Interviewee: Robert Everett
Interviewer: Henry S. Tropp
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EVERETT:

We really got started getting into the control analyzer in '44, and at that time we were talking about an analog. There was a tendency, so I recall, for MIT to sort of be identified with analog computers, as opposed to Aiken's work at Harvard. I remember when the first announcements came out about Aiken's work, I talked to some people in the computing business at MIT, and they all said: "Oh, well that's just a big adding machine. The differential analyzer, that's the way to go."

So, when we started working on it, we started working on an analog computer, and it presented very serious problems. And as I recall, it was in 1945 when we'd built amplifiers and servomechanisms and multipliers, and done a lot of planning work and so on, and had run into very serious difficulties in making a machine which would solve that elaborate set of equations. Jay would be a far better man to talk to about this, but he'd heard about the digital business, so he started. He got the thing converted from an analog machine to a digital machine in 1945.

TROPP:

So, it was kind of Jay's impetus and pressure that—

EVERETT:

Well, he--I guess I've forgotten now who it was that talked to him. But anyway, he got invited to a very select meeting about digital computers at MIT, and came convinced that the way to go was the digital computer, which was a very sound improvement on his part. In the way of the world, in 1945--I may have the years wrong. Let me think. No, that's right, I think it was 1945. In the way of the world, in those days, Jay, as far as I know, pretty much without discussing it with anybody, Jay just changed it from an analog machine to a digital machine. I can imagine what would happen to you today if you wanted to do a thing like that.

TROPP:

[Laugh]

EVERETT:

Back in those days, he was able to do it.

TROPP:

Let's back off to the analog period though, for a minute, because that's really the beginning of the Whirlwind project, and in terms of an analog machine, your main input were the Bush derivative differential analyzers. Is that the kind of machine and machine technology you were thinking of?

EVERETT:

No, they weren't. The first Bush machine and the second machine, called the Rockefeller Differential Analyzer, were essentially mechanical devices. The first one was strictly mechanical, all hooked up with shafts and what not. And the second one was essentially the same thing except that instead of hooking the integrators and whatnot together with shafts, they were hooked together electrically with servomechanisms. So really, it was the application of the servomechanism to the same basic device, which was mechanical in nature. We were talking about an electrical one.

TROPP:

An electrical –

EVERETT:

Where you used amplifiers and things of that sort. The questions of just how we were going to carry out the multiplications and integrations I don't think we ever quite settled. It's not all that easy to multiply electrically.

Now, people since then have built, it's well within the state of the art, for instance, today, to build a--in fact you can go out and buy an analog computer which would do that job. But the things weren't available in those days. Electrical integrators with the kinds of accuracy and the kinds of dynamic range and so on, that were required, were just not state of the art. They were difficult.

TROPP:

So, the state of the art for an analog device in the original conception, really was not up to the level that your needs required, in terms of the kind of machine that you wanted.

EVERETT:

That's right. Now, also the idea that Jay had, was that he didn't want to build a machine which was just specialized for that particular thing, he wanted a machine with some generality, so that you could do other things with it and you could also change your mind just about how you wanted to do that job, change the equations and so on. And that meant the machine had to be pluggable and had to have flexibilities to it, which made the job more difficult.

TROPP:

I think that's an important point. Would you say that at that point in time, Forrester was really thinking about a computer that could be made into a general purpose machine? Because that was really one of the big breakthroughs of von Neumann's kind inception.

EVERETT:

I think, as I recall it, he was interested in a machine which had considerable flexibility of use. No analog machine probably has the kinds of flexibility that the general-purpose digital computer has, but I think his conception was, in part, that to do the job properly, even that job required flexibilities. Also, with a machine of that size and complication and cost, it should not be so specialized that it could not be used for other purposes. He may have also had it in the back of his mind that if he didn't have it generalized to some extent, he might not be allowed to finish it. I can't say, you can ask him that. Anyway, when he saw the digital computer, I think he was struck that not only was it a solution to such problems as dynamic range and what not, which were troubling us in the analog machine, but it was also a machine that had fundamentally much more generality and much more flexibility.

TROPP:

The original ENIAC, which was the only thing around in '44, was a very special purpose electronic machine. It was built strictly to solve ballistics tables problems.

EVERETT:

That's right, and it was very hard to re-set it, although you could do it. But at the time of these meetings in '44, the basic conceptions of von Neumann and others about stored program calculators, were known, they had been invented. The realization had not come through, but they had been invented.

TROPP:

So those key papers of von Neumann, Goldstine, Burks, Mauchly-Eckert involvement in the ENIAC generalization—

EVERETT:

I don't recall that there were any papers that von Neumann—

TROPP:

That's right. The papers don't come out until '46. That's right.

EVERETT:

So, but we went around and looked at the ENIAC and talked to the ENIAC people. It was fairly obvious that you wouldn't want to build a thing like the airplane stability control analyzer using ENIAC technology. But at the same time, you could if you had to. As I say, the idea of stored program calculators, that idea existed, and proposals to build things like EDVAC, using acoustic delay line memories were in existence.

TROPP:

So, in terms of the state of the art, then, you mentioned really two key things. One, the notion of the stored program concept, a general-purpose machine, and then the technology from radar that enabled you to think in terms of acoustic line memory.

EVERETT:

Yes. Fundamentally, the technology that we proposed to use, did come out of that radar business. The use of vacuum tubes and trigger circuits, and things of that sort, had been developed for radar, and the storage devices which were being thought of at that time were essentially those which had been developed for NTI work in radar. The acoustic delay line, which is still used for NTI work, and the storage tube, which is still used for radar, and there was also talk about magnetic memories which I guess I don't know exactly what they were used for, but certainly they were around as drum memories, although they were very much slower. The kind of thing we were talking about, we were talking about acoustic memories or storage tube.

The first machine we laid out, which was laid out, I guess, in '45 or into '46, was a serial type machine. The idea was to use cathode-ray tubes. Cathode-ray tubes had been around for quite a long time, but their use in the war as indicators and what not in radar, had greatly improved the situation. Incidentally, there was the existence of such things as synchroscopes, so there was a certain amount of instrumentation available.

TROPP:

So that was already available by '45-'46.

EVERETT:

Yes. The oscilloscopes and so on. Well anyway, this machine that we were talking about was originally intended to have two kinds of memory. Memory for instructions, and there we were going to use cathode-ray tube with a card in front of it, for which you punched out holes if you wanted zeros or ones. Then, you point the electronic beam at the hole with a photocell that was on the outside, and if the hole was there, you got a signal, and if there wasn't any hole, you didn't get a signal. That was supposed to be the memory for constants and instructions.

You could modify those instructions in the machine by changing their addresses, and things of that sort, but you could not modify--that is, you take the instruction out of the memory, modify it and use it, but then you didn't put the modified instruction back. You could keep track of the indices in other ways.

And then we were talking about using cathode-ray tube storage of a kind which had been developed at the Radiation Laboratory for moving target indicators. A fellow named Forbes, as I remember, Donald Forbes –

TROPP:

Don Forbes. Yes that—

EVERETT:

Jay hired him from the Radiation Laboratory, and he worked with us for a while. And this was a serial machine and it was supposed to have these two kinds of memory, the idea being that it was hard enough to build a storage tube memory, an erasable storage tube memory, without proposing to store all the instructions in it. There were a lot of instructions.

Well, we laid out the system organization for a machine of that sort, and an order code for it, and in fact, in 1946, I wrote a program, which is probably in one of the notebooks someplace, of how to solve the astral [?] equations using that machine.

Anyway, talking about the state of the technology, the knowledge of how to build trigger circuits and what not, and multi-vibrators and so on, existed because those had been developed during the War--before the War, actually. In fact, there was a course on that kind of circuitry which was given at MIT, by what's his name? He was head of RLE for a while and may still be.

TROPP:

We can always look that up.

EVERETT:

I know him perfectly well. Anyway, he started to give a course immediately after the War. A whole bunch of us from the Servo Lab went and took it, including Jay and me. It was, you know, a pretty straightforward business. It was different from the sort of continuous way of electron tube design that we were used to, where you could draw operating lines and so on, tube curves and what not. He broke it up into sections, you know, turn the tubes on and off and what not.

Anyhow, that business was, you know, well known, and it was reasonably straightforward. Mostly the tubes that you had were tubes that had been designed for other purposes. The reliability and so on of the tubes, the life was not much, but you could make counter circuits and things of that sort, switches.

TROPP:

So essentially, that layout of your machine in '45-'46, all the technology that you needed for that particular kind of machine with the two kinds of memories, the serial approach, was totally feasible within the technology of the day.

EVERETT:

Well, you know, one of the problems with life is what does "off the shelf" technology mean? There are varying definitions of it, depending on what you are trying to do, who you are or who you're talking to. Now really, "off the shelf" technology means that you can build it, or you can get somebody who knows how to build it, and the stuff is available, and you can make it, and it will work, and there really isn't any question about it. That's what I call "off the shelf." It's fairly customary in proposals and so on, to talk about "off the shelf" technology as technology in which, you know, you can't really prove that you can't do it.

TROPP:

Mhm.

EVERETT:

That's a big difference.

TROPP:

Right..

EVERETT:

I would say that the technology at that time was more in the latter category. There wasn't anything about it that you had any certain knowledge that you couldn't do, but very little that had actually been done. For instance, if it was slow enough, that's a big advantage, but to operate at the speeds that were required for astral [?] problems which required, it's hard to estimate, but up to 100,000 instructions per second, that was nontrivial. 1,000 instructions per second, maybe, it was pretty straightforward as far as the speed of the tubes were concerned, but 100,000 presented problems.

Secondly, as I said before, the questions of life and reliability were just unsolved. There were many people in fact, that said that you couldn't make a machine of the size required, and keep it running for more than a few minutes at a time. The matter of the memories, the read-only memory, the one with the card in front of it, led to questions about how precisely and accurately and constantly you could point the beam of the electron of the cathode-ray tube. Because, you know, if you pointed it at the hole, it was not where the hole was.

TROPP:

I'd like you to again make that distinction of these two ways of looking at the state of the art, in terms of the "off the shelf" kind of technology, as opposed to the other kinds of things that you really can't say won't work attitude. And also, in terms of that kind of state, who there was to talk to in terms of what other people were doing, because again of the time period, the wartime security problem, the input from various wartime projects. Maybe that's too broad a thing, but—

EVERETT:

You want me to say this again?

TROPP:

Yes.

EVERETT:

Well, what I'm saying is that the question of whether technology is available or on the shelf is a very complicated question, and it arises all of the time in the business, it arises all of the time in military procurements and so on. I say it means different things to different people, ranging from one extreme, which says that available technology means that nobody can demonstrate it can't be done. It is not known to violate any of the fundamental laws...

TROPP:

Right.

EVERETT:

... which is the first law of thermodynamics.

TROPP:

Right.

EVERETT:

And to some people, you know, it's a solvable problem. The other extreme is to say it really is off the shelf, there's a book that tells you how to do it and people do it all the time and you can handle it." [?] And then, there is everywhere in between. So the issue then, is how available was the technology at the end of the War, as far as being able to build computers. I'll sort of give you my impression of the state of it, and I'll let you decide whether it's "off the shelf." By my definitions, it was far from "off the shelf."

TROPP:

Right, and I think I agree with you.

EVERETT:

It certainly met the criterion that there was no way to prove that you couldn't do it, and that turned out to be correct, because we did do it. At the same time there were lots of problems.

TROPP:

I guess that's why I'm asking the question about communications, because different people had different experiences. There were a variety of wartime problems. The problem of communication, of knowing what other people had done or were doing, what they had tried in terms of applying technology, was available to some and was not available to others. I guess part of the state of the art question is your accessibility to known work of other people involved in radar communication theory and so on.

EVERETT:

I would say that communication among the people who were involved in the budding computer business, both among themselves and with the people who had been through the radar business, was very good.

TROPP:

Kind of an invisible university?

EVERETT:

They knew each other, but there weren't all that many of them. You could talk to them. For instance, at MIT, the MIT Radiation Laboratory was one of the big centers of work in the radar business. They had done a great deal of fundamental stuff. There were a lot of very bright people there, and many of them were still part of the MIT community. Either writing the Radiation Laboratory series of books on radar, or as part of the Institute in one form or another, and you could talk to those people; they were perfectly open about it.

TROPP:

So, part of the state of the art, then, is really the talent and the experience that was available at that time and in your particular locale.

EVERETT:

Yes.

TROPP:

And a very important part of the state of the art, then, was the talent.

EVERETT:

The trouble comes, of course, because even though you had access to these people, it turns out that they can't really answer your questions. The reason they can't answer your questions is that the things that they were doing were somewhat different for different purposes. They could tell you all about how they did whatever it was they did, but then if you said to them, you know: "I've got to have a machine, you know, in which there are 10,000 vacuum tubes, and they work all the time, and if any one of them makes a mistake, it is all shot", he says: "Well, I never built anything like that. Radars aren't like that." [laugh]

TROPP:

That's right.

EVERETT:

So, this happens all the time. I think, in general, in technology, with the exception of certain kinds of artificial security, and by artificial I don't mean by that that it's bad, I mean that it's deliberately created by either military security or industrial security. Except for that, I think communication is really quite good amongst technical people.

But there is the problem that if you go to a guy you know, he'll tell you what he knows, but if you ask him something he doesn't know, you really can't expect him to stop working on what he's working on and work on your project. So, if you ask him, and he says: "I don't know the answer to that one," then where do you go from there? He'll talk to you about it, and say: "Well, I did this thing, which is not all that different, and it came out this way, but I can't say what would happen if you did it your way."

TROPP:

This is, then, in a sense, the kind of the state of the art that you were faced with in that '45-'46 time frame.

EVERETT:

Yes. A lot of these things had been done, the kinds of circuits had been built, cathode-ray tubes had been built, and there were certain kinds of instrumentation, but it turned out that nobody had ever done anything like this in almost any of the details, and these things stand or fall on details. Sure people made flip-flops, but nobody had made them in large quantities that had to work all the time, and nobody had made them so that they'd work as fast as you wanted. People had built gate tubes, but they generated different kinds of gates for radar purposes, not to pass the kinds of pulses we were trying to talk about. People had built synchroscopes, you could buy or scrounge synchroscopes, but they weren't just what you needed for computer work, so you had to modify them and build different kinds of scopes.

People had tape recorders, you know, which they used for recording conversations, and under certain circumstances, even recording certain kinds of digital information out of radar, but nobody had built a tape recorder for recording digital information in which, you know, every bit had to be there all the time, and no extras. People have built deflection circuitry for cathode-ray tubes for display purposes and so on, but nobody had built one that had to point the beam at a particular spot on the tube, and hit that same spot three weeks later. It wasn't that you couldn't, but that nobody had had to do that before.

So, it turned out that most of the things that had been done in the computer thing were different in major or minor ways than anything done before, and we had to do them for the first time. And when you do something for the first time, you get a lot of surprises.

TROPP:

I think that almost summarizes the whole thing, Bob, because what you're really saying is that the theory was there, even some of the practice was there in conceptual levels, but you were faced with brand new applications of-- Some of these things had only been around for a few years, and some of them had been around for a number of decades, but you were faced with brand new applications, brand new problems of reliability, brand new problems of checking capacity and accuracy, that had not been faced in the earlier applications of the same thing.

We might just take the flip-flop as a single example and sort of trace it through in terms of how it began, and how it had been used up to this point in time. And you might just take that as a single example to illustrate this kind of thing in some detail.

EVERETT:

Well, I wouldn't propose to drag out of the disordered storehouse of my memory anything very reliable about the state of the art flip-flops, but there had been Eccles-Jordan circuits...

TROPP:

Right.

EVERETT:

... and things around for a good many year.

TROPP:

Right. For 40, thirty years, anyway.

EVERETT:

If you took a course in vacuum tubes in engineering school, you might very well have put one of them together, and they worked. They sat this way, and then they flopped over and sat the other way. So you'd say, you know, there is such a thing as a flip-flop. Now, if what you are talking about, however, is a flip-flop which has a certain set of characteristics, it has to operate at a certain speed, maybe five megacycles.

You know, man always has a tendency to say: "Well, if the state of the art is... [?] a speed of one megacycle, I'll design the machine for five megacycles, because I ought to be able to get a factor of five sometime in the next year by working on it. In fact, if I can get a megacycle by just going out and soldering the thing together, I can get a factor of five." Well, maybe he can and maybe he can't, you see.

Secondly, you say: "Well, I want one that will stay on whichever side that I put it, you know, indefinitely, whatever indefinitely means--weeks--not subject to malaise in the circuits. I want one where the trigger which turns the thing over - I can have a reasonable tolerance, like I generate the trigger and perhaps I want to run it through a set of gate tubes, I've got to cascade these things. I don't want to have to reshape the trigger every time it goes through a gate tube, and that means the trigger that I start out with gets smaller and smaller as it goes down the line, so there's some maximum size for the trigger, and I want the thing to operate in some minimum size. That is, I want things to be reasonably far apart, as the thing is extremely sensitive to the size and shape of the trigger, and it is going to be a big job to make the machine. So, I want it very tolerant to the size of the trigger and its shape.

I want to be able to hang a lot of gate tubes on this, because I want to do some very complicated mathematical functions, logical functions, I want to be able to hang a lot of gate tubes on it, so that my flip-flop not only has to work, it has to work when it's got all this load on it. I want remote indicators so that when the flip-flop is a hundred feet away in another room, I can sit at a console and see what's in it.

I want it to be settable from either side as well as triggered from the middle. I want the thing to be able to work with the kind of power supplies that I have. I don't want to have a hundred different voltages or power supplies all carefully regulated to make for [?] the machine work. I want a relatively small number of voltages. The less demand upon the ripple and shock noise and what not of this thing, the better off we'll be, so I have restrictions as to what kind of power supplies.

I want to cascade vacuum powered gates. I don't want to have to build the thing up so that as I go through this gate, then it changes the voltage level of the pulse, so now the next flip-flop has to exist at a different set of voltages from the first. I don't want that, so I've got to couple these things together. I couple them together with capacitors, and then how do I make the thing work for one pulse, and none for a long time, or a whole string of pulses, like a thousand pulses in a row? How do I do that?

These problems all arose. So, you looked in your college textbook, and it told you how to make an Eccles-Jordan circuit, but that wasn't all the problem that you had when you started to make a machine.

TROPP:

I think you've really hit the sort of key theme for this section, is that [?] in a sense, the state of the art was there, but in another very real sense, it wasn't at all. You had - everything else was so new that it was almost like starting from scratch. You knew the kinds of things that you were going to have to work with, you knew the kinds of constraints, but nobody had ever done it before, and you had to see whether it was even feasible in every single instance. Maybe that's too compact a summary.

EVERETT:

This isn't new, you know. It's true in any new field. The ballistic missile people had it when they were starting to build ballistic missiles. People had built rocket engines and they had built tanks for cryogenic temperatures. People knew how to build gyros, and they knew how to build communication sets and things of this sort. The problem of making all of this stuff and putting it all together, and making a missile in which the stuff would all work and all work at the same time, and all work under realistic conditions, you know, required the solution of a vast number of problems. They were getting smart people and they were working very hard, and spending lots of money and solved these problems. Some arts are harder than others.

It turns out, and this may be sacrilege from me on this subject, but it turns out that building computers wasn't all that hard, because it turned out that there were a lot of groups that worked on it. It turned out that almost without exception, each group ended up making a machine that more or less worked. So, you know, that proves that it wasn't all that hard. People can make computers now. Every once in a while they make a...

The real question, though, was how well did you do, how much did you push the state of the art? It's relatively easy to stay within those things you know, accept the limitations. But if you start asking, you know, for example, you've got a one megacycle flip-flop, do you build your machine around one megacycle flip-flops, or five megacycle flip-flops or twenty megacycle flip-flops? And the people that do best are the ones that say: "Now, who can make that choice best? And are able to follow it up?" And the guy says: "Well, I'll build around 10 megacycle flip-flops if I can…."

TROPP:

But in almost every case of the early machines, and I think Whirlwind is a beautiful example, you did advance the state of the art if in no other way than to show that with the so-called existing technology, things were feasible that perhaps nobody had ever thought could be done.

EVERETT:

That's right.

TROPP:

And just to say that a number of groups were able to do this, is merely, I think, to restate that same concept.

EVERETT:

And because people had a certain degree of flexibility in what they were trying to accomplish, they could modify their plans and take more time and so on. That's one of the reasons that they succeeded.

It also turned out that vacuum tubes were better than people thought and could be made better still. One of the things that the Whirlwind group did was to really put a lot of work into the question of reliable vacuum tubes. That is a point that I think I made earlier with you, that I think one of the reasons that the computer business has gone so well, is because people were really concerned about reliability and paid a lot of attention to it, they were able to build machines.

This is the same thing in the missile business, in the satellite business. People put up some very complicated satellites to go around the earth and run for five years. That's amazing. But it's not so amazing when you think of the fact that the guy who designed it knew it had to do that, and put a lot of attention to it and put a lot of money…

TROPP:

But even this concept of reliability at that point in time was new. And I'm now recalling my conversation with Dr. Killian, who mentioned the difficulties that he saw when people from the Navy came to MIT worrying about the amount of money that was going into this project and the amount of time, because of the project's insistence on reliability, and the careful circuitry that was being designed and planned that was just costing more and taking more time because that was essentially a new concept, even though the word reliability had been around from the year one. You were really breaking new ground, there were no standards.

EVERETT:

The Whirlwind had a reliability which was orders of magnitude beyond anything that had ever been built in the electronics business before. It was orders of magnitude, and it was different in kind too.

TROPP:

Yes. That's essentially, you know, one of the things in terms of the state of the art, you could really come up with a new definition of reliability and a new position of reliability, relative to the machines that you were working on. And I think that's an important aspect of the state-of-the-art question, because the standards don't show up for another decade. Nobody even asks the question of what's standard for one of these electronic machines.

EVERETT:

I think it's like the old leaky tank analogy, you know. If you've got a complicated thing that has to be reliable, it has to be reliable all over. It doesn't do you any good to have a tank that is nice and tight at the seams and leaks through a hole at the bottom. So, everything has to be reliable. That's one reason why the machines took longer, and cost more than people expected.

Now you know, there's a lot of trouble today with military equipment which turns out to be more expensive than it's supposed to, and doesn't work as well, and has a lot of reliability problems. One of the reasons is that they're not given that kind of treatment. If somebody - suppose there were no digital computers, and you had to build a digital computer today using present day government procurements. First of all, there would be a lot of analyses that would show you that it doesn't violate the fundamental laws of nature, and then a lot of people would make bids for how long and how much it would take to do it. And these bids would depend on the guy and how optimistic he is, but in general, I think engineers are reasonably good estimators if you leave them alone and let them estimate. But then we go to the guy, and we say: "Well, you said that you wanted all this money to do this thing. Why?"

And he says: "Well, you know, most of this stuff has never been done before. I've built things before, and I know that very often they don't work. You get all kinds of unpleasant surprises and you have to rework them. Some of these things are really very chancy, and I think we'd better try several parallel approaches and make sure that we get along." "What you mean is that you think there will be a lot of things go wrong, but you can't say where." He says: "That's right." So, you say: "Well, suppose I just give you enough money to do it if nothing goes wrong?" And he says: "But something will go wrong." And you say: "Well, show me where it's going to go wrong." By that time, he's getting kind of nervous, and he says: "If I knew what was going to go wrong, I wouldn't do it that way." So, you say: "Aha! You don't really know what you're doing." Then he says: "Okay, damn it! I'll take the small amount of money, but you'll be sorry."

So, he takes the small amount of money, and after a while, he gets into trouble, and everybody's sorry.

TROPP:

laughs]

EVERETT:

But, you know, if you work at it, it comes out all right. And in every case that I know of where the thing has been done that way, you came out with a product. In the case where it isn't done that way, you get into trouble.

TROPP:

One of the military machines that I can think of in the Whirlwind period is RAYDAC, which went through lots of plans and were never realized until finally a machine was built about three years later than was originally planned for, I think, one machine that cost more than what the original proposal said two machines were going to cost. But it's not something that you would fault, because I think that it was in that vein that you're talking about, where nobody knew where things were going to go wrong.

EVERETT:

That's right.

TROPP:

When finally, those things were sorted out, they ultimately got a machine, and the order of magnitude really wasn't that big.

EVERETT:

You know, really, it's hard to get a guy to say-- You know, he tends to be optimistic and he tends to think that he knows how to go about it, and to say: "Well, I think if everything went right, I could build it for a million dollars, so that I think five is about right." He just won't give it to him, so he knows enough not to say that. So, he promises to do it for a million and a quarter, and it costs five.

TROPP:

Yes, but a good engineer does know the kinds of things that you indicated are going to happen on a project of a new sort.

EVERETT:

I think an experienced engineer who's been through things, who knows the art as well as it's known, when the thing is not, you know, completely out of range, can give you a reasonable estimate. What a reasonable estimate is, depends on how different the thing is.

Our estimates were way out, our estimates--I'm speaking not [?] only of Whirlwind estimates, but the estimates of every group that made the original--were way out. Why was that? Well, really, nobody knew about a lot of these problems. Nobody had ever really ever done anything like this before. The groups were all young and inexperienced, and not accustomed to building these large factors of safety into their estimates, as they would have after they got through this once.

EVERETT and TROPP:

[laugh]

EVERETT:

But you know, that's not surprising.

TROPP:

I guess, in retrospect, we're very lucky that some of the things really did happen. I think you're saying that if they were started today, it might not have occurred, beginning from scratch under the current environment.

EVERETT:

The way things happen today in the new areas, is partly just a process of going through over long periods of time, and partly, that if something gets to be a sufficiently high priority, then it will get the kind of money and attention that's required to overcome this.

You know, take the NASA people going to the moon. Now there was a case where once again, it was within the state of the art in the sense that as far as one knew, it was within the laws of physics. But there were - many of the things were part of the state of the art, but almost everything had to be improved. They needed bigger engines and stronger stuff. They needed life support, better guidance and communications, and practically everything in it, you know, was an extension of what had been built before.

TROPP:

They needed a level of reliability that had just not been reached in almost anything else.

EVERETT:

So, now they did it and they did it on schedule, and how did they manage to do it? Well, I think that they managed to do it partly because it was a very new and exciting and high priority program, and lots of good guys wanted to work on it and did, but also, it was a high priority program and they got enough money.

If somebody had cut them down to one third of the money in the beginning, and you know made it difficult to get any more on the way through, they would still be fussing around. But they gave them enough money, and they did it.

TROPP:

They gave them essentially—

EVERETT:

In fact, if you look at it, I think you'll find that they did a lot of things that they didn't need to. And then if you were able to predict in advance, which those things were, you wouldn't do it. That's my feeling, you know, and that's the same argument that says it is clearly absurd to go to the racetrack and bet on the horses that don't win. Why does anybody do that? It's absurd.

EVERETT and TROPP:

[laugh]

TROPP:

But they all do, including me.

EVERETT:

It's the same way in the technical business, you know. Before you start, you don't know which horses are going to win, and you have to bet on the ones that seem most likely.

TROPP:

So, this was a kind of wartime atmosphere then, that was a carryover, and that also is a part of the state of the art, because many of the wartime projects, when you look back, you find out that a variety of approaches were attempted, different groups were working on them and maybe one of ten panned out. But that one was the big payoff.

EVERETT:

That's the way to do something when you've got to have it.

TROPP:

That's right. And in a sense, opportunities to attempt to build the large-scale electronic computers were operating in a spin-off of that environment.

EVERETT:

To some extent. You know, there's the large and the small, and in the large, there were lots of different groups with their varying approaches toward the problem. You might ask why is it that the government was apparently willing to support all of these. Who knows. I would say that it was probably something like equal parts of a fairly clear impression of the importance of the digital computer as felt by various people in the government, plus the lack of organization of the government to keep the thing from happening.

Like we were talking about earlier, about MIT. You know, it turns out that there were a couple of groups at MIT working too. If somebody had really been put in charge of the government and had started out to "neaten" up the situation, and had gone over all this business, and shot down all but one or something, and then told one guy that he could only have a limited amount of money, they could probably have slowed the thing up enormously, but not completely because the thing was sufficiently important. So I think it was a combination of these things, and where something is important enough, it will get done.

The issue comes down very often to the question of what's important, and who gets to decide. What happens when the organization is distributed and non-centralized is that there are a lot more people who can decide to do things. When you get a centralized organization, there are very few people who decide to do things, and often, they decide not to, and it makes it very hard to do. Now of course it stops a lot of things that you shouldn't do if that's what you're interested in, but those are easy to stop. You just do nothing, and get them over. The problem in life is to, you know, support all of those things you have to do, a lot of those things you should do, and not too many of the things you shouldn't do. And if you've got the thing on balance, you're concentrating on not doing anything, or not doing the wrong things.

There's always a fuss about, you know, avoiding duplication. And traditionally if two groups are doing the same thing it's duplication. It's obvious that one of them shouldn't be doing it. You may not know which one, but it's obvious that one of them shouldn't be doing it. It's obvious that if you are doing something twice, you're wasting half your money. The argument goes that if you stop one of them and give the other guy twice as much money, you'd obviously be further ahead. Now, that's not necessarily true at all if that's the case. It appears to be true to the casual observer, but it is simply not true to the guy who has been through things of this type.

Very often, if you look into it, you'll find that no two groups are doing anything the same. They may be both trying to do the digital computer, but you'll find that one of them is vertical and the other one is horizontal or something, and you wouldn't be caught dead doing the same thing.

TROPP:

Right. As I look at a lot of what I call the von Neumann derivatives, the machines that came out of the Institute concept at Princeton, I find that a lot of them were [?] you know, looked essentially like the Institute machine, but they were polished and refined at a level that was not possible at the Institute because they had the advantage of looking at the thinking that was going on here, looking at what was going on, looking at what they wanted to do with it, and saying, you know: "Hey, here's a way we can make this more reliable and here's a way we can do this faster or quicker or better." And if there hadn't been that kind of simultaneity, that would not have occurred.

You could argue that: "Why have all these different projects? Why not just have one place that's building ten copies of the same machine?" But you would have ended up with something different had you done that, very different from what occurred. You wouldn't have gotten the polish and the engineering refinements that each of these machines in turn contributed.

I guess the other thing that bothers me about this time period, is where the foresight came that we would need this kind of computing ability, because to the casual observer of that time period, it didn't look like there were that many problems around to require an awful lot of computing ability. It looked fairly limited.

EVERETT:

Well, there were some people who said that, and Aiken was one of them.

TROPP:

So was von Neumann.

EVERETT:

And I think that, you know, the problem comes, and so often does, if you try to predict the need for something which is, you know, sort new in kind. For instance, suppose you said to yourself, you know, this happens in the road business, for instance, you say: "Should I build a road from City A to City B?" So, you make an analysis, and you find that there are only two hundred cars a day going from City A to City B. So, you say: "Well, that's not very big. I'll build a two-lane road." So, you build a two lane road, and you come back and find that it's clogged to the ears. There are two thousand cars a day going, so you say: "I better build a four lane road", and it goes to ten thousand cars a day, and so on. The thing is, you know, that when there was nothing but a goat track between the two cities, there weren't very many people could go. So, it really isn't fair to say that's all the need there is.

TROPP:

Mhm

EVERETT:

So, it turns out that before there were digital computers, the amount of computation that was actually carried out in the United States wasn't all that great. There were a lot of multiplications, but multiplications were expensive. And so, if you believed, for instance, that a million multiplications a day was all the country needed, then you said that one or two of these machines, or ten at the most, could solve the problem.

A more sophisticated approach would be to say: "Well, we do a million multiplications a day, and they cost us a dollar a piece, so we're spending about a million dollars a day on computing. And that will support a thousand computers, so what we need is a thousand computers, because there is a need for three hundred million dollars worth of computing." It turns out that the third order estimate is better, which is: the cheaper the multiplication, the more dollars will get spent. So, it turns out that instead of ten machines or a thousand machines or even ten thousand machines, when the cost of the multiplication gets down to a fraction of a cent, people use them like mad.

TROPP:

Right.

EVERETT:

So how do you make these estimates?

Well, once again, because there were so many groups thinking about it and working at it, it was not possible for those who didn't believe you didn't need new machines or more than one or two of them, to keep the others from working.

TROPP:

It's the same thing that you were talking about earlier.

EVERETT:

Now it turned out that people like the MIT group, Forrester and the rest of us, we understood that there was just a bottomless need for machines. We were thinking about things like real time control, simulation and what not. There were other groups who were thinking about making out payrolls, and doing bookkeeping and accounting and so on. And fortunately, the Aikens and the von Neumann's and the people whose stock in trade was to do scientific calculations and tables and so forth, were not in a position to prevent the rest of us who had something else in mind from doing our thing.

TROPP:

I think that's an important-- I think part of what we call the state of the art, then, is state of the environment, and it's a very important part of why and how things happened. And in terms of the Whirlwind project, the environment, I think, had been gone into in our discussions a good deal, as well as the fact that you had all this talent that came out of the Radiation Laboratory, out of the Instrumentation Laboratory and other wartime projects that had used the technology you were going to be faced with. But you were now faced with a new application, a new order of magnitude in almost every instance. And I think that in some sense, that almost thumbnails it.

EVERETT:

I'll make another comment about the state of the technology. Although all of - practically everything had some foundation, and practically everything required fairly major modifications and improvements, as usual it turns out that you know those ranged over a spectrum ranging from those things which were pretty easy, to those things which were hard, to those things which were extremely hard. And one question about whether you could get something done, was how many extremely hard things you had to accomplish, and a well-managed, manned, funded top priority project could cope with a certain number of very difficult things, but not very many. It turned out that in the computer business, the thing that was hardest was the memory.

There were other things that were difficult, such as magnetic tape units and so on, but in general, the mechanical devices and the memories were the hardest. And they were the limit for many years on computers, and probably still are.

TROPP:

Mhm.

EVERETT:

It turned out that the acoustic delay lines had very serious drawbacks, logical drawbacks as well as physical drawbacks. The practical realizations were expensive per bit, and they had long time delays in them. The storage tubes, at least in the form for which they'd been developed for NTI, turned out to be totally unsatisfactory as far as storage capacity and reliability were concerned. And all of the first computers that used acoustic lines had very limited capability, and so the next generation that came along and used storage tubes, they were pretty hairy operations to keep going. It really was the invention of core memory that was the real breakthrough.

TROPP:

Right.

EVERETT:

We are now getting to see large scale integrated circuit memories. Of course, they were totally out of the question.

TROPP:

I remember talking to or looking at a discussion with a gentleman from Rand Corporation on JOHNNIAC, who described the order of magnitude of the circuitry problems on the Williams tube as being much more difficult than the similar engineering problems on, say, the Selectron Tube. Which may not have been an easier problem, but it was an easier problem after having fought the Williams Tube.

EVERETT:

If the Selectron Tube had worked.

TROPP:

Yes.

EVERETT:

The circuits were easier, but it didn't work.

TROPP:

Yes, that's right. But, as I say, the order of the magnitude of the circuitry problem was a really tough one on the Williams Tube, and had the Selectron done what it was supposed to have done, then that same problem would have become some orders of magnitude simpler. And, as you said, until core memory came along, really, the whole problem wasn't solvable within the state of the art.

EVERETT:

All the Williams memory - it worked well enough to get the early machines going. He originally invented it as a serial memory, and as a serial memory it was easier to handle because, as you say, it had 32 lines or bits on each, and you re-did a line each time you wrote. And there was a maximum of 32 readings and writings of a single line before you came back and refreshed the adjacent lines. But one of the problems that arose when you started using it as a parallel memory, because it had 32 x 32 matrix limit too, then you could - then you read one and refresh the memory. Then you could read or write a thousand times in the vicinity of a spot before you had to go back.

I remember when Jay and I were down at the Institute for Advanced Study talking to them, and they'd just been to England and come back with the Williams Tube. And they were talking about it and they said: "Oh, this is the problem. If you read or write in the vicinity, of course there's a certain amount of spillover from those actions. The question is how often can you do that before you'll damage one of the spots. And since you re-write it once in a thousand times, it seems like...Goldstine said: "Gee, that sounds like an interesting question." Then he went out and picked up some circuitry, and he came back and said: "Well, as far as we can tell, it's some place between 8 and 64."

TROPP:

[laugh]

EVERETT:

"It looks like it ought to last for 8 samplings in the vicinity, and I'm pretty sure it won't last for more than 64." As far as I know that pair of numbers was not refined in the next three years.

TROPP:

[Laughter] That’s a wide range.

EVERETT:

...

TROPP:

Yeah.

EVERETT:

And it was one of the fundamental limitations of the tube. It turns out, of course, that you say: "Well, 64 is a lot. How often could that happen?" And the answer is that it will happen the first time that you write a program.

EVERETT and TROPP:

[Laughter]

EVERETT:

Also known as the cussedness of the ...

TROPP:

[Laugh]

When you were talking about the state of the vacuum tube technology, which was all there, but again I think back to all the material that Dick Mertz collected in connection with Whirlwind, and the realization that suddenly you had to establish brand new standards.

TROPP:

Mhm.

EVERETT:

You had to almost start from almost scratch because you were using the tubes in such a different way, and there was nothing to base it on. So even though one might say the state of the art, the technology was there, there was a whole new ball game in terms of how the tubes were going to be used, and what standards you could accept as a minimum, in terms of their reliability and their usage.

It also turns out that, you know, it's really not surprising, but it turns out that if you look beneath the surface of almost anything, you'll find that very little is known about it. And the reason is that it costs a lot of money to find out about things, and there's not much point in finding out more about them than is necessary to sort of make the thing go. Beyond that, it's not really very cost effective, and everybody's got a lot of things to do. And if you found that a guy had really been looking into depth about all the things in this business, you'd wonder how he stayed alive, and he probably couldn't. And it turned out that that was true in the vacuum tube business. They could make vacuum tubes, but boy, they didn't know much about them.

TROPP:

Mhm.

EVERETT:

And as far as I could see, they had these machines for making vacuum tubes, they'd start them up, and they'd make no good vacuum tubes. And so, they'd pour out the other end of the machine, you know, and they would get thrown away, and they'd change this, and they'd change that, and finally, some afternoon, they'd start making good vacuum tubes. "Don't touch a thing!"

TROPP:

[laugh]

EVERETT:

…they used to say. If they discovered that the manager of the factory was standing on his head in his office when they started making good 6SN7's, that poor guy had to stand on his head until the run was over.

TROPP:

[Laughter].

EVERETT:

And you know, nobody knew why they were sometimes good and sometimes they weren't, and they just knew that if you fiddled long enough, they would start making good ones, and if you left it alone, there probably would be good ones for quite a while. They made them so cheap.

EVERETT and TROPP:

[laugh]

EVERETT:

To some extent they make transistors that way now.

TROPP:

It's like the basketball coach who happened to wear red socks the day his team finally won [laugh].

EVERETT:

He keeps right on.

TROPP:

He keeps right on wearing red socks.

EVERETT:

When we were making the storage tube, we used to have that, you know. We'd be making storage tubes and then some day we couldn't make them anymore.

TROPP:

Of course, that's a whole other open-ended question, in terms of the elements that went into the machine, and that's the individual aspects of quality control on the items, that were also, in a sense, a new art.

EVERETT:

Mhm.

TROPP:

Even though quality control, I guess you could say, had been there from the day the first manufacturing plant on anything opened, you were really faced with a brand-new set of problems in terms of quality control on individual elements.

EVERETT:

Mhm.

TROPP:

Again, you could argue the same "off the shelf" approach to that aspect.

EVERETT:

Then, there are fundamental ways of attacking that. One way is the vacuum tube that I said, which says: "Well, we'll figure out some cheap way of making them. If we can just get a yield of a few percent, they still won't be very expensive."

This argument came up in the early days of the core memory when nobody quite knew why the cores were the way they were. Some cores were good and some were bad. And they had to be reasonably alike, in fact quite alike, in order to make memories that would work and have reasonable tolerances. So, one approach was to say: "Well, let's work on the business of the materials and how they're formed and heat treated, and one thing and another, until we know how to make them, you know, that are essentially all alike as they pour out of the machine."

And the other argument was: "Well, let's just make them, and then we'll just build a great big sorter, and sort them into a hundred bins, and when we get enough in the first bin, we'll make a memory out of that."

EVERETT and TROPP:

[laugh]

TROPP:

That was the vacuum tube approach. [laugh]

EVERETT:

That reminds me of a story that a fellow from G.E. told me. Maybe I told you this story. He said some guy had been making transistor radios in the United States and selling them at a profit, and you'd wonder how he could do that. The way he did it was that he bought reject components from GE. And they make transistors and they make capacitors and so on, and these are tested. They try to get the distribution as sharp as they can but you know there's bound to be some ends anyway, and there were quite a few that they didn't know what to do with, so he came around and bought them for practically nothing. And then, he didn't test them. He'd gotten them for practically nothing, he had a line set up with a bunch of girls, and they'd take these completely untested rejected components, and they'd solder them into radio. And when they got down to the end, they'd try them. If it worked, they'd sell it, and if it didn't work, they'd throw it away.

TROPP:

[laugh]

EVERETT:

And because he didn't have any testing costs, and because his component costs were way down, he was able to make the radios cheap enough,

TROPP:

[laugh]

EVERETT:

and to sell them at a profit in competition with the Japanese.

TROPP:

[Laughter].

EVERETT:

I think that's a lovely story. I'd hate to have one, because, you know, it might quit tomorrow. [laugh] But, you know, it's a different approach to the problem. Instead of testing each one of these hundred components at great expense, he soldered the whole thing together and tested later.

EVERETT and TROPP:

[Laughter].

TROPP:

But you couldn't afford to do that on Whirlwind. You weren't in a position to do that.

EVERETT:

No, you know the answer. You could have made Whirlwinds from now to eternity, and you'd have never gotten one that would've worked.

EVERETT and TROPP:

[laugh]

EVERETT:

There were just too many parts.

EVERETT and TROPP:

[Laughter].

TROPP:

Can you think of any other aspects of this open-ended kind of question state of the art that we haven't touched on?

EVERETT:

Oh, we could sit around for weeks while I dredge things out of my memory.

TROPP:

Mhm. Well, do you think this is pretty good for openers?

EVERETT:

I think it's pretty good for openers.

TROPP:

Okay, let me turn it off then.

[END OF INTERVIEW]