Interviewee: Robert Everett
Interviewer: Richard R. Mertz
Date: January 26, 1970
Repository: Archives Center, National Museum of American History

MERTZ:

This is an interview of Mr. Robert Everett of MITRE Corporation conducted in his office on the 26th of January 1970.

Mr. Everett, would you like to describe your early schooling and education?

EVERETT:

You mean starting with college, I assume.

MERTZ:

Starting even before then, grade school and high school.

EVERETT:

Well, there was nothing very exciting about the grade school and high school, I guess. I did my undergraduate work at Duke University, graduating in June of 1942 with a Bachelor's in Electrical Engineering, and went from there directly to MIT.

MERTZ:

What was it that attracted you to MIT?

EVERETT:

Well, I never thought of doing any graduate work. At that time, graduate degrees for engineers weren't as common as they are now. And so, in the spring of that year, I interviewed a lot of people that came around the University along with, as is customary. And the man from GE suggested that since my academic ratings and so on were good, that I might consider joining their 6-A program, which is the cooperative program between GE and MIT. This is ordinarily a combined undergraduate and graduate program, but apparently, they did let in a few people at least at the graduate level.

So, I wrote to MIT, and as a result was admitted to the graduate school, with the understanding that it was one of these alternate terms at MIT and at GE. It turns out I never did get to GE, however.

MERTZ:

When was this in '42, do you recall?

EVERETT:

You mean—

MERTZ:

In the fall?

EVERETT:

No, I entered in the summer.

MERTZ:

In the summer.

EVERETT:

Yeah, that--well, the war was on and they accelerated the class, so we actually graduated from MIT--from Duke that is, the engineers did the first of May. So, I just went home and did nothing until June, when summer school started.

MERTZ:

Where was home?

EVERETT:

In Niantic, Connecticut. And so I went to summer school at MIT and--okay, I went to summer school at MIT, and in just about the first of August, I went to work in the Servomechanisms Laboratory for Jay Forrester.

MERTZ:

Now this was…had you known anyone at MIT prior?

EVERETT:

I didn't know anybody. I had paid a visit to MIT, but I didn't know anybody else. I was just a student.

MERTZ:

What led you to the Servo Lab?

EVERETT:

Well, the war was on, and there were a lot of things going on. And in talking to faculty members, it seemed like it might be a good thing to get a job as a student. I was in the Electrical Engineering Department, and the Servo Lab was one of the EE laboratories. So, it was suggested that I talk to Jay Forrester, and he came and interviewed me, and I guess he was desperate for people to work for him, so I went to work for him as a student.

MERTZ:

Had you, at that time, a particular focus of interest in the EE field?

EVERETT:

No. At that time, you know, EE was just sort of a thing. There weren't really specializations within it. Most of the emphasis was on electric machinery and things about what specific things I wanted to do. I had had a very limited amount of experience in electronics, for instance.

MERTZ:

Did you work out an arrangement then as a student and part-time work, or were you working full-time?

EVERETT:

I was...I was a full-time student, and then worked in the Servo Lab. Started out, it was supposed those were war years, you know, and you worked all the time. So essentially, I used to get up in the morning, go to school in the morning, do my homework, eat lunch and then after lunch, I'd go to work and I'd work until late in the evening. So, I was carrying essentially a full-time load as a student and I was also working about forty hours or so a week at the, at the Laboratory. It didn't seem so bad then, but I don't know if I could stand it now.

MERTZ:

At that time, correct me if I'm mistaken, if I recall correctly, the…Jay Forrester was working on a hydraulic mechanism for a gun mount, or was this later when he got involved with the, the stabilizing of radar antenna on ships?

EVERETT:

Well, he was working on hydraulic servo mechanisms and hydraulic pre-amplifiers. I guess he'd been working on a gun system before I got there. When I got there, he'd started working on a stabilized radar mount for ships, and that was the job that I worked on.

MERTZ:

I see. And how did the division of labor break down on this project? Was there any particular area that you were responsible for?

EVERETT:

I was primarily responsible for the test portion of it. Jay had decided to develop some new pumps and motors as well as a modified amplifier. And... I was also

And this was on a, a modification of the hydraulic pre-amplifier with different kinds of dash-pot arrangements. That's what I did my thesis on.

And then he built the pumps and motors and eventually we put the whole thing together, and I was sort of doing the testing on the pumps, and then eventually on the servo as a whole. There were only a few people working on the job. There was a design engineer, and…

MERTZ:

Do you recall who they were?

EVERETT:

Well, Lennie Dozier, Leonard Dozier was the design engineer, and Steve Dodd was working at the same time with me. He'd, he was also in 6-A at MIT, so he was also a student. Dozier, as I remember, was a full-time engineer. And there were a number of technicians and so on that were working on it as well, and eventually a few other people joined the group. Jay was the boss and he was the head designer as well as the manager of this group.

MERTZ:

Then, who then headed the Servo Labs?

EVERETT:

Well, not very much. He was there, and Gordon always makes his presence felt, but I was working directly for Forrester, and by that time Forrester and Brown had come to an understanding, which was that Forrester ran his own part of the show. So, I knew Gordon Brown, of course, but he didn't intervene very much in what was going on.

MERTZ:

Also, if I recall correctly, in the history of that particular radar mount, that had gone in, started to go into production just about the end of the war.

EVERETT:

That's right. We built a small number of them, like two or three or four ourselves, in the Servo Labs shops, and then a contract was let to the Westinghouse Air Brake Company, and they built a number of preproduction models, so-called, a dozen or so. And I went out to Pittsburgh to oversee the tests of those models, and then the machine was put into production. And then the war ended and the whole thing was canceled. As far as I know, none of the production units ever came out. They were in various stages, but none of them were ever completed. The pre-production models were used for some stabilized radar for some of the large carriers, the Midway and the FDR, I believe, and they were used for several years in that purpose, but eventually they were replaced with newer models. These were essentially orphans, and the spare parts and maintenance, and so on.

MERTZ:

Well, they didn't have much chance really to be adequately field tested.

EVERETT:

Well, the first ones, of the ones that were made in the laboratory, one unit was put on a light ship up in Portland Harbor and it ran for, oh, a year or two on that job. I remember we used to have to go up and service it once in awhile.

MERTZ:

I bet that was a cold venture sometimes.

EVERETT:

Climbing up that mast coated with servo oil, while the…you…was one of the things I liked least. [Laugh] But .. as I say, the preproduction models were quite thoroughly tested and eventually were used And as far as I know, it would, it would have made an adequate, certainly an adequate machine. It's, it was not that there were not other servos suitable for this purpose available, it was just that this was a smaller, lighter, less expensive device which was hoped to be in wide use for stabilized ship antennas.

MERTZ:

This brings us up, I gather, roughly to about 1944 sometime, or is it even later?

EVERETT:

That was about 1944. And... the war was coming to an end group do? And by some process that I can't quite recall, Jay began talking to the Special Devices Center and Luis de Flores, about the possibility of building this Airplane Stability Control Analyzer.

MERTZ:

There was a former MIT graduate, Perry Crawford, who at some point in the early history of this, did wind up, I think, in the SDC group. I'm not exactly know if he knew Forrester before he --

EVERETT:

I think they knew each other, but I didn't know Perry for a couple of years after that. I was just a sort of test engineer at that, at that time.

MERTZ:

You had graduated?

EVERETT:

I got my degree from MIT in, in June of 1943. After that, I was a full-time staff member in the Servo Lab.

MERTZ:

Were there any other alternatives that you considered at that particular point, once the war was over, and it looked as if maybe the group would be disbanding? Had you at any particular time considered industry, for example?

EVERETT:

I don't recall thinking about it very hard. I wasn't married and I was making enough money to live on. And the work was interesting, and the people were interesting. And I never seriously considered looking for a job. By the time the war actually ended, we were involved in, in other things and there seemed to be continuing support, and it looked like an interesting place to stay. I guess I always assumed that some day I would leave and go into industry, but nothing more specific than that.

MERTZ:

There were a number of members of Forrester's group in the Servo Lab who stayed on in this transition period, in what you might call the pre-Whirlwind phase of the activity, the aircraft stability things.

EVERETT:

The Servo Lab was somewhat different from the Radiation radar lab. It was sort of created for that purpose. It was very large, and it was disbanded at the end of the war. The Servo Lab really had its beginnings before the war. It was run by Gordon Brown. As far as I know, there was never any presumption that it would disappear when the war was over. So a large number, I think, most of the people in the Servo Lab continued at MIT, and when the war was over, doing various things. They gradually distributed over the various and sundry places, but there was no mass disbanding of the Laboratory.

MERTZ:

One of the perhaps logical elements in considering this particular project for the Navy was the devices. Was that, at this particular point, the main thrust of the involvement of Forrester and, and his personnel, in an attempt to adapt the design?

EVERETT:

Yeah. There was no consideration certainly in my mind, and as far as I know, in Forrester's mind, in 1944, of building this device out of anything other than analog computers of one sort or another – analog servos and things. The ones that had been built by the Special Devices Center, which were specialized to particular airplanes, used electric servos.

There were also problems of building a cockpit that would be realistic and have the realistic forces on the controls and so on, which was a considerable part of the problem, and those were fairly obviously suited to hydraulic servo mechanisms. So, it seemed like a, a reasonably close fit to the interests and experiences of the people in the Laboratory.

MERTZ:

At this point how, roughly, who were the people who carried over into this project, from the earlier radar stability?

EVERETT:

Well, there was Forrester himself, of course. There were several other people in the very beginning, I guess. Steve Dodd was still on the project, and Hugh Boyd, and I believe, George Schwartz, and a couple of other people whose names –

MERTZ:

Was Weiser –

EVERETT:

Weiser was in the Laboratory, but he was not working on this project at that time.

MERTZ:

You were in that group also?

EVERETT:

I was in the group, yes. I worked for Forrester from the day I went to the Laboratory until he left in '66.

So, we started working on it as an analog computer and there were a lot of very serious problems about it, having to do with its dynamic range, and trying to make it flexible and so on. And this led Jay when he…to become interested in the digital computer approach, which I guess really, he started thinking about seriously in late '45.

MERTZ:

The, in that period, a number, I take it a number of visits were made to see some of the advanced, then advanced models of the Link Trainer, and other…

EVERETT:

Yeah.

MERTZ:

…devices used.

EVERETT:

We went around and visited those and saw the various trainers and so on, that the Special Devices Center had built. We were working with the Aeronautic Department at MIT. They were responsible for the aeronautical, the wind tunnel measurements that would enable you to the parameters in. So, we spent a considerable amount of work on the mathematics of the problem as well. Yes.

MERTZ:

One of the questions there was, what were the principal considerations in your view that led to the abandonment of the, of the analog approach to aircraft stability or simulation of real flight?

EVERETT:

Well, I think that if there had been no such thing as a digital computer, or if the art had been less far advanced, that we would probably have gone on and built an analog computer and it would probably have worked. Not probably; it would certainly have worked. The problems as we saw them, were that limits in the dynamic range of the analog signals, problems of noise, the problems of the very large number of integrators and multipliers and so on, that were required to solve these extensive equations, all this would lead to a very large and expensive machine. We also felt very strongly that we ought to have a flexible machine. If we were going to build a machine of this size and capability, it should be useful for other purposes as well, so we wanted to build the thing with plug-boards so that you could reconnect it. And this led to some fairly serious problems.

Then when the digital computer came along, and it seemed to offer a solution to these problems, because inherent in the digital computer are such things as large dynamic range and high flexibility and so on, that it just seemed like the right thing to work on. The potential capabilities inherent in the digital computer approach seemed to us to be so much greater than the analog computer, that we switched over.

MERTZ:

Wasn't there another group in the Servo Lab that indeed went along with the analog approach? Perhaps not in the flight simulation field but –

EVERETT:

Yes. This was Al Hall's group. There were several senior people in the Servo Lab, of which Forrester was one and Hall was another. He is now a Vice-President at Martin. And he had been working on a simulator for testing a guided bomb. And he broke off from the Servo Lab and formed a group called the Dynamic Analysis and Control Laboratory. And they, in fact, built quite a significant digital -- eh, analog computer for flight simulation and testing of flight components, guidance systems, and things of that sort, along with a quite elaborate platform that went with it.

MERTZ:

This was roughly contemporary with, if I'm not mistaken, Forrester and his group.

EVERETT:

That's right, it was contemporary. There was also the group at the Institute that had run out of the old Bush Differential Analyzer and then the Rockefeller Differential Analyzer, and were giving thought to digital differential analyzers. That was under Sam Caldwell.

MERTZ:

Right. To what extent, if you recall, was there much inter-influence on the part of the, the Hall group, the Caldwell group and the Forrester group? Were these viewed as more or less independent activities?

EVERETT:

From my vantage point they were quite independent, especially after we went down the digital computer to visit back and forth, but only as sort of friendly visits, not that we were working together.

MERTZ:

What at this point, if you recall, were some of the more important influences that affected the decision to explore the digital computer route as an alternative to what seemed to be at least an extremely difficult and possibly not feasible analog approach? That is, the work of which groups of people came to the attention of you and Forrester in this?

EVERETT:

decision to go from an analog machine to a digital machine was made by Forrester. And he went to some sort of meeting that was held at MIT on digital computers and thought about it a little bit and decided that was the best way to go, and said we were going to go that way. So it seemed like an exciting prospect to me, but as I recall it, neither I nor any of the rest of us in the group played any particular role in that decision.

MERTZ:

Did this did he at that time -- oh, the people who had participated in this conference obviously had been some of the influences which had been brought to there was, I gather, a period of education on the part of the principal people in the group learning about digital computing machines or the present existing state of the art at the time. Which of the ones that were in existence, or in the planning stage at that point, were the ones that seemed to be the most interesting in terms of your own activities?

EVERETT:

Well, there were only really two, two groups. I guess, there was some relay computer work at Bell Labs, but I don't recall that we had much contact with that. There was Aiken's group at Harvard, who built the Mark I and was working on the Mark II, and there was a group at the Moore School, at the University of Pennsylvania, the Eckert-Mauchly group. And they had, were just about finishing up the old ENIAC computer, which was the first of the vacuum tube machines which was designed for trajectory calculations, and were talking about building the stored program machine called EDVAC, which was also sort of closely attached to the Moore School group was Von Neumann's group at the Institute for Advanced Study. He was a major participant in, in what was going on at the Moore School. ….them, of course, and talked to the, to the various people. We got some people out of the Radi--curcuitry and storage tubes and things of that sort, added them to the group.

MERTZ:

Do you recall who some of these people were?

EVERETT:

Oh, dear. I guess I'd have to struggle with that. Let's see, Norm Taylor came aboard at that time. He didn't come from the Radiation Laboratory.

MERTZ:

I think he came from Bell.

EVERETT:

He had come from Western Electric, I believe, via another company whose name I've forgotten, which had gotten into some sort of difficulties. And Dave Brown had joined the group at about that time.

MERTZ:

Youtz?

EVERETT:

I don't know whether Pat had arrived at that time or not. Harris Fahnestock, I think he came out of the Radiation Laboratory. Pat .. –

MERTZ:

Boyd?

EVERETT:

Hugh Boyd had been with us before. He was still with us. We were still in the Servomechanism Laboratory building at that time. And Clint Ryder, I guess we got him from Radiation Laboratory, and Fran Swain, and Dick Brown.

MERTZ:

At this point, when you were more or less exploring what others were doing, did...I understand you got together every week, or fairly periodically, to discuss what each of you had been finding out about the digital computer field. Was there any division of labor as such, in terms of looking into different aspects of what it would take to build a digital machine?

EVERETT:

Oh, yes. There were…some of the people were more concerned with circuitry and some who were concerned with storage devices. I was primarily concerned with the

At that point, I guess, the, the Von Neumann-Goldstine, Bigelow work had been printed and circulated to…I don't recall anything in late '45. The times get all of mixed up. There was considerably more activity in the field the next year. By the next year there were several groups working and the Moore School held a summer session in the summer of 1946 in which interested people and practitioners, fifty or sixty of them altogether I would guess, went to Philadelphia and spent oh, like a couple of months listening to lectures and engaging in discussions, and so on, on this.

I went for the first couple of weeks. We handled it by a rotating period arrangement in which we sent a different one or two people every couple of weeks. There is in existence a report on that session which was issued by the University, and there are also reports written by our people from, of that work. That summer Jay took six weeks off and went back to his ranch. The Institute sort of gave the senior people, who had been carrying the burden, a six weeks' vacation, and he took his and he sort of left me in charge.

I guess that was about the first time that it sort of became apparent that I was the, the number two person in the group, although there was a lot of space, I might add, between number one and number two. Six weeks doing two things. One was to write a computer program to see how many instructions, and one thing or another, and what the problems were of writing the instructions that would enable the machine analyzer problems, because we were still working on that.

MERTZ:

You were using the ASCAP equations?

EVERETT:

Yes, we were using the equations which had been generated by the Aeronautics Department. I had been working before then, a considerable part of my time, on the hydraulic servos that went with the force generation equipment for the controls, but we had sort of stopped. That work had tailed off by this time.

The other thing is that we had begun with the intention of building a serial type machine, which was the kind that the Moore School people were promoting in the EDVAC. Primarily in discussions in the late spring and also in this summer course, the Von Neumann ideas for a parallel machine presented somewhat different problems of implementation, but also in my opinion, considerably greater possibilities. I was looking into that, had made some preliminary block diagrams of logical organizations for machines of both types.

So, although the program that I wrote in the summer of '66 -- '46 for solving these equations was based on the tentative serial type machine, I had pretty well convinced myself by the end of the summer that we ought to switch to a parallel machine, and succeeded in persuading Jay of that when he got back in the fall, in September.

MERTZ:

You mentioned that.

EVERETT:

So, we switched from the serial machine to the parallel machine.

MERTZ:

In the summer, there was a, a rotation of the personnel of the project in Philadelphia, and you went for a couple of weeks. Did you go with someone else when you went down there? The first of the two week –

EVERETT:

I'm pretty sure I did, and I'm trying to remember now. It seems to me that Jay went for a couple of days, and I think Dave Brown was there for awhile.

MERTZ:

But did everybody more or less, who was immediately on the project, manage to spend a little time down there? Was that the idea?

EVERETT:

Not everybody. I think by that time we had .. oh, there went, but a goodly number of them went for part of the each guy came back, he was expected to write up…ely a set of notes on what he had been listening to.

MERTZ:

And in addition to the...the problem of serial versus parallel, what other considerations in terms of design of the machine synchronous .. considerations, for example, of whether you're going to have a –

EVERETT:

Well, two things. One is that we decided not to build the full machine at first, but to build a smaller machine. In fact, we first of all built the so-called five-digit multiplier to test out the circuitry and the maintenance procedures and marginal checking. We then planned to build a machine of a short word link, 16 bits, and that was a sort of a prototype machine in order to make sure that we knew what we were doing and everything would function. And that machine would not have been adequate to do the airplane stability control problem. We were talking about building a still larger machine after that, a machine with more storage capacity and a longer word length.

Now as far as matters of fixed-point arithmetic, and so on are concerned, those things were all known talks about today were known at that time. And the they weren't done was that...twofold. One of them is that they added considerably to the complexity the machine. Nowadays flip-flops are very small things and people shovel them into machines by the thousands, but in Whirlwind, a flip-flop was two feet wide and a foot high and you didn't put in an extra…

We examined the mathematics of the problem and we convinced that it would not be necessary to put a fixed, a variable point machine, at least not to the variable point machine. In the case of the bit machine, we put in some special instructions to facilitate double length arithmetic, so that you could get 32-point precision if you wanted. And it turned out that -- although we thought we were building a very short length machine, and 16 bits is pretty short, it was very short by the standards of the people who were concerned about mathematical calculation such as Von Neumann and Aiken -- that there are a lot of machines today, you know, that have a register length of approximately that size. We chose 16 bits because it showed that was enough to provide an adequate instruction set and address the 2,000 words of memory that we were proposing to make. And that was the shortest one we could make. It was also 24 which was nice, but hardly necessary.

Such things as whether the machine was synchronous or asynchronous. The serial type machine was sort of almost by definition a synchronous machine that had to have a fixed clock. And when we came to the parallel machine, we decided to make one which would do a step every time you gave it a pulse. And this meant you could run it at whatever speed you could get away with, and also that you could stop it or step it through step-by-step and so on if you wanted to, and gave you considerably greater flexibility, as opposed to the certain other kinds which sort of did whatever they did, and then would go on and do the next thing and you sort of couldn't stop them.

MERTZ:

Do you have, do you have any recollection now of the chronology of some of these decisions that were made with regard to the, the word length, or the synchronous –

EVERETT:

Well --

MERTZ:

For example, the size of the memory, was that fairly early determined?

EVERETT:

That was determined fairly early in the game. There are certain things you have to determine. Much of it is a recursive process in which you make some tentative decision and then you come back later and you may change it. We started in that fall, and through the fall and winter, made the logical design of the machine, which was eventually printed in 1947 in, what is it, R-127, of which I guess you have a copy.

[END OF SIDE I]