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Interviewee: Russell Kirsch
Interviewer: Richard R. Mertz
Attendee: Robert Elbourn
Date: October 8, 1970

MERTZ:

The following is an interview conducted on the eighth of October 1970 by Dr. Richard Mertz at the National Bureau of Standards with Mr. Russell Kirsch, one of the engineers involved in the design and development of the SEAC computer. Would you like to describe your early background and training and what led you into the field of computers?

KIRSCH:

Yes. My own interest in computers, as I was just telling you, has evolved fairly widely over the whole spectrum of computer activities ranging from actual soldering of computer hardware to abstract studies of theoretical capabilities of abstract computers. And, as I understand from the preliminary discussion that we've had, you would prefer me to concentrate on the early history, especially that part which is connected with the development of, early developments in computer hardware. Presumably early developments here at the Bureau of Standards that I was most intimately connected with. My own connections with the computer activities started in 1950 when Peter Elias at Harvard gave a seminar on computers, and I was a graduate student there at the time. This was a seminar in which each of the students reported on some activity in the computer field, and of course the computer field was barely in existence at the time. For essentially no particular reason I happened to agree to report on the, what was, I believe, at that time called the NBS Interim Computer which some people up at Harvard believed there was under construction at the Bureau of Standards at the time. For this purpose I got from Scudder McDowell, who was an Air Force officer stationed at Harvard, and Elpert Little, I believe, who may also have been in the same status, I got three or four blue dittoed sheets that gave block diagrams of this computer that was purportedly under construction at the Bureau of Standards.

MERTZ:

This was in 1950?

KIRSCH:

This was in 1950, yes. I can document the actual date on that because I think I still have

that around someplace. These four or so dittoed sheets were all the documentation that existed on this machine, and they included a block diagram. In fact, they included a five-block diagram which is the canonical diagram of computers. Taking this as a starting point I figured out not how the Bureau of Standards, I reported not on how the Bureau of Standards computer works, but on how such a computer, given such a block diagram, would have to work. I remember presenting a seminar on the, what was then, I believe, being called the SEAC computer, SEAC standing for Bureau of Standards Eastern Automatic Computer, later on Standards Electronic Automatic Computer. "Eastern" was to distinguish it from another effort going on at the West Coast part of the Bureau of Standards, at the Institute for Numerical Analysis in Los Angeles. That ultimately yielded the SWAC computer, that's S-W-A-C.

MERTZ:

If I may just backtrack a second, you were a graduate student in engineering at Harvard?

KIRSCH:

Yes. I was a graduate student in the Department of Engineering Sciences and Applied Physics at Harvard.

MERTZ:

Aha. I see. And how had you--if we might got back even earlier, you might want to describe very briefly your secondary school before you went on--

KIRSCH:

Oh yes. I received my scientific training at the Bronx High School of Science in New York City. Subsequently I attended various universities at which I received no scientific education whatsoever, including New York University where I got a Bachelor of Electrical Engineering degree, and then Harvard, where I got a master's in the Engineering Science and Applied Physics Department. And then, some years later, MIT and various local universities in the Washington area. But my last formal degree was from Harvard in 1951.

MERTZ:

And it was this seminar that was your actual first introduction to computers.

KIRSCH:

That's right. Yeah. My more immediate connection with computers beside that seminar came from an interview I had, during the end of the semester sometime I had received a job offer from the Naval Research Laboratory, and I flew down from Cambridge to Washington to be interviewed for the job at the Naval Research Laboratory. But, as I

recall the situation, I got into Washington somewhat ahead of time for my appointment and I thought it might be interesting to go out to the National Bureau of Standards and see this computer and verify whether or not it indeed was doing the things that the block diagram said it might do and that I had conjectured it does do. And when I got out here I had a very interesting time with Sam Alexander and Al Leiner and Ralph Slutz and some of the people who were only far distant mysterious names to me at the time. And, as a matter of fact, we had such an interesting time that I remember canceling my appointment at the Naval Research Laboratory and I don't believe I got to the Naval Research Laboratory for many years, until many years later. Sam Alexander gave me a job on the spot, and I went to work at the Bureau of Standards where, shamefully enough, I am still employed.

MERTZ:

You were just finishing up then at school?

KIRSCH:

That's right, yes.

MERTZ:

So that this all came about roughly at the same time.

KIRSCH:

That's right. I started here at the Bureau of Standards in September of 1951, which is, unfortunately, a long time ago.

MERTZ:

Then you were working with Leiner and Slutz and Alexander?

KIRSCH:

Yes, there was a very small group. Essentially everybody was working with everybody. And the group included Sam Alexander, Ralph Slutz, Al Leiner, Bob Elbourn, Ruth Haueter Cahn [Recorder off]. To continue with the people who were present at the Bureau of Standards when I got here, at the risk of repetition: Al Leiner, Sam Alexander, Bob Elbourn, Ralph Slutz, Ruth Haueter--H-A-U-E-T-E-R--who later became Ruth Haeuter Cahn, married Leonard Cahn who was also here on the project, Richard Witt, Dick Witt, I've probably forgotten the most important person, but, I guess, that's all I can remember offhand.

MERTZ:

Was Ida Rhodes?

KIRSCH:

Yes, Ida Rhodes, of course. The organization of the activities here at the Bureau of Standards was into two separate groups, a mathematics group and an engineering group. And I really just listed the people in the engineering group. In the mathematics group, however, there was another entirely separate group of people, and a large group, too, including Ida Rhodes; let's see, who else?

MERTZ:

Her boss is still here, I believe.

KIRSCH:

Let's see. Well, now, her boss, at the time, I believe, was Ed Cannon, who is now my boss.

MERTZ:

Ah.

KIRSCH:

Yes, that's right. I'm not sure whether he was her boss at that time in 1951, but, I guess, he probably was. I'm not entirely sure of the timing of that.

MERTZ:

And his boss was?

KIRSCH:

John Curtiss was his boss. I'm not exactly sure of when Cannon came.

[Recorder off]

Yeah, this is just a reminder of when you talk to Margaret Fox to find out whether the correspondence date file which was kept in the old computer division still exists for some of the early years since it's a rather compendious collection of the correspondence activity and quite complete, as far as I can recall.

MERTZ:

Do you happen to recall offhand whether or not there was any vital document program

where they put on microfilm such things as the diagrams of the circuits for the SEAC? This was true of some computers of roughly that era, vintage. The program that the government had to preserve vital documents associated with different things. They photographed on microfilm, for example, the entire sequence of block diagrams and schematic diagrams for ILLIAC and a couple of others. Which is another way of attempting to retrieve drawings in a very convenient form for us because of the microfilm.

KIRSCH:

I certainly don't remember anything of that sort being done with the SEAC. In fact, I'm pretty sure that there was no documentation of the SEAC drawings. The reason I say this is that I remember going to considerable effort putting together a complete set of working drawings myself, which I had, and which I protected very carefully, because I knew there was no other such extant copy. But where that exists right now I don't know. One thing I do have--which this reminds me of, and I bet I can find it in some of my old files right now, is the logical drawings of what we called the Army computer. Do you remember that, Bob? That was the one built up at, I guess, at Ward Circle by NSA by, let's see--

MERTZ:

This is Abner? The computer called Abner?

KIRSCH:

I don't remember it that well, whether it was Abner or not, but it was very close in style to the SEAC computer, and I had a set of working drawings which were paradoxically not at all classified. I bet I could find those modular diagrams. ... Yes, Abner was the name, wasn't it?

ELBOURN:

The name Abner rings a faint bell, but I--

KIRSCH:

I know we called it the Army computer.

MERTZ:

Well, there was an Army project and a Navy project. At the time they had several computer projects. Abner, I believe, was the Army project.

[Recorder off]

KIRSCH:

Let's see. We're picking up now from where I was listing the various people who were here in 1951. Among the people who were concerned with the construction of the computer and the operation of the machine there is also Sidney Greenwald who should be listed along with the other engineers. Then, among the people in the mathematics division, there is some question as to whether John Curtiss was still here at the time, in September 1951, when I arrived. And there was Ed Cannon, who was the head of the mathematics activity. Then Ida Rhodes and Joe Levin who was the head of the group most intimately concerned with programming. And then a very large group of people who were programmers and mathematicians and I wouldn't pretend to be able to remember all those names. They have virtually all gone away from the Bureau of Standards over a period of years, but a couple of them are still here, and I can remember some of those names, of course. There is Irene Stegun who is still here at the Bureau of Standards, Ida Rhodes, Ruth Zucker, Ed Cannon is still here, Margaret Fox was the administrative assistant to Sam Alexander, who was the head of the group of engineers. She is still here. Ethel Marden was one of the mathematicians and she is still here. That's the extent of the list, I guess, of the people who are still here. A very large number of people who are elsewhere. ... Another thing, while we are listing names, I won't try to list these either, but a useful thing to make note of is the fact that there were quite a few people working here on the SEAC computer from elsewhere. That is, they were people who were not really at the Bureau of Standards, but who were connected with various projects. For example, a project at the RAND Corporation, a project at the Atomic Energy Commission, and these people did indeed spend a great deal of time here, using the SEAC computer, and, in those days, were effectively indistinguishable from the staff at the Bureau of Standards. But they never really were administratively affiliated with the Bureau and are nevertheless part of the de facto alumni association.

MERTZ:

Were there rosters kept of the people who were active at different times on the ...but not Bureau of Standards employees?

KIRSCH:

No, as far as I know there were no such rosters. The nearest thing to that that I could think of that would ever have existed and very likely would not exist any more would be a log of all the operations on the SEAC computer which was kept fairly scrupulously and did include the names of all the people who were running on the computer. And in that type of log you would find names like Wesley Mehlan, who is the head of Systems Development Corporation. Freddy deHoffman who is Vice President of General Atomics Division of General Dynamics, ...and, again quite a few people who were independent of the Bureau but did spend a good deal of time here, so on the list of names you really have to somehow or other dig out some of these.

MERTZ:

Excuse me. Do you think that possibly there is any place around the Bureau of Standards where that log might be? That Margaret Fox would know?

KIRSCH:

A person who might possibly--Margaret Fox might be one. Another person whose name now I also recall as part of the engineering group was Phil Shupe, who was one of the people who were in charge of maintenance of the SEAC computer, and he is still here at the Bureau of Standards and he may remember where some of those records exist, if they exist at all anyplace. That would be rather hard to dig out and I would say it probably wouldn't be worth your efforts to try. Let's go on with additional stuff.

MERTZ:

Yes. When you came you had, if I might backtrack just a little bit, you had had a little bit more exposure to problems of electronic computation or electromechanical computation than the one seminar.

KIRSCH:

Right. I failed to mention the fact that there was one additional activity at Harvard, a course that Howard Aiken gave on the organization of large-scale computing machinery, which I had taken, I think, concurrently with the seminar that Peter Elias gave. That one was more ostensibly on computers and was the only other exposure that I had, with the possible exception, and this really shouldn't be included, a course on the switching technology in radars given by Harold Mimno at Harvard. I guess one must say that that did in fact influence my thinking at the time, since going over to the Signal Corps SCR 584 radar was enough to impress any young person at the time with the fact that one could construct large systems out of electronic components. And perhaps other people were influenced, as I was, but that was the extent of the direct computer influences that I got at Harvard; the three activities being Elias' seminar on computers, Aiken's course on computer organization, Mimno's course on radar systems.

MERTZ:

In connection with the Aiken course, was this largely a descriptive course or did you get involved in doing any actual work with--

KIRSCH:

It was entirely descriptive, and, in fact, as I recall, it was mostly taken from the Harvard Annals book which was just appearing at that time, The Synthesis of Electronic Computing and Computing Controls. And that was the textbook, and essentially the main purpose of the course was to follow that textbook with particular emphasis on the Harvard types of techniques for Boolean minimization and design of switching functions the arithmetic, algebraic techniques that Aiken used at that time.

MERTZ:

Did you have any familiarity--did you get involved with the machine itself, there?

KIRSCH:

As far as I can remember there was essentially no involvement with the Mark I or the Mark IV which were both in the computer lab at the time though there were other people who did become more involved. I can recall no involvement with the machines as such.

MERTZ:

Did part of his class involve anybody going down and taking a look at it or seeing how it worked?

MERTZ:

Well, the machine was displayed out in an open public area, so you couldn't help see it, but there was very little actual laboratory experience, none at all as far as I can recall. There was some laboratory experience that other people did have. There was a course given in numerical analysis in an office right next to the main computer room in which there was a lot of desk computer work done by students, and some of them did in fact make use of the Mark I or possibly the Mark IV. I didn't myself.

MERTZ:

I was going to ask you if you'd had any courses in numerical analysis.

KIRSCH:

No, no, I did not. Though that was offered at Harvard at the time. Many of the students who stayed there for several years became exposed to that. Some of the alumni of Harvard in fact were the ones who had taught that course, Bob Ashenhurst and Peter Calingaert later on taught that course, and subsequently taught that at other universities where they went. Should we check and see if—

[Recorder off]

These comments will sort of be a continuation of what I was saying before, with an attempt to cover certain specific topics. You asked me to say something about the general climate at Harvard. In particular, you asked whether there was any contact that I can recall, competitive or otherwise, between Harvard and MIT. And, as of about 1951, I can recall no contacts at all between the groups at the Rad Lab and at Harvard. My experience was that that was insularity but that may not be representative of what everybody else thought at the time. The other things having to do with the general climate

at Harvard, I wish that I could say that there was a great deal of excitement about computers and things of this sort, but that really wasn't the case. The first excitement over computers that I can recall was here at the Bureau of Standards when I got here. So this again is rather impressionistic judgment. The emphasis as I recall at Harvard in 1950 was in treating computers as essentially objects of mathematical study. That is, there was a great deal of admiration at the time for what the Soviets were doing in switching theory, the emphasis being on switching theory. It was a respectable theory and one could use respectable mathematics in studying it. It perhaps is not too much of an inferential jump from that to the conclusion that the grubby details of the practical construction of real machines was less favored by the people of Harvard at the time than were more theoretical questions like how many switching functions in n variables there were; what systematic methods could be developed for synthesizing switching functions with a minimal number of switching components; with a certain amount of grudging acquiescence to the fact that the criterion of minimalization might have to be one of counting grids in vacuum tubes or counting diodes in switching networks rather than a more pristine mathematical characterization of minimalism. So this is the general flavor of the work as I remember it at Harvard, mathematical in its orientation, abstract in its preferences, practical in its degree of acquiescence to the realities of the construction of real computing machines. ... Let's see. When I got to the Bureau of Standards I was, since I had been trained as an electrical engineer, my assignment was, at first, to work on the construction of what we called an out scriber, o-u-t-s-c-r-i-b-e-r. This was to be an imitation of the device that was built by the EDVAC group for transcribing data from magnetic recording media to punched paper tape. The technical problem in doing this as we were doing it was to take a magnetic wire cartridge which was one of the two magnetic storage media that we had on the SEAC computer, to take the magnetic wire cartridge and run it slowly enough that the paper tape punches that we had at that time which punched out ten characters a second, or it may have been as little as six characters a second, that these paper tape punches could keep up with the rate at which the magnetic medium was moving. The technical problem involved in doing that, of course, was that the magnetic medium would have to move slow enough that the potential generated in the reading head was very, very small, and the signal to noise problem became something of a technical engineering feat to overcome. Which we did with the out scriber, and we were able to transcribe from magnetic wire recordings to punched paper tape, using a special nickel alloy wire which gave a somewhat higher signal than the original steel wire which had been used in the recording cartridges, which were modified from office dictating machines that we used on the SEAC computer.

MERTZ:

Were these Dictaphone machines, or do you recall?

KIRSCH:

Well, they were made by the Pierce Recording Company.

MERTZ:

And once they introduced this new nickel alloy wire did the company produce them?

KIRSCH:

No, the company didn't use that wire. The company just had manufactured these as dictating machines with steel wire in it, and we got someplace or other, from Navy surplus, some of this alloy wire which had a higher, gave us a higher signal for getting around the recording flux. And this was the standard input-output device that was used on the SEAC. It actually was a rather important innovation on SEAC because it became possible for each person to have his own private files in a reasonably portable medium and an easily modifiable medium, which he could take with him. And there was a superstition believed in those days, which is still true, which is to say that I believe the superstition, that one is not safe unless one carries with him on his own body his own programs and data and doesn't entrust it to the vagaries of what a computer and its operators will do on valuable data and programs. And these wire cartridges made it possible for each user to take his own programs and data home with him and only let them on the computer when he was satisfied that they were safe. So the first problem I was assigned was to assist in the design and construction of this out scribing device on which I did a combination of electrical design, logical design, and wiring with soldering irons.

MERTZ:

With whom did you work at this point?

KIRSCH:

The person who was supervising this project was Ruth Haueter, and she was my first supervisor for a short period of probably two months. I don't remember quite how long that was. As a result of my doing this work it became obvious that I should learn something more about the SEAC computer, and at this time Ida Rhodes was teaching a course on how to program computers so simultaneously I took Ida's course on programming and joined the group that was in charge of maintaining the SEAC computer and keeping it in operation. There was one group that had responsibility for both the electrical and the mechanical operation of the machine that is, operating it from all but the programming standpoint and also doing testing and diagnosis of the behavior of the machine. And I was assigned to that group. While I was learning the operation of the SEAC computer I was also learning something about programming it from Ida Rhodes.

MERTZ:

About when was this? You came on in--

KIRSCH:

I came in September of '51 and I would suspect that this must have been fairly early in 1952. I had only been here a few months.

MERTZ:

Who was in charge of the maintenance....

KIRSCH:

Maintenance of the computer was under Phil Shupe who is still here at the Bureau of Standards, so he was my second boss. I remember at the time being very impressed with sort of the question of how does the soul enter the machine? What starts this thing going, and there seemed at the time to be an insuperable obstacle to overcome between inanimate devices which were easily understandable and the ability of the machine to exhibit behavior in some sense or other, and my being impressed with that has I'm afraid still survived to this day. I'm still not entirely convinced that I understand how these devices manage to exhibit manifest behavior, even though I can put them together and take them apart. One of the interesting things that came out of Ida Rhodes' course was a certain degree of confidence in one's programs. Ida was a member of the early school of programmers who believed that one should write programs correctly, and I remember that my first program that I wrote in her course was a program to compute e to the x power by a series expansion, and the first time I put it on the SEAC computer the program did not work, and I said what I have subsequently heard many times over many years thereafter, "Oh, the computer must be broken." But, in this case, since I was on the maintenance crew on SEAC I went in to find out what was broken about the machine and indeed found out that the machine was broken, and so I was in that very unusual position of having my first novice program not work only because the machine was broken.

MERTZ:

The program worked when the machine was fixed?

KIRSCH:

The program was correct, yes. And I credit Ida with having given me what's technically called enough “chutzpah" to make so pontifical a claim as that my first program was correct and that the machine was broken, when in fact it was.

MERTZ:

How long was Ida's course?

KIRSCH:

It was a fairly short course, probably only a few weeks. And she was teaching mostly the people who were in the mathematics group at the time. That was the one of the two

groups at the Bureau of Standards who were concerned with the computer, the engineering group concerned with the construction of the machine, the exploration of new components, the modification of the computer, and the mathematics group, which was concerned with writing programs, doing numerical analysis and interpreting scientific problems in terms of their, the programming necessary for them.

MERTZ:

In general did the engineers do their own diagnostic programming?

KIRSCH:

Yes. This raises an important issue about diagnostic programming. The diagnostic programs that we used on SEAC were all written by engineers. This should be perhaps slightly qualified. There were a few sort of functional tests that the mathematicians wrote, mostly to assure themselves that the machine was OK. So many of them had their own private diagnostics. Sometimes these diagnostics would consist of nothing more than a standard program that they had run before and got satisfactory results from. They had very little diagnostic value, however, because they weren't intended to reflect anything about the structure of the hardware. All of the programs that did reflect anything about the structure of the hardware were written by the engineers who were familiar with the logical design of the machine. And we had quite a powerful set of such programs, at least powerful for those days. One program that we had made use of the fact that when memory failures occurred they would tend to be local within the memory rather than global, and consequently a memory testing program could be loaded into the acoustic memory in one half of the memory and test the other half such that by throwing toggle switches one could put the diagnostic program in whichever half of the memory was working and test the other half and thereby localize the troubles to within one logical unit which was a single mercury delay line of eight words of memory. This program was extensively used. There was another program that I remember writing, probably the first one that I wrote of any size, which was used to test the switching circuits, the so-called G generators, which were used to decode the memory address into an acoustic tank selection signal. And the way in which we used these diagnostic programs reflected a property of the SEAC circuitry. The SEAC circuitry was a dynamic pulse repeater circuit, which communicated between circuits through a coupling transformer. This meant that the output of the circuit was a pulse train fed through an output transformer to subsequent diode networks which the circuit would drive. By taking the return from the output of the coupling transformer and varying the DC, the direct current, bias level to which this transformer was returned, one could, in effect, shift the level of all of the pulses either up or down. Since the nature of most of the troubles that would develop in this type of circuitry were that pulses would not be sufficiently large to drive subsequent circuits, by the very simple artifice of changing the DC bias level to which the transformers returned, one could, in effect, uniformly weaken signals all over the computer and thereby induce failures. This was a very powerful type of marginal checking which was already in use when I arrived in 1951, and was very likely in use from the very beginning on the SEAC computer since it was so easy a check to effect.

As I say, it's the kind of thing that was a particular property of the dynamic pulse repeater circuitry that it was easy to do this.

MERTZ:

Was it customary for many of the people on the engineering staff to go to Ida Rhodes' course and take it, or?

KIRSCH:

I don't remember how many of the people in the engineering staff had taken Ida's course. There probably weren't many and this was for the peculiar reason that most of the people in the engineering group were already sufficiently familiar with the machine that there was very little reason for taking Ida's course. That is, the machine was totally understood once you understood sixteen instructions plus a few details, and one could comfort oneself into thinking that one understood all about the machine by understanding these 16 instructions. This was, of course--it was quite a different matter to understand the niceties of doing computations, but most of the members of the engineering group were less interested in numerical computation than they were in logical programs. And Ida's course was more devoted toward doing numerical computations, which, of course, is what the mathematicians and the members of the Mathematics Division were interested in at the time.

[Recorder off]

MERTZ:

At the time that you were taking Ida's course what was your initial assignment with the maintenance crew?

KIRSCH:

The initial assignment I had in the maintenance group was the same as essentially everybody else--to be available on tap when the machine broke down. I wish I could give you a reasonable recollection of mean time between failures, but I can't remember that well. It was such that you didn't feel secure about taking off for the afternoon, certainly, if your job was to be the maintenance engineer on the machine. There were various activities that we had which now, in retrospect, should be considered to have been just WPA holding types of operations. One of them was a preventive testing, preventive diagnosis of the machine, in which we systematically pulled out all of the diodes which were mounted in plug-in old vacuum tube bases, pulled them out of the machine and tested them on a diode tester, testing their reverse resistance and their forward resistance characteristics. In retrospect it's perfectly clear that first of all this didn't detect intermittent diodes which were our main cause of trouble, and secondly induced many more bad solder joints and troubles than it ever cured; but it did keep a bunch of people busy who might otherwise have been unproductive.

MERTZ:

When you were first involved as a maintenance engineer on the machine did you work with someone or were you pretty familiar with most of the machine by [then].

KIRSCH:

I was pretty familiar with most of the machine by that time, and each person worked essentially by himself, though typically when troubles would develop there would be enough time pressure on trying to fix the machine that you would quickly call in everybody else who was around for consultation purposes. I should say that the machine was being run around the clock twenty-four hours a day, seven days a week, which meant that a sizeable staff was needed to maintain the machine. And, for a period of a few years, this task of being the maintenance engineer rotated, not only among the nominal maintenance staff, but also among all the SEAC designers and engineers, so that there were periods of time when the more senior people who had charge of general planning and design--I believe not including Sam Alexander, but probably including everybody else--did serve there their periods on the graveyard shift from midnight until 8:00 a.m., and all other shifts during the day, as resident maintenance engineer. There grew a situation fairly soon in which the people, especially some of the members of the maintenance staff, became sufficiently expert at the machine that they were able to do a pretty good job of diagnosis of programming errors. And the combination of the comparative ineptness of the mathematicians in the intricacies of the machine's operation and the maintenance people's ability to diagnose troubles rather extensively made the maintenance people reasonably good assistants in diagnosing non-strictly mathematical types of errors in the mathematicians' programs. And so this provided another type of activity that the maintenance people did for the mathematicians.

MERTZ:

You might want to describe, perhaps just something briefly, the physical location, layout, of the machine itself and if there were any environmental factors which had effect either on the operating conditions of the machine--

KIRSCH:

Well, I can't really think of any special environmental factors. The machine, of course, was very large, and it occupied what would now be the equivalent of about two rooms worth of equipment.

MERTZ:

This was in the old Bureau of Standards.

KIRSCH:

This was on the old campus at Connecticut and Van Ness Street. The computer was built in a building on the north side of the campus, in Building 16, and some several years later it was moved to the south side of the campus and I have a photograph someplace around here in which I am seen sitting at the console of the machine with something typed on it that said "SEAC's last printout ever," because I was firmly of the belief that the machine would never be made to work again once we took it apart and put it back together. I carried most of the machine in my lap from the old site to the new site because I was afraid to let anyone else touch it, and my predictions were proved eminently wrong when in a period of something like about a month or so we were able to take it apart and put it back together again. In fact, in putting it back together again we discovered that there was at least one error in logical design in the machine that had been there all along and that was enshrined in the wiring diagrams and all, and that had never apparently been detected in the half dozen or so years that it was in operation, until, in the process of putting the wires back together again, I happened to ask the question why a particular wire went to a particular place and discovered that in fact it was a design error in the machine. So there has been precedent from the very beginning for machines that weren't correct from the very beginning to keep operating with nobody detecting their logical bugs.

MERTZ:

And to produce mathematically useful results.

KIRSCH:

And to produce mathematically useful results.

MERTZ:

All right. I'd like to reverse the side now. This is the end of side one.

[End of Side 1]

KIRSCH:

On the general question of the operation of the machine and operating experience with it, the best reference I can think of is a paper by Phil Shupe and myself in the Proceedings of the Eastern Joint Computer Conference of December 1953, published by the Institute of Radio Engineers. One of the questions you asked about on memory problems bears a few comments. First, the SEAC computer had two memories that were radically different in their, both in logic and in their physical nature, the serial mercury acoustic delay line memory and the parallel electrostatic cathode ray tube memory. The SEAC computer was a serial computer, which meant that it was necessary to convert the information stored in parallel on forty-five cathode ray tubes into a serial representation, and this was

done by feeding the information out in parallel from the cathode ray tubes to a shift register and then sequentially passing it on to the rest of the machine. The fact that the two memories were so different in nature made it possible for us to use one to test the other, as well as to use one part of one to test a part of the remainder of the machine. Each of these memories had 512 words of capacity. Typically, a failure in the mercury memory would be in a single mercury tank, mercury delay line, thereby destroying eight words of memory, whereas a typical error in the electrostatic memory would be such as to ruin one binary digit position in all of the 512 words. By the nature of what the machine was like, therefore, there was more likelihood of being able to use a part of the acoustic memory when it broke than a part of the electrostatic memory. However, in the electrostatic memory we had three spare-tube positions, three spare cathode ray tubes, and this enabled us to get reasonable reliability somewhat less, though, out of the electrostatic memory than from the acoustic memory. The actual techniques that we used for making the electrostatic memory operate were twofold. First, using the spare cathode ray tubes, and secondly, using the resources of John Rafferty, who was able to perturb the deflection voltages in the x and y direction in such a way as to solve the necessary forty-five simultaneous equations to dodge the blemishes on all forty-five spots. It's not entirely clear how John Rafferty did that, or whether in fact somebody can do that, but it was a fact that he did do it.

MERTZ:

These electrostatic storage tubes were manufactured by--were they ordinary--?

KIRSCH:

The initial ones that we used were ordinary production-line cathode ray tubes. Later on, we got more reliable operation out of the electrostatic memory by using special ones that were manufactured by RCA for the purpose.

MERTZ:

Selectron?

KIRSCH:

No, not the Selectron. There was a Selectron tube that had been connected and tested before I got to NBS. It was never used as part of the actual memory of SEAC to my knowledge. These were ordinary single beam cathode ray tubes with an ordinary phosphor. One of the things that made the special tubes more reliable was a more carefully deposited phosphor, as well as more careful attention to the electron gun mechanism in the design of the tube. But that came along quite late in the use of SEAC, by which time the electrostatic memory was already being supplanted by the ferrite core memory, so that although probably we can say that as a result of some careful redesign work that we did, especially that Bob Elbourn did we were able to get reliable operation out of the electrostatic memory. This became a moot point because of the superiority of

the new technology of core memories. So, the electrostatic memory should be considered to be one which died, not for reasons relating to it but rather for reasons relating to the existence of the new, more stringent species [?] that replaced it.

MERTZ:

About when was the SEAC reequipped with a magnetic core memory?

KIRSCH:

SEAC was never equipped with a magnetic core memory. My point is that, although we continued to use the cathode ray tube memory until the very end of SEAC's existence, it was no longer appropriate for anybody else to plan on using one even though it was clear from our experience that one could get reliable operation.

MERTZ:

And there was some experimentation with other types of electrostatic storage tubes.

KIRSCH:

I believe, yes, we did have a quartz delay line that we tried, but I don't believe it was ever actually used in the SEAC for ordinary computations. And there was a nickel-delay line that we experimented with, that I think we got from the Elliott company in Britain. Again it was not used on the SEAC computer. But we did experiment with several different types of devices. We also experimented with magnetic core storage. Max Klein built a small piece of magnetic-core storage and some special test equipment for testing it, but again it was never used on the SEAC computer. SEAC maintained its ordinary acoustic and electrostatic memory to the very end, including an additional bank of acoustic memory that it got toward the end of its productive life.

MERTZ:

Speaking of test equipment, was there any special test equipment built or designed for use on the SEAC as such?

KIRSCH:

The test equipment consisted of a vacuum tube transconductance tester for testing the 6AN5's which were the standard tube used in the pulse repeater circuits; a diode tester, which tested the forward and reverse resistance of the diodes under low and higher currents and voltage conditions; and--a, yes, there was also a bench tester for the acoustic delay lines. However, as I recall, we made very little use of the off-line tester, since it was much easier to test the exotic types of conditions that would occur in an acoustic delay line by testing it with the computer. The problems in testing acoustic delay lines were not so much ones of detecting traumatic and catastrophic failures as determining

marginal failures which would be caused by various reflectance conditions, and these would only show up under certain kinds of pulse patterns, and this made it more desirable to test them on the computer than to test them off the computer.

MERTZ:

Now, during the time you were at the--involved as a maintenance engineer, I take it, there was some time which was available to you for fiddling with the machine. Would you care to comment on--

KIRSCH:

For purposes of the historical record your choice of the word fiddling was at my suggestion.

MERTZ:

Yes. This was a term which was introduced into the history of technology by Mr. Kirsch.

KIRSCH:

Yes. The point that I wanted to make was that from the very beginning of our use of the computer there was a small privileged group of people who had access to the computer and an even smaller group that had privileged access to the computer, which made it possible for those of us fortunate happy few who had such access to experiment with uses of the computer which were less immediately concerned with productive problems and more with more speculative uses. I remember myself being very much impressed, from the very beginning of my contact with SEAC, with a fact, which I still consider to be the case, and I think many other people now also consider it to be the case, that the computers even that we had then, certainly the computers that we have right now, have vastly more capability than we know how to successfully exploit. And this motivated me personally to think about some of the more science fiction-like uses of the computer. In particular I became interested in what we now consider to be artificial intelligence questions, questions about getting machines to exhibit the artifacts of intelligence insofar as one understands anything about what intelligence is. And I became interested in writing programs for the computers to play coin matching games. The motivation for playing coin matching games with the computer was first of all to discover whether an algorithm of reasonable complexity such as we could write on the SEAC computer could play against a human opponent and beat a human opponent. And I was able fairly quickly to demonstrate that a computer could, but this was more an indication that people are bad random-number generators than anything else. We became interested in a more complicated version of the problem. Ralph Slutz and I challenged each other to coin matching with the computer. Each of us would be allocated half of the machine's capacity. Ralph occupied, say, the lower half of the memory, and I occupied the upper half of the memory. And we established conventions, both of a programming nature, how we would transfer results to each other's programs and transfer control, and also

conventions of a gentlemen's agreement nature to the effect that we would not allow our programs to look at the other fellow's program to find out what he was doing. I think in those days we didn't understand that there was very little to be gained by looking at somebody else's program. However, we did play a coin matching experiment and, according to some sequential sampling theory that we used to determine who was the winner I am honor-bound not to say who was the winner because the results were considered to be statistically insignificant. I however have just recently discovered that I still have the magnetic wire cartridge with Ralph Slutz's program stored on it, and I am still under the agreement that Ralph has never released me from not to look at it. So someday I must try to transcribe his program and find out how he was playing against my program.

MERTZ:

Speaking of the time involved, though, roughly when did you have the chance to do these things?

KIRSCH:

This was usually done in various times like when we were doing preventive maintenance on the machine. Sometimes we would have the machine scheduled for testing and there would be no mathematical computing scheduled during those periods. Sometimes I can confess to having stolen machine time from purportedly more useful products like the thermonuclear weapons calculations and things of this sort. I wasn't aware at the time of what I was stealing time from. I'm not entirely sure that I would have done differently had I known, but it was possible to get a certain amount of computing available at very attractive rates, namely free. I should mention here, on the subject of fiddling around, that there has been a history, not only with the SEAC computer, but certainly with most other computers, of people being in this privileged access position who have been encouraged thereby to try things which could hardly be justified in a cost-conscious environment, and mostly these were people who had access to the machine for engineering and maintenance reasons or for operating reasons, and who by the vagaries of accounting procedures were able to get undue amounts of computing and try things out. This I think has been a powerful influence on the development of new uses of computers, because things which, reasonably speaking, could not have been justified were in fact done and demonstrated and justified after the fact. I can think of a few instances of things of this sort that we did. Among the things that I personally have been involved in which required access to the machine for unreasonably large amounts of machine time were not only these problems in game playing with the machine, but problems in sound generation, acoustic signal generation, which perhaps has now--not in the case of SEAC, but in the case of other people--led to some results in speech synthesis. Problems in feeding in exotic forms of information, most notably pictorial information; it was my interest in feeding in two-dimensional rectangular arrays called "pictures" that led to the first instance, to my knowledge, of feeding in a picture into a computer and processing it. And now, of course, this is a fairly common type of operation, many people are doing it. This was first done by us in about 1955. And, as far as I can recall, my number one son

was the first child ever to have been fed into a computer and come back out. The device that we built for that was a rotating drum scanner that enabled us to feed a two-inch square picture into the computer as an array of 176 by 176 binary digits.

MERTZ:

From half tone?

KIRSCH:

No, not half tone. Just black and white. Another technique that Leonard Cahn invented as a modification of the scanning device enabled us to feed halftones into the machine. But the important fact was that we were able to feed in pictures drawn on a piece of paper or photographs on a piece of a paper, into the computer's memory. And we were able to demonstrate that one could separate out physically separate objects in a picture with computer programs and analyze them in individual detail. This made unreasonably large demands on computing capability at the time, and in fact still does today with the much more ambitious things people are trying to do. But I suspect had we had to justify what we were doing in terms of the cost of the machine which was, as I recall, then something like $120 an hour for what we now call connect time rather than CPU time, we would not have been able to justify uses of this sort. There are other examples of what I'm calling here "piddling around" which could probably also be listed like some of the early uses of the computer for display purposes, where a valuable computing instrument was being used to put a few spots of light on a cathode ray tube for visual purposes. Probably a hard-headed person would have been able to think of much cheaper ways of doing this than what we were doing with the SEAC computer, but one must credit some of the motivation in the development of graphics technology to the constructive demonstration in this somewhat irresponsible way of the capability of the machine for producing visual graphic output.

MERTZ:

Speaking of data output, what--particularly, do you recall any specifically designed data output of the display elements that were associated with SEAC that were unique or unusual to machines of its vintage? ...

KIRSCH:

On the subject of picture input to the SEAC computer there's a paper that I presented with Leonard Cahn, Chuck Ray, and Genevieve Urban in the 1957 Eastern Joint Computer Conference Proceedings published by the Institute of Radio Engineers on "Experiments in Processing Pictorial Information with a Digital Computer." And that gives some of the descriptions of the pictorial input device that we built and used on the SEAC. You mentioned questions about environmental conditions on the early SEAC. There were some environmental problems of various sorts that we encountered. The air-conditioning problem for the SEAC was reasonably critical, since the reliability of the germanium

diodes that we used seemed to be influenced by the temperature and humidity such that maintenance of air-conditioning, hot air quality, was reasonably important in order to prevent machine failures, as was the maintenance of temperature conditions for things like magnetic tape use. I'm sorry: Not temperature conditions as much as humidity conditions for magnetic tape loss.

MERTZ:

When they moved the machine from its original location to the building in the south part of the old NBS campus, were there certain things put into the building first which took into account these environmental factors? In some cases some of the machines didn't always take cognizance of some of these problems that might be posed by humidity and/or air--

KIRSCH:

The new installation had well designed air-conditioning equipment and it presented no new problems, and the operation was at least as good in that respect in the new installation as it was in the old one. There was one other type of environmental condition I can think of on the original outscriber project, the very low signal levels that we were working at made us particularly prone to any noise that was available in the environment, and indeed, there turned out to be a burst noise, occasional bursts of noise that would interfere with the operation of our equipment that we couldn't seem to track down. The nature of the signals that we received made us suspect the tube laboratory at the Bureau of Standards, which was reasonably near to us, but the directions from which the signals came made us suspect some other source which was located in a slightly different direction. The nearest source of any radio interference in that direction was up at Ward Circle, which was where National Security Agency was at the time, and so we abandoned any attempt to track down the source, but we were reasonably satisfied that we knew where it came from. Such problems, of course, could easily be solved by use of suitable shielded rooms and conditions of this sort, but techniques were not really that advanced in our particular laboratory to be able to use things of this sort. The other environmental conditions we had involved the use of magnetic tapes. The tape units that we had on the SEAC computer were a triumph of engineering simplicity. Jim Pike designed tape units which consisted of rotating drums with jam rollers which would pinch a piece of quarter inch magnetic tape between them and start moving, and then the servomechanism for feeding and taking up magnetic tape consisted of two glass bins, one of which received the tape and the other which simply furnished the tape. The glass bins were a quarter of an inch wide such that the tape would not tangle but just drop into the bins, and this served not only to work admirably but it was a very attractive demonstration because of the appearance of tape being spit out and then picked up from a magnetic tape drive. It did, however, present the problem of dust and other particulate matter being picked up from the atmosphere and cleaning of the magnetic tape units became something of an art such that one could only really successfully make use of the magnetic tape units in the presence of a suitably trained artist. Such problems have continued to crop up in the magnetic tape industry but they are handled more as a science than as an art nowadays. I

guess, that's all I can think of on the subject of environmental conditions.

MERTZ:

To get back, chronologically, how long did you remain as a maintenance engineer then with the SEAC?

KIRSCH:

I had a somewhat tenuous status as a maintenance engineer. It's not at all clear when my interests stopped, being--I'm sorry: It's not at all clear when my assignments stopped being maintenance. It's clear that my interests became less matters of maintaining the computer than questions of trying to do things with computers that hadn't been done before. And certainly, by about 1955, I was no longer doing more than occasional maintenance of the computer, and was instead concerned with writing programs for new uses of the machine. By about 1955, I was already heavily involved in building what may be the first time-sharing system, although it's not generally recognized as a time-sharing system. This involved the DYSEAC computer which we built here at NBS unwisely enough in a mobile van such that the sponsor, the Signal Corps, was able to take it away when we finished building it. Before that, however, we succeeded in demonstrating the ability to run three problems simultaneously on two computers. The DYSEAC computer had two interesting capabilities. One was the ability to drive certain types of input-output equipment, most notably cathode ray tubes and other types of displays; and the second was a reasonably sophisticated interrupt capability, which was probably not on any other machine until that time. This enabled us to demonstrate the following capability: we were able to simulate on the cathode ray tube display of the DYSEAC computer an image, a radar image, of ships maneuvering on an ocean under the kinematics of realistic naval vessels. Simultaneously with this program running on the DYSEAC computer there was a sorting program for some type of numerical data running on the SEAC computer, which was across the road from where the DYSEAC computer was, and there was a thin wire strung from the window of the SEAC computer over to the DYSEAC computer, which we hooked up by simply running such a wire from one machine to the other. The SEAC was sorting items in a file, and then when it encountered an item which was to be posted against the master file, it transmitted over this line to the DYSEAC computer an interrupt signal, which caused a temporary interrupt in the DYSEAC ship maneuvers simulation program, caused the DYSEAC to take this numerical data and post it to a master file, and then resume the simulation program allowing the SEAC to go on with its sorting program. So this was an instance of real-time interrupting of a program for performing another type of program, and in at least one occasion when I remember that we did this, we had some naval people watching this ship maneuvers calculation, the program for which I wrote, and they were impressed with the realism of the program, totally unaware of the fact that the same computer was, in fact, running part of another problem for another computer at the same time. Which, of course, is an effect which nowadays is fairly commonly understood, and in fact requiring in the time-sharing system that the other users be invisible to a given user.

MERTZ:

At the time that, I gather, the DYSEAC was being thought of, at the time that SEAC was running during these years, was there any particular experience gained from SEAC in a negative sense which affected the plans for DYSEAC?

KIRSCH:

Yes, I think we could say that we satisfied ourselves on the SEAC computer that investigating exotic hardware was unwarranted. All our experience which we had been accumulating on using new types of circuitry ... essentially led to the practical, negative conclusion that the DYSEAC should be built with the same vacuum-tube pulse repeater diode logic type of circuitry that characterized the SEAC computer.

MERTZ:

Serial?

KIRSCH:

Serial in nature. And all of the innovation in the DYSEAC computer went into the logical design. The only engineering innovations in the DYSEAC computer were connected with the special input-output equipment, and that should properly be viewed as an effort somewhat independent of the DYSEAC computer. So the answer to your question really is that the SEAC computer encouraged us at least in the DYSEAC design to be intellectually John Birch Society with respect to engineering innovations. And, for future purposes, "intellectually John Birch Society" means being conservative. One must avoid topical references in historical documents.

MERTZ:

You did mention, I don't believe it was recorded, an amusing situation that occurred during one of the problems run on the SEAC for the AEC when the normal paper that the machine usually to print out its results was in short supply.

KIRSCH:

Yes. One evening Genevieve Urban was running on the computer and the teletype paper ran out. And the computations for that evening were finished on a roll of toilet paper procured from the ladies' room. There were other instances of resourcefulness of a comparable sort. There were also instances of frivolity, and very often in fact directed against the same young lady, who was a very good programmer, using the machine at the time. Such cases as having the machine halt spontaneously whenever she did something not involving the computer at all like standing up or moving across the room. Of course, the machine's response was induced by somebody who was sitting inside the computer at the time. And there were other such instances of having great fun. One source of fun that

was a peculiar property of the SEAC computer was the fact that it had a typewriter with only sixteen characters, the hexadecimal characters, and one bit of challenge for the users of the machine was to try to figure out many combinations, obscene or otherwise that could be made out of this limited repertoire of ten digits and a few alphabetic characters. And there were indeed some triumphs of hexadecimal poetry done on the SEAC computer. As a matter of fact just a month ago or so I saw in Datamation magazine a poem written in hexadecimal which would have been the envy of most any of us in the old SEAC days, and that certainly goes to show how civilization has advanced in the intervening years.

MERTZ:

[laugh]. Were there any attempts by budding young musicologists or composers to score music by the SEAC to accompany the theme of the coin matching?

KIRSCH:

There was no work on music synthesis or analysis to my knowledge. I know that occurred to me several times, and I convinced myself that it was beyond the capacity of the SEAC computer. The nearest to that that I can think of was a talking dog routine that I wrote. This was occasioned by an open house at the Bureau which became pretty burdensome and boring for those of us who were demonstrating the computer. And necessarily anything burdensome and boring in the vicinity of the computer was an open invitation to the use of the computer. So I wrote a program which exploited the peculiar properties of the input-output devices that we used, the Pierce wire recorders. I recorded a demonstrating message, a lecture, on the recording machine, and then wrote a program which read this message in as if it were digital information. Since it was not, in fact, digital information and was analog in nature, it read in as gibberish to the computer, which was in turn looking for a particular flag on the wire recording. Not finding such a flag it continued to read in the message, thereby producing on the audio monitor the recorded message that I had produced. At each juncture where there was an action that the machine was to undertake to demonstrate what had just been described there was in fact a digital signal that I recorded which stopped the magnetic recording medium, caused the tape units to move or some other thing that was being demonstrated, and then after the demonstration to continue on with the recorded message. So this was an instance of reversion in the use of an input-output device to its original intended use as an office dictating machine. And we had this talking dog routine, which we used many times, to automatically demonstrate the machine, while giving a short speech describing its characteristics. There was, however, no serious work on speech analysis or synthesis on the computer, and I'm pretty sure that could not possibly have been done with the very limited capacities of the machine with 500 or 1000 words of memory and relatively unsophisticated input-output.

MERTZ:

In what respects did DYSEAC improve the possibilities in--

KIRSCH:

Well, DYSEAC improved the possibilities considerably even though we had it for a very short time and weren't able really to exhibit its capacities. First of all was the input-output equipment, which was much more sophisticated on the DYSEAC. The cathode ray tubes, the existence of a light pen, the existence of a two-coordinate joy stick for putting in x-y information from a rod-type of controller; and, secondly, the ability in the DYSEAC to trap on various conditions, trapping being the logical condition in which when something happens in a program the control is transferred to some standard location where the consequences of this trapping condition can be further pursued. There were fairly sophisticated traps available in the DYSEAC computer and fairly sophisticated types of external interrupt features whereby some external signal could interrupt the program and initiate some new action and cause the original action to resume. All of these were exploited in the short time that we had the DYSEAC computer and they represent significant innovations of a logical or programming sort rather than an engineering sort on the DYSEAC computer.

MERTZ:

But that was, I gather, from what you say, did tend to be the main thrust of the engineering staff after SEAC rather than innovation in circuitry...a non-logical design.

KIRSCH:

That's not entirely fair to say, because, although this was what characterized the DYSEAC computer, and even the PILOT computer which we built many years after that, there were simultaneous efforts in the design of new components and research of new devices, which didn't impinge themselves on the construction of new electronic computers until many, many years later. But that was going on simultaneously as an independent and somewhat less related effort, related, that is, to the construction.

MERTZ:

I was distinguishing here engineering as specifically applied to the machine in terms of something different from logical design, as specific components for the machine rather than component research in general, which is not necessarily machine-related to a ...

KIRSCH:

I think the only component design that we did, or any component research that we did, which did have direct influence on the computers that we constructed or used was related to magnetic tapes. We had reasonably extensive research going on magnetic tape units, and we had, over a period of time, several magnetic tape devices that we used on the SEAC and on the DYSEAC computer, which were consequences to some extent of our research on magnetic tape recording. So in that sense there was a direct connection. But

it's probably the only significant connection between our research on devices and components and our construction of computers. The best demonstration of this fact is that the PILOT computer, which we built was built out of essentially the same electrical components as the SEAC computer of more than a decade previously, a decade or so previously, when probably the consensus of judgment outside was that transistors were the appropriate tool to use and that vacuum tubes then were hardly the same vacuum tube circuitry that was used in the late 1940s and early 1950s.

MERTZ:

We've got some more items on the list here. Other--

KIRSCH:

Contacts with other groups was another item that you mentioned. Let me very rapidly point this out. In the early SEAC days, the computer was available to many people from different organizations who were doing government-sponsored or directly government-related research. And without attempting in any way to suggest names, which I wouldn't be able to do without more serious study, I would say that the effective SEAC alumni should be considered to be a much larger group than the actual duly constituted SEAC alumni since there were many people from organizations outside of the government who spent a great deal of time using the SEAC computer and who became, in effect, members of the staff and who have gone forth and have been fruitful and multiplied in the computer field. This was of course true also for other computer installations, perhaps somewhat more so, because, in the case of the SEAC, because as a government computer it was available to more diverse organizations than would be the machines that were used by a single organization for its own purposes. So we probably had a more illustrious group of alumni than most other comparable organizations, comparable in size to ours.

MERTZ:

Right. Could you perhaps rather briefly characterize what if any relationship there was between SEAC and SWAC?

KIRSCH:

I had virtually no contact with the SWAC computer, and I couldn't really say much about that. The people who would be able to give you better information about that would be Dr. Cannon and some of the mathematicians who were the ones connected with the SWAC computer through the joint arrangements with the Institute for Numerical Analysis and the Mathematics Division.

MERTZ:

Correct me, it is my impression, however, that SEAC was indeed in some respects a more

successful effort in the machines. It was running earlier--

KIRSCH:

Oh, yes. It certainly was running earlier and had a--it both was running earlier and, I'm afraid, later also, than the SWAC. And SEAC had a very large influence, of course, both in and out of the government because of its early date and--

MERTZ:

Also it had something of an open door policy, didn't it, in the sense of people coming?

KIRSCH:

Yes. Again, the only pretense for qualifications in the use of the SEAC was connection with some sort of government contract that would pay for the use of the machine. And this provided a rather successful entrée for a rather diverse group of people, ranging from people who did research at universities to industrial organizations and other government laboratories, as well as the Bureau of Standards itself. Which was perhaps more conservative in its own use of the computer than would have been warranted by the uniqueness of its situation with respect to...

MERTZ:

Which, it is my understanding, contrasts a bit with the way SWAC functioned and its exposure to--

KIRSCH:

[Inaudible].

MERTZ:

We are running to the end of the reel.

KIRSCH:

Out of tape and history.

MERTZ:

Thank you very much.