think in terms of what you're going to do, you've got to put together a team of people, and there are really still only the two of you, as I see it.

### MAUCHLY:

Well, we had a lot to think about, of course, but one of the very obvious things was, where are you going to get the people to do this? In other words, we couldn't do it alone, and so we had to try to gather together a staff of people to do it. The second obvious thing is, we had to have something more concrete than that August '42 proposal for them to work by, and so we had to begin to lay out what was the work that those people would do if we got the people. Those two were like, you can't do anything without both.

So I had already, of course, laid out things which are now in the appendix of the proposal submitted to Aberdeen, which showed the step by step integration of something like trajectory integration. We already had the modular concept, if you like, that we were going to have a certain number of things that we called accumulators. Again, these things are in the appendix of the proposal. We were going to have certain other things which controlled the repetition of various steps in the program. We were going to have something which would be specialized to multiplication. We were going to have something that was specialized to division. We were going to have something that was called a function table, and maybe more than one of them. In part, of course, one of the things before us was to be sure that the number of each of these types of units was sufficient to do the kinds of jobs which Aberdeen would like to have done and for that we were going to confer with Leland Cunningham and some other civilians at Aberdeen to find out what they conceived to be the proper amount of equipment to provide them with the power, capacity they needed for what they anticipated would be the workload of the ENIAC. So in that respect, of course, why we merely had to wait till we had the conferences with the Aberdeen people. But in the meantime we had plenty to do in trying to describe in more detail now what an accumulator is? What's it going to be? What kinds of things go into it regardless of its capacity? The decade counter boils down, of course, to being the building block, you might say, of arithmetic operations in the accumulator.

So immediately Eckert started gathering all of the information he could as to what kinds of electronic counter circuits had been published or were available for us to find out about so that we could try to test them for reliability and either choose one that was most reliable or modify something to get one that was, had the reliability. This took a couple months actually to get through that phase, and during that time, fortunately, Eckert, Brainerd other people around the Moore School that were able to lay their hands on people and recruit them into the staff. I guess about the only person I ever recruited for the Moore School staff had nothing to do with this except before the event. The only person I recruited was Chapline who serviced and maintained the differential analyzer,

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and who was the one who referred Lt. Goldstine to come up and see me, so that I made contact with him. Chapline had nothing to do with the ENIAC as such.

**TROPP:**

Before I get on to what will be a broader question, we're nearing the end of the tape. You mentioned that when you were hired at the Moore School, Professor Travis had gone on active duty and so he wasn't there. Did you have any contact with him at all during the intervening period from the time he left when you had joined and, say, this period in 1943?

**MAUCHLY:**

I think up to that time I can't remember any contact. He did visit the Moore School occasionally and he might have come through and said, "Hello" or something, but then he would have gone out to lunch with Brainerd and, Cornelius Weygandt was the name of the faculty member who was worried about the operation--

**TROPP:**

W-y-g-a-n?

**MAUCHLY:**

W-e-y-g-a-n-d-t,

**TROPP:**

d-t.

**MAUCHLY:**

I believe. And normally known as Corny.

**TROPP:**

I'm going to turn this over so that we don't run out in the middle

**MAUCHLY:**

Sure. Go ahead.

**TROPP:**

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Before we talk about the team that you built up, let me ask the specific question about your own role in the continuity of the conceptual development, from the time of this initial proposal as you began implementing.

**MAUCHLY:**

Well, maybe the first thing to state is that I was never a full-time employee of the project, the ENIAC project. I was a little uncertain, I guess, as to what title or what my role was supposed to be, but Professor Brainerd at various times, I think, defined it as being a consultant and so stated in a footnote of some paper, I think, that was written—

**TROPP:**

The parent takes on a new role.

**MAUCHLY:**

Yes. See, I had not only what you might call a full-time teaching role, I continued teaching the engineering students that were the normal civilians enrolled in the Engineering School, but they also had additional students there who were called Navy something.

**TROPP:**

Navy 12 program? Navy Student Training something.

**TROPP:**

Yes. V-12 perhaps and we had another group of students that were Army student training. So there were various extra classes for these people to be taught, too, so I had what would be considered at least a normal teaching load in the university there.

**TROPP:**

Just to get it on the record, do you have any idea how many courses you taught, or how many contact hours a week you had in the classroom?

**MAUCHLY:**

It wasn't an awful lot, but it was I guess something like two hours a day, most of the days of the week. Call it something like ten hours.

**TROPP:**

A normal teaching load varies from six to twelve by current standards.



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### MAUCHLY:

Yeah. I know that at some period during this year I was a kind of an assistant to Dr. Pender, in that he and I together gave a course for the seniors on network calculations and things of that sort. Dr. Pender had gotten sort of enthusiastic and in love with matrix methods of calculation and I, too, was somewhat enthusiastic. I was, for instance, very much interested in some papers published by Gabriel Krone somewhere in that era, and how you could tear networks apart and analyze them that way. Also I had gotten quite a lot of kick out of what is called the symmetric components method of analyzing three-phase power networks, which I had not known a thing about before I came to the Moore School. So there were a lot of things where Dr. Pender and I had common interests, you might say, in this network analysis program. So this course given to the seniors was nominally given by him and he wrote all the notes and he kept rewriting and revising his notes, not only from year to year but it seemed like from day to day sometimes. The mimeograph notes, the mimeograph machine was a great instrument in those days--nothing like the Xerox of today, but it enabled any teacher to write his own textbook and give it out to his class in a way that was impossible in the earlier days, you know. So there were just mountains of mimeographed notes emanating from Dean Pender on this course in network analysis. It was my function to go in there and teach this by his notes. Occasionally he would come in and lecture, and most of the time I was in charge of that class.

### TROPP:

Well, I guess that—

### MAUCHLY:

So I had a full teaching load, I'd say, and when this project first was implemented, the ENIAC project, I still had not finished the Signal Corps project, you see. So I had to finish up on computing the--some of these patterns, radiation patterns, for the Signal Corps project during the time that the ENIAC project was getting under way. But at the same time I had this job which I just outlined a moment ago, trying to specify what it is we're trying to build and lay it out in enough detail so that we could begin to educate the group of people that we were collecting. Where those people were collected from is still somewhat a mystery to me; I know that some of them were former students at the Moore School and, of course, the faculty kept in touch with some of those people and they came back, and they were available and known. And so we got Bob Shaw back from somewhere; I don't know where. We got Jack Davis. I don't know where he'd been before, but suddenly he was available to us, you see.

### TROPP:

Of course Burks was already there.

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**MAUCHLY:**

Then there was another man, an RCA man who knew a lot about RCA test equipment and he too was enlisted and put on this thing. I can't remember his name at the moment but he was one of the regular staff.

**TROPP:**

Was Sharpless there, too?

**MAUCHLY:**

Sharpless, of course, was one of the people who had worked on this Signal Corps project, on the experimental end of it.

**TROPP:**

The same one that you were doing the theoretical?

**MAUCHLY:**

Yes.

**TROPP:**

Portion of? Who recruited Harry Huskey?

**MAUCHLY:**

Well, that was later, as I say. I don't know how he happened in on all this, but since part of his work later was with Dr. Rademacher on rounding off errors and things, why I wasn't sure but what he'd come in through the Math Department, but this is not really something I know about, where he came from, how he got there.

**TROPP:**

You were getting put together this team then.

**MAUCHLY:**

This is certainly a question which Harry Huskey would know better than I.

**TROPP:**

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I've asked Harry Huskey about; it was really unfair to ask you.

**MAUCHLY:**

Yes.

**TROPP:**

What role did Jack Davis play? Let's just take each of these people and what roles they--

**MAUCHLY:**

Jack Davis's first job was to put together--actually assemble, wire up some of these circuits for--I mean counters, and test them under conditions which would show how sensitive they were to variations of voltage, and to what frequency of pulse repetition rate--what range of pulse repetition rate rather--they would give adequate response to. We also had the problem which we recognized early in the game, that you'd better standardize your pulses, which was a thing which lots of people using counters did not do and was the cause of a lot of grief to people. That if you didn't have something that shaped or standardized your pulses preceding the counter then it is very hard to predict how the counter would react to a single pulse or to fast repetition rates where they trip over each other, you might say. But if you start out with a pretty good pulse shaping device and then can vary the repetition rate and likewise vary the power supply voltage levels and anything else that might seem pertinent--I don't think we had anything that was particularly temperature dependent--so you didn't have to, we weren't testing this under environmental conditions like -40 or +180 Fahrenheit or something--they worked in a normal environment as you would expect in a laboratory, but that was good enough. But you certainly have to be sure that they weren't excessively voltage sensitive. We also were interested, of course, in how sensitive they were to the aging or changing of the parameters in the circuit. Davis was directly involved in that kind of test work. As I say, we had a few other people helping at that time because whoever we acquired, we didn't want them just sitting around. We put them on to whatever was the most useful immediate thing to do, and getting those decade units were settled so we knew what we were going to build was the most immediate thing. Next question.

**TROPP:**

I guess I want to stay on the roles of the main team.

**MAUCHLY:**

I see.

**TROPP:**

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Continue with people like Shaw, Burks, Sharpless, Huskey, Betty Holberton, Kaye(?), Goldstine, Von Neumann ultimately.

**MAUCHLY:**

I see. You're going away out yonder now.

**TROPP:**

We'll get out yonder. I think if we just stay in the time frame and, you know, generally build up the team in terms of the roles, I think that's the general trend. I guess I'd like to interject one question that has bothered me. At what point did you and your colleagues realize the magnitude, the size of this thing that you were beginning to build? At what point did that realization occur?

**MAUCHLY:**

Well, I don't know which interpretation of magnitude you mean. Do you mean how many tons? How big a room? Or do you mean in what it was eventually going to be able to do, or what?

**TROPP:**

I guess at least two of those in terms of, you know, the size. You know, 18,000 plus tubes still astounds me, the absolute size of it.

**MAUCHLY:**

As I said before, how much of this equipment was going to go into ENIAC for Aberdeen depended in part on what the Aberdeen people said they thought they needed. In other words, we could lay out the characteristics of what an accumulator is going to be like but we were leaving to them the question of, is it going to work to 8 decimal places or 10 decimal places or 12 decimal places, or what.

**TROPP:**

That's one of the questions that I wanted to get to, was the discussions with them.

**MAUCHLY:**

That has some bearing, you see, on the size. Twelve decimal places is 50% more than 8, and we were building it as a somewhat parallel machine. That means 50% more equipment. And so if you then say, "how many multipliers do you want to have, is one enough or not?" If they'd said, "we've got to have two," we probably would have built two. But with our statement as to what we thought a multiplier could do, how fast it

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might produce a multiplication, and with joint understanding, you might say, as to how frequent is multiplication going to be compared to addition and things of that sort, we never had any argument over that as far as I know. Everybody seemed to agree that with this machine we'd only need one so-called high-speed multiplier; and clearly, the way we were building the accumulators, we could do small trivial multiplications with accumulators. We could multiply by 23 by adding twice in the tenth place and 3 times in the nth place in any accumulator. So you could do small constant multiplications without having to depend upon a shifter or tie up the main high-speed multiplier. So with this entire thing, why, as I say, we didn't really have any big problems. Nobody seemed to be concerned about, do we need two high-speed multipliers. Aberdeen was satisfied; we were satisfied. One's enough.

There may have been some discussion as to whether we needed a divider or not. If we had known about Aiken and consulted him I don't know what he would have said either. He might have said no. If we'd known about some other people I think their advice would have been no that you can essentially get the effect of division with subroutines, and why not use them. But we thought, since we were trying to build something fairly high-speed that we didn't want to have one of its weakest links spoil the game, so to speak. That in case you had a fair amount of division, you don't want to have that holding up the works. So let's put in a divider. If you had gone that far it didn't seem to us that it would take much more equipment to make that divider equipment also do square roots, so we planned it as a divider-square rooter combination. The biggest question, I guess, was how many function tables we needed. In that there was amplification. In other words, I think we originally were thinking about two, but from our consultations with Aberdeen it got stretched to three and something like that happened to the number of accumulators also. In other words, we originally, I think, proposed some smaller number of accumulators and we wound up with 20. At any rate, during the extensions of the contract while we were building these things the specifications of how many of these things did go up a bit, and I think it was in those two categories, an extra function table and a couple extra accumulators. Well, that, of course, made it take a bigger room, you might say.

### **TROPP:**

Let's now go back to the decision to even have function tables and how you arrived at it and who was involved in that decision. That turns out to be a fairly critical part of the machine—

### **MAUCHLY:**

I think the primary thing there was that Aberdeen was used to having things like function tables. That is to say, they were not used to trying to get an analytical expression to represent an empirical curve which they derived from the measurement of drag functions on the firing range, on the test range. So this could also, of course, enter into exterior ballistics or other sorts of things. There may be other empirical curves which had to do with burning rates or what else. They had all kinds of problems. If you're trying to do

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work which would help design the ogive curves or something for the front of an artillery shell or something where they would normally use a wind tunnel, somewhere out of this you may be faced again with empirical curves. Now you've either got to represent the empirical curve analytically before you can use it in a high speed calculating machine, or you've got to have the ability to go and get the values of an empirically tabulated function and have it tabulated at close enough intervals so that interpolation is tolerably successful, you might say.

**TROPP:**

I was thinking about that, you know when you were talking about the decision to put in division. It would seem if you are only faced with the idea of square root

**MAUCHLY:**

Yes. That would have more naturally gone into your function tables.

**MAUCHLY:**

At that time, at the time that we were doing this, we didn't even stop to figure out, I think, how long it would take the ENIAC to compute an elementary function like a sine or a cosine, but sines and cosines were the very essence of a lot of artillery calculations. You had to work in two dimensions and calculate from angles to the components of the vector and so on. And so you were going to continue to need sines and cosines in some form. Where were you going to get them from? As we visualized it; why you might well then want to have tables of sines and cosines to feed in, not because you couldn't calculate them, but because it would be more efficient actually to set them up once and for all. The same old sines and cosines would never change, we'll say.

**TROPP:**

Just like you and I would look them up

**MAUCHLY:**

Yes. Rather than use an expansion to calculate them each time.

**MAUCHLY:**

So that was the primary reason for having function tables, because there were these drag functions and other things which they were used to dealing with as empirical functions and they were put into their calculations in that way from a tabulated function in what they did before. For instance, on the differential analyzer where they were computing firing tables, one of the adjuncts they had put on to the differential analyzer was what we called a curve follower which was designed to--a photocell--so that you drew a heavy

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black India ink mark as a curve on a piece of drawing paper, put it on the table of this curve follower, then as the ordinate position of the photocell was changed by a screw responding to the analog shafts of the analyzer, that the curve follower would tell you--was the abscissa position rather--the curve follower would tell you the ordinate position on the readout. That was a very important part of the analyzer for their work. So it was sort of natural, you might say, nobody really questioned much whether or not we should have a function table; the question really was, how fine-grained should the function table be and how many of them do you want?

**TROPP:**

Right. Really, how few of them can you get away with,

**MAUCHLY:**

Yes. Which may be the reason square rooting ended up in the machine, the hardware itself rather than the functions.

**MAUCHLY:**

Well, square rooting was so easy.

**TROPP:**

Yeah. Once you were into division,

**MAUCHLY:**

That's right. Square rooting is easy. The decision to do division was made.

**TROPP:**

Let me ask a very simplistic question. I'm kind of getting off the track in one sense from where we started, but in terms of a highly oversimplified look, I was thinking of characterizing, say, the first machine that Aiken built at Harvard. It struck me, again from a simplistic view, that it did mechanically solve problems much in the way that I would if I were using pencil and paper. I'm trying to draw a similar analogy in terms of ENIAC, and it strikes me that if you look at the over-all architecture of the machine, its closest relations are these calculating devices we've been looking at, that go back to the Marchant days and that you see in catalogs today. Is that a fair oversimplification?

**MAUCHLY:**

Are you trying to say that these are different in this respect? Are you saying that one is doing it differently from the other?

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**TROPP:**

They seem to be slightly different, but I'm not sure. I mean, one seems to be a mechanization of me as a human being and the other is an extension of the mechanization.

**MAUCHLY:**

Well, I can't quite see your point yet because you compared one of these methods to what you'd do on a calculator and the other as you do—

**TROPP:**

Right, right. Okay, what I'm saying—

**MAUCHLY:**

In my concept what you do with pencil and paper and what you do on a calculator are identical.

**TROPP:**

I guess, as I say, I was oversimplifying. What I'm trying to do I guess is sort of draw a chain from the human being to the mechanization of the human being to another one; and the other one out here is a version of the mechanization rather than the human being. It's an extension of the mechanical.

**MAUCHLY:**

I would say offhand that in many ways Aiken's MARK I was closer to the punch card machines of the day than was the ENIAC, not now because of the fact that the ENIAC was electronic. I'm talking about the system organization.

**TROPP:**

Right. That's what I'm thinking of too.

**MAUCHLY:**

And the thing is that the punch card machines of the day did one task at a time just as you would do with pencil and paper. They serially more or less (maybe there was some parallel function, but serially more or less) the Aiken MARK I addressed itself to a multiplication or an addition or division or a multiplication and so on—



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**TROPP:**

Or it stopped to do interpolation of something you had looked up, or—

**MAUCHLY:**

Or it went to some special gear which could produce a logarithmic or a sine or something for it. And so it did step by step through those things, and it was instructed to do that by instruction tape; so that each step forward from the punch card equipment was really, much of the advance came about because in going from one of these steps to the next, it did not have to punch out a card and then have a human being carry the card to a different machine which input the same numbers in order to do a multiplication and then, say, take the results of that and bring it back to another machine (a counting machine or what) which did an addition, and so on. You didn't have to keep carrying the data in punch card form from one machine to another, but rather remained resident as counters in one machine and systematically your taped program told it what to do step by step.

Now, here you see comes the distinction between the MARK I and the ENIAC in that that's all you could do with the MARK I was this step by step stuff. And if you wanted to do the same thing a second time in MARK I you had to have the same extended detailed set of instructions on the tape and feed it again. You couldn't say, do what you last did again. In fact, Aiken subsequently designed an exterior machine which he (I forgot what he called it, but it's sort of a program tape manufacturing machine) which could punch the instruction tape from MARK I and thereby put in the instructions again to do once more what you'd already done once. He could then repeat the same process over and over again by punching more of the program tape, but there was nothing internal in this machine which would do these things, which essentially would be classified as subroutines in our modern terminology. In the ENIAC we had the ability, almost as much as you might wish--there was, of course, all of these there was limited capacity, but we provided an extraordinary amount of capacity in various ways including something called a master programmer, to repeat sequences of instructions so that once they were wired up or programmed in the machine you could merely say, go back and do this thing again with the new data. Over and over again you could employ that technique, you could use many subroutines.

**TROPP:**

The reason I used oversimplification is I wanted to get your

**MAUCHLY:**

Yes. Simplified approach on this tape because in future years lots of different people

**MAUCHLY:**

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Yes. Of varied background might be listening to these.

**MAUCHLY:**

Yes. Well, I was trying to make it very clear.

**TROPP:**

I think you have done that very well.

**MAUCHLY:**

Yes. That the Aiken machine was more than just a collection of punch card machines.

**TROPP:**

Oh yes.

**MAUCHLY:**

It was electrical and it was integrated, you might say, so that you could do these steps one after the other without input-output all the time, but it wasn't what many people imagined it to be, a completely flexible thing that could call upon subroutines. It was exactly not that to start with. Any subroutine facility they had was added some years later by putting extra tape readers on and adding loops of tape which could perform subroutines. Even there you had only two or three of these available in the machine, where you could have had many subroutine loops in the ENIAC.

**TROPP:**

Let me get back--I keep getting away from the main thrust that we started on this side, but these questions keep occurring to me. What contact did you have in this period of time when you're beginning to work on what is to become ENIAC, with Aiken at Harvard or with Stibitz at Bell Labs or with any other groups who were concerned with the same computational problems that had been concerning you?

**MAUCHLY:**

Well, the simple answer is none. In the case of Aiken, we didn't know he existed until he was about finished. In the case of Stibitz, it was obvious I did know he existed.

**TROPP:**

Yeah. I did know about—

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### MAUCHLY:

I knew he was a Bell Labs man with his relay computer, but I also knew somewhere as we were doing the ENIAC, of course, that the Bell Labs people had plans for bigger computers and some of their intermediate sized relay computers--I think one of them was at Naval Research Laboratory. I don't know when it arrived there. I never saw the thing, I don't know how much it was used or how well it was used, but Naval Research Laboratory, I think, did buy a relay computer from Bell Labs. There were several such, I think, intermediate type relay computers. We were also aware somewhere along the line from the Aberdeen ballistics people that they had gotten IBM to agree to deliver to them some kind of a big multiplying machine and when it would be delivered--I don't know when it was delivered, but at whatever time it be would delivered, why it was going to much facilitate the process of doing some of their card computations by possibly speeding up multiplications. I never knew exactly the specifications for that, but I do know that other people like Dr. Goldstine credit the staff at Aberdeen with soliciting and getting IBM and Watson interested in their program and interested enough to build something which as far as IBM was concerned, was a special purpose device that never had a market for. Just something special for them.

Later, of course, we knew that the Bell Labs relay computers were sufficiently planned out so that the Aberdeen people could put in an order for a relay computer to be delivered to Aberdeen and, indeed, they made that contract with the idea that they were sure that such a relay computer could be built because Bell Labs had built others, whereas no one was sure whether we could succeed in what we were doing, and so this was a kind of insurance for Aberdeen. At least they would have this relay computer which was programmed by punch paper tape whether ours worked or not.

### TROPP:

At what point in time do you remember the outside world in terms of people interested in computation (that's a limited outside world, I realize) becoming aware of what you were doing and where you were going?

### MAUCHLY:

First of all, of course, our work from the beginning was classified. The classification wasn't anywhere near Top Secret; it was Confidential, I think. As things go, Confidential was severe enough to impress us and we were not about to talk about this project or tell anybody what we were doing ourselves. We were somewhat surprised, you might say, at the apparent freedom with which the people above us, Goldstine and others, what they could say about the project to other people, you know. Our biggest surprise, of course, came later when Von Neumann was introduced to it and it seemed as if classification didn't bother him a bit. All he had to do was to invent a ew nomenclature and he was free of all classification.

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**TROPP:**

In this early period after the proposal had been given a green light, we discussed your role to some extent and understand what Pres Eckert's role was, and you mentioned Jack Davis's. What was Goldstine's role during the early period, after the '43 acceptance?

**MAUCHLY:**

Well, his role did change. It was my belief that in the, at the time we first met him that his chief responsibility was what went on in Philadelphia, but that included all these other things computationally and not just the ENIAC project. It included something which, I think, was referred to as Project H, on which my first wife worked for a while, which was the reduction of data from some kind of 35 millimeter films which were taken of shots on the firing range, things of that sort, and they were trying to get triangulations on those. I never knew exactly what it was all about, but all of these different computing projects going on at the University of Pennsylvania for Ballistics Research Laboratory were under the direction and cognizance of Lieutenant Goldstine. At some time, I don't remember when, he was stationed fulltime in Philadelphia so he could better oversee these things. It was clear that he was the one officer from Aberdeen Proving Grounds and Ballistics Research Laboratory, who was the man to work with us and see that we were properly cared for, you might say, as to our needs and to report back to Aberdeen, of course, what our progress was. So it's my understanding that he made periodic progress reports to Aberdeen as to what we were doing, although I never saw one of those progress reports.

**TROPP:**

I guess my question either should have been or will be then, at this point did he play any roles in the conceptual development of the machine?

**MAUCHLY:**

None that I know of. As I say, as to the quality and amounts and things, why it was people like Cunningham, who was an astronomer from the West Coast, but stationed in Aberdeen at that time. Cunningham and possibly Ted Sterne is it? An officer named Major Sterne.

**TROPP:**

S-t-a-r-n-e?

**MAUCHLY:**

S-t-e-r-n-e, I think it is.

**TROPP:**

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e-r-n-e.

### MAUCHLY:

Yes, who was mathematically trained. I never knew exactly his background, but people like that who mainly came from fields such as astronomy. Dick Lehmer came from the theory of numbers. Those people were interested, of course, in what our machine was going to be able to do and whether it was satisfactory for their purposes and things of that sort. That was the major influence, you might say, of any Aberdeen personnel on our work. Goldstine, who became Captain Goldstine somewhere in there, was kept pretty busy, I thought, by seeing that administratively we weren't tied up and that we had access to information. In fact, from our point of view, we got access to information we didn't even ask for. We learned of some work done at National Cash Register, and we learned of some work that had been done other places. Perry Crawford's thesis, apparently, of which we got a—

### TROPP:

Is that Perry Crawford's thesis about 1942?

### MAUCHLY:

Yes. We got a photostat of things like that without ever asking for it. We didn't know these things existed, but they were put in front of us. And presumably it was through the official channels of Army Ordnance and work through the Pentagon and having contact with other Government projects that these things were found out as of possible benefit to us, and so Goldstine requested them and we had them, as I say, put in front of us. We looked at them and didn't see much relationship in general to what we were doing. The work at National Cash on thyatron ring counters and things like that

### TROPP:

Right. This was Joe Desch's--

### MAUCHLY:

He gave us no real indication as to what the reliability of those counters were. You could make some estimates about their speed and so on, you know, but as far as we were concerned, once we'd gotten on stream, so to speak, none of this outside information coming from RCA or anywhere else made much difference. The counters that we used in the decade units of the ENIAC and in some of the programming units too, were in a sense modification of something, a counter that had been done by Grosdorf, I guess it was, at RCA.

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**TROPP:**

What was that name again, I'm sorry?

**MAUCHLY:**

G-r-o-s-d-o-r-f; maybe a double s, I don't know. At any rate, early in the history of the project, as soon as it was under contract, I guess, why Goldstine said, "Well, we ought to visit the RCA laboratories and see what they're doing." I think we made at least two visits to the RCA laboratories where Goldstine or somebody drove up there in his automobile. And we went through the clearance requirements and everything and we were introduced to Jan Rajchman and Grosdorf and a few other people working in those laboratories. And we found out some interesting information about, for instance, a big vacuum tube which was a large bell jar with a lot of pieces stuck in it experimentally, which was to become a grand belfon(?) tube. This thing would do binary multiplication on something like say a 6-digit number by a 6-digit number, and give you out at least a 6-digit product, all very fast with secondary emission characteristics of electrodes that he had in there. A grand concept, all-in-one vacuum tube, but entirely foreign to our way of approaching this because what we wanted to do was to use the most reliable components we knew how, and you can't know anything about the reliability of something unless it's an already manufactured component which is manufactured in quantity, so that you can test these things and find out how well it will hold up and last and change.

**TROPP:**

That leads me to another question I hadn't thought of before, and that is that essentially you and Eckert were primarily concerned, for whatever reasons, with primarily off-the-shelf, then, technology.

**MAUCHLY:**

Off-the-shelf from the point of view of components.

**TROPP:**

Right.

**MAUCHLY:**

Almost off-the-shelf from the point of view of things like counters. We would have gladly used an off-the-shelf counter, so to speak, if we could have taken one and found that it was reliable. So what happened was that we got a book on counters, Lewis or somebody from England. We built up one of his scale of five counters or something and found that it wasn't too reliable, and we could see reasons why it wasn't. To a person like Eckert it was pretty obvious that some of the things that people did, they shouldn't ever

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have done. You know, it didn't make sense. Just like Von Neumann could do calculations in his head, why Eckert could immediately arrive at the fact--"well, there is an obvious defect in this. Why would anybody be so stupid as to do it this way? They should have done it this way," you see. So when it came to these counters that we were testing which I believe were the Grosdorf counters from RCA, why they were voltage sensitive. If you varied the supply voltage by maybe 2 volts out of 200, the damn things might not work. Well, this we couldn't tolerate! We weren't about to put in voltage regulators which kept voltages within 1%, because if you had that wouldn't have solved the problem yet, because how do you know that the next counter you build by the same plan isn't going to require the voltage to be 1% different from the last one you put in? You've got to have something that works over a wide range. So what we did was to stabilize our counters by a feedback arrangement which apparently no one else had thought of; but which was plain and simple and it worked to the point where the same counter, without varying a thing in the circuit, would be stable as the voltage changed by a factor of 2. If the tube characteristics changed by a factor of 2 it still worked. If the resistors changed by a factor of what--I don't know now, but say 50%--which is rather unlikely for a resistor to change when you age them, because we weren't, again, using wire round resistors which were stabled to a tenth per cent. The shop people made such resistors, you know, for meters. If you tried putting those things in, why you'd wait forever to get them because they were of low manufacture and they cost like \$5 a resistor. We wanted to use the 2 UNK, 300 resistors which are carbon gunk made by International Resistance Company or Bradley, or something like that, you see. You get down to saying I want cheap components and I'm going to let them vary over a certain tolerance span and I don't have to worry about a thing as long as they don't vary outside of that, why you have a chance of success in using them, and yet not run your costs out of line.

**TROPP:**

Let's get back then to my original question and the role that various people on the main team that you built up played,

**MAUCHLY:**

Yes.

**TROPP:**

People like Shaw and Burks, Sharpless and Huskey,

**MAUCHLY:**

Okay, let's do that.

**TROPP:**



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and so on. Even though the time at which they entered—

### MAUCHLY:

Yes. Well, I mentioned Jack Davis as working on the counters. I can't say exactly when these other people were available, but Sharpless was around there and whenever his contract work on the Signal Corps project (he was on the experimental side as I remember)--whenever his contract work made him available he was put on. And we outlined various things which went beyond just the accumulators then; such as, we have to design this multiplier. It was a one of a kind thing, but it's got to be done, again, so it works reliably. Burks, of course, was very available. Again, he had been on the Signal Corps project, and he was one person that we knew could very quickly absorb the logical requirements, you might say, the mathematical requirements of it. And so he was a very able aid to Eckert and myself in getting the main features planned out, you might say. I can't remember exactly when he became available, but this staff grew adequately fast so we kept going without much mismatch or gaps.

Oh yes, one of the fellows that I said was in RCA made measurements and standards and tests and things, a fellow named Chedacher. He worked with Davis, I think, on some of these early tests. He also designed some of the test equipment to test the decade units and things of that sort. You can't test something unless you have something to test it with, which has got to be a darn good thing too. Then Shaw, when he was acquired, brought along a pal of his named Gale. Gale was almost always an assistant to Shaw; the two of them always worked together, like the buddy system, you might say. This was somewhat desirable in that case because Shaw had some serious physical impediments that--as an albino his sight was not very good and he also didn't have full coordination of his extremities and sometimes had to use a cane to get around and things of that sort. Well, Gale was a very adequate and sympathetic assistant for him, intelligent assistant, and helped him read the diagrams and write down the values of things and so on. Shaw himself would have to get within about 3 inches of a diagram to see what he was looking at and that meant he didn't have a very good field of view, you might say.

So those people, I think, were assigned mainly to the design and the circuits of the function tables. That means the electronic part of the function tables. The part of the function tables which we called the portable part which is switches and resistors, that kind of thing built up on a caster or frame. There was nothing difficult about the design of that except to arrive at what would be suitable value of the resistors, you know, after you had done the specifications of how many switches there should be and that sort of thing. And that, of course, dated back essentially to Aberdeen's agreement with us as to how many numbers and how many digits per number should we put into those things for adequate capacity and empirical functions. So we always sort of agreed that physically a portable function table could be this high and it could have this many switches on it so that you could easily take care of a hundred arguments. But, if you want interpolation then it would be a desirable thing to put 104 arguments on it so you would get 2 below the so-called zero and 2 above the hundred, so that you could be second or third or fourth



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difference interpolation, functions, things of that sort. There were a lot of other peculiarities about the function table, but probably the most interesting one, of course, was that when you consider that you've got a function table which can feed out any arbitrary preset set of signals in response to any one of a hundred or so inputs, you can also use it for programming. So we equipped it with the proper ways of feeding what we called program pulses in, so that you could feed out program pulses to initiate various operations in the rest of the computer, instead of feeding out data which could be fed into as operands in the rest of the machine. So here three function tables, any one of which or all three of which in combination could be used for programming purposes if you so desired. (End of Side I). I don't know if they were ever used that way much, but at least the germ of the idea was there.

**TROPP:**

This was the area that Clippinger was talking about at the ACM meeting.

**MAUCHLY:**

Yes. That's right. And so the equipment was there, the concepts, the ideas and everything were there. It was almost the one like, if you're going to have a hardware divider, why not have a square rooter, you see. If you're going to have a function table, why not let it be able to control programs as well as control data. It was a very minor change just to what pulses you fed into it really and how you integrated the program circuits to make use of them. It didn't cost any more to make it do that, so why not do it? Contrary to the present day concepts of the ENIAC that have been....., I regard the ENIAC as having stored part of its program. (LAST SENTENCE IS UNCLEAR)

**TROPP:**

There has been a good deal said; much of it needs correcting

**MAUCHLY:**

Yes. And I think this, for example: the ENIAC has been characterized throughout a good portion of the literature as being a special purpose machine in the sense that it was built primarily to do ballistics tables.

**MAUCHLY:**

Well, that, of course, is absolutely wrong. Let me emphasize very strongly--that was wrong from the very beginning, because even though the people in 1943 when this project started did not have the view that Deterick had expressed to me the year before, "Oh well, it'll take a year and the war will be over then," nevertheless in 1943 when we started this, Cunningham--everybody--said, "Whatever you build, we want it to be as general as possible because we want it to be useful after the war. This is not to be a

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machine just to do artillery firing tables. This is to work for exterior ballistics. This is to work for any problem that we can conceivably put on it, except for the limitation, of course, that we're not going to put so much equipment in there that we never know how to use it."

**TROPP:**

That was the point I was—

**MAUCHLY:**

"It will have to fit within the confines of our budget and our room."

**TROPP:**

That was the point I was going to make

**MAUCHLY:**

Yes.

**TROPP:**

That when you bring people like Cunningham and Lehmer in, whose interests

**MAUCHLY:**

Yes.

**TROPP:**

Ranged far beyond

**MAUCHLY:**

Yes.

**TROPP:**

The tabulation of ballistics tables, I was going to get to the point that you just mentioned,

**MAUCHLY:**

Yes.

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**TROPP:**

The impact that they had

**MAUCHLY:**

Yes.

**TROPP:**

On the machine; because Lehmer had more than a decade earlier, already built

**MAUCHLY:**

Yes.

**TROPP:**

The prime numbers in

**MAUCHLY:**

Right.

**TROPP:**

And his interest in machine assisted computation was already well known.

**MAUCHLY:**

Yes.

**TROPP:**

What I was trying to emphasize here was the fact that the Aberdeen people--? for that, but I do know that other people like Dr. Goldstine credit the staff at Aberdeen with soliciting and getting IBM and Watson interested in their program and interested enough to build something which as far as IBM was concerned, was a special purpose device they never had a market for. Just something special for them.

**TROPP:**

Of whom Lehmer and Cunningham were two.

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**MAUCHLY:**

Yes. Those people were very strong on this point of view and so even if we had not been why they would have pushed it that way. But in spite, whether they had or not, I was advocating that anyway, you see. The only reason that I put the firing table example into the August '42 memorandum was because this was a concrete example of what they were interested in.

**TROPP:**

That's politically important.

**MAUCHLY:**

Yes, it's a selling point.

**TROPP:**

I'd like to make, I think, an important kind of summary for what you've been saying from a limited point of view

**MAUCHLY:**

Yes. And that is that in 1942 when you submitted the proposal, it appears to me that you were told, "the war's going to end pretty soon. We won't need anything like this. Why bother?" In 1943 you were told, "Well, the war's going to end pretty soon. We want to make sure we have something we can use afterwards, that in that year essentially a shift had occurred in thinking. Now maybe I... it that way.

**MAUCHLY:**

That's partly the case, but another thing is, of course, regardless of when the war was going to end, I guess everybody had confidence that we would be the victorious ones.

**TROPP:**

In '42 most of our planning was short term. By '43 we knew it was going to last at least two more years, possibly four.

**MAUCHLY:**

Yes. We didn't think we were building something for the enemy to come in and clobber, but that sooner or later we were going to be victorious, whether we were right or wrong, that's the way we felt.

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**TROPP:**

I guess what I meant to say was that your '42 proposal appeared to have no impact on the war effort, which was one of the reasons for its rejection; that no one could see it being completed in time to help in the war.

**MAUCHLY:**

Yes.

**TROPP:**

The '43 proposal, then, you're saying, was to as much postwar

**MAUCHLY:**

Yes.

**TROPP:**

As it was wartime problems. And so the characterization then of ENIAC - as you've often said yourself, and I've heard it, but and as I say, I think it needs to get into the literature, as a special purpose machine, is an erroneous one that needs to be corrected.

**MAUCHLY:**

I didn't realize that anybody had ever even suggested that it was a special purpose machine.

**TROPP:**

Well, I'm sure you've heard--

**MAUCHLY:**

I've heard many people argue that it was almost useless because the input-output was so limited, the capacity for storage was so limited, all kinds of limitations so, gee, the thing really wasn't much, you know. Well, you could say that of a national accounting machine or whatever counters you used for difference table generations, and so forth. It's a special purpose machine, but you made a hell of a lot of good use of it, you know. So somewhat, you might say, special purpose or not in the eye of the beholder or the user. But the point I was trying to make a moment ago was wider than that, you see, broader. That is, not only was it a general purpose machine within the limitations of its capacity, but it was a stored program machine.

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Now the fact that you could not completely store the entire program in it doesn't prevent it from being a stored program machine. It's just that it didn't store everything. Everything that we thought was important to store, because it was electronically, it was necessary for it to be electronically fast in supplying and controlling the program, was electronic. The program counters in every accumulator were electronic. How many times an accumulator was to add something that was being received at its gate, was controlled by an electric counter. Now the fact that you had to turn a switch manually to say what that counter was to count up to is somewhat deceiving, because if you like to, if you like, say, play tricks, what you can do is to preset the counter electronically by some other part of the program. So even if it only counts up to 8, we'll say, you can have it effectively do this 3 times or 5 times or something like that. But that's a sort of a trivia. What's more important is that the iteration steps, all the things which govern these subroutines I was talking about, were all electronic. That's what the master programmer was all about, and yet I've seen descriptions of the ENIAC which totally ignored the fact that there was a master programmer in it.

One of the little gripes I might as well put on the tape right now.

**TROPP:**

Oh, I think this is great.

**MAUCHLY:**

One of the little gripes I might as well put on the tape right now is that my own brother-in-law, Dr. Joshua Gray, at the Moore School, who himself should have been intimately acquainted with this ENIAC, because he and one other engineer were given the job of moving it to Aberdeen and getting it back into working order after it was moved and had to know the blueprints, had to know every element that was in that thing--later wrote a book on digital electronics or something, how to design computer circuits, and put in it a history which includes an account of what the ENIAC's all about. He came to me and said, "Would you read this book and see what you think?" I said, "I'm not competent to say anything about how good this chapter on solid state logic and how good this chapter is," something like that, but I said, "but this history you've got, you described the ENIAC and you don't even mention the master programmer. You say that Von Neumann had the stored program; we have stored program in this master programmer." Did he change the book? No. He thought that, he didn't want to lean over backwards and yield to the bias of being my brother-in-law. He printed the book just the way that would still say that the ENIAC was a very limited machine and forget all about the master programmer, you know.

The people who don't know what computation is about and only look at the electronics of the thing somehow missed the point, that the iteration control is the heart of the problem control.

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**TROPP:**

Of course Aiken saw this in his description of the, in his 1937 paper when he described what a calculating machine should be able to do, the one that, long before the MARK I was built. But he saw this iterative procedure as being the really important characteristic--

**MAUCHLY:**

Why didn't he do something about the MARK I? That is the big question, see. I didn't know anything about his 1938 paper.

**TROPP:**

'37. In that '37 paper he makes a big point of the key role that the iterative procedure plays in the kinds of problems in a sequential...

**MAUCHLY:**

You can see how ignorant I am. I don't even today know--what did he publish in '37 that says this?

**TROPP:**

It wasn't published.

**MAUCHLY:**

Oh, it wasn't published.

**TROPP:**

This was the paper that he presented, much as you presented your '42 memoranda.

**MAUCHLY:**

Yes.

**TROPP:**

to state what he felt ought to be done in the way of mechanically assisted computation, and who he felt were the best people to do it, in this case namely IBM, which also led to the contract between Harvard and IBM, and the building of the MARK I. So it was as if was as if this paper played much the same role as your--

**MAUCHLY:**

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Well, it may be that somewhere along the line he had to compromise, you know.

**TROPP:**

I'll have to send you a copy of that paper because I think you'll enjoy reading it.

**MAUCHLY:**

I'd enjoy reading it.

**TROPP:**

He's got many examples in there--in fact my memory tells me that almost a third of the paper consists of examples of the iterative procedure, which indicates how important he felt it was,

**MAUCHLY:**

Yes.

**TROPP:**

in the whole process of sequential computation. It's hard for me to imagine what went on from that point on because--perhaps he felt that merely the ability to repeat these instructions, paper tape instructions,

**TROPP:**

Would give him this iterative capacity would be adequate for this

**TROPP:**

Yeah. I'm sure.

**MAUCHLY:**

But it isn't adequate as far as I can see. Because for one thing, for instance, if you want to iterate N times, but you don't know what N is until you're doing the calculation, you've got to have a control which is responsive to N, responsive to the calculation so as to produce what N has to be, you know.

**TROPP:**



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Except--this is why I made the allusion to a person doing this. A person doing an iterative procedure--

**MAUCHLY:**

Knows when to stop.

**TROPP:**

Right, he knows where to stop.

**MAUCHLY:**

Or should know when to stop.

**TROPP:**

Right, and this is the same thing then when you're punching in this large roll of tape. You can do essentially the same thing. You know when to stop the iterative procedure as you repeat these instructions over and over again.

**MAUCHLY:**

Well, my point is, of course, that you really don't know, because what I was postulating here was that how much you should iterate depends on the result of calculation, and you don't know that beforehand. So the only thing you can possibly do, it seems to me if you have a machine programmed like the Aiken Mark I was originally, without any subroutine loops; the only thing you can do is to have it skip past regions of tape where you provided for repeating this thing and you say, now I don't want it so I'll read that part of the tape fast, and skip over it. You might do that.

**TROPP:**

They eventually made some provision for that. I've forgotten what exactly, but it's clear that he felt exactly the same way that you feel; having done as both of you had, a great deal of manual computation, you both arrived at the, you know...independently; as anybody would who had done a lot of computation.

**MAUCHLY:**

I don't know how much you know at this time about the master programmer, but the usual examples of iteration that are given just for simplicity say that the iteration, for instance, might be terminated when a certain quantity reaches zero. That's about all they say, you know. The master programmer didn't have any inherent limits which said that had to be the mode by which you terminated something, you know. Anything that you

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wanted to program in the way of iterative techniques and criteria for stopping them or taking alternate paths and doing alternate program steps was always available to you, because you could redirect the computer program in each sub module, if you like, of the master programmer. You could have for a program pulse indicating the termination of some process, you had 6 outputs. You could initiate any one of 6. We had many modules like this so each of the 6 could go off and branch again if you liked, you see, so that it wasn't limited to 6. Moreover, when it stepped from one of the 6 to another one of them, you could control that by plain counting, control it by, for instance, something reaching zero any means that you might devise and wish to have for controlling this was available to you. You just couldn't provide input capacity of this sort, but neither do we have input capacity in a 370/185 or whatever they happen to have now; you know 158, at any rate. Even with virtual storage you don't have infinite capacity. You just have a lot.

So in a sense, why all these machines I've mentioned have virtual storage too, but that's another story.

**TROPP:**

I'm glad we got that onto the tape, and I will send you a co<illegible>

**MAUCHLY:**

So the general thesis I was trying to put across here is one which I never really verbalized until a few years ago when I began to see how the words were coming out of other people's mouths as to who did what and what was what, you know. I us to sort of passively withdraw, you might say, when somebody said, "well, of course, the ENIAC didn't have a stored program and the EDVAC did, regardless of who thought of it." Now I regard that as a misconception.

**TROPP:**

Of course one of our problems is the problem of language.

**MAUCHLY:**

Yes.

**MAUCHLY:**

Yes.

**TROPP:**

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As you indicated, one of the papers that Atanasoff showed me guess came out of the litigation, and this is the paper in which you had some notes about your trip; and you indicated that he had characterized to you the difference between an analog computer

**MAUCHLY:**

Yes, yes. And what we now call a digit computer. He had a totally different name for it at that point in time.

**MAUCHLY:**

Yes. Impulse.

**TROPP:**

No, that wasn't it; it was something else.

**MAUCHLY:**

A digital computer he called an impulse computer.

**TROPP:**

An impulse computer?

**MAUCHLY:**

Yes.

**TROPP:**

And the idea of a stored program computer was a whole new concept in our language. Even the word computer

**MAUCHLY:**

Yes. Was a totally new usage

**MAUCHLY:**

Yes. Of a word that formerly meant a human being.

**MAUCHLY:**

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That's right. It's very hard to take yourself back and remember what the language was or where things sat as of the time.

**TROPP:**

The phrases "general purpose computer" and "special purpose computer" are again, difficult to assess

**MAUCHLY:**

Yes. from this vantage point, because I read articles today that say, "well, maybe solve some of the problems that our big computers can't solve, we're going to have to go back to special purpose machines." But I don't think they mean special purpose machines in the sense of the differential analyzer style.

**MAUCHLY:**

I don't think so.

**TROPP:**

I think we've got confused with phrases, general purpose and special purpose, and I think our whole terminology is taking on a new form and this makes it difficult to go back into the embryonic period

**MAUCHLY:**

Yes. And say yes it was or not it wasn't, because the terms have changed.

**MAUCHLY:**

Fifteen years ago or less I was much taken with this idea of a hybrid computer; that you would get some of the advantages of the analog and some advantages of the digital and you meld the two, and somehow the whole thing works together. The only advantage you could get as far as I could see from this was, again, the economic advantage. In other words, that with this combination you might solve some problems faster and cheaper. I've never had the opinion, of course, that you had to have the analog computer in order to do these things. Everything that you ever want to do can be done, as far as I know, on a digital computer if you're willing to devote the time and expense to do it.

**TROPP:**

Right. That is, you have more of these and they're faster, the cost per unit operation becomes lower.

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**MAUCHLY:**

I don't know whether this is in any sense an extension of Turing's concept about a machine or not, but as far as I was concerned when I once learned what people said was a Turing machine and what Turing had said about a Turing machine, as far as I was concerned that said it. That was it! That any one of these machines, if it had certain fundamental capabilities could mimic any other one of them, and that maybe in itself does not say that you can solve every problem you ever want to solve with it, but to me it seemed to say that. That anything that the human mind could ever conceive of as a problem to be done, and they could write a specific program for, could be done by a Turing machine. And the rest was all a matter of efficiency, not feasibility or capability any more, but efficiency.

**TROPP:**

There are still people who are advocating hybrid computers, but I think they're advocating them from the standpoint that you mentioned earlier,

**MAUCHLY:**

Yes. The standpoint of economy.

**MAUCHLY:**

That's right. Yes. There was never a thing in Turing's theorems or papers that had to do with economy at all. It was a question of, is it or isn't it?

**TROPP:**

I've gotten away from what was going to be the initial thrust of my question at the very beginning of this tape. You mentioned the role that Shaw and Gale

**MAUCHLY:**

Yes

**TROPP:**

Played jointly and Arthur Burks,

**MAUCHLY:**

Yes.

**TROPP:**

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And I don't think you ever said anything about what Sharpless was primarily involved with.

**MAUCHLY:**

I don't remember exactly when he was made available. I mentioned that he was, I thought I said that he was put on the multiplier and that he and Burks

**TROPP:**

He worked with Burks?

**MAUCHLY:**

Did a lot on testing out our ideas for a multiplier. The ideas about how that multiplier was supposed to work were essentially mine, and were in a sense derived from what I learned at the World's Fair and subsequently about a true multiplier machine which Rem Rand had on the market (and which never got a very great market incidentally). But here was at the World's Fair, '39 this is, in New York.

**TROPP:**

I visited that, but I don't remember that exhibit at all.

**MAUCHLY:**

Yes. Well, I haunted the place for a day or two, I guess. Here on the one hand is the IBM, took 6 seconds or something to go bp-bp-bp-bp- through a 6-digit decimal number on a card and then some seconds later you could punch out a 6-digit by 6-digit product. Here in the Rem Rand exhibit was standing a small machine, you know just about maybe no more than 2 feet wide, less than that in depth, and standing about waist high with a roll of paper coming out of it, and this thing has a printing multiplier. You feed cards in which had two 6-digit factors on it and in one second the thing had punched out a product and printed the product on the paper and go ahead to the next one, you see. How did it do it? It's mechanical, you understand. It isn't electronic or electrical even; it's all mechanical.

So I went back to the Sperry Rand, and Remington Rand's offices, sales offices and things, and I tried to find out who was able to tell me how this worked, and I finally got somebody in the service department to open up a service manual and show me the contoured plates, you might call them, the templates, that went into this thing which, like the things that were inside that millionaire multiplier positioned themselves differently for each digit of the multiplier and, therefore, gave you the product of any 6-digit number in this case by any one decimal digit. If you can do 6 of those in one second you've got the answer, you see. You didn't have to grind away with repeated additions up to 9 times

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to get a multiplication by 9. One turn of the crank, so to speak, on the millionaire gets you the multiplication by 9. So one bang in this mechanical template system of the Rem Rand thing gave you the multiplication by 9. So I said, that's what we've got to design if we want fast multiplication in our ENIAC multiplier. So what we did was build something again which worked on a kind of function table principle, you see, that if the digit 9 came in through one set of tubes and things were activated that fed out partial products which were 9 times the digit that was coming in from the other factor.

**TROPP:**

Right.

**MAUCHLY:**

Now you summed the partial products and you got this all over with, you see, in a small number of addition times. If you had 9 digits, for instance, in the multiplicand and 9 digits in the multiplier, why essentially for reaching the partial products only took you 9 addition times, not 81.

**TROPP:**

Right. Then that brings up the question of the timing within the multiplier, the idea then that this naturally led to of requiring it to be asynchronous, because you had to wait until each of these popped up before you could then transfer the whole product of all these partial sums.

**MAUCHLY:**

I think what you're saying is that you had to wait different lengths of time depending on what you required in the multiplier.

**TROPP:**

This led to the requirement that you have an asynchronous timing mechanism.

**MAUCHLY:**

No, if you had done it by repeated addition you would have been worse off, you see. Then you could have anything from zero to 81 addition times to do 9 digits—

**TROPP:**

I'm not talking about the number but I'm talking about the—

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### MAUCHLY:

If you know you've got 9 digits by 9 digits you only always wait 9 times to get the partial products.

### TROPP:

This way you can time it up to a maximum

### MAUCHLY:

--But if you say I only want 5 digits then you can get out of the multiplier the product, faster. The real place where you have the asynchronous thing, as I understand it, is in division which had to be done by repeated subtractions or in square roots which had the same limitation, so you never knew how long a division or multiplication, division or a square root was going to last. You couldn't know that in advance without, of course, some very complicated testing or maybe using extra function tables. The multiplication is pretty damn predictable, more predictable than if you had done multiplication by addition. But in any case you might, apart from all those things, have two different sequences of things going on simultaneously in the ENIAC. The ENIAC was a very parallel machine, so even though it had to be serial in the sense that if you wanted two multiplications to be done, you'd have to do them one after the other--u couldn't do them simultaneously-- you wanted two divisions to be done you'd have to do them one after the other--you couldn't do them simultaneously because there was only one divider. But in most other respects the ENIAC was a parallel machine. The digits were transmitted in parallel, all 10 digits from one part of the machine to another, one accumulator to another, and while that addition was going on you could have in parallel 3 other additions or subtractions going on. You could have a multiplication going on. You could have a division going on, you see. Lots of things could be done in parallel.

### TROPP:

Lots of things could be done in parallel but in the case of each arithmetic; had to be done in serial.

### MAUCHLY:

Yes, and so somewhere if you do have this parallelism you've got to bring things back together again. And people, of course, are now studying this effect in multi-program machines and so forth, that, for instance, if you have to search a file to get a number you're going to use in a computation, the search time is not necessarily always the same. So if you try to do two different things, one of which involves a file search, you don't know which thing's going to be done first. Then you've got to provide for this, what we



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call an interlock, to be sure that both things are done before you go ahead and try to use the result of the two. And, of course, the interlock is, you might say, almost essential in almost any kind of input-output. So we had two places where interlocks were very vital to us; one was we had a whole bunch of interlock units associated with the divider-square rooter. We had another set associated with the input-output, the card reader and the card punch, so that you could read, write, and compute simultaneously, as we said--as we now say in present day language--read, write, and compute simultaneously. It was hailed as a big invention by a certain IBM Company years later; when they finally got around to thinking maybe it was worthwhile to put buffers in to do it. We had the buffers there in terms of relays in the input-output because of the fact that our machine was going to be so much faster than the card read or the card punch. We couldn't afford not to have that machine doing calculations overlapping the reading and writing of information.

**TROPP:**

I'm sitting here with so many questions I don't know which one to throw out first. You have stimulated so many things that you have triggered ideas.

**MAUCHLY:**

Yes.

**TROPP:**

I'll try to sort them one at a time. You have a big problem. Here you're building something that's never been done before.

**MAUCHLY:**

Yes.

**TROPP:**

At some point in time you get all this input from Aberdeen. You've got your own problems of design, the realization of the conceptual ideas. At some point you know you're going to have to worry about how to program things.

**MAUCHLY:**

Yes.

**TROPP:**

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At what point in this development stage did you start thinking about, "how are we going to run a particular problem through?" Suppose you wanted to do something else. At what point in time did you face up to the realities of the use of this machine?

**MAUCHLY:**

Yeah. Well, in a sense you've put your finger on something which relates back, of course, to that question of whether it was a general purpose machine. That is, it is a fact that when we said we were designing this for as general a usage as possible, we didn't ourselves consider it as being the ultimate answer to having a machine that was all things to all people and that would do everything.

**TROPP:**

Oh, yeah. I think you made this clear earlier in terms of the time pressures.

**MAUCHLY:**

So as a matter of fact, the thing you just mentioned, a prime number search or something, was not one of the kinds of problems that we were trying to provide efficient implementation for. It's true that Dick Lehmer and I spent a Fourth of July or something putting on a program which relates to the theory of numbers, and which used a lot of this master programmer and other things very nicely to do this. We had lots of sieves going at once, you might say, in parallel. To answer what I gather to be the real content of your question now, why maybe we might say that we were a little cavalier about this part of it. We didn't think that we should spend our time worrying about figuring out programming methods and programming aids and all that. We felt that if we had the machine capable of doing all these things, including the iteration controls and the master programmer and all that, that there would be time enough to worry about those things later from the point of view of a flexible wide application of things.

**TROPP:**

Of course, if the way--

**MAUCHLY:**

The war didn't really come to an end till about the time the ENIAC was finished.

**TROPP:**

I guess I was thinking in relation to both statements your wife made the last day or two about her experience, and my conversations with Maurice Wilkes

**MAUCHLY:**

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Yes.

**TROPP:**

During the building of EDSAC

**MAUCHLY:**

Yes.

**TROPP:**

And Wiener and Gills who were at various stages of that.

**MAUCHLY:**

We'll get to that.

**TROPP:**

But you know, I was thinking in terms of the fact that here were these people who had to learn, as she put it, the back of the machine

**MAUCHLY:**

That's right. Yes.

**TROPP:**

In order to do a problem or program a problem. I guess my question was--

**MAUCHLY:**

My transition sentence here, you see, if you can hold your question a minute,

**TROPP:**

Sure.

**MAUCHLY:**

Was that at the time we were finishing ENIAC, the war was just about closing. It isn't a question of its having already been over with, and so we were still, during the time we were building this thing, primarily concerned with getting that hardware built so it would

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properly work. Our first concern, of course, was to do what we called the two accumulator test just to prove that a piece of it--a modular piece, two accumulators--would do what it was supposed to do because if we had had to redesign them then there was a lot of reproduction to do, because we were already producing more accumulators, you see, to go into the machine. So we satisfied ourselves in '44 with the two accumulator test that only a few minor changes had to be made, and in essence the whole thing looked as if it was going to fit together and work properly; subject to final tests, of course, which might discover some more little things that ought to be changed, and we did find those things. But we were still concerned, not with how you program it, but with getting the thing finished and working reliably. Now, the War Department or the Ballistic Research Laboratory in particular, as soon as they saw the thing seemed to be on the right track and it probably would work, they concerned themselves with the programming problem and began to say, "Well, we'll train some people to try to use this thing when it's finished." So Dr. Goldstine, I think, claims that he picked out the girls who were to be the yeomen to get in there and first learn how to do this. I never could figure out how he happened to pick some of the girls because we know, for instance, Frances Bilas came in as a last-minute just because she thought it would be nicer to work at the Moore School than at Aberdeen, I think. And that's not a good reason for picking somebody. But, at any rate, three of the girls at least had some mathematical training and ability and they took to it and they in turn, I think, helped the other girls to accommodate themselves to...

**TROPP:**

Why don't you mention these names as long as you've identified them?

**MAUCHLY:**

Oh, well, Kathleen McNulty, who's now my wife--

**TROPP:**

Her background was in mathematics.

**MAUCHLY:**

And she was a math major from Chestnut Hill. Actually Frances Bilas, who's now Mrs. Homer Spence, was also a math major from Chestnut Hill and the two of them simultaneously, together, applied to Civil Service and became the first employees for doing trajectories by this hand Friden calculator method when they started building up the staff at the University. Then when they tried to put the analyzer onto two-shift operation, they had to get more staff, and they asked some of these girls that they had hired to volunteer for transfer to the analyzer, and those two girls volunteered and became differential analyzer girls. They had a bunch of other girls working down at the differential analyzer that got there the same way, but none of them as far as I remember ever then were transferred to the ENIAC program.

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Well at any rate, continuing to name these other girls, the third girls who had some mathematical ability, but was not a math major was Betty Snyder,

**TROPP:**

Now Holberton.

**MAUCHLY:**

Who married the man who was put in charge of all these people at that ENIAC project,

**TROPP:**

John--

**MAUCHLY:**

John Holberton. John Holberton had formerly been the supervisor for this group of girls doing the desk calculator step by step integrations, pencil and paper work. So he was picked--I don't know whether he wanted to be, volunteered or anything--but at any rate he was picked by Goldstine to be in charge of this group of 6 girls who were going to then learn how to use the ENIAC, and set up some firing table calculations and things of that sort. In addition to those three then--well, I forgot to say that Betty Snyder had started out as a math major at Penn, and the story that I remember she gave me was that the instructor she had in her first year math turned her against it, so she majored in journalism. But when the opportunity came to be hired by this department unit she thought she would be interested in doing this math work. So she too then was one of the early employees doing this pencil and paper Marchant desk calculator, not Marchant, Friden desk calculator integration.

So those three had a considerable math background. Another one, Marlene Wescott, who married a dentist by the name of Phil--can't remember his last name, now--was an interesting person of an entirely different sort. She had trained to be a teacher of high school history. Her major was history; I don't remember what school. I hired her--I don't know how she got to come to us or who recommended her or what, at any rate I hired her as one of my computers on this Signal Corps project to do these step by step integrations. She didn't have to know any mathematics to do that. Neither did anybody else.

**TROPP:**

It was all laid out in tabular form.

**MAUCHLY:**

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It was laid out in tabular form, step by step do this, do that, you see. But the interesting thing was we checked everybody's work by having somebody else do some kind of checks... things. If the other person disagreed with what she'd gotten we always told the other person to go check. She never made a mistake!

**TROPP:**

Incredible!

**MAUCHLY:**

That girl, when she punched a key on the machine, by God it was the right key. When she copied a number out on a piece of paper it was the right number. I don't remember any instance in which she ever made a mistake in any of those tabular desk calculator machine things, you know. It was like an automaton, if you like, you see. She was letter perfect, or number perfect, on the stuff. That doesn't mean that she had any great knowledge of mathematics or understanding

**TROPP:**

You didn't need any for that particular work.

**MAUCHLY:**

for these things, you see. And so for some reason after she was transferred to the numerical integration work and did well at it, she volunteered and was accepted by Goldstine apparently, to go to the ENIAC thing. She had no interest in mathematics; she really didn't understand much about the ENIAC. It was just a great romance, I think, you know that here's a chance to do something new and novel--why not? Now another girl, Ruth Lichterman, whose married name is Teitelbaum, came into the group some way. I didn't really know her, I think, until after she was chosen and started working with the ENIAC group. I think she had been a member of this desk computation group before. There were six of them there. Who have we lost?

**TROPP:**

The only name--don't think she was part of that group, but another woman's name that I remember is Adele Goldstine.

**MAUCHLY:**

Adele Goldstine

**TROPP:**

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I was going to ask you about her role.

**MAUCHLY:**

Was already an accomplished mathematician

**TROPP:**

Right.

**MAUCHLY:**

And she was in the role of a teacher for the people who were doing the desk integrations. So was my first wife, Mary. They wanted somebody that already had math training. She was a math major and knew something about math beyond what other people might know. She had worked as an actuary for a while, and so she and Adele Goldstine and another woman who I guess had had math training before--must have--whose name was Mildred Kramer and whose husband was Noah Kramer, was a well-known, what do you call it, an archeologist, essentially--he deciphers things like Rosetta Stones, you know, that sort of thing, at the University of Pennsylvania Museum--. So Millie Kramer and these other two women became the instructors, and their purpose, their job in life, was to instruct girls in how to do the step by step integration. Then when the ENIAC became something to plan for, then what Goldstine did was to have his wife transferred to writing the manuals for the ENIAC. The other girls then had nothing else to do. So my wife, my first wife, Mary, got transferred to the project H which was doing an entirely different thing off campus for the Ballistics Research people, but Millie Kramer, as far as I know, didn't have much else to do.

Now there was a sixth girl on the ENIAC programming project, Betty Jennings, who became Betty Bartik later, and she was, I guess, a math major. At any rate she was pretty good at this and worked later with Von Neumann in designing a code for revising the way in which you directed the ENIAC to accept instructions, something which was implemented at Aberdeen much later. After raising a family, why she's back working with computers just as Betty Holberton is, but some of the others have all just become housewives and have retired from the computer business.

**TROPP:**

That's a good point to close with.

**MAUCHLY:**

Yes.

**[End of Interview]**

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