Computer Oral History Collection, 1969-1973, 1977

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Interviewer: Henry Tropp
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TROPP:

We'll just talk informally and then, Dr. Alt, and then we can—

ALT:

You asked about forerunners of the stored-program concept. Richard Clippinger had some thoughts ...that were forerunners of what we later called the new programming system for ENIAC and didn't go as far as those. ...That was--that might have been 1946. Then a year later they developed a real new programming system and it was still quite handicapped by the fact that there wasn't any internal storage on the machine. And then they added hardware to store 100 words; and that made it into a real powerful system....The instructions were set up on the function tables. One difference, of which I don't know when it was installed, was about having branch instructions so that you could loop back. I think Clippinger did not have that in his original concept, but I'm not --I'm not sure where all this comes in. That's my [recollection] now.

TROPP:

Of course memories are vague and the reports are there but they are not absolutely factual either.

ALT:

Right. Right

TROPP:

When Clippinger talks about seeing how to use the function tables after Adele Goldstein showed him how to use ENIAC. And then he mentions that she told him to see Von Neumann or told Von Neumann about what he said and at that point he went back and spent some time with Von Neumann. Then came back and talked about his ideas in more detail. And I think the problem that Metropolis ran was the first one that was run using the function tables and a kind of a stored program. Now that's, you know, very crude. Does --how does that gibe with your memory?

ALT:

I think that Metropolis's problem, ...it was a fluid dynamic model of the atomic nucleus, ...I think that problem was first run under the old program method, set up the ordinary way, and later on they used it as the guinea pig for the new programmers.

TROPP:

I see.

ALT:

I believe that's true. ...I happen to remember that Nick Metropolis's problem was labeled problem number three. It was in fact the third one run on the ENIAC, officially. There were --there were a few fore-runners that were not numbered. ...I don't know whether I can still identify all the other numbers but I do seem to remember that number three was Metropolis'.

TROPP:

Aha.

ALT:

Number one was almost certainly ...most certainly some kind of firing tables. Perhaps [inaudible] kind of firing tables....One of the very early ones was just interpolation, just sines and cosines, some --something like that would be used only as a test problem. [Clears throat].

TROPP:

Was Lehmer running one of the early problems?

ALT:

Lehmer was running one of the unnumbered early problems. There was a famous Lehmer problem, ...July 1946, I believe I am alluding to that in this article in Communications 1972. That's one of my most treasured recollections.

TROPP:

Aha.

ALT:

Lehmer --I --I was not connected with ENIAC at the time. I had been discharged from the Army, gone back to civilian life and took a year before I got back as a civilian to Aberdeen Proving Ground, back into contact with computers. ...And Lehmer's problem was run during that time. But it happened that somewhat later I was rummaging through the files of the Computing Laboratory at Aberdeen and I found a carbon copy of this ...letter Lehmer had written to Colonel Simon, Director of the Laboratory, in which he told him about --about it. He said he ran it over the July Fourth weekend, [cough] he said he had received permission to do that because the machine would have been kept turned on anyway, [so it] cost [nothing] extra - [one did not dare turn it off, for fear of blowing tubes] and it took the whole weekend, I believe, two days and three nights, and he and John Mauchly were there personally all that time.

TROPP:

[Laughter]

ALT:

Well, the machine was not yet in routine operation; ...it took special people to keep it running.

TROPP:

But it hadn't been at Aberdeen very long then.

ALT:

I --it was still in Philadelphia.

TROPP:

Oh-

ALT:

This was done in Philadelphia.

TROPP:

That was done in Philadelphia.

ALT:

Yes. [Pause] ENIAC was not moved to Aberdeen until early 1947.

TROPP:

I see.

ALT:

...That problem was a problem in number theory. The purpose was to determine whether a number, integer, was a prime number or not. ...Since prime numbers have a very irregular distribution the only practical way to do that [cough] --well, you can try to divide by all possible divisors, but that takes a very long time, for a large number. Another way is to have a list of all prime numbers in the computer, but that gets out of hand when you go to large numbers. ...Number theorists have a test, which is almost but not quite foolproof. ...[If] I remember correctly: 2 to the power a divided by a leaves a remainder of 2 --may be wrong, something like that --if and only if a is a prime number. The statement is not quite true but it has very few exceptions. And it's a very easy thing to test.

TROPP:

M-hm.

ALT:

And so the purpose was to list all the exceptions and Lehmer did that on the ENIAC. And he found about 30 exceptions up to some very large prime ...six or eight or ten digit numbers, billions of numbers, and only 30 exceptions and it would be a fairly easy job for a computer at any later time to have a list of these 30 exceptions in memory and then if any number is given try this rule and see whether (a) the rule is satisfied and (b) whether it happens to be one of the exceptions. That would determine the prime ability of the number. That was the purpose of it, and it took about 2-12 days [clearing throat] to find these 30 exceptions. That had to be done the slow way, because each number had to be tested for prime ability by essentially trying all divisions --with a grain of salt.

TROPP:

Looking at all those which had a residue of zero.

ALT:

Yes. That was Lehmer's problem and the letter that he wrote about it, the report that he wrote about it, was quite enticing; very well done; I've treasured it for a long time….

That was the July Fourth weekend of 1946. There is some debate about whether anything had been done on ENIAC before that time, certainly test problems. ...And it's not --it's even hard to know where to draw the line. You know, Lehmer's problem could be called a test problem because that's what it was for the Army. ...It was a useful problem in its own right from somebody else's point of view. Some of the earlier things ...one could argue even more whether there was any use for them or whether they were merely test problems. I don't know what they were. Certainly the machine was being tested from late 1945 on until about, oh, the middle of '46 or so; around that time it began to produce good results. Why did we get into this?

TROPP:

Well, we were talking about --we started to talk about the origination of stored program ideas on ENIAC. And I guess the only way to get into that is to go back to ENIAC and its early --as you were doing, go back to ENIAC in the beginning and what it was like. And I would gather if Lehmer's working papers were available, that was kind of a brute force approach to the problem.

ALT:

Yes. Lehmer had no share in the so-called new programming method, stored program method. That was after his time.

TROPP:

That was later.

ALT:

After he had left Aberdeen. Yes.

TROPP:

You mentioned in an earlier conversation that you and Lehmer and Cunningham and somebody else were originally on a computing committee--

ALT:

Yes.

TROPP:

at Aberdeen

ALT:

Yes.

TROPP:

and that would have been during this same period or even before.

ALT:

That was well before.

TROPP:

Well before.

ALT:

Before the machine was in existence. That was in 1945. I got to Aberdeen something like March or April 1945. Cunningham was there before me. Curry and Lehmer arrived the same summer or late spring. And I left again in October '45. Lehmer stayed another year. I don't remember when the others left, certainly after me. But the middle of 1945 was the time when the so-called Computations Committee was in existence. And what we did would now be called programming for machines that weren't there yet and we divided up our interest ...I had a predilection for something concrete; and so I did not concern myself with ENIAC, which was not yet finished, but I did concern myself with some forerunners, relay calculators made by IBM, rather small ones, which were there and weren't being used much, because nobody knew much what to do with them.

TROPP:

Did they have the early Bell relay machines there then?

ALT:

That was not there yet, that came later. That was still being built in New York. Also one of the interests of the Computations Committee, but at that time, 1945, nobody, as far as I know, had any special [interest] in it.

Cunningham interested himself in the standard punch card section that we had at the Computing Laboratory. And he tried to streamline them and get them into better production. I worked with relay calculators, IBM relay calculators. I programmed some problems with them and still remember the problems too.

TROPP:

Aha. These were primarily problems connected with ballistics tables?

ALT:

One --we had a ...we had a ballistic range at Aberdeen in which small samples of --of ammunition were fired in a tunnel in which high speed photographs were taken all along the path and one could therefore determine very precisely the behavior of that small shell, ...its spin, its tumbling, ...its yaw and things like that, the curvature of its trajectory, things which would be somewhat harder to see on real live ammunition. [Cough]. And the laboratory got ...reams of photographs taken of these shells and they had to be evaluated; that is, mathematics had to be used to deduct from these photographs the characteristics of the different shapes of shells. And it was that kind of mathematics or some of it that I put on the relay calculators. If you have the X-Y coordinates of a shell on successive plates, and the timing between ...exposures, you deduce from that the location of the shell in space and its velocity and its angular velocity and characteristics like that.

TROPP:

Both an interpolation and an extrapolation problem?

ALT:

It's interpolation and some ......trigonometry, if I remember correctly.

TROPP:

Linear transformations. ...

ALT:

Yea. That was one problem. Another was matrix multiplication. Multiplying two matrices; that came out very prettily.

Lehmer and Curry were studying the ENIAC at that time. There is a paper in existence, or at least a report, by Curry on high order interpolation on the ENIAC, but ENIAC was still almost a year away at that time. Everything was still in a vacuum. We had a large differential analyzer. One of the things that struck me in retrospect was how much unrecognized influence the differential analyzer had on the design of the first digital computers. Nowadays one wouldn't think that there is any connection at all. But at Aberdeen they had a lot of experience with the differential analyzer. That was patterned on the first MIT differential analyzer which had been built by Vannevar Bush --about 1930 or '32.

TROPP:

Right.

ALT:

A --an amplified copy of that was in existence at Aberdeen, I think another one at Philadelphia, and its size had been determined for the requirements of trajectory computations. For example, they had found they needed so many integrators, I forget whether that was twelve or fourteen or twenty, some number like that - integrators. Now normally on a differential analyzer one integrator is used to perform one integration in the course of the computation. If you write differential equations of a trajectory, you know it's a sixth order differential system so you think of six integrators, and it turns out that one needs a few extras for ...the value of any special functions, for instance, if you have an exponential function anywhere in your problem the differential analyzer ...evaluates that as the solution of a differential equation; that is to be carried along in the system. It sounds like a round-about way to us, but on a differential analyzer that's a natural thing. And so for ancillary systems like that one needs a little extra, ...integrators, and so the size of the analyzer was I forget what, around 14 integrators. And it's for that reason that ENIAC, for example, and also the Bell Laboratories ...relay computers had on the order of 20 ...now what do we call them --adders or --or registers or something like that, units which could add numbers. Nowadays we have only one of those and switch numbers in and out of them, but that hadn't occurred to people. They thought that if you wanted to solve a differential system of this order you have to have 20 places where you can accumulate numbers.

TROPP:

M-hm.

ALT:

Accumulators. ...After ENIAC was built, it was Von Neumann who observed that one might get along with one adder and otherwise only storage ...components. So that, even with the techniques that we used for ENIAC, if we were designing a computer now with the components available at that time, we would build 20 storage registers and one adder. ...In fact I think the Bell Laboratories computers were designed that way; they had storage registers and only one ...arithmetic unit.

TROPP:

M-hm.

ALT:

But on ENIAC one thought, because of the influence of the differential analyzer, that one needed 20 accumulators. One hadn't yet had the concept of --of separating arithmetic functions from the storage of numbers.

TROPP:

That's an interesting theme. Can you think of any other aspects of the differential analyzer in --in terms of how it did things sequentially and the design of machines like ENIAC? In terms of the impact? I think the --there is a tendency now, as you say, to totally divorce the analog or differential analyzers from the development of digital machines.

ALT:

Yes. That's right.

TROPP:

And I'm interested in --in following up your theme and idea to see if there are any other characteristics that you felt were carry-overs.

ALT:

I --I don't think there were. One might think of the function tables on ENIAC. But, on the contrary, on the differential analyzer the corresponding thing, a function table, ...was one of the weakest points. We did either have them or began to introduce them at that time. I remember that there was talk about schemes of making graphs of functions, carry the [?] functions that would be needed and [cough] having them on --on matrices [?] half black and half white with photoelectric follower which would ride along that boundary. One needs things like that because in ballistics critical functions occur which are not easily expressed by analytic formulas. Air resistance, for example, as a function of speed has a very str --has a very strange behavior. So one --one has to have some way to look up the values of a function which is defined only ...empirically, not analytically. And on the differential analyzer ...the solution to that problem was quite crude and giving a lot of trouble at that time. I guess it is --well, one could say that the function tables of ENIAC played a similar role and maybe that's why they --why it occurred to them, but I can't really say that they were patterned on the analyzer. [Short pause].

TROPP:

Well –

ALT:

Perhaps the fact that we had three might have been taken from the analyzer experience, but I can't be sure of that.

TROPP:

Well then, let me toss a broader kind of question at you that you can use stories, or, or what have you, to illustrate. And, in reading through your articles on the history of computers from your viewpoint and your experience, ...what would you identify as the real major milestones in the early period? And how do you see some of them as having occurred, again from your own experience?

ALT:

Oh, more than one milestone. ...The relay computers were not a milestone. They were some years old and they --as we look back now that was a blind alley. ...They looked impressive, the Mark I of Harvard, for example. ...That had predecessors designed by Stibitz at Bell Laboratories, computers which were remotely accessible. ...They were blind alleys. Well, I --I think we'll see that more easily when I --when I say why electronics was a big step forward. ...

ENIAC was essentially the first electronic computer. There were forerunners too.

TROPP:

M-hm.

ALT:

One hears about them now, --

TROPP:

Right.

ALT:

but they weren't recognized at the time. ...Lehmer tells me that he had designed a little electronic computer specifically for factoring numbers I believe.

TROPP:

He called it a prime numbers sieve.

ALT:

Number --yes, sieve. That's right.

TROPP:

It was partially electronic, it had a electronic-device.

ALT:

And somebody else --somebody else, can't think of it now.

TROPP:

Well, John Atanasoff had a --

ALT:

Yes, yes that's the other one.

TROPP:

had part of a machine going.

ALT:

ENIAC was the first one that was really integrated and really operated too.

TROPP:

And also sequential. It --it operated in --in a sequential sense. I don't know that any of the other

ALT:

Yes.

TROPP:

precursors really had that capacity.

ALT:

I think the thing that we learned with this high speed was that ...you had to have a way to program ahead of time --long sequence of operations. ...On slow computers you can add manually, you can request manually the performance of each arithmetic operation. But a fast computer is useless unless you have some way to program it. ... The idea of a stored program, which I think is attributed to Von Neumann, ...couldn't have come up before we had the ENIAC because it would have been useless. But with ENIAC it was mandatory to have something like that. ENIAC itself still worked without stored program, and worked somehow after a fashion, made fairly good use of its speed. It was limited to having one problem to perform --performed a very large number of times –

TROPP:

M-hm.

ALT:

Because of the absence of stored programs it took so long to put on a problem that ...that any problem would have been a bottleneck unless it was to be repeated thousands of times. But ENIAC was the first time that you could really make good use of a very high speed computing and the stored program concept extended that to much smaller problems. I guess those were the two milestones, electronic arithmetic and stored programs.

Stored program brings with it the basic ideas of operating on the instructions; that is, putting an instruction into the computer and then modifying it under the control of the computer. ...Also the concept of branch points. Those two --those two elements of stored programs are what give the computer so great power. They can modify their own instruction and follow different paths at different times under their own control.

TROPP:

Now when you say that this was essentially a Von Neumann contribution, it is a conceptual contribution or the force of the personality?

ALT:

There is some argument about that but I don't want to be too apodictic. I wasn't even there.

TROPP:

Oh, I see.

ALT:

When I came into play the stored program concept existed in reports which were then being written.

TROPP:

Right. There's a re—

ALT:

About 1945. And I can't be sure who contributed what to the idea.

TROPP:

Yea, in Clippinger's '46 report he gives great credit to Von Neumann ...for the idea and -- and for the various elements.

ALT:

Yes. But there was so much --there was so much interaction between different people. I think Mauchly will probably take some credit for --for the thought, the idea.

TROPP:

Well of course the really big jump in --in terms of something like ENIAC is to go the major step away from this machine that was designed to do one problem over and over again into a more general purpose machine. Now Mauchly talks about having this idea and building in the capability to ENIAC even though it was only going to do the ballistics problems over and over again. Now how well that was known at the time I'm not really sure; or how valid that statement is, I don't know. You remember, he talked about it that evening in Boston. [Pause].

ALT:

I --I guess what --what is meant was that he conceived ENIAC as a general purpose computer, while before that it was considered as only a ballistic computer.

TROPP:

Eh-hm.

ALT:

Yes, even the name indicates that. But that has a forerunner, the differential analyzer although it had been you know, installed merely for ballistics, it was already known to us that [it'd] solve a great variety of problems. ...At least all kinds of differential equations. That's what it was designed to do. We learned later that one could do even problems that were not differential equations. A differential analyzer can do all sorts of things; it is primarily designed for differential equations, but even that's fairly general purpose. ...The same with ENIAC. The justification for building it was ballistics. I thought it was quite well understood that it would be at least a general purpose integrator. At least as general as a differential analyzer. And it turns out to be quite a bit more general.

TROPP:

Now you tended to dismiss the --the Bell relay machines or other relay machines as technologically a blind alley. And I don't think anybody would argue with that. But I'm --I guess the other question, in terms of their role, is what conceptual impact they had.

ALT:

Oh, they were miraculous in their --in the cleverness of their design.

TROPP:

I mean it's things that they did that later turned out to be valuable on the electronic machines.

ALT:

Eh - ?

TROPP:

I'm thinking of the Bell machines and particularly the first use of binary arithmetic, ...a checking code –

ALT:

The Bell machine was not binary. One of its predecessors may have been.

TROPP:

The c--I was thinking of --of Stibitz's first machine --

ALT:

Ah, yes. Yes, yes, yes, yes.

TROPP:

--the complex calculator which was a binary machine. The idea of an excess three checking code which showed up on later machines.

ALT:

Yes. ...But most of all the basic idea of having every step checked. The basic --the other basic idea of having the machine operate without human supervision.

TROPP:

M-hm.

ALT:

On the Bell machine we would load up 4 or 5 problems in the evening before going home and in theory expect the machine to pick up one after another overnight.

TROPP:

M-hm.

ALT:

In practice, in those days, it always ran into trouble after a few hours. The longest I remember its running was till five in the morning. I can't remember any occasion when we still found it running when we got back at --at nine. ...Relays weren't that reliable. They were too easily subject to little specks of dust and malfunctions.

But ...in the --in the design of the machine there were a great many fine points which were quite absent in electronic machines for years.

TROPP:

M-hm.

ALT:

We who knew the Bell machine and others, and could compare designs of different machines, kept arguing for more self-checking, for example. Nowadays it's understood that on the better tapes you have longitudinal and lateral checks. ...It took years to convince designers of machines that that was necessary. In the early days they wouldn't be bothered with that.

I remember conversations, I remember, with people with whom I talked about it and they began to scratch their head and say, "from that viewpoint, if that's true, we have to do something."

TROPP:

[Laughter].

ALT:

The high degree of automatic control in the machine; going on --carrying on with the problem, checking every step: we didn't perform a multiplication without checking the result, at least for internal consistency of the stored numbers. ...There was some checking after every --after every relay operation practically. ...And then if a check did not come out correct there were all kinds of built-in procedures for what the machine would do next, a record [?] of intelligence for re-input.

TROPP:

Well, as you indicate, these things didn't show up for a long time, a decade or more.

ALT:

For a long time and I think some have never shown up. Some may --may have [inaudible].

TROPP:

Do you think they got into the electronic machines through that area?

ALT:

Oh yes, because some of us knew this was possible. Yes, yes. Well, for example, we now speak of Hamming codes. Hamming, Richard Hamming --

TROPP:

Right.

ALT:

--gave his name. He was at Bell Laboratories, and he knew about the different systems of number representation on relay computers or --or relay switching networks and he made a systematic listing of the different checking methods, like "excess-three," or "two-out-of-five," or whatever they are.

TROPP:

H-hm.

ALT:

And added some to his list that hadn't been implemented yet. It wasn't that difficult to see what could be done next. Some had the property of picking up one malfunction provided there weren't two of them: there could be two troubles that occurred simultaneously, but there were systems in existence that could discover two simultaneous malfunctions. And there were even some self-correcting systems and he generalized that, and --and so the whole theory of checking of number representation goes back directly to Bell Laboratories practice there, Hamming's theory for one thing helps [though] now we're using only the most rudimentary parts of [it]. That's for number representation but also checking the successive steps of an operation. The ideas were there. They were --they didn't have to be reinvented. They were technically there.

TROPP:

So from one standpoint the relay machines were a technological dead end. From an intellectual standpoint --

ALT:

Yes. Logically --logically they were a long step forward.

TROPP:

Well, I think even people like Stibitz realized very early in the building of relay machines that the way to go was electronic. ...When you run, and again this is my reading, this is not in, in a textbook or from talking to people, I would guess that the pressures of wartime, the pressures of getting things done, the difficulty to try something brand new as ENIAC was a --a sort of a --a sort of a counter example of, would have been difficult during that period, even if the ideas were there.

ALT:

...At Aberdeen Proving Ground it was known, documented and understood by all of us that we had given Bell Laboratories the contract for their relay computers as a fall-back position in case ENIAC should not work out.

TROPP:

I see.

ALT:

But that was quite well understood.

TROPP:

Mm-hm. But had Bell Labs or say Stibitz and Sam Alexander come in and said we want to try to build an electronic machine you would have said no we've got one going over here?

ALT:

I can't say.

TROPP:

I am just --I'm conjecturing.

ALT:

I don't know what would have happened. ...Mauchly had that ...pipe dream idea of an electronic computer and the Army decided to take a gamble on it. I don't know whether they would have taken a second gamble on a similar wild idea.

TROPP:

Yea, and also, as you indicated, the gamble wasn't that major, because they did have fall-back equipment.

ALT:

Yes.

TROPP:

And you still had the IBM relay machines, you had the differential analyzers and you had the Bell machines coming that you knew were going to do a particular job.

ALT:

Yes.

TROPP:

So the ...the first major milestone that you see then is ENIAC, and another major milestone is the stored program.

ALT:

Is the concept of a stored program computer, yes.

TROPP:

Between that concept and between core memory, do you see any other major milestone?

ALT:

Core memory was the next, I think. Yes. In all the early years it was memory that was the bottle-neck for us, ...and cores were the first time that --that we had something reliable and something that didn't give --give any more trouble.

And after core memory? I don't know if there has been anything that important. Once we had core memory computers worked well enough, not so different from the way they work today. [Pause] ...There have been a number of small --smaller improvements; each has given us, say, an increase in speed, but as I said once before, all your increases in speed adding up since ENIAC amount to only about as much as the one step from hand computing to ENIAC. ...

That was not the major step forward. Storage capacity, yes. The early machines were far too limited and it was core memory for the first time that made large storage possible.

TROPP:

It was interesting to hear Mauchly talk about the actual effective memory on ENIAC and realize how small it was, that sometimes it took 50 to 100 tubes to get a single digit. Today that sounds incredible.

ALT:

Well, ...it took 20 to store a digit. ...The number of tubes in all was 20,000. And it had --and there were --there was storage for 200 digits. So the average number of tubes per digit was 100, but that includes all the access and all the arithmetic circuitry, divider for example; adding all that in, the number of tubes was a hundred per digit store. It took 20 tubes to store one digit and then some access circuitry that was used for nothing else but that storage, maybe 30 or 40 tubes.

I have no idea ...how many ...diodes or transistors we would need today. That number wouldn't be so terribly different if we tried to store numbers in --in diode memory as we sometimes do. [Clearing of throat. Pause.]

TROPP:

In terms of your own experience, do you remember aspects of these various developments and problems that you were working on where you felt the need to be able to do things better?

ALT:

The big problem that I remember all the time with ENIAC and later machines was utilizing the time of the machine. It always seemed that ...you ought to do a problem you could calculate that it'd take so many minutes. And actually it seemed to be taking so many days.

TROPP & ALT:

[Laughter]

ALT:

[Cough]. The arithmetic speed was very high and most of the time went into subsidiary operations that weren't foreseen, ...shouldn't have been there, checking, trouble-shooting, starting and stopping, all --all kinds of things that it is hard to identify. It doesn't mean that the machines were out of order during all that time --they were out of order, too. ...In the early days we had perhaps two-thirds of machine time in good order and one-third out of order due to maintenance. That --that was good performance in those days. But --but even of the time that the machine was in good order only a very small fraction was really used in computation. ...That got better rather rapidly on successive machines, but was still a problem ...as long as we had one-of-a-kind machines. They were never quite as reliable; they --they didn't enjoy as much confidence as they do now. Whenever there was a strange result you just thought it must be the machine.

TROPP:

[Chuckle].

ALT:

Nowadays it wouldn't occur to us. We check the program twenty times before we begin to suspect something is wrong with the machine. [Pause]

TROPP:

Well you --anyway, you see those as the --as the main --major milestones. ...

ALT:

Yes.

TROPP:

It's interesting that even before ENIAC was completed that people like Von Neumann and Mauchly and Eckert had ideas which were going way beyond it.

ALT:

Yes. I don't even think that that's so terribly unusual. I --I have the impression that that's the customary thing.

TROPP:

Well, if that had been Babbage they would have stopped, ENIAC would never have been finished. [Laugh].

ALT:

The --the planners and designers had turned their backs on ENIAC as soon as the design was frozen and ENIAC was to be produced. That had to be done sometime. Naturally enough, in the early part of the effort they kept on changing their minds, and redesigning and improving, and the --the Army kept urging them to produce something usable, and the only way to produce that was to stop the design, freeze the design at some stage. I can't say when that was done. My impression is that it was sometime in 1944, although probably again not one instant. I imagine what happens in a case like that, that somebody says now we are freezing the design. And then he over-rules himself another dozen times. Especially on smaller points.

TROPP:

[Chuckle]. Yea.

ALT:

But essentially somewhere about 1944 they decided not to admit any more improvements. So people didn't stop having ideas. So those ideas were put into a --a pool for the second machine.

TROPP:

Mhm.

ALT:

By the time we --I began to read the reports, say late 1945, that second machine had a name, EDVAC. And it was --very clearly ...taking on its --its own identity. Von Neumann's reports about stored program computer, about the binary system and all that were directed specifically for ...EDVAC. It's a project for it, and ...there was a separate contract for its design.

TROPP:

Hm. ...You stayed on the East Coast during most of this period?

ALT:

Yes.

TROPP:

The early fifties you were on the East Coast that whole --that whole time?

ALT:

From early 1945 on.

TROPP:

So you had ...very little contact then with the early developments that were going on on the West Coast in the --say '47, '48, '50, '51?

ALT:

Oh, wait. By '47 I had a lot more contact with them. Say by '48. By that time I had moved to the Bureau of Standards, and the Institute for Numerical Analysis was part of the Bureau of Standards --

TROPP:

Right.

ALT:

--it was in the same division, it was a sister section to the one I was operating. We all kept traveling back and forth and exchanging personnel, exchanging programmers. We knew quite a bit about each other. Yes.

TROPP:

So your contact was the Institute for Numerical Analysis, the SWAC that was being built on the West Coast.

ALT:

Yes. I don't --

TROPP:

In terms of the SEAC; but --

ALT:

There were some other developments on the West Coast about which I didn't know quite as much. I guess they were started when Lehmer returned to Berkeley in 1946. ...There was Professor Paul Morton at the University of California at Berkeley who built a computer of his own.

TROPP:

Right. There was a University of California computer about this time and, looking at one of the reports, apparently they had conceived this computer as its building being a teaching device -

ALT:

Yes.

TROPP:

- and being primarily a teaching-oriented machine.

ALT:

Yes.

TROPP:

And I never heard a --an acronym or name connected with it. It was merely called the University of California computer. ...At least it is in the reports.

ALT:

I can look that up.

TROPP:

Yes.

ALT:

You probably have the same reports I have.

TROPP:

Wait a minute. I'll turn this over.

ALT:

1953.

[Machine off].

[End of Side I]

TROPP:

I don't remember any acronym being attached to it.

ALT:

It does have one here, but it wasn't widely known. Here in this report, 1953, Office of Naval Research Survey of Automatic Digital Computers, it is called CALDIC: C-A-L-D-I-C. And that's [the] Electric Engineering Division of the University of California, Berkeley. But nobody ever called it that.

TROPP:

[Laughter]. Yea, I'd never run into that. ...There are the other developments that I was thinking of that were going on in closer proximity to the Institute for Numerical Analysis were the developments that came out of Northrop and Hughes ...ended up with --[Pause] a number of companies.

ALT:

I don't remember the chronology of those things. I don't remember which comes early or which --which comes first.

TROPP:

Well, I'm interested in the --the Bureau of Standards thing. I --I'd like to go back to that, because that's, I think, a whole --a whole area of research all by itself.

Did you come to the Bureau of Standards before they were talking about building their own machine?

ALT:

Not before they were talking about it, but before they had actually --started.

TROPP:

Could you --?

ALT:

Just --just around that time. ...I came to the Bureau in October of 1948.

TROPP:

M-hm.

ALT:

The Bureau of Standards at that time had been involved in electronic computers for a couple of years, primarily as advisors to other government agencies that were interested in acquiring computers. ...The Navy Department, the Census Bureau --the earliest --the earliest two, I think. And they turned to the Bureau of Standards for technical advice on where to buy machines, what --what sort of contracts to enter into. ...The Bureau of Standards monitored those early contracts for the Census Bureau and for the Navy Department. They were with the Eckert-Mauchly-

TROPP:

Right.

ALT:

Computer Company for Census, and with Raytheon for the Navy. And then, as I am told, ...it turned out that both of these developments would be slower than had been expected and also the EDVAC at Aberdeen Proving Ground was slower than had been expected, and so people at the Bureau of Standards decided to build an interim computer. ...I suspect they were anxious to build it anyway [laughing] and were glad to have an excuse for doing so. It was patterned on EDVAC more than anything else. ...In particular, it had an acoustic memory, mercury delay line memory, like EDVAC, and many of the circuits were originally ...to be taken over. But then, ...people are always irrepressible, and they kept on redesigning and changing, and it turned out in the end to be a fairly new system, new especially to most elementary parts of ...componentry. They built up, as I remember it now, arithmetic functions from very simple logical functions. They built circuit elements for the simplest logical functions AND, OR, NOT, and put together the more complicated functions like ADD and CARRY ...from those simple ones. That --I think that was new. I --I don't remember seeing that kind of thing in EDVAC reports although I may be wrong. I think --

TROPP:

That may be checked on.

ALT:

--in EDVAC the --the arithmetic was the primitive element. To add two numbers to something to indicate it was not further reduced to simple elements.

One designed a circuit to add two digits and provide a carry [digit if] necessary. In the Bureau of Standards this was expressed in terms of the simple logical operations AND, OR, NOT; one knows that everything can be expressed in those terms and they provided circuits and made practical use of what the theoretical logicians had known for a century.

I don't think that was the only thing that was new in the Bureau of Standards computer SEAC. They must have changed the circuit design in many ways that I fail to appreciate because I'm not an electronics man. They must have, because their circuits worked very promptly, while at EDVAC they had untold trouble for years. The Bureau of Standards machine was gotten into production rather quickly. Between late 1948 and the spring of 1950 all design and construction was done; in the spring of 1950 we actually put the first real live problems on.

TROPP:

Then, if EDVAC was its, in a sense, parent, you had then stored program capability and you had branching capabilities and --

ALT:

Yes, yes. We had a memory of about 500 words.

TROPP:

You had a way of modifying.

ALT:

Oh yes, we had branch instructions. We were doing arithmetic operations on instructions. We were doing all these things. Very small ...list of instructions. I know it was less than 16 because they put only four bits to a code; I think it was around 12 or so, but that included the essential ones of, of matching, ...shifting, and --and the arithmetic operations, transferring of control. I don't remember whether we had logical operations, separately. It was a binary machine so that there isn't that much difference between arithmetic and logic. ...

TROPP:

Eh-hm.

ALT:

I remember the first problem we put on. I had written a test program of about, oh, 300 or so instructions to decompose an integer into its prime factors. There are a few alternatives of doing that but I chose one that seemed to be about the right size for a computer like that. I wrote the program, I asked a colleague to check it, and he found half a dozen mistakes in it and in the process of correcting them, introduced a couple of new ones. ...I punched the instructions myself. I wouldn't trust anyone else --

ALT & TROPP:

[Laughter].

ALT:

--with this important task. And I believe there were no errors caused by keypunching. We used paper tape, punched paper tape as input into the computer at that time; we didn't have magnetic tapes yet. And I remember the afternoon when Ralph Slutz, one of the principal engineers on the machine, called up and said he thought this evening we would be ready for a real-live test. And we decided to do it after hours so there wouldn't be any disturbance around. He --he functioned as machine operator and I was there. I put the tape into the reader and I said what do I do next. And he said push the start switch. And I couldn't do this.

ALT & TROPP:

[Laughter].

ALT:

I was afraid the thing might blow up or something.

TROPP:

[Laughter].

ALT:

But I finally did, and in the space of a second the first answer was printed out on the printer, on one printer. And then it stopped and that signified to me that there was something wrong with the program, it should have gone on printing. But even getting the first answer meant that the machine had gone through almost all the instructions perfectly well.

The machine was exonerated, it was probably the program. Perhaps it was a good thing that I've never had very much self-confidence. I immediately suspected the program and not the machine --

TROPP:

[Laughter].

ALT:

--and I discovered very quickly where the trouble was, and I corrected in on-line which was possible on that machine; it had a little keyboard and you could actually key numbers of instructions into the machine, so I could correct a few instructions that were stored in memory and the machine went right on and Ralph Slutz and I that evening tested, oh, several dozen numbers for prime factors.

TROPP:

Numbers of what magnitude?

ALT:

Up to ten digits.

TROPP:

Ten digits.

ALT:

In fact, very soon. The first number I put in was 15, just the number fifteen. But after that we, almost at once, we said let's go to some real big numbers and so we started with a set of ten 9's and then went down from there: nine 9's and a 7, nine 9's and a 5. Then we left out some trivial ones where we could see the prime factors ourselves. And after a few of those ...we put one number in and the machine failed, or seemed to fail, nothing --nothing was printed out. We waited for a good many minutes and finally said something must be wrong.

TROPP:

[Laughter].

ALT:

Yes. I forget what the number was.

TROPP:

Do you remember the --the procedure that you used for the decomposition?

ALT:

Oh, very precisely, yes. ...Testing successive divisors. Now in principle one would have to test only the prime divisors, but that would require a list of prime numbers and that -

TROPP:

At least up to the square root of the number.

ALT:

Up to the square root of the number. But it's impractical to have that many primes stored in the machine. One goes almost to the opposite extreme. The only primes stored were 2, 3, and 5. And from then on you avoid all multiples of these numbers as divisors. And they, ...if you write down the list of all the multiples of 2, 3 and 5, they are a very nice periodic set, so you can program that. You don't have to check for each number the multiples of those; you know how they occur. They occur in a period of 30, and all you have stored in the machine is which, in every set of 30, are not multiples of 2, 3, and 5. And you go through successive set of 30 divisors picking out only the 8 I think that are not multiples of 2, 3, or 5 and those are the only trial divisors you use.

I said 2, 3 and 5. I remember when I told ...another mathematician about this problem, J.C.P. Miller from England, who visited and was very much interested in this, he said why didn't you go to 7.

TROPP:

[Laughing].

ALT:

I hadn't given it any thought. It turned out that I might have and it would have saved a small fraction of the machine time; it cost a great deal more storage and I didn't want to bother anticipating whether I would have enough storage space in the machine. Just instinctively I chose 5 as the largest.

TROPP:

Then you would have had to look at period of 210 and wouldn't that have been a larger search?

ALT:

...A larger period but I would have, it would have slightly reduced the number of trial divisors.

TROPP:

I see. That's right.

ALT:

...It would have left out, I think, one out of every seven, something like that. [Laugh].

TROPP:

Aha.

ALT:

That was that problem. ...We demonstrated that to any number of people in the Bureau of Standards, the press, and I remember how embarrassed I was when I invited somebody to put some numbers into the keyboard and test them and he asked me "what happens if I put in the number one?" I myself didn't know. I had to think for quite a while before I decided what the program would do on that. But what was so embarrassing was I hadn't thought about it ahead of time. One --one ought to design a program so that it foresees all possible circumstances. And to this day we never succeed in writing a program that really foresees all possible circumstances. That [is] our greatest problem to this day. I frequently cite this example as one very critical one. All along we never seem to be able to think of all possibilities.

TROPP:

I remember in the very first relay machine that George Stibitz built, ...you know, complex calculator, he had something built in, in case somebody tried to divide by zero.

ALT:

Yes. But that's been drilled into us in school. That's not the only trouble that can occur.

TROPP:

I notice in Lehmer's table of primes, if I remember right, I could be wrong, to avoid difficulties he lists one as a prime.

ALT:

That's right, yes.

TROPP:

Then that avoids some of the problems that you would get into of this relatively trivial nature. Well, getting back to the --to the Bureau of Standards, when the Bureau decided to build its own machine, ...what kinds of problems were they thinking about in terms of machine applications? This is a, really a new kind –

ALT:

Our thinking was relatively advanced at that time. We could anticipate quite well that there would be a large number of different problems, coming from at least physics and chemistry, all the physical sciences. We also knew that the Census Bureau was anticipating a number of problems. We also ...by that time there was a third big customer the Air Force had come into the game at that time, and they were beginning to develop the techniques that are now called linear programming. They had --they had them on paper and they were looking for a computer for that kind of calculation. So we knew quite well that there were all kinds of problems, but especially physics. The Bureau of Standards is essentially a research organization in physics and chemistry, and we knew there would be partial differential equations, for example, fluid dynamics --one of the very earliest test problems, not really test problems, real problems, that we put on the computer was [ray] tracing for optical design. By 1950, we had --we had a good idea of --of the power of computers and their applicability.

TROPP:

That's interesting, the ray tracing problem, because I think that was also one of the very early problems on the Harvard Mark I was a ray tracing problem.

ALT:

I wasn't aware of that. But it --I --I'd never been aware of that, but it's quite --plausible.

TROPP:

I think it also was one of the first three problems that --that they were working on. And that's interesting. So the SEAC was designed then primarily as a scientific computer.

ALT:

Yes.

TROPP:

You weren't really anticipating applications like the Bureau of the Census.

ALT:

No. For that we would have needed somewhat different equipment. Not greatly different, but it wasn't intended. By the way, the Bureau of Standards almost simultaneously started two machines. SWAC on the West Coast was started almost at the same time as SEAC.

TROPP:

Right. Now Harry Huskey was in charge of the SWAC development. And was that independent or ...were the two kind of ...dependent?

ALT:

It was rather independent, just as SEAC was patterned on EDVAC, had a lot of contact with Aberdeen Proving Ground, Harry Huskey's development was closely allied with the British efforts. Huskey had spent a year in England -

TROPP:

[inaudible] know that.

ALT:

...learning, I forget where, at the National Physical Laboratory and I think also Manchester.

TROPP:

Manchester. Right.

ALT:

He got his storage ideas from Manchester. There was little in common between the two machines. ...I'm not even aware of the differences, but I know that they were rather fundamental in organization, and programming was rather different.

TROPP:

When did the idea of the Institute of Numerical Analysis come into being? And what was its --

ALT:

...There is an early report on that written by John Curtiss, first Chief of the --what is now the Applied Mathematics Division of the Bureau of Standards. Curtiss had been with the Navy during the war and was transferred to the Bureau of Standards. I think it must have been early 1946. I'm not quite positive, ...and immediately began to work out the plans for what an applied mathematics division would do at the Bureau of Standards. There wasn't any organized mathematics before him, but Ed Condon recognized something like that was needed and one of the functions needed would be statistical engineering and a computer laboratory at least. Presumably some advisory functions in applied mathematics and presumably along with it some research in pertinent areas of mathematics which later jelled into mainly numerical analysis. And so Curtiss began to lay out a table of organization for a division of that kind but I believe that almost from the start he envisaged a separate West Coast section to concentrate on research.

TROPP:

I remember seeing some of Condon's early reports in which he advocated some of these ideas.

ALT:

I remember a report written by Curtiss which was, you know, marked as an outline of such a division.

TROPP:

There is, there's another interesting report that I can't seem to find a copy of and --and even ...professor Curtiss hasn't been able to turn one up. And that's a report that was written to him by Von Neumann, Aiken and, I think, the third member was Stibitz, I'm not sure about the third. In a sense, a report ...concerning itself with the future of high speed computers. What the needs would be. And Curtiss apparently was very upset at the short --what he considered to be a shortsighted view. There are stories about that. I've not been able to find a copy of that report.

ALT:

Well, it I --I --it is true that the trio is Von Neumann, Aiken and Stibitz. Those three were the original --

TROPP:

That was the --

ALT:

National Academy Advisory Committee --

TROPP:

Right.

ALT:

--on High Speed Computers --not "Advisory"; Committee on High Speed Computers.

TROPP:

Right. And they were appointed in '46/7.

ALT:

Earlier than that; during the war.

TROPP:

During the war.

ALT:

I think '44 or '45. Later on, others were cooperating with them. Archibald came in later on, Lehmer. But it was those three originally. I said National Academy. I probably should have said National Defense Research Council. NDRC.

TROPP:

NDRC - that's right - they were --

ALT:

I think those three did that.

TROPP:

That's right. They were an NDRC committee.

ALT:

...And I don't know which of their reports comes f--

TROPP:

This would have been a 1948 report. And he didn't refer to it. Other people have told me about it.

ALT:

Umm. That sounds very late.

TROPP:

It could have been '46.

ALT:

Those three --those three were a committee much earlier than that, during the war. Now they may still --

TROPP:

The earliest that could have been would have been 1946, but I --'47, '48 kind of rings a bell.

ALT:

I see. So by that time Curtiss was at the Bureau of Standards. I don't know why they would have written a report then, but it might --it might be that I wasn't aware of it.

TROPP:

So that's not a report that you have seen.

ALT:

I --I never heard of it, no.

TROPP:

Do you have any reports written by that trio?

ALT:

Some --[Pause]. I wonder if there could be a confusion because it's even unlikely to me that I wouldn't have heard about it. I --I was at the Bureau since 1948, and if there had been something shortly before that somehow it would have come to my attention.

TROPP:

Well the story is told about John Curtiss --and he doesn't deny it, he just can't find the report --essentially flushing this report down the toilet because he was so upset with it, or he disagreed with it so violently. Now whether he did it literally or only in a figurative sense.

ALT:

Aiken tended to be very negative in those days. More so than the other two. Aiken tended to say that there wasn't enough need for computing in the whole country to keep one electronic computer pretty busy. And even the most optimistic ones among us thought in the early days that maybe we'd need three computers for the United States, of which the Bureau of Standards was about to supply two.

TROPP:

[Laughter]. I find it highly unlikely to have Stibitz take that position, because as early as '46 he is beginning to design a desk-size electronic computer for business applications, for the Barber-Colman Company. That's a machine that never actually was sold, but was built. And so I know that he felt that there was a need for electronic computing.

ALT:

I --I remember there was a Barber-Colman Company --

TROPP:

Right.

ALT:

--I think some place in the East.

TROPP:

They're still in existence they're in Rockford, Illinois.

ALT:

Rockford, Illinois [inaudible].

TROPP:

They actually built two models of this machine and then decided for various reasons not to go into mass production and not to market it. But Stibitz's early documents proposing this machine and outlining it I have found go back to 1946.

ALT:

I didn't know that Stibitz was connected with it.

TROPP:

Well, apparently at NDRC one of the members on the committee whom he got to know was the president of the Barber-Colman Company. And they got to know each other. And apparently this gentleman was taken with Stibitz's ideas and went ahead with the development of Barber-Colman Company with Stibitz, I think, as a consultant, to the operation.

ALT:

I just noticed that in the 1953 Navy list of computers, which is about the earliest that I have, that was not listed.

TROPP:

It wasn't completed until '55.

ALT:

On the other hand I remember visiting him well before that.

TROPP:

It was a desk-size machine and I've seen pictures of it. In '55 they made the decision, for various financial reasons, not to market such a machine. Their financial position was such that they didn't want to go into the public market.

ALT:

If I am not mistaken they were in an entirely different line of business normally,

TROPP:

Right.

ALT:

building the machine just on the side.

TROPP:

That's right. Engineers were sort of --.

ALT:

A hobby of the president of the company.

TROPP:

Right. So as early as --

ALT:

It could have been successful. In --England, the Leo --

TROPP:

That's right, the Lyons Company.

ALT:

The Lyons Company got into the field that way.

TROPP:

Well, apparently Ed Berkeley did a survey for Barber-Colman in '49 which indicated a market for something like 2,000 machines.

ALT:

Gracious.

TROPP:

[Laughter].

ALT:

He was certainly way ahead of his time.

TROPP:

Yea, yea. So that's why, you know, I heard about that particular report and, you know, its expression of pretty much the sentiments you indicated of a small handful of computers would be all the computational ability the United States would need for a long time to come.

ALT:

Yes.

TROPP:

On the other hand there were people who were thinking in terms of other applications, other than scientific applications.

ALT:

Also we are talking about a small computer.

TROPP:

Right. And that's something else.

ALT:

Even though it is electronic I don't know what a small computer would have accomplished. For instance, ...I'm not --I'm not clear that it would have been a stored program computer.

TROPP:

No --

ALT:

Not clear that it would have had any sort of memory.

TROPP:

It had a small memory; ...on the order of 250.

ALT:

That much? Well --

TROPP:

...A- Again, it was designed for primarily business purposes.

ALT:

Yea. But just there a small memory can be a very bad thing. So it would have been good for only a fraction of even business-type problems.

TROPP:

By --you know by current standards, and by what the Lyons Company was doing, it was a very small machine. Not as large as this desk, and, I would say, two-thirds the size and probably a foot higher.

But going back to the Bureau of Standards, ...what are some of the impact that you see in the development of computers of the early work at the Bureau of Standards? For example, the Institute for Numerical Analysis ...looks like a major milestone to me because numerical analysis is such a new --

ALT:

Yes, That's borderline to computers. The computers that had been built had a certain impact, that's one thing. ...What we did with numerical analysis had a much greater impact. It was quite unique, in fact. The Institute was dissolved after some time, but by that time it had established numerical analysis, had put numerical analysis on the map, as a discipline, where before that hardly the name existed. It was somewhere in the back of textbooks and was rarely taken very seriously.

TROPP:

When I was a young student of mathematics I don't remember seeing the name numerical analysis.

ALT:

No. No. ...There were a few books. I think they were in existence, which was of interest to astronomers primarily, because they were doing large-scale calculations. There were texts on numerical methods of numerical computation. ...The word "numerical analysis" was used in a slightly narrower sense than nowadays. It referred to only numerical approximations for the infinitesimal calculus. But, I --I remember a book by Scarborough with the title Numerical Analysis which may not be quite --it may not be earlier than this but the word was very well established at that time. In other words, we didn't invent the name numerical analysis.

TROPP:

No, I realize that. But as a mathematical discipline it was totally unknown to me.

ALT:

It was certainly --we made a big field out of it. I say "we" --I myself had very little share in it. John Curtiss and those whom he acquired for the California section in the early days.

TROPP:

This is people like Lehmer, and George Forsythe.

ALT:

Hartree, the physicist Hartree, was the first director for a year. Probably the most famous man, one of the most famous men we had. ...George Forsythe was one of the earliest members, so was C. B. Tompkins, ...Wolfgang Wasow. In fact we always thought of Forsythe and Wasow as a pair of people who were there, the principal ...stable staff members.

TROPP:

Is Wasow --

ALT:

A great many visitors and a great many factions.

TROPP:

Is Wasow still alive?

ALT:

Oh yes, I think so. ...He --he went to the University of Wisconsin, to the Army Research Center in Wisconsin in later years, somewhere in the late fifties or so. I can look him up. [It is?] important. [Walks across room] University of Wisconsin. He is still listed there and this is the 1972--3 membership list of the Mathematical Society.

TROPP:

Fine. I don't have that one. I have the --the one -

ALT:

This just came out.

TROPP:

That just precedes it.

ALT:

Yes, you get them in alternate years now. If you're a member of the [inaudible - laughing] clique.

TROPP:

Right. Oh, Van Vleck Hall. In --oh, very good.

ALT:

Yes, he can be found at Wisconsin. I don't know how much contact he still has with computers. He was always a little less interested in computers than Forsythe, for example. He couldn't get very far away from computers at the Institute of Numerical Analysis. It was a matter of degree. Later on Motzkin and the Todds, John and Olga Todd, were at least frequent visitors. They were --they were stationed in Washington --same place with me --but they spent at least summers in California. [Pause]. Oh, I don't remember -

TROPP:

But that, I'm interested in the West Coast development which was a kind of a parallel development to what was going on in the East Coast. I mentioned some of the early ...things that came out of Northrup, the ...MADDIDA --

ALT:

Yes, yes.

TROPP:

magnetic drum differential analyzer, the CPC.

ALT:

The card-controlled calculator of greatest accomplishment.

TROPP:

Right. Right. And I guess one of the questions I'm interested in, in looking at that area, is the impact that SWAC and the Institute had in terms of interaction.

ALT:

I believe that was very great. I think SWAC was the most important of all the West Coast developments. Perhaps I'm ...incompletely informed, but to me it always seemed that, of everything that went on at the West Coast, SWAC was the kind of center, and the others were satellites around it.

Partly I get that impression because in California there was always a great deal of emphasis on small computers. ...A number of people went into the business of building small computers. ...And of course nobody had the kind of environment that we had for SWAC, this large research institute. ...A relatively modest budget for the whole thing. I should remember. I --as I recall, it was $200,000 per year for --for the Institute, but it came from the Navy Department mainly, with some contribution from the Air Force.

TROPP:

In terms of this question that I raised not too long ago to a group of individuals that I also think is worth looking at, ...I guess there are three parts to it, one is the independent development of numerical analysis as an independent mathematical area of research, the other two, I guess, are the inner relationships between the development of numerical analysis and what computers came to be and conversely. The ability of machines to do certain things and their impact on development of numerical analysis. And I wonder if you have any thoughts on some of the major aspects of that?

ALT:

It seems to me that the influence is mostly one-sided: because we had computers, numerical analysts began to be interested in things which they otherwise would not have picked up. For instance, differential --partial differential equations, all the stability questions. ...As long as you compute by hand you're not much worried about stability [of] computations because if anything tends to get out of hand you would notice soon enough, but when you leave things to cook in a computer by itself you have to anticipate the growth of errors, which is an old word for stability. ...

So all these theories that grew up, partly at least, from numerical analysis, in large part in New York at the Courant Institute, ...about methods for integrating partial differential equations, for instabilities that were introduced only by the numerical methods and those that were inherent in the problems, of these two kinds, the effect of rounding errors and truncation errors and their different behavior all that was possible, was made interesting, by the existence of computers and wouldn't have been picked up otherwise. ...All studies of very large matrices, for example. George Forsythe excelled in that field. A number of people: Stiefel and Hestenes. All that. Many things become of interest when you have matrices of very large order, say 20 or 100 or something like that, which would have been of no interest to anyone before computers.

In the other direction, ...of course it's trivial to say that we needed numerical analysis, we needed the progress of numerical analysis in order to do more things with computers, but I think it's an over-statement. That is, ...people would have done things on computers with or without numerical analysis and they still do. Many problems are being solved on computers by people quite ignorant of numerical analysis. So they fumble around a little bit, and experiment, and run into trouble, and dig their way out of it somehow. Computers could well exist and be almost as well off without numerical analysis. It's nice to have it; it's better to have it; and one would wish piously that more attention were paid to numerical analysis, but I --I don't think that the development or the progress in numerical analysis had any influence on the development of computers. I don't think one can say that.

TROPP:

You wouldn't say that in order to solve certain types of problems that had --that were difficult ...in terms of their numerical procedures would have led to characteristics in a computer that wouldn't have been there otherwise? That what machines are able to do were going to be there regardless.

ALT:

I hardly think so. ...Ever since floating decimal points, for example, that --that's the last time that any concession was made, and that's so primitive that one didn't need numerical analysis to accomplish that. As a matter of fact, even the Bell Laboratories

TROPP:

Right.

ALT:

relay computers had floating decimal --

TROPP:

Right.

ALT:

points. No, I don't think that there are any design features of computers that were caused by numerical analysis, that --that wouldn't have been recognized [inaudible] development. And anyway that's not the bottleneck. The bottleneck is in the programming. Now there are or could be design features in computers that are intended to facilitate programming or to facilitate trouble shooting, diagnostics, ...There are perhaps a few and there could be many more, that would be highly desirable and would be a direction we ought to go. It is not overly likely that much will be done, because that isn't something that sells a computer. It's very hard to convince a customer that he's getting his money's worth if the cost of the computer is increased, say 25 percent, by added features which facilitate programming. That's so abstruse, so hard to understand that it's not likely to improve sales, and therefore it's difficult to convince manufacturers that should be done.

In the early days Aiken invented a coding machine or programming machine for one of his computers, either Mark III or Mark IV. He built a separate little machine to write programs. ...That also seemed highly a blind alley, highly a wrong concept, and it had been so labeled, so recognized by Von Neumann, whose --whose theory was quite the opposite, the computer should be very simple, very general purpose and you can do anything on a computer that can perform just the simplest operations fast enough. That is, Von Neumann would have said use the full size computer as a programming machine, and Aiken would have thought that's a waste, you don't want to tie up this big machine for something relatively simple. Nowadays I would think in the first place, programming is one of the most complicated activities and we use the entire computer. And in fact that's what we do. You see we now have compilers on the main computer that ...that do the programming, do assistant programming for us. And they are among the more complicated programs; a good - a good size of the portion of the computer's memory and time is devoted to compiling, storage of things, or diagnostics. One does need the full power of the computer for these activities and there still is not enough of it. ...There isn't enough because we don't --we don't know what to do, don't have good methods and that goes back to the fact that we are mentally not capable of seeing through the tremendous complications, all the consequences of a computer program. If you write a program of a few hundred instructions, simple program, it is already impossible to foresee all its consequences, all the conceivable circumstances. ...We don't know what to do about making programming safer, more foolproof.

TROPP:

Most people still tell stories of programs that have been around a decade and still have errors in them.

ALT:

Yes, yes, yes. I am conservative and I assume that after a program has been debugged and put into operation, ...say for six months after that one will still find troubles; that's what I expect. But I do find it happens 2 and 3 years after a program has been in operation, and if we don't find them any longer than that it's probably mostly because the programs are not used any longer than that. They go out of fashion, they become obsolete at that time. I know very few programs live that long. And so sometime before its death every program by definition comes to an end.

TROPP:

[Laughter]. There is one other aspect of programs that's interesting and as I look at some of the more contemporary programs, look at the problems in numerical analysis, ...for example, the problem of dealing with a large matrix inversion, extremely large one, 10,000 or so, ...devices that in a sense keep reducing this matrix to one in which you have great big regions that you don't have to worry about and one in which you will have negligible round-off errors in --in large regions, to get away from the problems that numerical analysis was designed to solve, ...ways of redoing the mathematical entity in such a way that some of those errors, if they occur, occur very infrequently and can be taken up as separate ...elements.

ALT:

I don't [inaudible].

TROPP:

Somebody was just showing me a program for a very large matrix ...in terms of what he called spikes and, and things, where you can take this large matrix and essentially reduce it to one where there are very few multiplications which will occur in the inversion that will lead you to round-off errors.

ALT:

I --I haven't heard of that. It strikes me as dangerous, because when you say "very infrequent" or "negligibly small" that's just what is so dangerous.

TROPP:

Right, right.

ALT:

But I don't know.

TROPP:

I don't know either. I had this explained to me in like 5 or 10 minutes with a quick sketch and a reference to a paper that I haven't had a chance to look up yet. ...But I wondered if this isn't one of the off-shoots of some of the difficulties of computation, to try to go around the corner or through a back door. If that isn't an attempt to offset some of the current weaknesses. But again, I don't know. I guess I just --I've lost touch with recent developments in numerical analysis and some of the problems that are still outstanding. [Pause]

ALT:

I don't --I don't know myself what the problems are in that area now.

TROPP:

How long did you stay at the Bureau of Standards?

ALT:

Until 1967.

TROPP:

Till very recently.

ALT:

Yes, yes. But even in the last few years there I had rather little contact with interests in what is called scientific computation or technical computations. More and more in information and auxiliary language handling, all kinds of non-numerical applications.

TROPP:

In terms of the --of the history of this whole field of computation, I guess an interesting period would be the Bureau of Standards prior to SEAC. Now, by the time you came, ...SEAC was ...conceptually thought about. Did people talk much say about the decade prior to SEAC and the kinds of computations that they were involved in and what they were doing and how they were getting them done?

ALT:

No. ...One could hardly identify anything like that. ...It wasn't an identifiable activity. It wasn't organized, wasn't talked about. People had assistants to do computing. If you asked anyone, in fact we did ask people, "what are your computing needs" and the word "computing needs" didn't ring a bell at all. They weren't --they weren't recognized. Of course, everyone picks up a piece of paper once in a while and does some computing with it.

The largest activity in computing that was isolated was the Mathematical Tables Project in New York, which was carried on under the supervision of the Bureau of Standards. ...That --I forget how many people they had here, maybe there were a hundred or something. ...That was a large computing laboratory. Other than that I am not aware there was --not aware of any organized computing.

TROPP:

So really you were building a whole new environment then as well as a whole --as a new machine. And environment that says --

ALT:

A service computer. A computer as a service operation. That was one of the interesting things. There was no precedent at all for that. We had to begin to find out how to charge people for time and how to keep books, how to keep track of what we were doing, how to acquire customers and how to predict the costs of a problem. We are still not very good at that.

TROPP:

[Laughter]. Do you have any recollection --and you've been in the --the field almost from the year one --of when the word "computer" started being used for a machine as opposed to a person who did calculation? [Pause]

ALT:

It had to occur --as long as --as far back as I can remember the words "calculator" and "computer" began to be separated at that time. ...One began to say "calculator" as something that does arithmetic, a "computer" does more than that; it does a sequence of operations, it does some logic. ...It occurs in the name of ENIAC, for instance.

TROPP:

M-hm.

ALT:

The acronym ENIAC has the word "computer" in it.

TROPP:

So that may have been the beginning of that separation, because prior to that, except for the Bell machines and the Harvard machine, there really are no functional machines that in a sense do more. I guess the differential analyzers do, but nobody called them computers.

ALT:

Nobody called them computers, but the Bell relay computers were computers. It was around that time. And a component in them was called a calculator. What we would now call the arithmetic unit was called the calculator.

TROPP:

It must have happened sometime around 1940-ish.

ALT:

Yes --yes. yes.

TROPP:

When that separation -

ALT:

But it antedates the electronic computer. [Pause]

TROPP:

People at Bell Labs ...when they used to solve network problems, used to have a bunch of girls working at these Monroes or Fridens or something, and the people who did them I think were called computers,

ALT:

Yes.

TROPP:

people who did the calculations.

ALT:

Oh yes, oh yes, oh yes. People were called "computers."

TROPP:

And this was in the thirties.

ALT:

There was one well organized computing laboratory in the British Admiralty. Well organized due to one man - [Pause]

TROPP:

Can't think of it; I should know the name too.

ALT:

Comrie,

TROPP:

I keep wanting to say Comrie, but he was - [Note: The conversation went on while tape was being changed. I mentioned some of L.J. Comrie's accomplishments in organizing (human) large-scale computation, such as the design of type fonts for mathematical tables.

[End of Side I]

ALT:

For easy visibility the numbers should not all sit on one line and all have the same height. ...They should have risers and descenders just as some letters of the alphabet. A nine should have a tail which drops below the line --

TROPP:

So that your eye can scan -

ALT:

and a few others like that.

TROPP:

the difference quickly.

ALT:

And then it's easier to read. Yes. It helps the computers look up numbers more quickly and with fewer errors. But nowadays the typewriter or --or print-out is designed from some ...so-called esthetic standpoint ...which means somebody likes its appearance, but there is no systematic knowledge involved on what's easy to use.

TROPP:

[Laugh]. What are --what are some of the other old stories or anecdotes that you remember about the early days that might shed some light on the kinds of things you were concerned with, some of the problems when you were at Aberdeen and your early days at the Bureau?

ALT:

At Aberdeen we had visitors from all over the world ...even while the war was still on, but primarily in 1946 --in 1947 ...Sweden, for example, England had people come to see what we were doing, to learn, try to ...do the same thing at home, and we had discussions about whether a laboratory should build its own computer as a bench project or whether they should be built by experienced electrical firms, industrial firms. And how big a country had to be before it could rate its own computer.

Sweden was, in the early forties, and just --

TROPP:

That was the BASK --

ALT:

I know that. Yes, yes one of the earliest computers was built in Sweden other than in England and in the United States.

TROPP:

That was the BASK --

ALT:

Another thing that is not widely known is that the British got there first. The first stored program computer ran in Great Britain rather than in the United States.

TROPP:

EDSAC, by actually 2 or 3 years. [Laugh].

ALT:

That was built ...by Wilkes ...in 1949.

TROPP:

Did you have any contact with BINAC while it was being built in Philadelphia?

ALT:

No, I don't think so. I knew about it and got some of the reports. I never visited there. I knew Mauchly and Eckert at the time, but I had no direct contact. And I have never quite found out what happened. It was shipped to California --

TROPP:

Shipped to California, to Northrop, never really got running.

ALT:

--and they had a great deal of trouble getting it going --

TROPP:

It never really got running.

ALT:

But one may suspect that it was not the fault of the machine, but ...it didn't have the right sort of people who knew how to use it, knew how to maintain it. Perhaps it was delivered prematurely. I don't know.

TROPP:

It's hard to tell because the people from Northrop did spend considerable time out here during the building of BINAC and were supposedly trained in its use and maintenance. And ...there --there was a team of people that spent considerable time in Philadelphia, from California. And --I don't think anybody really knows why it didn't work. Whether it was conceptually something that just couldn't have worked.

ALT:

It antedates UNIVAC but by a short time. I wouldn't be surprised --I wouldn't understand why there was a great deal of difference between the two machines.

TROPP:

One of the problems was that it was, I think, part of the SNARK project, and it was supposed to fit into a bomb bay door and everybody knew that it was never going to be airborne. But it was supposed to be designed as an airborne computer.

ALT:

This --this 1953 list says that BINAC was first ready for use in August 1949.

TROPP:

M-hm.

ALT:

That's not terribly long before the first UNIVAC.

TROPP:

Right, they were almost simultaneous.

ALT:

...1951.

TROPP:

Right. I think BINAC was started earlier.

ALT:

Would you say it was --it had stricter requirements which made it hard to make it reliable?

TROPP:

I'm not sure I have that particular report. Maybe we ought to put the number down on it. It's a Survey of Automatic Digital Computers,1953, Office of Naval Research, Department of the Navy.

ALT:

That was the first in a series of such reports.

TROPP:

Prefaced by Captain Hart. I was just looking to see if they had any number on this, because I --I don't have a copy of it and it would be of interest. I have earlier ones that were put out by the Ballistics Research Laboratory. [Pause]

ALT:

The earliest, I think [walks across room]

TROPP:

Yes, that I --yea, that I have. And, excuse me, that's earlier.

ALT:

Do you think so? December 1955. That's the first one.

TROPP:

'55. Oh, that's the first?

ALT:

Yep. Yea. The second one is 1957. The third one is still later.

TROPP:

Aha. Well, this I do have.

ALT:

Yea. The Navy report is the first one, and now that we see it, I think the Navy did not produce any more because Aberdeen --

TROPP:

Aberdeen took over.

ALT:

was coming out with their report.

TROPP:

I wonder if I might borrow this, Xerox it and return it to you

ALT:

Oh yes.

TROPP:

because I don't have a copy of this. And there are some expository surveys. For example, before the Bureau got involved in building its machine, Jim Wakelin had done a survey and he --but it's written in expository form and this would be 1945, '46. And this led to his advocating and Condon's later advocation of --

ALT:

That's a very interesting thing that I didn't know anything about. I know the name Wakelin comes up there, one of the first books on computers --the first book on computers.

TROPP:

Right. The ERA publication.

ALT:

Yes, yes. And Wakelin was involved with ERA; I never quite knew how that came about.

TROPP:

Well, he was --he was involved.

ALT:

He was in the Navy.

TROPP:

He was in the Office of Naval Research or NDRC, I've forgotten which now. But --but he did a survey and he went around the country to find out who was doing what. And this would be in the '46, '47, '48 era. It would be, I guess the latter half of the forties, because that survey is kind of repeated in the book you're talking about. And it may have been as late as '49 because this material doesn't differ substantively from that first book on computers which has a '51 publication date.

But I keep thinking there was an earlier one. And I have a copy of it. It's an expository report describing –

ALT:

Was John Curtiss involved in that? Did he work for Wakelin, maybe?

TROPP:

I don't know if he worked for him or side by side with him, but they --they did work together. And, let's see, Condon is one of the gentlemen and Ernest Riaveck is another name that comes in there.

ALT:...

TROPP:

Yeah, but these are some of the names of some of the people who were --who were advocating ...the development of what --well, it had to be NDRC, because one of the things they were advocating was ONR. And the eventual ...development of what is now the National Science Foundation. ...The proposals appear in some of their early documents, in the postwar period,

ALT:

Oh, yes.

TROPP:

in the postwar period. But one of those documents is a --an expository survey of what's around and what's coming.

ALT:

The National Science Foundation is in some sense an offshoot of the old Office of Naval Research. Waterman, the first director, had previously been with ONR and went to the Science Foundation from there. And in many ways we on the outside could see how the tradition of ONR was transplanted to the National Science Foundation.

TROPP:

Right. Because the original tradition of ONR was not to tell people what to do but to say: if you've got interesting things, submit them to us. It was a kind of a puritrose to what the National Science Foundation was in its early years. And the two looked very much alike except for the name and the affiliation. ...But I would like very much to borrow this and return it to you.

ALT:

By all means.

TROPP:

I'll --I'll xerox a copy because some of these are hard to get at this point in time. And I didn't realize that this work of --was it Wick?

ALT:

Weik.

TROPP:

Weik was that late. And --the surveys are interesting in that they give you a kind of a state of the art at a given point in time,

ALT:

Yes.

TROPP:

a year or so preceding the publication date.

ALT:

Do you know about the ONR Digital Computer Newsletter?

TROPP:

No.

ALT:

That was a periodic publication --publication isn't the right word. It was a series of quarterly reports.

TROPP:

M-hm.

ALT:

...If you give me a second I can find out how far that goes back.

TROPP:

M-hm.

ALT:

Because when we started the Journal for the Association of Computing Machinery in 1954, we made arrangements for including the Digital Computer Newsletter as a supplement to each quarterly issue of the Journal. But you see the first volume of the Journal to appear is dated January 1954 and the appendix to it, the supplement to it, is Volume 6 --

TROPP:

Six.

ALT:

--of the Digital Computer Newsletter.

TROPP:

So that would make it 1948 or 49.

ALT:

And that would make it 1949 for the first volume. I didn't realize they went that far back. That's very surprising. I don't have any of those old copies. That would be very interesting to get hold of them.

TROPP:

...Would you have any idea as to what [is] the best place I could search for that? I may have a few isolated ones.

ALT:

In the first place the Office of Naval Research Computer Branch still exists; I believe.

TROPP:

You think they would probably have those on file?

ALT:

...[Walking. Pause]. One of the old timers in there was Mr. Goldstein. [Pause]. I don't see his name here. ...I can't think of his first name although I knew him very well in Washington. He's quite possibly still there. I would go to the Computer Branch of the Office of Naval Research --and see him.

TROPP:

And they're in Washington?

ALT:

In Washington, Office of Naval Research, yes. ...It was discontinued, the Digital Computer Newsletter was discontinued, oh, one day we can find out when. I think we find it in the Journal to he end, that will tell us something. Oh, we didn't have it. Collaboration with the Journal as --was discontinued at some time, I don't remember why. Then it continued to exist for a while and ...went on separately, and in the end they found that there was less need for it now, the field had become so well established that the Navy didn't have to provide this service any more. I don't know when. In the early sixties maybe.

TROPP:

That will be an interesting source to look through.

ALT:

Yes. Yes, I should say that would be very interesting.

TROPP:

Yes.

ALT:

That seems to go back to early 1949. Ah --Gordon Goldstein, Gordon Goldstein.

TROPP:

Goldstein. -S-T-E-I-N?

ALT:

Ya; ya, that's correct. ...I can't be sure that he was there from the very beginning ...but he got into the game quite early. I think in 1954 when I negotiated with them about ...including the newsletter in the newly-founded journal of ACM. I think it was Mr. Goldstein to whom I talked. ...

The earliest chiefs of the Computer Branch, or, members of the computer branch were C. V. L. Smith and A. E. Smith. They both are still around Washington.

TROPP:

Yea, in fact I know C. V. L. Smith is around.

ALT:

Yes;

TROPP:

A. E. Smith I don't know.

ALT:

A. E. Smith, I just saw him at the --at the [chuckle] at the Computer Conference in Boston last month.

TROPP:

Aha. But they shouldn't be hard to find then. They'll be listed in the ACM membership list.

ALT:

...C. V. L. Smith is, you say you know where he is? He is either with the Atomic Energy Commission or with NASA or some place like that. One of them.

TROPP:

...Don't tell me. He's right there, right on the periphery of Washington; I'm trying to remember the name of the place now.

ALT:

He's easy to find. [Pause]. Those were both in the old Mathematical Sciences Division of the Office of Naval Research of which Mina Rees was the head, and the Smiths were under her. ...very important to her. She had a mathematics branch and a computer branch and a statistics branch. In the computer branch were both Smiths. ...They might know something. They might even have copies of the early newsletters.

TROPP:

...I --I'll be seeing Professor Rees while I'm in New York, and --

ALT:

That's a good source, of course.

TROPP:

Perhaps you can tell me some of the areas that I ought to be asking her about. Now ONR and that whole period would, is obvious, is clearly one. ...Their supervision of some of the early computers.

ALT:

Yes. She --she awarded the contracts, for instance for the Institute for Numerical Analysis. That was entirely --

TROPP:

Whirlwind?

ALT:

...Whirlwind is another one. Yes, that was one of her projects. ...But the Institute for Numerical Analysis, that was an idea jointly between her and John Curtiss.

TROPP:

Aha. So she awarded the contract then for the SWAC?

ALT:

I should think so. Although the Air Force was also involved in that. But I think it was mostly Mina Rees. ...During the war she was working with the National Defense Research Council, I think for Warren Weaver. But that antedates computers. There wasn't --there we weren't really concerned with computers. But she would undoubtedly remember about Aiken, Stibitz, and Von Neumann era,

TROPP:

Right.

ALT:

and the activities of that committee. ...She got into the Navy after the War. She was with the National Defense Research Council during the war, then joined the Navy, joined ...the Office of Naval Research about the time that it was founded out of the old Office of Research and Inventions. That was its predecessor in the Navy.

TROPP:

All of those originally come under Bush during the War, weren't they?

ALT:

No, I don't think the Navy Installation --Navy's organizations did.

TROPP:

They weren't.

ALT:

I don't think so. Bush was head of NDRC, presumably. I think that's true, but that was not the Navy.

TROPP:

I see.

ALT:

That was --be reported directly to the President.

TROPP:

I see. [Pause]. It would be interesting to talk to her about that.

ALT:

Yes. About the contributions of Von Neumann. She might know something about the priority questions that have come up. She knows a bit about the Bureau of Standards Mathematical Tables Project in New York. She had something to do with financing of that organization.

TROPP:

Very good. [Pause]. You mentioned that --the sort of counterpart of INA here in New York, the Courant Institute. The Courant Institute came along much later.

ALT:

That was also a Mina Rees

TROPP:

Was that also?

ALT:

organization. Yes, she --she put that one on its feet.

TROPP:

When, when did the Courant Institute?

ALT:

Well, it was started before that but not under the same name. But Professor Courant came to New York, oh maybe in 1937 or '38, and became professor at NYU and established a circle of applied mathematicians there, not yet an Institute of its own. I remember talking to him in 1938 shortly after I had arrived. ...I told him that I had been preoccupied for a while with mathematical economics and he said yes, that an interesting area, and he had given some thought to getting into it himself.

TROPP:

[Laughter].

ALT:

...But I know that the Courant Institute was --or, or, or that Courant's group at NYU was greatly strengthened by ...grants from, coming from Rees's organization. The Navy. That was really the starting point. That made them what they are now. [Pause].

TROPP:

That was a ...a conceptually a different kind of applied mathematics group.

ALT:

Right, right. They got into computers later on. Originally they were theoretical applied mathematicians. Paper-and-pencil applied mathematicians. ...In one way they got in touch with computers when the Atomic Energy Commission established its computing facilities at NYU. Neighboring to Courant Institute and I think with some joint management. ...And that --I don't know when that happened; I would say maybe 1952 or 3.

In another sense one of their staff members or one of the NYU people, Fritz John, joined the Institute for Numerical Analysis for a year as its director. I should know which year, I think '51 to '52 perhaps, plus or minus one. ...We hired Fritz John as Director of the Institute for Numerical Analysis. Up to that point he had known rather little about computers, but he had --well, you see, that --that makes it very clear: the Courant group in the early days was interested in partial differential equations, but not terribly specifically in numerical methods.

TROPP:

M-hm.

ALT:

Perhaps that came in somehow, but mainly they were interested in partial differential equations. [ ] and I don't know what the words are in that area. And then at some point they became interested in numerical methods and in stability. Richtmyer was there --and around that time Fritz John went to INA in Los Angeles, California and became --and, and became even more interested in computers because the computer was there and because he could put things on and see what sort of problems could be attacked. And he then really specialized in the numerical approach to partial differential equations. For a while that was the great contribution of the Courant Institute.

TROPP:

He returned to --?

ALT:

He returned to New York. After a year, yes. ...As a matter of policy, the directorship at the Institute was rotated on a 1-year basis. Each director was hired for one year and then went off again, went home again. On the other hand there was under him a small permanent staff, especially as I say, Forsythe and Wasow --Forsythe, Wasow and later on Motzkin and --I'm not quite sure of the other names. ...And then there was a very large number of visitors, many during the summers and some for semesters or years ...that came and went.

TROPP:

I think that idea of a rotating director is an interesting one and I wonder what brought it about? What was the thinking that led you to --what today looks like a very good management decision?

ALT:

I think it was intentional in order to get fresh blood all the time. It may have been sour grapes, I'm not sure. Perhaps they were unable to hire someone permanently, and so they settled for this. But I think we talked about it from the start as something that had many desirable features.

I doubt it would be possible to find a list of successive directors. I don't have it.

TROPP:

I'm sure it's --

ALT:

I know that Hartree was the first, from the summer of '48 to '49. ...I think Fritz John was the third from '50 to '51; and then came Lehmer - Derrick Lehmer, and he stayed for at least two years. By that time we were glad to have somebody ...and --correct '51 to '53. By the summer of '53 I had a great deal of direct personal involvement with INA. ...In my early years at the Bureau I was mainly concerned with the Washington installation. We had four sections. INA was one, ...out on the West Coast. Washington Computing

Laboratory was two, it was second to this; it was mine, I was then its Deputy Chief of that; John Todd was Chief. The other two were small. A statistical engineering laboratory; and a ...a fourth section which played different roles, for a while it was called Computing Development, it was a small group.

My interest in the early years was the Washington Computing Laboratory. ...We maintained a lot of contact with INA. We exchanged ideas, we exchanged personnel, we visited back and forth. ...There was a slight feeling of ......intellectual competition. Trying to be as good as the other --

TROPP:

Ah ha, ha.

ALT:

and maybe we were a little more business-like, they were a little more scientific then we were. We tried very hard to be --to, to come close to their reputation.

But around 1952 John Curtiss, Chief of the Division, left for various periods of time visiting some place. ...It wasn't exactly a sabbatical, but I think he was on leave of absence for a time, went to NYU for a time. More and more I became Acting Chief of the Division. Especially we had a very unpleasant crucial period somewhere between '52 and '54 and for a good portion of that time I was Acting Chief of the Mathematics Division. Unpleasant because oh, it was a period of very severe cuts in funds, ...re-organization. It was around that time --the end of that was that we gave up the Institute for Numerical Analysis, transferred it to the management of the University of California.

It had --very indirectly it was connected with the ...Eisenhower presidency, and in that --in that era it became less easy to operate a scientific laboratory directly in Civil Service.

It became more --it became easier to do --to transfer it out on contract, to a university, for example. That was one of the reasons for that, but that --that whole transition was a very difficult and unpleasant thing and –

TROPP:

That was also the period of the witch hunts in the academic world.

ALT:

That was about the same time, yes.

TROPP:

Places like Berkeley were being decimated, particularly in physics at that period of time.

ALT:

Yes.

TROPP:

The loyalty oaths in the state of California occurred about that time.

ALT:

That may have been one of the contributing factors. It was generally considered unpleasant to be in Civil Service. So the people at the Institute for Numerical Analysis, I don't think unanimously, but the --the prevailing feeling was that they should not remain in Civil Service.

Yes, Lehmer was Director until '53 and then C. B. Tompkins, who had been a staff member, became Acting Director for the last year and then took over the Institute when it was transferred to the University. Tompkins had come to the Institute in '52 as a permanent staff member, from the George Washington University Logistic Research project in Washington.

That nearly gives us a complete list of directors

TROPP:

Right.

ALT:

except for one year that I can't remember. '49 to '50. It might have been --it's likely that John Curtiss was Acting Director during that period I have in mind. That's what I seem to remember.

TROPP:

You said you became heavily involved with, with INA about 1953.

ALT:

'2, yea '52 or '3, something like that, because John Curtiss was increasingly away. ...He finally resigned from the Bureau of Standards, I don't remember exactly, maybe '53 or something like that, but even before that he had been away for sometimes a semester at a time. More and more I was Acting Chief of the Division during that period.

The person who had been second in command, Ed Cannon, was on loan to the George Washington
University Logistics Research Project, where Tompkins had just left to go to California.

TROPP:

[Laughter].

ALT:

Cannon, in a game of musical chairs, took over as director of that project.

TROPP:

Aha.

ALT:

It was in Washington, D.C. That is, from the summer of '52 for about three years Cannon was there. During that time whenever Curtiss wasn't there I was Acting Chief of the Division. Then Cannon finally came back.

TROPP:

Did you have to spend a lot of time on the West Coast during that period?

ALT:

...Not a lot of time. I visited several times. But I had to spend a lot of time on their problems.

TROPP:

[Laugh]. In the --in their early years, before they became part of the University of California, how closely were they tied to the academic, intellectual world?

ALT:

Oh, they were on campus and there were a number of joint appointments. ...Hestenes and --[inaudible] were professors at the University and at the same time staff members of the Institute.

TROPP:

So it wasn't then --

ALT:

I can't remember --he was chairman of the mathematics department for a time. But at the same time a staff member of the Institute.

TROPP:

That's a name I should know too. [Laughing].

ALT:

I --I'm poor at names these days.

TROPP:

I think you're doing very, very well.

ALT:

...Yes, there was a good deal of contact between the mathematics department and the Institute.

TROPP:

How was the decision made to then put it on the campus in Los Angeles as opposed to Berkeley?

ALT:

I don't know. Ask Mina Rees, she will know.

TROPP:

Fine. Did, in --in terms of the mathematics at Berkeley --

ALT:

Berkeley was a real department, yes. By far, yes, yes, yes.

TROPP:

Berkeley may not have been quite as heavily oriented towards applied mathematics at that period of time. I'll have to ask her that question.

ALT:

Probably --probably that's the reason. Or it may be that UCLA had more space, better facilities.

TROPP:

In the postwar period everybody was pretty bunched for space.

ALT:

Another thing was closer proximity to other projects. The big aircraft --companies were down there --

TROPP:

That's --that's what I wondered.

ALT:

--big computer developments were down there. Another thing was --I, I know that one of the considerations was right from the start they planned to have summer institutes there. They planned to invite people to spend summers there, and they may have been looking for just physically pleasant surroundings,

TROPP:

M-hm.

ALT:

which was more easily, ...which was more easily found in Los Angeles. I know that was one of the considerations in establishing it in California.

TROPP:

M-hm. It's --you know it's difficult thinking back in time to the days before jet aircraft and the ease of flying cross-country to imagine the Bureau establishing its largest computational facility 3,000 miles away.

ALT:

Well --it wasn't the largest and SWAC was intended to be less a service computer. The East Coast computer, SEAC, was meant as a service to the Bureau of Standards and other government agencies.

The Air Force was one, Atomic Energy Commission. I think AEC never put any funds into it, but almost from the start preempted all of its time. The Air Force had financed building the SEAC and demanded a portion of its time. The purpose of SEAC was to be a service computer mostly. The purpose of SWAC was first of all to be an experimental tool for the numerical analysts out there, for numerical analysis research. And then if there was any time left over it was to be a service mostly to Air Force West Coast Contractors and the Air Force financed part of its building for that reason. So you might say that was one subsidiary reason to locate the Institute in Los Angeles.

TROPP:

The AEC usage is interesting because at that time they were funding their own computers at Argonne, at Oak Ridge, at Los Alamos.

ALT:

Yes, yes --yes and those were all slower in being completed.

TROPP:

They were modeled after the Institute for Advanced Study Development.

ALT:

Yes. People from Los Alamos first --like Metropolis --first came to Aberdeen, and then, the moment SEAC was completed, they came to us in swarms and demanded a very large portion of SEAC's time and they had their fights with the Air Force, and we stood by.

TROPP:

[Laugh]. And so until they got MANIAC then they were a heavy user of SEAC.

ALT:

Yes.

TROPP:

People at Argonne got AVIDAC and Oak Ridge got ORD --ORDVAC?

ALT:

ORACLE.

TROPP:

ORACLE, ORACLE. That's right. And --that was just built, I guess at Argonne simultaneously with their machine. The two were built almost together.

ALT:

I thought it was --wasn't it built in Oak Ridge?

TROPP:

No, it was built in --that was the one that Alston Householder was involved with.

ALT:

I suppose that is somebody else whom you might contact.

TROPP:

Yes, I have contacted him and I --through the offices of SIAM --am interested in this question I raised earlier of the, kind of a history of numerical analysis as well as the impact relationship that we talked about. And I'm hoping to get Alston involved in this study now that he has sort of semi-retired and I'm using the offices of the people in SIAM to try to get him involved. [Pause].

I've taken a good deal of your time and I've really picked your memory for all kinds of things.

ALT:

Oh -

TROPP:

I'll turn the machine off.

[END OF INTERVIEW]