

An Interview with  
ETHEL C. MARDEN  
OH 51

Conducted by William Aspray

on

20 October 1983

McLean, VA

Charles Babbage Institute  
Center for the History of Information Processing  
University of Minnesota, Minneapolis

Copyright, Charles Babbage Institute

Ethel C. Marden Interview  
20 October 1983

Abstract

Marden discusses the early use of computers by the U.S. government as seen from the National Bureau of Standards, where she was employed following World War II. She discusses the results of the construction of the Standards Eastern Automatic Computer (SEAC) and points to the prominent role in its design of people who had worked on ENIAC. Marden describes the enthusiasm and work environment of the SEAC project, including accommodations for women to hold professional positions at the same time they were raising families. She points to the success of SEAC as measured by the many government offices that used it. She describes the interactions of NBS with other government agencies and other major computer projects, and describes how NBS recruited talented personnel.

ETHEL MARDEN INTERVIEW

DATE: 20 October 1983

INTERVIEWER: William Aspray

LOCATION: McLean, Virginia

ASPRAY: This interview with Mrs. Ethel Marden is being conducted at her home in McLean, Virginia. Mrs. Marden was interviewed a number of years ago. This interview is intended to supplement the earlier interview, now on deposit at the Smithsonian Institution.

Please tell me about the early development of the SEAC at the National Bureau of Standards.

MARDEN: The SEAC was dedicated in June of 1950. After that time many bugs were found--as are found in all new computers. Malfunctions would occur and then the engineers would determine where the malfunctions were. Sometimes a basic fault was found in the logical design of the computer which only certain types of programs would be able to find, after very simple programs had run beautifully. But, occasionally, even after the passage of months, a particular occurrence of a combination of instructions would send the computer through a path it had never traveled before and the engineers would find another bug in the hardware. That happened once in one of my programs. Although the program had run correctly before, this time it did a low-order multiplication incorrectly and the engineers found another bug in the hardware.

In the beginning, i.e. from the time of the dedication, there were only three programmers working with the computer: myself, Ira (Corky) Diehm, and Otto Steiner. Florence Koons, who had worked with us, and who had actually taught the three of us what little we knew about computers, was moving to Census to work with the UNIVAC, which had not yet been completed.

ASPRAY: About what date would this be?

MARDEN: This would be in the mid 1950s. Perhaps after the dedication, because I'm not sure whether Florence was there for the dedication. As you may or may not know, the SEAC was built because of the delays in completion of the UNIVAC and of the University computing machines--actually there had not been enough basic research in components for people to know enough about the reliability of the various components. The Bureau of Standards was asked to build a small "interim" computer, the SEAC, in order to help solve some of the problems in basic research which were delaying the completion of these other machines: the MIT machine, the Princeton machine, and the UNIVAC.

ASPRAY: Asked by whom?

MARDEN: Probably the National Science Foundation had a hand in it; probably Census, and probably the Pentagon. The Air Controller's Office was to get the second UNIVAC. I'm not sure who asked the Bureau of Standards to do it. I think probably a consortium of representatives--concerned representatives of other government agencies.

ASPRAY: The Bureau was a reasonable group to do this?

MARDEN: Yes, it was then the principal scientific agency for the government. You probably know the NBS played a large part in the development of many, many things during the Second World War, among them the proximity fuse.

ASPRAY: Yes.

MARDEN: That was accomplished at the Bureau of Standards, when Dr. Condon was the Director; he was Director of the Bureau of Standards at that time. To return to research in components, the SEAC, therefore, had two kinds of memories. It had a mercury delay line memory, which was an acoustic memory, and then an electrostatic memory which was essentially a TV tube--the cathode ray tube--which Professor Williams in England had first developed. We were to test the reliability of these components. A group of dedicated and very capable young engineers was

working on putting together the computer based on the logical design of Dr. Samuel Lubkin. Among these young engineers you might consider one the foremost, Dr. Ralph Slutz, who, in my opinion, was a brilliant young engineer. The engineers who worked on SEAC actually improved on the logical design in many respects. They added things that were not in the original logical design. And when the computer was finished it was really much better than expected, or than it had any right to be.

ASPRAY: These engineers, were they hired particularly for this project, or were they already employed by the Bureau of Standards?

MARDEN: Undoubtedly a few were in the Electronics Division, but I think many of them were hired particularly to work on the SEAC.

ASPRAY: I see. Do you know anything about the background of these engineers?

MARDEN: I think Ralph Slutz had been at Princeton and at MIT; I'm not sure at which school he got his doctorate; but I believe he had been at those two schools.

ASPRAY: They were primarily electrical engineers?

MARDEN: Yes, electronic engineers, and the kind who would drive sports cars or put together an automobile out of odd pieces. They were the kind who tended not to be the ordinary run of engineers, but they tended to be, well, if they were pilots, I would say they would be test pilots.

ASPRAY: I see now what you mean.

MARDEN: I have a pilot's license and I tend to relate some things to that. They tended to be more unconventional innovative, and eccentric.

ASPRAY: That seems out of keeping, at least with certain standards one associates with government hiring practices.

MARDEN: The Bureau of Standards was not like other government departments. I only went there in 1948. Before that I had been assistant to one of the commissioners at the FCC. I had no chance to practice mathematics until I went to the Bureau of Standards. The engineers were already there working on the computer when I arrived, so some of my knowledge about the historical background of their employment is very vague.

ASPRAY: But it does seem to you that the Bureau of Standards was somewhat special in personnel practices in terms of government standards?

MARDEN: Yes, by far. People felt much more dedicated to their work. You didn't have people rushing for the door at 5 o'clock. Many, many nights engineers would be there all night. There was no question of payment for any overtime. The group never wanted or asked for overtime pay. Its members just worked very long hours, as did the mathematicians, with a motivation which is hard to understand unless you have worked in a pioneering environment where you feel you're developing something new and exciting and you can hardly bear to leave it to go home.

ASPRAY: I see. Although many of the early pioneers have told me about this, it is hard to understand.

MARDEN: Well, actually some of the wives of the engineers were jealous of the computer. They could never get their husbands home for meals. So they would come in at midnight and have a snack. I worked many hours at night, primarily because one of the duties that Dr. Levin assigned to me, in addition to setting up the training classes and teaching, was to find somebody to work with the outside people who came in. By "outside" I mean people non-government or non-NBS personnel, such as the University of Michigan group, for example, who were working on the Bomarc Missile, who had asked for computer time on the SEAC. I also worked with a Princeton group which was engaging in hydrogen bomb calculations. If I couldn't find somebody who was free to work with such groups,

then I stayed--coming home at five or six in the morning, frequently. There was a little bar about three or four blocks away from the Bureau of Standards, and sometimes when the computer would break down--as it often did in the early days--we would go over there for a pizza and a beer to wait until the phone would ring and the engineers to say the computer was up again. Then we'd dash back. You could always find three or four engineers in the laboratory in the evening until the computer reached such a stage of reliability that one duty engineer was assigned to remain, with others to be on call if need arose. Of course that's hard to understand now because of the reliability you find in computers now. In those days there were those two types of memory and they were outmoded within two or three years of their development. We didn't have the reliability in components which exists today. We didn't have transistors: we had vacuum tubes and diodes.

ASPRAY: Can you give me some idea of what the level of reliability was? How often did the machine go down?

MARDEN: Sometimes it would run for two or three days without a malfunction--so far as we knew. Sometimes the group would solve a problem; they would have SEAC "up" for fifteen minutes and then it would break down again. What the particular breakdowns were, I don't recall now. They would frequently find joints. At the very beginning, some difficulties arose because the engineers wanted to continue to add on equipment--tape and wire drives for additional storage and things like that. (Oh, this is an aside: our first programs were running on punched paper tape, by teletype, and we later had magnetic tape and wire cartridges on which auxiliary information could be stored.)

The engineers wanted to continue to work with equipment which would be added on in various stages. They also wanted to do preventive maintenance. At the beginning they had the computer for about 3 days a week and we, the mathematicians, had it for 3 days. Later, when the Atomic Energy Commission asked to use the computer seven days a week, 24 hours a day (This I have been told, and I believe it.), they were told they couldn't have it for that much time. They said, "if you don't give it to us, we'll go to the Joint Chiefs of Staff and we'll get it anyway, so cooperate with us, and we'll give you whatever time we can't use." Of course, they couldn't use it efficiently that much. In fact, we trained their people because they wanted to run SEAC themselves. Three unmarried Bureau mathematicians, Joe Wegstein, Bill Hall, and Frank Stockmal were assigned to work with the AEC because they were bachelors and could

best meet the peculiar hours of the Atomic Energy Commission. (Actually, Joe Wegstein was a physicist from the University of Illinois.) As AEC promised, we would be told occasionally that we could use the computer, sometimes for two or three days. At such times there was quite a waiting list for use of SEAC.

ASPRAY: How did your group, that is the mathematics section, feel about a large number of people from other divisions coming in to do the programming? Did you feel as though it would have been better for your group to have handled the programming for these other organizations?

MARDEN: We did the programming for almost everyone. But in the case of the Atomic Energy Commission, they wanted their own people there. Our mathematicians worked with them--I think always--but they wanted their own people to have control of the machine and I believe they were motivated primarily for reasons of security. Actually, what we tried to do when we worked for another agency was to have one of their people work with us and learn programming while our programmers were solving their problems for them. In this way they could carry on and could without us when the job was completed and could modify their own programs as time passed. This procedure helped us because we had more requests than we could handle after the computer was finally initiated. The most reluctant to use SEAC, I think, were the people at the Bureau of Standards themselves. Dr. Astin encouraged them to make use of it, but the Bureau of Standards had a group of very conservative scientists who weren't sure that they trusted something such as a computer. And, of course, at first they didn't know what use they could make of it. Dr. Astin arranged for them to have free time on the machine and encouraged them to put projects on it; encouraged each division to make use of it in some way. Once people began to see the potential of the computer for their needs, he didn't have to pursue that course any further. Some of the early work that we did as projects for the Bureau of Standards were crystal structure problems and the ray tracing problems. The problem demonstrated on the machine at its dedication was the first of a whole family of ray tracing problems. We also used SEAC to calculate thermodynamic functions. Function tables of course lend themselves to filling up time on a computer, while the programmer is engaged in trying to find time to program other problems.

ASPRAY: You had mentioned that very shortly after that machine was operating there were more users than there

was time for on the machine. How were priorities set on who would get use of the machine?

MARDEN: Dr. Levin, who was in charge of the computation lab assigned the priorities. He was always very reasonable about it. He used great discrimination and great judgment. He was marvelous. He looked toward the future. He saw the need for developing software. He saw the need for having good organization instead of some loose arrangement where everybody came when he wanted to, the way many of the university machines were operated. Of course, we operated in a different environment.

ASPRAY: Did people have to schedule certain times for use of the machine in advance?

MARDEN: Oh yes. Dr. Levin's secretary, who had a great deal of mathematics herself (but not enough to be classified as a mathematician), was very interested in the computer and in learning to program it herself (as she later did). He put her in charge of assigning time under his direction. She kept very tight schedules.

ASPRAY: Were there some projects that were turned down because it was felt that either the problems were inappropriate for the machines, or they were just too time-consuming and there wouldn't be time for other worthy projects?

MARDEN: I don't know of any that was turned down. I think the time-consuming projects were simply shunted off to the graveyard shift. If you had a long-running problem you asked for graveyard shifts because you could get eight hours at one go. Many times I had gone in with one or two people who might be working with me, to get everything set up about 11:30; this was for a Midnight computer shift, and we would be delighted to have the privilege of that shift: midnight to 8:00 a.m.

ASPRAY: I see. At the time the project was first conceived to build the machine, was it envisioned that a group of different government agencies would be coming in to use the machine, or was it felt that this was mostly a Bureau of Standards machine?

MARDEN: No, I don't think it was ever felt that it would be just a Bureau of Standards machine because it was built to meet of the requirements of other government agencies, the Census Bureau and the Air Controller's Office in particular. I think it was felt that this machine would give other government agencies an introduction to computers. I don't think anyone then envisioned the rapid advance of computer technology.

ASPRAY: I see. Did your group or the Director of the laboratory go out and proselytize, try to convince other agencies to come over and use the machine?

MARDEN: We didn't need to do that, even in the beginning. They heard about it. There was enough publicity in the newspapers and in government circles. There was an effort to convince other people in the Bureau of Standards to make use of it. And at a later time, as people came to us or we heard of needs, it may be that we did solicit work from other government agencies. But usually the initiative came from the other agency: Bureau of Outdoor Recreation in the Interior, Agriculture, NIH, Radiological Health, Navy Department, Defense Intelligence, and other agencies in the Pentagon. Many, many people came to us. One of the early projects that we had was the design of a variable nozzle for a wind tunnel in order to create different Mach numbers in the same nozzle.

ASPRAY: We were talking some before we turned the tape on about some of the other uses of the machine. Would you like to describe those again to me so as to get it on record?

MARDEN: Can you give me one example?

ASPRAY: Well, for example, you were talking about the use of it for bomb calculations.

MARDEN: Oh, for the Bomarc Missile?

ASPRAY: Yes, for the Bomarc...

MARDEN: Yes, the Bomarc Missile at the University of Wisconsin, I mean...

ASPRAY: Was it Michigan?

MARDEN: Michigan, Ann Arbor. The Bomarc Missile work was being carried on there under contract. Much government work was contracted out to universities. Michigan came to us with a simulation of the missile. We did lots of simulations on the computer. At the same time we were doing some work for what was then known as the Long-Range Proving Ground, Cape Canaveral. The Bureau of Standards was building, that is helping them to build, a copy of SEAC with certain modifications at the same time they were helping the University of Michigan build a copy of SEAC called the MIDAC. We worked with the people at Cape Canaveral in simulating characteristics of the machine. Later some of the problems which they wanted put on their computer were put on the SEAC. And yes, as I think I mentioned earlier in connection with the AEC, the hydrogen bomb calculations were made on the SEAC.

ASPRAY: You mentioned to me the demonstration problem that you'd done with ray tracing. What other sorts of programs did you do for the SEAC?

MARDEN: I worked on crystal structure analysis. I worked on the Loviband network, which is a color network; I worked with the Federal Reserve Bank in analyzing bank loans.

ASPRAY: This must have been one of the earliest programs involving banking in computers. Is that right?

MARDEN: Yes. Most of that work, however, was done on the UNIVAC because one of the members of the Federal Reserve could get access to the UNIVAC at Newark, Delaware. We went up to Newark and used it for a great deal of the work because we got free time on it and it was thus a lot cheaper than using SEAC would have been. Some of the work that I did in chemical structure--preliminary work--was done on the SEAC.

ASPRAY: What about your colleagues in the Bureau of Standards? What sorts of problems did they work on?

MARDEN: Well, one of them was working on computation of a trajectory to the moon; many of... Remember this is about 35 years ago.

ASPRAY: Yes, I understand.

MARDEN: So, there are many things that I don't remember; another one of the things which they put on the computer immediately was a routine for finding prime numbers larger than anyone had found before.

ASPRAY: This was done by?

MARDEN: That was done by Dr. Alt.

ASPRAY: One question I had when I read your Smithsonian transcript was about the relations between the engineers and the mathematicians, especially as the machine was first being built and first operable.

MARDEN: That's a very interesting question. I worked with the engineers a great deal because I needed to know how the machine would behave under certain circumstances. I would go over and ask them questions. Say, what happens if so and so occurs? For example, one of the questions I asked them was; is plus zero equal to minus zero? They said, "Oh, let's look." They got out the engineering diagrams and told me, "You can have it either way. Which way do you want it?" Well, I shouldn't have been the one to decide that, but I said, "I think zero ought to equal zero whether it's plus or minus." So that's how it was done. We had very amicable relations until the computer was ready for us to use, until it was finished I'd say. I have to modify that word "finished" because when it was supposedly finished, you know, there were still little malfunctions that would show up--little things that hadn't been caught until it was used. After that, an intense rivalry developed. Not only did the engineers not want to give it to us, they wanted to have it. It was their baby. They wanted to have it all to themselves. However, the mathematicians felt that

now the engineers had made it, their work was done and it was ours, and that they shouldn't have anything more to do with it. An interesting development arose: every time a program didn't run right the mathematician would rush in to Dr. Levin, "The computer's down again, and it's an engineering fault. The computer's broken down." The engineers would say with disdain, "It's a cockpit error," which meant a flaw in the code. I think the score was about 50-50 as to how often it occurred in was each case. For the most part, however, the relationships were amicable.

ASPRAY: This brings me to another question that I had about training. As you said, when you were first learning to program the machine you had to learn a fair amount about the engineering side of the machine in order to program in an efficient way and to understand the operation of the machine. Now later on you had a major responsibility for training others to use the machine. Did you try to instill some of that knowledge about the engineering aspects to other users that you trained? For example, what topics would be covered in your training programs that would last for several weeks?

MARDEN: Three weeks.

ASPRAY: Three weeks.

MARDEN: Primarily, I had to start out by teaching them the binary number system. Many of them who had doctorates even, had never had any acquaintance with binary numbers. And it was very difficult for them to think in anything other than the decimal system. I found that it was usually easier to talk to high schools and junior high schools and explain to them binary numbers and base sixteen number systems than it was to get similar understanding from our professionals in training. We worked in base sixteen a great deal. We called it hexadecimal. To explain it to the children, then put problems on the blackboard and have those little kids giving me the answers after only a few minutes of explanation was surprising and exciting! I found that they learned it much faster than these college graduates did, because few of the college graduates had had very much numerical analysis, and almost none of them had any acquaintance at all with binary numbers. They had great difficulty at the beginning with that. Initially, we covered number conversion routines. I wrote decimal to binary and binary to decimal conversion

routines for SEAC so that we could enter our numbers in a decimal system and run them through a sub-routine for conversion to binary for use in SEAC. It was also necessary for the programmers to know how to convert manually. At first we converted our constants ourselves manually. In my training courses, I started out by teaching them something about number systems and explained to them how the computer worked; that this computer could do arithmetic but that long division was the most difficult thing it could do. It could compare numbers. It could add and subtract. But in essence it really understood only two states: 1) you could say that the switch is off, that there is no electronic impulse; alternatively, that the switch is on, that there is an electronic impulse. Therefore everything that's computed could be translated in terms of ones and zeroes. I found it more difficult to get some of these people with doctorates to accept this whole new way of thinking than to get those little kids in junior high to do it. Once I got them past that stage I had to explain to them that when they're dealing with numbers, the machine doesn't care what it's working with; that you have to tell the machine where to find the number. You can't tell the machine to add five and six. You have to tell it to go to the number which is stored in a particular address and add that number to the number in another address.

TAPE 1/SIDE 2

MARDEN: I drew an analogy to a post office by saying that if you addressed a letter to a post office, where the recipient had a numbered box, you addressed it to a numbered box in the post office; and that you could consider that an array of boxes in a post office all had numbers. They had addresses, in other words, and in a way that's what we were doing with the computer. We were telling it where to find numbers that we wanted to operate with or addresses that we wanted to go to or to modify.

ASPRAY: I see.

MARDEN: Then I taught them the machine language because it was required that they learn that at the time. All of those people who learned to code in machine language then used the simple assembly language which Joe Wegstein had developed in order to assign the final computer addresses for constants and instructions. I think I mentioned this

before you started recording this conversation; that our simple little assembly language simply consisted of our assigning an address for instructions, the numbers R1, R2, and so on, where "R" represents "relative address". When R4n5 reached 100, we chose another letter, N, for example, and began N1, N2, etc. Temporary addresses likewise became T1, T2, and so on, and constants, K1,...K4n. It was a very simple method compared with what you find nowadays.

ASPRAY: Before we take just a short break I want to ask you this question. Could you describe briefly who these people were that you were training? What kinds of backgrounds did they have? You mentioned that some of them had PhDs. Were they primarily scientists? Were they...

MARDEN: Most of them were mathematicians. What we were doing, in effect, was something nobody else knew how to do. If another government agency came to us with a mathematical or a scientific problem, those problems were very easy to handle with a computer. But many problems were large, unstructured, "so-called" data processing problems which had never been completely defined. Many problems that people had and didn't know how to do would be brought to us, and I would in effect tell the people I would assign them to, "Look, here's a problem. We don't know how to do it; the people who brought it to us don't know how to do it; you find out." Mathematicians are used to tackling problems that they don't know how to solve and then attempting to solve them. If they can't solve them, fine. If they can find a solution, they feel a tremendous sense of gratification. If they can't, sometimes they restructure part of the problem into one that can be solved. But we also had other professionals; one or two physicists were on our staff. Later we had chemists. We had one post-doctoral that I brought in who had a doctorate in biology. He became interested in using computers in biology and then became interested in computer science. But almost all the first staff members were mathematicians. In brief, they were all in a professional category. We had no non-professionals who were programmers in those days. We were all systems analysts as well as programmers.

ASPRAY: All right, why don't we begin again, then.

MARDEN: At the time that I was talking about, the Bureau of Standards as then constituted was made up of divisions, and each division chief reported directly to the Director of the Bureau of Standards. At that time the Division of Mathematics and the Division of Electronic Engineers (I think it was called the Electronics Division) were two separate divisions in the Bureau's structure.

ASPRAY: Who were the heads of those two?

MARDEN: The head of the Mathematics Division, Applied Mathematics Division, when I went to work there was John Curtis. At a later time Dr. Edward Cannon became chief of the Applied Mathematics Division. Dr. Astin was chief of the Electronics Division when the SEAC was finished. He later became the Director of the Bureau of Standards and Sam Alexander was made chief of the Electronics Division. At a later stage, the Center for Computer Sciences came into being, and that embraced a: (1) Computation Laboratory which had at one time been in the Mathematics Division, (2) the so-called "Engineering Division" and, (3) the Research and Development Division. I headed the latter for a time. It was made up of software people. In a later reorganization of the Bureau when it was changed to form institutes there was an intermediate level between the division chiefs and the Director. There were institute directors, and each such institute might be made up of two or three or four or more divisions. Does that cover it?

ASPRAY: I think so. What kind of practical significance did that have in the development of projects or in the autonomy of certain directors or managers at certain levels to do things that they would like to do? Did this reorganization make it easier for the computing center to do the kinds of things...

MARDEN: I had very little to do with the Computation Laboratory after that because they became a separate operating division. They had problems from the Bureau of Standards but also a great many problems from other government agencies because the Computation Lab at the Bureau of Standards constituted a central government facility for other government departments which did not have their own computers or which had overload on their own computers. In my case, the Systems Research and Development Division, most of the work came from other

government agencies. Now we are speaking of a later stage, you understand, in the mid 1960s and onward. Most of our work came from other government agencies. I found it very difficult to get money for research in software which our people wanted to do because I could get enough outside money in transferred funds. There was always competition for available in-house money. The money which came to the Bureau of Standards, appropriated by Congress, was divided up according to need of the various divisions. If you had enough money from outside agencies and somebody else was crying for funds, it was very hard indeed to get funds to carry on the research which you felt important for your own division to do.

ASPRAY: I see.

MARDEN: I don't know if you have those problems in universities.

ASPRAY: There are similar sorts of problems, I'm afraid. Can I go back to examining the question of your training courses for programmers again? You have described to me the background material in binary arithmetic and in machine language programming. How much did you teach the new programmers about the architecture or electrical engineering of the machine and how important was this? Did you feel it was important for teaching efficient programming techniques? Can you elaborate on that?

MARDEN: Yes. At the beginning because of the limited memory we had to try to program as efficiently as possible. We created a library of subroutines, of the usual transcendental functions, square root and so on, but we tried to make our programs as short and as compact as possible. But this wasn't the main stress in teaching programming courses. We wanted to get people programming as soon as possible. After they had done some programming, knew what it was all about, and had put their own problems on the machine, they were much better able to understand programming for other computers, for example. At the end of the training course and at the end of their little project which they all were required to do, each one was placed as an apprentice with a more experienced programmer to work as that person's assistant for awhile until he or she was given a project to do alone. Many times most of our work was in teams, anyway, the team made up of two or three people or more. This procedure had many advantages;

if one person happened to be away, the project could be carried on. Since mathematicians ran their own programs on the computer, if one person was not available to run the program another one could. But after the IBM 704 was developed, which I think happened about 1954, the principal computer in use at the Bureau of Standards in the computation laboratory was the IBM 704. We then had training courses by IBM personnel in the use of their machine. Our people who were already programmers easily learned how to program the 704. They learned FORTRAN, and in some cases COBOL, and began to program for the 704 or for UNIVAC. Our people were working also with agencies which had UNIVAC machines, and they were able to adapt quickly, after having had our basic course in programming which had featured programming in machine language. Now, it isn't that I'm saying that programmers should learn machine language first. I don't mean that at all. It was the only thing we had when SEAC became operational. We had no other languages, and it did permit programmers to understand how the machine worked. They were in a one-to-one correspondence with the computer.

ASPRAY: While we're on the topic, what was the attitude of the more experienced programmers at the Bureau of Standards toward the introduction of higher level languages? Was there a certain resistance to them?

MARDEN: Most of them welcomed it because it made their programming so much easier. It also eliminated many opportunities for error, which was one of the main advantages. Every programmer's nightmare is debugging a program. Anything which eliminates chances for error is always welcomed, I think. Haven't you found it so?

ASPRAY: Well, generally that's right. The reason for my question is that when I'd been exploring the history of FORTRAN several years ago I found that there was a certain reluctance within the first year or so after FORTRAN was made available at the big research centers where scientists were already very competent at machine level programming because they felt that they could code much more efficiently than FORTRAN was able to.

MARDEN: That's quite true. But then you had enough memory. The 704 had enough memory for most of the requirements of the Bureau of Standards. So that there wasn't the necessity to code that efficiently. Now, with the SEAC, I perhaps should go back and mention that it was a four-address system originally. The first two addresses

would contain your operands, the third address would be the address where the result of the operation would be stored, and the fourth address indicated where you wanted the computer to go for its next instruction. In other words it didn't go sequentially unless you told it to. We later could increase our memory by changing to a three-address system, which we did, which eliminated the fourth address. Then when we had a jump in the program, we had to have a complete instruction which took care of that. But at the beginning, when we had a four-address system, when we were coding for the acoustic memory, we knew the length of time it took sound waves to cycle through the mercury delay line. By choosing an optimal address for the next address for the computer to go to you could cut down on the execution time of the machine. We did that as well. But any time when we could save an instruction, we did.

ASPRAY: I'd like to go back and ask some other questions about the early days of SEAC if I may.

MARDEN: Certainly.

ASPRAY: One of the questions that comes to mind is brought on by your comment that there was competition regarding reliability between these two techniques, the mercury delay line storage and the electrostatic storage. What was the outcome?

MARDEN: Well, there wasn't really competition between them. The idea was to try both of them and see which might be more reliable. I don't know that there were advocates of one system over another. It was a typical scientific approach: let's see which is more reliable under all conditions. We had temperature and humidity control, of course. The mercury delay line memory was operational first and seemed to be more reliable at the beginning than the electrostatic. The electrostatic did come along and was faster, so that there were certain advantages of the electrostatic over the mercury delay line in that respect. But by the time any decision might have been made to say let's build a big computer with just this type of memory, there were other types of memories.

ASPRAY: I see.

MARDEN: So that the question didn't arise.

ASPRAY: Yes. You've also mentioned a number of other projects going on at the same time: the UNIVAC project, the Institute for Advanced Study project, the Whirlwind project. What kind of communication was there between your group and those other groups? What did you learn from them and what did they learn from you?

MARDEN: I don't know if this should be off the record. Would you turn it off for a minute please? Well, in the case of the Whirlwind people a professor and two or three graduate students came down and did receive instruction from me in the very early days. This was shortly after SEAC was finished. In the case of Princeton, I had mentioned that I worked with some of their atomic scientists. The mathematicians working on some of their equations came down to Washington and I worked with them. I did not know what the equations meant at the time, for example what the Xs and Ys referred to. But my mathematics was adequate for me to work with them and program with them the equations to be handled by the SEAC. In addition, Von Neumann and Goldstine of course were programming for the machine later called the MANIAC, which was being built at Princeton. They sent down some subroutines and some programs to be looked over or checked by personnel in our section of the Mathematics Division. (Incidentally, that machine, as well as the SEAC and possibly others came about because of the ENIAC.) Everybody who had worked on the ENIAC seemed to go away with an idea for a logical design for a computer. One of those people was Dr. Samuel Lubkin, who designed the SEAC. I have not mentioned anything about the SWAC. SEAC meant Standard's Eastern Automatic Computer. There was a design for Standard's Western Automatic Computer built out at UCLA. That computer eventually, I think, became operational or semi-operational, but was never used.

ASPRAY: That was the Harry Husky design?

MARDEN: Yes.

ASPRAY: So if I may summarize what you've said for your agreement or disagreement, a number of other groups, for

example the group at MIT and the group at the Institute, came down for additional training, especially in learning to code the machines or coding procedures.

MARDEN: Yes, it may be that they just wanted to see how we operated and to get acquainted with a machine which was operational when they did not have one.

ASPRAY: I see. And that you, in turn, had learned primarily, that is your group had learned primarily, from the ENIAC in terms of the overall logical design for a machine. What about the UNIVAC machine which was being built at the time for...

MARDEN: Yes, Eckert and Mauchly had been with the ENIAC. That computer was constructed at the University of Pennsylvania and every computer in this country, so far as I know, that was being designed coincidentally with SEAC, was designed by people who had been involved with that project at the University of Pennsylvania, with the ENIAC.

ASPRAY: I assume.

MARDEN: Certainly Dr. Samuel Lubkin was there.

ASPRAY: I guess the only exceptions I can think of are the Bell Labs developments and Harvard, but yes, I think that's right.

MARDEN: Harvard didn't have one yet. They had very fast calculators, but theirs wasn't a computer. I went to a symposium at Harvard about the time the SEAC was finished, maybe slightly before or slightly after.

ASPRAY: There were conferences in 1947 and 1949.

MARDEN: It may have been in 1949 then. I heard Dr. Aiken speak. Someone asked him if Harvard had any ideas or any plans to build a computer, and he said "not at that time" because the components were not reliable enough.

ASPRAY: Your group obviously disagreed with that?

MARDEN: Yes. You can say my group because you don't have to include me personally. I wouldn't have known enough to agree or disagree.

ASPRAY: Would you like to make any further comments about the importance of your group as the first group in the government knowing about computing, being the experts, and giving advice to a variety of other agencies of the government? What impact did that really have? Did other groups come to you when they wanted to design or purchase their own machines or write up a set of specifications for bidding procedures on machines?

MARDEN: Yes they did. Particularly to Dr. Cannon. I would not have been involved for the most part in that because they would go to the head of the Math Division or Electronics Division and then be shunted to at least a Section Chief. Dr. Cannon was chief of our section then. I don't know if Dr. Cannon is still alive; but if he is, he would be able to answer those questions much better than I, and so would Dr. Ralph Slutz. I was lower down on an operating level and the people coming for advice would have been coming to the chiefs. But we were involved when we were called in to help. We were the working stiffs.

ASPRAY: Would you like to make some overall evaluation of the SEAC project? What do you think were the most important features of success--the overall importance of the machine, or programming of it, or use of it. We've talked about a lot of the specifics.

MARDEN: Well, I think it has an astonishing significance which has been mostly overlooked. People say the ENIAC was the first computer. Of course it was. But it was not a computer as we know them now because you couldn't write a program in advance and prestore a program. The SEAC was the first computer with a memory. It's

the one on which everybody, you might say, cut his teeth. The Atomic Energy people learned from it; the Pentagon learned from it; other agencies of government came in. It's the one that taught people what computers could do, and from that there was sort of an explosion. But anytime when you're doing an experiment, you have to have a beginning and you learn from the first experiment. In my opinion, this was the first experiment in computers as we know them today--computers with memories. We learned a lot about numerical analysis. People had been used to solving problems analytically--particularly mathematicians. They had to reorient their thinking to solve problems by numerical approximations because that's the way the computer handled things. So, from that evolved the new math. You may recall in the mid-1950s the professional societies had committees--three different ones--which decided, made recommendations, about curricula for elementary schools, high schools, and colleges and universities. From this evolved the new math. So, it affected drastically the teaching of mathematics in schools, from elementary schools up through the universities.

ASPRAY: What was the importance in terms of particular technical developments? We have already discussed, at least briefly, the issue of the mercury delay line versus the electrostatic storage and the fact that by the time this experiment was over, the decision had been made because technology had passed both of those by.

MARDEN: Yes.

ASPRAY: What other sorts of lessons did one learn from the SEAC in terms of technical training or technical issues, if any at all?

MARDEN: I think probably we learned some things about magnetic tapes. UNIVAC was having difficulty with its magnetic tapes at the beginning. Eventually, you know what they were using? Fishing line because of its elasticity for stopping and starting the tapes. If you suddenly stopped the tapes, they tended to break or overrun. The Bureau of Standards had magnetic tapes and the research that they did on magnetic tapes probably helped the industry in general; I don't know how much. We also stored information on magnetic wire. We had a machine which was developed just to read the wire and read onto the wire, I mean inscribing onto the wire from punched paper tape.

Then you could have another wire cartridge with data on it. We could write on the wire directly from the computer at a very rapid rate, whereas before we had only had the punched paper tape. Then a machine called the outscriber would read the wire and produce a punched paper tape which then could be put into a teleprinter. So far as I know, no other computer did that. The need was bypassed for that too with more modern advances; but it was a very rapid way of getting in and out of the computer, much faster than punched paper tape.

ASPRAY: I see. What did you learn and say, other agencies of the federal government learn about taking problems and making them suitable for solution on the machine? setting up big problems? about which were suitable and which unsuitable problems? You told me earlier--I've forgotten whether it's on the tape or not--that there was a lot of difficulty in understanding how to put data processing problems on the machine.

MARDEN: Yes, because they had never been properly defined. The idea was to sit down with the people who knew the problem, who'd been working with it for years (we didn't know the problem); sit down and form a flow chart sitting around the table. In making a flow chart which is an exact design for the programmer to follow, in creating a program for the computer, you have to ask certain questions.

TAPE 2/SIDE 1

MARDEN: The only way to define these problems was by writing a flow chart which described it in detail. And a flow chart is so definite that people are forced to define the problem. If you have a flow chart from which a programmer is going to write detailed instructions for the computer, you sometimes have to face questions never faced before, certain situations that had never been thought through. They had vague opinions and vague definitions in mind, but nobody had ever forced them to sit down and define in detail every aspect of a multi-faceted problem. And in having to define it, in developing a flow chart, for the first time a problem would be defined.

ASPRAY: Can you give me some examples of the kind of large data processing problems that were successfully solved on the SEAC?

MARDEN: Well, large inventory problems are one of them. And then the problem for matching chemical structures that I worked on is certainly not a simple problem. Consider that there are probably three million organic compounds that have been synthesized and chemists may draw diagrams, chemical structures, in different ways which may be mirror images of each other or which may be drawn in other ways; the same structure may be drawn in many different ways by different chemists. You can't do an overlay sort of match of a structure if you want to find a part of a structure combined in a bigger configuration. You've got many problems to face. You've got the question of whether they're single or double bonds. You've got many different problems to face, and I don't think that problem had ever been thought through carefully enough. We thought that we had developed a system which was fool-proof. We had put in lots of screens in order to cut down the running time. Our biggest problem in that one was the accumulation of the files to be searched--the file of chemical structures, accumulating it in an unambiguous way. The system that we developed (for the Bureau of Standards and the Patent Office), with several of us working on it over a period of two or three years, was one which one of the chemical companies--I think Monsanto--later developed and gave us credit for. Then, since it was done with government money, it was in the public domain. Later it was the one which Chemical Abstracts took and which they employ today, I believe. I'm not up to date on that; but previously they had tried to have a linear description of the structure, a line notation which would be unambiguous. They had never developed a satisfactory linear notation. Dr. Dyson in England was for a time working with Chemical Abstracts and there were other chemists here who had developed linear notations. Not one had ever been found to be completely unambiguous.

ASPRAY: And yours, your system was?

MARDEN: Ours was. It was not a matching of linear notations. It was, in effect, very simple-minded; just sort of a topological tracing.

ASPRAY: I see. Let me continue in the same vein on this question. Concerning the more strictly scientific and mathematical questions, which were really issues of numerical analysis --I know there were a large number of

high-powered mathematicians who were later hired to work on them. Would you say that there were advances in connection with work on SEAC in the development of a new kind of numerical analysis for application on the computer?

MARDEN: I did not see any great developments in that area, no. There were many high-powered mathematicians brought in. There were some foreign mathematicians who could not have been employed by the government who worked with us. We had an arrangement with the American University and they were employed by American University. We had a financial arrangement with American University whereby they could come over and work for us. They were actually in the employ of American University, but we paid the University in some way. They had to teach a course at the University. Most of those people simply tended to have a project and worked on it by themselves and I did not see any great advances in numerical analysis as a result of their work. Many of them continued their own research and published papers and that's very good; it's good for the government. It needs to be done.

ASPRAY: The reason I had asked this question is that there was a very early paper by von Neumann (sometime in the 1940s) in which he pointed out that many changes would have to take place in numerical analysis to accommodate problems to computer solution, and he called on the community to work on this problem.

MARDEN: Well, we'd have to break down hyperbolic differential equations into simple arithmetic steps, for example. But most people with a fair amount of graduate work didn't seem to have any trouble. I don't think we had to bring in lots of specialists for most of the flow-charting or the programming of problems which might involve some fairly high-powered mathematics.

ASPRAY: Would you explain on tape to me how you helped to improve working conditions for women at the Bureau?

MARDEN: I think I initiated part-time work for women at the Bureau of Standards. There were two mathematicians

and one physicist who were very competent, reliable women who had children at home. One of them could only get a babysitter for two days a week, and she wanted to work those two days. Another woman wanted to work when she could get away from her child so that she could keep in touch with the development of the profession and then return to work full-time as soon as the child went to kindergarten. A third one had three children in school and wanted to be able to go home at 3:00 so she would be there when her children came home from school. I discussed with our personnel department the problems attendant upon having these women work part-time. They gave me all of the disadvantages to them, for example, health insurance record keeping for this type of work, etc. But I told them that I believed the talents of these women should not be lost to the country. They agreed to let me employ them on a trial basis only, and it was more successful than we had dreamed possible, because these women were thinking all of the time. They didn't turn off their brains when they left the office. They were thinking about problems from work when they were doing the dishes or feeding the children. As a result, at the end of a week they turned in almost as much work as the people who were there working full time. When you're doing creative work, or when part of the work is creative, you're thinking about your work when you're asleep. You're thinking of it certainly the minute you wake up. That was the pace of these three women. As a result, other divisions began to request part-time workers as well.

ASPRAY: Just for the record, the personnel office was quite reluctant and was only willing to try this on a trial basis...

MARDEN: On a trial basis. We did it for a year and there was never any question about it after that.

ASPRAY: Could you describe something about the day-to-day activities in the laboratory? What were your days like? What was the interaction between staff like?

MARDEN: Well, every day when we went into the office, I think all of us went in with joy in our hearts because we loved our work. We were doing something nobody had done before and we were all excited about our work. We were given assignments by the chief of the section, Dr. Joseph Levin, and at least half of the time most of us didn't know exactly how to carry out an assignment. One tried--as I mentioned before, mathematicians tend to try to solve

problems they don't know how to solve. We all had blackboards in our rooms and for the most part a couple of people shared an office. When we were faced with a problem that we didn't quite know how to tackle, or perhaps we could see a way but we didn't know the most efficient way, sometimes we would get together, five or six in an office, for a brainstorming session--somebody at the blackboard describing the difficulties, and everybody seemed to have input. We helped each other because we were, in a sense, all stumbling around in the dark doing something that we had no clear pattern for. Sometimes even if we could see how to do something we were unhappy because it seemed inefficient. We didn't have, in the mathematical sense, an elegant solution. We would try to find the elegant way to do something. It was a lot of fun because we were all exchanging ideas in a very cooperative and friendly way. There was, as I recall, no rivalry and no vying for position. Everybody was helping everybody else. It was a fun time.

ASPRAY: You mentioned to me off the tape that after a certain period of time a more formal structure came into the laboratory.

MARDEN: Yes, Dr. Levin tried to bring discipline and an orderly way of proceeding into the laboratory. He set someone to scheduling computer time. Time was scheduled on the computer rigorously. People were required to write reports of their projects. He began to accumulate a library of subroutines. From time to time we published in the Bureau publications (primarily in the Bureau publications), which were available to other government agencies, results of work being done in the lab. I should refer again to certain mathematicians from this country, and certain foreign mathematicians who were working partly at American University teaching courses and partly spending time at the Bureau of Standards. Those people were brought in primarily with the hope and the thought that they would help in numerical analysis because of the new way problems had to be handled when using computers; instead of being handled analytically, numerical approximations were employed. Most of these people were concerned with the ongoing work they were engaged in before joining NBS. Their work was largely supported by funds which the Bureau had appropriated directly as well as by some transferred funds from Atomic Energy Commission (which was using the computer heavily), and some funds from the Pentagon. These people published very useful papers during the period of time they were there. However, after Dr. Astin was fired after the ADX 2 scandal, the battery additive

scandal, the Bureau began to be broken up and part of it, the part that developed the proximity fuse, was given to Army Ordnance; part of the Bureau was transferred out to the Navy in California; and the radio propagation division was moved to Boulder. (And I might say that we used to get time signals beautifully when they were broadcast from the Bureau of Standards here. We have a sailboat and time signals were very important to us when crossing the Atlantic. After it was moved to Boulder, we could never get proper time signals from there. We'd get them from Canada when we crossed the Atlantic; we could get the ones from Boulder only when we sailed in the Pacific.) The Bureau was cut down quite a bit in its funding so that we could not carry on as ambitious a research program as we had formerly, and the section in which I was working (which had been doing as much research or more than developmental work) became a sort of computation laboratory. After a couple of years I transferred from what had become only a service group in the computation lab to the Electronics Division, Sam Alexander's division. Does that answer your question?

ASPRAY: Yes. Were there difficulties in attracting good people to work in your laboratory?

MARDEN: One of our problems at the Bureau of Standards always was in getting top-flight people when we couldn't pay them as much money as industry could pay them. I was once offered exactly twice my salary to work for a company. People whom I trained went out and were making two and three times my salary in a short period of time. One of the ways in which we tried to make up for the lower pay was to get post-doctorals, who were paid for by the National Science Foundation. That way we could get some of the very brightest people in the country to work at the Bureau of Standards, and then we didn't pay for them. Many times they would stay on and accept the money that we could pay them after their year was up, when they could make more outside, because they liked the atmosphere and the work and the scientific freedom that we enjoyed there. In that connection, in the last two or three years that I worked as assistant to Dr. Grosch, who was director of the Center for Computer Sciences, I handled our international affairs and our post-doctoral research. I went to the top universities in the country which had computer science programs to recruit, and most of the post-docs that we got from the computer science programs would have had at least four years of post-graduate work. They would normally have a great deal of training in electronic engineering, mathematics, physics, and in many cases in psychology because some of them were working with artificial

intelligence. Of course, the Bureau of Standards was competing with other people who wanted these post-doctorals. They were very desirable people to have. After we would get them to make formal application to the Bureau of Standards, then I still had to lobby for them with other people at the Bureau of Standards, who wanted their own candidates; when there was only a limited number that we were allowed to take on every year. But this was one way of acquiring some of the top-flight people in the country and having them financed by other than our own meager funds. As I said, National Science Foundation paid for them in collaboration, I think, with National Academy of Sciences.

END OF INTERVIEW