INTERVIEWEE: Norman Taylor

INTERVIEWER: Richard R. Mertz

DATE: 27 January 1970

Mertz:

This is an interview with Mr. Norman Taylor, conducted in his office on Tuesday, the 27th of January 1970. Mr. Taylor, would you like to describe your early background and education? Where and when were you born?

Taylor:

I was born in England in 1916, in Manchester.

Mertz:

In Manchester?

Taylor:

Yeah.

Mertz:

And where did you get your early schooling?

Taylor:

Well, I emigrated to this country when I was about six years old and went to the public schools of--in New England; most of my secondary schools in Bangor, Maine.

Mertz:

I see.

Taylor:

And then I went to Bates College and got a Bachelors Degree in Mathematics in '37.

Mertz:

During that time, say, in your high school years, did you have any particular hobbies, or did you belong to any particular clubs, say in high school, like the Science Club. Oh, let's see. Did you build crystal sets?

Taylor:

I built--yeah, I built crystal sets and amateur radio things and things like that. And I came from a rather poor family so most of my free time was spent in spare time jobs. Things of that sort. I didn't have that much time for anything else.

Mertz:

Well, did you have any spare time jobs that related to...

Taylor:

No. I was a Printer's Devil for a while and did the usual odd jobs in high school.

Mertz:

Did you have a particular major in college?

Taylor:

In college--well, I really went to college where I could get the best scholarship advantage. So, when I went to Bates, I ended up majoring in Mathematics simply because it was easier to get all "A's" in mathematics and therefore that was a way to get a scholarship.

Mertz:

And you had done well in math earlier.

Taylor:

Yeah. Math and physics were always very easy for me, and languages were always very difficult.

Mertz:

Did you find the lab courses interesting, too, as well as the math courses, or--

Taylor:

Yes, although I wasn't very good in the laboratory.

Mertz:

Chem courses.

Taylor:

Chem? I was a mess in chemistry, and so actually I didn't become--show any tendency to engineering until about my senior year in college and I was about to teach high school mathematics as a career, and one of the professors at Bates suggested that he thought I had intuitive engineering ability and that it would be better for me to go to MIT, or some school such as that. And I walked down MIT cold one day and walked into electrical engineering and says, "I always wanted to be an Electrical Engineer." And one way or another I transferred to MIT and then got another bachelor's degree there. So, I spent--I did two undergraduate degrees, really.

Mertz:

Did you know anyone at MIT at that time?

Taylor:

No.

Mertz:

You just walked in--

Taylor:

I just walked into the office without any introduction. I remember Professor Timbey and Professor Carl Wildes were very receptive to my notions and helped me get started.

Mertz:

I see. Those were sort of two influences--

Taylor:

Yeah. Very strong influences.

Mertz:

At MIT. And about when was this? What year was this?

Taylor:

Well, that was the fall of '37.

Mertz:'37? And, well, that was still before the war, possibly--

Taylor:

Yes, that's right.

Mertz:

Still during the Depression.

Taylor:

Yes, that's right.

Mertz:

Then, you finished up your undergraduate work in ee--

Taylor:

Well, I actually did a master's thesis and didn't finish, because I ran out of money, and the war had

broke out in late '39--so I got tied up in the war.

Mertz:

And what happened then when you were caught up in the war?

Taylor:

Well, I worked for a few months in a small company in Ashland, called the Lombard Governor Company, which was quite closely associated with Fenwall [?], which has now become a big company. I did some research and development work on instrumentation of the temperature problem, in general. And it was an original kind of development work.

Mertz:

What kind of--was this thermocouple work?

Taylor:

Yeah. I worked for Henry Warren, who was the holder of the patent on the Telecoin [?] clock. I worked for him directly and it was pretty stimulating, even though I was only there about a year. At that time somebody at MIT received a letter from the Western Electric Company saying that they were needing very large numbers of young, bright Electrical Engineers. They had a--the war effort was starting and they had to go into communications--airborne communications. And so, I went down to the Western Electric Company and became a Test Equipment Designer for setting up production lines of radio equipment, both--all of the frequency bands from audio, you know, from broadcast bands up to the very high frequencies--and spent all the War years at the Western Electric--

Mertz:

Which plant was this--

Taylor:

Well, I've moved around. I started off in Carney, and then moved to Passaic. I ended up in the Bell Laboratories as a liaison engineer between Western Electric and the Bell Laboratories. I had some very interesting experiences there. In fact, this is the real reason that I got into computers, because there weren't too many people understanding about radars and we were--we started to build radars towards the end of the war, and so most of us younger guys went to school and started to design and build radars. And so, I moved from project to project, and each new project that came out of the Bell Laboratories, I was instrumental in putting it out for them into production.

Mertz:

I see. Could we backtrack just a little bit on the types of test equipment instrumentation.

Taylor:

Well, we had separate test equipment for all the radio frequency parts, such as front-end coils, IF strips. I designed a rather fancy way of testing IF coils which had multi-coupled coils so that the band width--they're very common in TV today, but in those days they weren't. Aligning these things was quite a trick, and so we developed a rather fancy set that did a frequency modulated scan of all frequencies, and then took a picture of the wave shape of the past band and compared it with the references of good ones and bad ones, and were able to get high school girls to be able to do this job which otherwise would have been kind of tricky. And later on, we did that kind of design on--it's now common, but we did that kind of design on antenna structures for beam widths and patterns, and—

Mertz:

Was this with Western Electric?

Taylor:

All with Western Electric, yeah. Then we designed test sets for the original magnetrons, and later some preliminary work on klystrons, all kinds of radio frequency. It was during this period that I got acquainted with the idea of rapid rising pulses and all the circuitry that had to do with sharp pulses. And even though it was archaic in terms of what was soon to come out in the computer business, in those days, like a one microsecond rise time was a very fast rise time, and a four-microsecond pulse was typical.

Mertz:

What kind of previous--some of this work had been done in the thirties before the war, but it certainly, got impetus from the war and intensified in a lot of the--

Taylor:

Well, yes, a lot of this work--well, the echos [?] joined the circuitry, many of us designed circuits of that sort never having read about them. The radiation series didn't have a--never came out until after the war, so most of us didn't have anything like that to depend on.

Mertz:

So, while your work was somewhat parallel to the Radiation Lab

Taylor:

Well, you see, I was very closely connected to the Bell Laboratories. In fact, I was in the Bell Laboratories for three or four years. I just resided there.

Mertz:

About what period of time?

Taylor:

'43 to '47.

Mertz:

And there, do you recall with whom you worked there?

Taylor:

Yes, Jess Wentz was one name that sticks in my mind, Morrison, Wentz--

Mertz:

Did you ever have anything to do with someone like Darlington?. Sid Darlington?

Taylor:

No.

Mertz:

Charley Townes. They were working on fire control.

Taylor:

Yeah. One of the things, though, that I forgot to mention that came along there was a thing called the AF, uh, we used to call it the Q5B. And this was an analog bomb sight for releasing the bomb from an aircraft. And the radar--it closed the loop between the radar detection and the release point. And that was one of my projects. And there's a little analog computer in there. I guess that's the first time we ever even thought of anything being a computer. This was probably '44.

Mertz:

Well, it even had a memory?

Taylor:

Yes, it did. It had a big capacitor, and the capacitor had to drain at the same speed as the airplane's approaching release point. It was very important to keep this thing linear. And so we had to work with pretty old-fashioned vacuum tubes, because the leakage--the leakage of these tubes--they were gassy at times, and the leakage of the tubes was such that there was no stability in this curve so that there was a lot of work going on in feedback circuits to improve this situation. But while I was there I got to realize all the little crazy problems that go with vacuum tubes, and also got to know all the Bell Labs' vacuum tube designers. So, I had a good background for, you know, learning about vacuum tubes. When we got into millions of them later on, I had some old friends to go back and ask questions from.

Mertz:

Sure. Well, at that point, near the end of the war, wasn't it true that there was sort of, at least on the part of the various components of the Defense Department, a negative--a rather negative view of the reliability of electronics equipment in general?

Taylor:

Oh, yes. It was terrible, absolutely terrible. And this was partly true bec--I think the componentry wasn't very good. For instance, in order to get some of these high performance circuits we had to use high performance tubes, which really had been designed for the entertainment business. And we had them in circuits they had no business being in, so they varied three and four to one in terms of tolerances. So, you have to select tubes, you know, in

order to make things work. And then as they age the qual--

Mertz:

Were these the standard production tubes?

Taylor:

Yes. Because there wasn't any other tubes that had been designed that would meet these things, and of course, the pace of development in the war is something I've never seen since. I can remember one year we put three new radars into production practically from drawing board to first article in nine months. And so that very often our production prototype was ahead of the model that we were building in the laboratories--in fact it was:--whichever one worked we'd ship, you know. And so we had to design whatever was available. And going back and designing tubes was not really in the cards. You'd take what you could get.

Mertz:

Well, this--do you recall any--during this particular period--any specific test equipment--you mentioned--

Taylor:

I mentioned the IF Tester.

Mertz:

Right--that had some later bearing on work that was going to become quite important in the problem.

Taylor:

Well, yes. I think we used to--the Ferris [?] Instrument Company in Buete [?], New Jersey made some radio frequency signal generators, and then they had pulse modulators--the

modulator. And then there was a Hewlett-Packard oscillator--square wave oscillator--we used in differentiating the square waves to get the pulses. And so that technology sort of immediately could be useful in a later generation. In fact, I think--

Mertz:

It was very important in terms of the synchronous versus asynchronous.

Taylor:

Yeah, yeah. And so, there were some of those techniques that we borrowed right away when we started this; we were borrowing designs.

Mertz:

Now that takes us up to the eve of your getting involved back at MIT.

Taylor:

Yes.

Mertz:

...from Western Electric or Bell Labs, where you had been working. Was there any work that you had done in this period that had involved you in patents of any kind?

Taylor:

There was a patent on that IF Test Set, I think. Right after the war I did work on--I worked for a consulting firm in New York for about a year. They went out of business. And that was when I went back to MIT. I just figured--after the war there was a very turbulent period of people getting relocated, and I worked in the Bell Labs on audio telephones for about eight months, and that--that's pretty boring after you've been dropping bombs. Well, a lot of the younger crew left, and--during this period that I was working in this consulting firm, we invented--a fellow named

Gerald Hess and I invented—

Mertz:

Hess?

Taylor:

Gerry Hess.

Mertz:

H-E-S-S?

Taylor:

Yeah. A patent that had to do with using a counter. Now Potter Instrument Company had come out with the first binary counter that I ever heard about, and I think RCA came out about the same time. This is just a strict counter. And we figured that you could convert a radar beam into a binary number by counting the time between the start of the pulse and the return of the pulse until you get a number directly out of these counters. And invented a system and proposed it to--some proposal, I've forgotten. I think it was a submarine detection system. Most up until that time, all these kinds of counters had been mechanical or something like that.

Mertz:

Was this a sonar system, or a--

Taylor:

No, it was a radar system. We had a radar--a submarine used to poke a little bit from the water--and then used to measure. And this patent was granted and--this had a pretty early date on it--'48 I think. And this was the first time that, I think, a binary counter was used to measure rain, which later became very standard.

Mertz:

And that was in the name of Taylor and Hess, was it?

Taylor:

Taylor, Hess, and Raymond Wolmock [?] who was the senior consultant who was pretty well known in certain quarters in Washington. I think we were joint inventors of it. And it's assigned to him. I don't know what ever became of it, but I do have a copy of it and I think that's the first--that's the first time it occurred to me that the concept of computation had something to do with the real world. And so that really brings me pretty close to starting the Whirlwind.

Mertz:

So, we have you in New York in the turbulent, immediate post-war period. You already knew people in the EE Department at MIT, of course,

Taylor:

Yes.

Mertz:

Years back. I take it you maintained some contacts with them.

Taylor:

Very loose contacts. I just thought it would be worthwhile dropping in at the alumni office, actually. Since I wanted--my family's from New England, my wife's from here. I really didn't want to stay in New York indefinitely. So--

Mertz:

Let's go back just a little bit. When did you get married?

Taylor:

Oh, right after I got out of MIT in 1939. I got married in '40. I guess just as the war broke out. As I--when I moved down to New Jersey. My wife is from Melrose, which is a suburb here. And so--

Mertz:

So, you went to the alumni office, and--

Taylor:

Yeah, and they said, you know, that things were not too good, but there was this computer project just starting up and would I be interested in it? Because they were looking for electrical engineers. And I had had a lot of circuitry background by this time, quite a lot more than a lot of people. So, I went over to talk to Jay Forrester, and he made me an offer right away, in writing. And those were the days when a lot of offers were "Yes, come around next week, I'm sure--." I'd been looking around for a while. So, I decided, well, I'll take the job. It doesn't sound like much of a job, building a computer, because everything about computers to me had been a pain in the neck, really. Just, you know, dull stuff.

Mertz:

So, was this in terms of analog?

Taylor:

No, this was--they had just decided that Whirlwind was going to be a digital computer. It had started out to be an analog.

Mertz:

Do you recall, roughly, when you had the interview with Jay Forrester?

Taylor:

Well, it was in the--probably around December of '46, because I started in early January of '47, I'm pretty sure. It might have been a year later--it might have been '48. I think it was right around there.

Mertz:

And did he outline to you what--the kind of thing that he thought you would be best suited to get involved in, so far as the project was concerned?

Taylor:

Well, I guess he was quite excited about getting an honest to goodness circuit engineer.

He had a--it turned out later I found he'd had a bunch of students out in the back and they had a vacuum tube stuck in a vice and they were trying to make an Eckles-Jordan circuit work, and had promised him ten megacycle "flip flops" and stuff, and so he said, "You know, this thing's all made out of electronics, and that's all you've ever done, so you can't help but help us somewhere." [Laugh].

Mertz:

So, then you started around January.

Taylor:

Yeah. And then I started to read about, you know, all the things. And they had just--the fellows had just come back from a course at, uh--

Mertz:

At the Moore School?

Taylor:

The Moore School.

Mertz:

That was that famous summer course.

Taylor:

Yeah, they had just come back. I missed that. But Dave Brown was there, Lou Wilson, and Bob Everett, and Steve Dodd. And they were all busily writing these logic diagrams, which were Chinese to me. I didn't know what they were all about. And so I just--you know, after being all these years in production and test equipment, I immediately said to them, you know, "what have we got to worry about--what's going to make this thing work?" And it was obvious they were going to make it out of flip flops, mostly, and gate tubes. So I went right after those two circuits--

Mertz:

Gate tubes and--

Taylor:

And flip flops, yeah, you know, and found a couple of bachelor students, thesis students, trying to make 'em work out there. And so, I decided that was ridiculous, that way to go about it. And so, I chose one little piece of Whirlwind logic which was the one bit of the add--the add and shift logic.

Mertz:

Had they moved in the Barta Building at this time?

Taylor:

No, no. They were still in the Servo Lab. And so, I started to build this up and see if I could make it work with the flip-flops that these students had. It wasn't long before I found out that they were completely--they were doing the same thing that we were trying to do back in the war. They were trying to put ten megaohm resistors in the grid of a 6AG7. This just won't work because the leakage is so variable that you never know whether the tube's going to be operating. So, using the standard design techniques for test equipment, which is, you know, a lot more conservative than the design for products, I went after these flip flops and started worrying about what made them work and what made them not work, and so on.

Mertz:

Do you remember any specific piece of test equipment that you thought was--that's particularly instructive in terms of designing?

Taylor:

Well, there wasn't any test equipment at that point. We were designing all of 'em, because we had to have rise times that were about ten times faster than anything on the market. So we designed our own signal generators. We started with Hewlett-Packard's and then quickly realized that wasn't working. And we had gas thyratrons. But it wasn't long before we had--in order to make circuits work at multi megacycle speeds, our test equipment has to work better than this. So, we had a hard job figuring out how to test the stuff. And so that's how we really started designing test equipment. The test equipment--really, Whirlwind was its own test set for a while. And then we'd have streams of pulses coming out at various speeds to find out, you know, if there was any sensitivity at variation in speed. So we designed these things, and it was

clear that once we had one set up and we got a few more students we had to have another set up, to have them do anything at all, and a set up was quite a lot of stuff. So, we started to build two or three test sets. At that time, it was clear to me, quite soon, that if everybody designed his own test set we'd never measure to any reference point. So, we simply had to get--we had a little--one guy who did all the test sets, and they all--and then we built three or four at a time.

Mertz:

Well now, when you started out the--Forrester signed a couple--or you found a couple of students, who were interested in this.

Taylor:

Yeah, mostly, mostly it was students--everybody there was an--I think I was one of the first full-time employees. I was not looking for a degree. Everybody else was getting a Master's degree. And I think Forrester had just gotten his, Amelia Erbert [?] was a full time employee--two or three of us.

Mertz:

So, you had, about how many working with you at the outset?

Taylor:

Two.

Mertz:

Two? And then eventually this--

Taylor:

Well, eventually we decided to build a--after we got one of these things going, I decided that the only way you'd ever find if all this logic worked was to build these five digit multiplier. So,

we--three of us built that.

Mertz:

Can you tell me about when that came up for discussion as a sort of pre-prototype for the Whirlwind?

Taylor:

I think it ran in '49, so I took about a year to build it, so it must have been in the middle of '48.

Mertz:

And by that time you had--you had already, sort of broken down the work on the circuitry.

Taylor:

(Inaudible) more or less full time on the test equipment for the arithmetic, or was...

Taylor:

Well, you see, [looking at photograph] all of this stuff here is test equipment. Down here, this is the test equipment we had to have to run this--this computer doesn't run, it needs test equipment. And here's a pulse generator and here's a pulse width control module; and here's a--this is a frequency unit that fed a certain frequency in to make the pulses a different width; and then this was another one that gave restorer pulses. That was another little trick we had to get around some dc coupling. So, all of this test equipment, whenever you did anything you had to design some, seek some sources of energy, and that you're sure you designed--you see, in authority on these--you could see these are not standardized--later on we realized what we needed to be standard. But everything is there that we really started with.

Mertz:

And all this was done, then, around 1948 then--during that year?

Taylor:

Yes.

Mertz:

What do you need to do that...

Taylor:

See, you'd have to find out how fast it works. Now, Whirlwind worked at two megacycles, but that thing worked at four. It actually ran out at 3.8, and the designed one was five. I went to Bob Irwin and said, "Look, you'd better slow this thing down. There's just as many tolerance problems in time as there are in voltage." And so, we slowed it down to two megacycles I remember making that--that decision was made about '49. And you know, there wasn't a computer that ran faster than two megacycles until about 1965. That was it.

Mertz:

...simply the practical problem, the design problems--what--did you write up any memorandum or any...

Taylor:

Oh, the...

Mertz:

...physically, with the problems of why it should be better to cut it back...

Taylor:

Oh, yes. There's a complete--there's a fifty or 100-page
write-up of that device with photographs of all the pulses and why they interfere, and how they interfere.

Mertz:

It's in the MIT Library?

Taylor:

Yes.

Mertz:

Was that one of the engineering notes, or...

Taylor:

No, this is an "R"(?) Report...

Mertz:

"R" Report?

Taylor:

It's about that thick. It's a real big report.

Mertz:

Right. You don't happen to recall the number by any chance?

Taylor:

I think it's R-134. I just...(inaudible).....that I was looking at my publications and there was an argument about when I was putting together my--somebody was putting together my application for a Fellow in the IEEE, it came up as to whether that was a publication or not. And I think it was finally considered that MIT Reports were publications. I have a list of my publications somewhere. And that's one...

Mertz:

Would it be possible to have a copy of that...

Taylor:

Yes. I think so. Why don't I try to have someone send you one?

Mertz:

O.K.

Taylor:

It's kind of a big document.

Mertz:

This was a report made in the "R" Series?

Taylor:

I'm sure of that. It is almost--it's so detailed that it's almost like a primer on how computers work. "Cause it had a picture of every pulse going...(laughter)...and every wire...

Mertz:

That's pretty essential.

Taylor:

It was pretty essential at that point.

Mertz:

Yeah, you weren't building too much on--did you--later you arrived on the scene a little bit too late for that summer course down at the Moore School, but later did you go down to see what Eckert and Mauchly were doing?

Taylor:

Yes.

Mertz:

They had already moved on--beyond the ENIAC at that time, they were in...

Taylor:

Yes, they were doing a thing in--I remember the first time I met Eckert and Jim Wiener(?) was at a back-to-back computer--I can't remember the name of it now.

Mertz:

The EDSAC, or was this a ...

Taylor:

No. Gee, I don't think I can remember that anyway. I remember it was in a room about this size. It had eighteen fans blowing out to keep it cool. It was a duplex machine, back-to-back of some sort. It was the first thing they did when they formed Eckert and Mauchly as a company. It was a contract of some sort. It didn't work.

In 1950, I was an acting chairman--Program Chairman of the first Computer Symposium, and we reviewed the thirteen computers in the country that worked, and I--and that's all documented--those are all documented. That is an interesting document. I don't hve my copy anymore, but that's available, I guess, at IEEE or IRE.

Mertz:

Well, now...

Taylor:

Now, that document...

Mertz:

Was that a Washington Symposium?

Taylor:

No, No. That was in Philadelphia, and it was sponsored by the--it was the first joint computer conference. It was sponsored by IEEE and IRE. And ACM wasn't even formed at that time. And I was Program Chairman of that. I can remember arguing with these various people, you know, I said, "If the computer didn't work, I didn't want anybody sitting there waving their arms about how great it was." And furthermore, I remember Goldsmith (Goldstine) from the Institute of Advanced Study, wouldn't give me a written paper. I said, well, if it doesn't work, then probably that's why.

But Von Neumann followed all this work along in very great detail.

Mertz:

But, then the IAS computer actually wasn't...

Taylor:

Never did work.

Mertz:

...wasn't inserted into...

Taylor:

No, you didn't need to be--Von Neumann gave us all those ideas and we implemented it differently. We had a different circuitry. Some of the circuitry that they had down there was pretty far out. It was more like--the circuitry was more like a physicist would design. But you know, I'd been brought out of Bell Laboratories and if it wasn't really solid I wouldn't have it around.

Mertz:

Well, which ones at that time--of that conference were, in your judgment, the--seemed like the most promising going, really functioning...

Taylor:

Well, EDSAC at the University of Cambridge worked very well. It was very slow, but it worked. We didn't consider the Harvard Computer was a computer because it was just relays and didn't have any stored program. Neither did the ENIAC. It just didn't do much but punch paper tape. If it didn't have a stored program, we didn't consider it was a computer. We still don't. You know, that's why we don't--but we did have the ENIAC. And Pres Eckert, he went along with that. And so, he didn't consider that he'd built one yet, at that time. So we didn't consider--we were pretty near the first. The JOHNNIAC would have been the--there was an ILLIAC at the University of Illinois, and...

Mertz:

Sanders, uh, SEAC...

Taylor:

SEAC didn't work, but we did have that on the program.

Mertz:

Eventually it worked.

Taylor:

Yeah, eventually it worked. But they couldn't get that little rotating cell idea to work, and the delay line flip flop. And it didn't work very long. We didn't say they all had to work reliably, but they had to have functioned to the point where we knew there was something there except a

big noise.

Mertz:

Now, did that meeting have any software people, any people who were interested in programming, or was this pretty much hardware...

Taylor:

Well, the software people were, you know, they were around because you couldn't--you can't make a computer work without programming--it doesn't know anything unless it's programmed a little bit. But the kind of programs that we had were pretty much test programs to show that the thing worked.

Mertz:

Well, you know, that raises an interesting thing...

Taylor:

The software was always delayed a year or two by the fact that there wasn't much to program. See, most of the people in those days--we didn't really have many programmers around when we first started. But then--about '50 when we started the--right after that--see, that only has--you program that by throwing some switches. There just isn't any program in that thing. That's just the arithmetic...

Mertz:

That is the five-digit multiplier.

Taylor:

Yes.

Mertz:

Well, this is an interesting thing that develops in pinpointing the time for a technical historian. When was the first actual program, that wasn't a test program, run on the Whirlwind Computer? Because many of the--much of the time spent in working on the Whirlwind was in testing.

Taylor:

Yes. Well, of course, I'm--because I had a lot to do with that, I really am a pretty good test programmer, but I'm not much good at anything else because I spent most of my time figuring out how to run tests. I think the first time that Whirlwind was programmed--it wasn't exactly a threshold, that I can remember, there were a lot of demonstration programs. Before we had electrostatic storage we had thirty-three registers of test storage. Five of those were variable, they were what is now known as a scratch memory. We added them there because we needed it to test, but it turns out it was a great idea and everybody's copying it. And Charley Adams wrote what I consider to be the first demonstration program that was not just checking out to see if the wires were in the right place.

Mertz:

That was ship stability--

Taylor:

Well, the one--the classic one before the ship stability one was the bouncing ball, the sine wave and then the bouncing ball through the hole and so forth. And that one, even though it was a demonstration program, it took some programming technique. It wasn't just a--you know, now the circuit's there and does this run around, error-checking kind of thing. That really did solve an equation and you had to do it--it had to work on numerical methods to solve the equation. You

couldn't just write down electrical engineering type stuff.

Mertz:

I--in talking...

Taylor:

That's the first one I remember. Now, from there on we did get--as soon as we got the storage tubes things went kind of quite rapidly. There had been a group of ten or fifteen guys who were programming as if the machine was there, you know. We go into--it took quite a while to build Whirlwind, even though it was pretty fast by today's standards. We started in '48 and I think it was running in '51. It really didn't run well until '52. But people had the model of that machine in their mind as if it was running. And the programmers had all these programs written before the machine was running. And they talked about it as if it was there. So when it finally worked, there must.

Mertz:

Well, then what would you do once you detected a change? Could you induce a failure...restorer pulses worked and all the flip-flops were following the restorers, you had a beautiful way of saying everything's all right.

Mertz:

How about in closing it down. Was there any particular problem there?

Taylor:

Well, of course, starting up logically is different than starting up just putting a voltage on the machine. We had an elaborate start-up procedure there in order to preserve the vacuum tubes. We started up--I forgot whether it's one or five minutes--but we heated up the film slowly, and

therefore, so the thermal strain on the inside of the cathode was minimized. We found an awful lot of failures in standard equipment in doing start-ups, where the shock of the--so we turned it on slowly, we turned it off slowly for the same reasons and, as I remember it, we brought up certain voltages--I don't know whether we brought the voltages up...

Taylor:

.....now we've found we've made some mistakes. It wasn't obvious where these things came from. And we had the trolley cars going by, and so all of these little, nasty electrical engineering things that we had all worked out. So, when we came to Whirlwind, we had a bag of tricks we could use. So, with circuitry. I think, though, that --you know, I shouldn't dwell too much longer on the circuitry because the real problem was always the memory and it really had a lot to do with most of what we did was the fact that the storage tube didn't work very well. And so, all of us turned our attention to the memory. And the Whirlwind circuit problems really became so minor after we...(tape stopped)

...I think that the--an awful lot of problems were worked out on the five-digit multiplier. And it ran for two or three years after Whirlwind ran. We used it as a test set for circuitry problems. Ed Rich ran the five-digit multiplier. And I remember we kept polishing the circuits and looking for weaknesses in the system. And I think the record run, I think was three hundred and sixty-seven days without an error on that thing. And once we got to that point, you see, we were in pretty good shape for predicting what we were going to have.

Mertz:

One of the things that comes up, and that is the role that some people play in stimulating ideas f or other groups and that doesn't always show up in documents and reports, and in talking to a

number of participants in Whirlwind project, the name of Bob Everett pops up a good bit, over and above the things that he is associated within the reports logic and the like.(inaudible)...and I think that you touched upon the scratch memory idea...

Taylor:

Yeah, Bob had--most of us who worked with Bob feel that he's--he was the genius of our group. I don't think anybody would quibble with that. And whenever we were in a jam, we'd go to Bob and ask, you know, "How are we going to get out of this mess," and discuss the problem with him. And he usually said, "Why don't you do this, or why don't you do that?" And we very often went back and did it and we'd be convinced in fifteen minutes that that was a great--you know, a lot better than our--we'd have spent weeks thinking out something that simple. And so most of us, I think, trace back things that we think Bob Everett invented. And then you'd talk to him about it and he'll say, "Oh, I didn't invent that. You invented that." I think he told me that about the light gun, for instance. And then later on he'd say, "You know, I really did invent the light gun." I said, "I'm sure you did, Robert." But he's so modest that it's hard to--and he didn't document things very well, particularly. He paid no attention.

On the other hand, he was always so gracious and generous with his praise. And when you invented anything--I can remember I came up with a cute little idea in the storage tube. We had trouble with the storage tubes and it turned out that the storage tubes splattered electrons and if you read the same spot the read beam splattered the spot around and got bigger, and pretty soon it would blemish. And so--and then if you'd re--when you read it you destroyed it so you had to rewrite it. So, one day I was working on this--as I said we ended up all working on memory--and I came up with a little cute idea that since we always read it before we write, if

we're going to write it again and if it wasn't going to be changed from what it was before, then let's not bother to write it, so we won't splatter it, you see. So, Bob said, "Oh, that's a great idea." Well, of course he gives me--I know we called this "only write when necessary", but it saved us an awful lot of splattering and it was a very simple thing to do. Well, E.T.(?) brought that up over and over again and what a bright guy I was, you know, and he'd done things like that hundreds of times without ever--

Mertz:

There was one more kind of question which always posed a big problem with the designers and people doing circuitry, and users, and that is that if you keep--in those electrostatic storage tubes--if you keep, maintain, the storage over a long period of time you're going to get some--

Taylor:

Creeping.

Mertz:

Right, long-term effects. And one of the questions was, "Well, you can't change the virility of those-- so maybe you can change what the location of the storage--so that you can more or less balance out and compensate for this problem. Was that ever done?

Taylor:

Oh, yes. You see the Williams tube didn't have that problem because you rewrite that one all the time. It doesn't have any static mode. And our tubes had a static mode. So, one of the things we did was rewrite the whole tube over again. It turned out you didn't have to--you really didn't have to move it. All you had to do was erase it and rewrite it and you got a fresh start. Whether you changed the physical location wasn't important, it was the secondary emission situation at

any one moment that was creeping.

Mertz:

This leads us up to something that I--

Taylor:

Oh, we did--I'm wrong in another sense. When we got a tube with a flaw in it--a small flaw--we could adjust the whole array, so that flaw was not under the beam at any one of the stop points. And we could save tubes that way. Once in a while we'd burn a tube--we'd move the whole thing.

Mertz:

Those tubes became a bottleneck in terms of the production, too.

Taylor:

Yeah, well the real problem with the storage tube is that all of these little ailing things could happen to you in the middle of a program, and then you couldn't tell whether your program was wrong or the hardware was wrong. And in the early days, since neither one was stable at any one time, we'd spend days looking for flaws, not knowing whether it was soft or hard. That's why we spent so much time with test routines, because we always had to fall back to test the hardware.

Mertz:

Well, this leads up to something else. Of course, that is one of the significant circuit and conceptual contributions of Whirlwind, and that is the marginal checking program as such, and one thing that I'm not clear on and that is how that was applied to the storage tube situation. Did it--was it used--

Taylor:

Oh, yeah. It turns out marginal checking--I guess I get credit for marginal checking, so I can talk about it.

Mertz:

I have your article.

Taylor:

It turns out that predicting when a thing is going to fail is possible in almost everything you do. But you have to be--you have to be clever in the sense that you just can't--you can't write a cookbook on how to do it ahead of time. You have to use your wits a little bit. And so, what you're really saying is that in certain analog circuits, which run the storage tube, can you do the same thing you do in digital circuits. In digital circuits you can easily see that you have to approach a threshold for the thing to trigger, and how much margin do you have before you get to the threshold is the thing you can measure. But in analog circuits, you can do similar things, and--for instance, you have to have a certain beam current to write a spot. If you have too much beam current you get into trouble, and yet--or you don't have enough. But one of the things is that if you leave everything alone, you might have the adequate beam current, but as the circuit ages it doesn't have--it runs out of current. When you do a certain amount of--so you put in some feedback, you see. One thing you can do is, "How much feedback do I have to put in before I fail to write?" If I remove the feedback and I put in more feedback, I will eventually not write. And if that--the amount of feedback changes from day to day, I know that my beam is changing. So, it turns out that you can do the same thing on amplifiers--in other words, how far can I move the bias before I start to get a noise out of the amplifier. And just by being a little

clever, you can actually measure "did this thing change since yesterday?" Regardless of what it is you're measuring. And if it changed, then it's not behaving the way we wish it were

Taylor:

Oh yeah, we always--we usually--the digital computer is the best detector of failures. And what you do is write a series of diagnostics such that at any one moment you're only testing one component. And if that fails, the nature of the patterns of ones and zeroes is such that you know which ones failed. So, you test one thing at a time. You know human beings can do well diagnosing one thing at a time, and they're awful at diagnosing multiple failures. It's terribly hard.

Mertz:

Well, the design of this actually--when was that begun?

Taylor:

Well, that was back in the five-digit multiplier. See, we all did--we learned about that--

Mertz:

What, '49?

Taylor:

Yes. '48 or 9. Forrester started to talk about this. And I don't remember--there was--it was in those days when four or five people used to talk about everything. It would be really hard to say how the thing came to be defined.

Mertz:

But it's largely the circuitry and the design of the marginal checking was your baby, so to speak.

Taylor:

Yeah. Well, you see, that's one of the things--I feel I get more credit for that than I deserve, and I did--and probably--

Mertz:

Less for--

Taylor:

Less for some other things that I think were more important. (Chuckle). But that's--

Mertz:

Well, in your--well, that's important--there's a general rule sometimes that many people get credit for things that they really didn't do, and then things that they did, other people get credit for.

Taylor:

Yeah, yeah.

Mertz:

That crops up often in technology, particularly in something like this where you're working on a lot, with different people. What do you feel--well, you've already touched on a lot of this--from the period of the marginal checking on, how long were you then related to--especially with the program?

Taylor:

Well, I was there for ten years. And I was in charge of the MTC Computer, and then the--

Mertz:

Now, that didn't come in until later—

Taylor:

'52, I guess.

Mertz:

After they got the core--

Taylor:

Well, I was in charge of the core development group. I was in charge of all the engineering for quite a while.

Mertz:

There's some interesting tricks down in the design of the core memory, too.

Taylor:

Yes. One of the things, for instance, that I feel that I did was to force Bill Peck(?) and his group to use pulse transformers to drive the core memory. They said it couldn't be done, and I said well, let's do it anyway, and then after it doesn't work, we'll argue about why it doesn't work. And of course, it worked. And so, when somebody came out we had a little diode trick there, too, that we were able to work out, so that we didn't have the problem of not--of getting the average value changing the base line. Without the pulse transformer, you can imagine the problem--the first memories were direct coupled without transformers. We had to have--they were just the minor--you know, quite large current coming right out of the tubes. It took great big bottles to drive it. In fact, the first core memory that Jay invented used a thyratron as big as that water pitcher there to drive one core, you know, and there were--well, I was in charge of the FSQ-7 Development, to put the importance of memory in its perspective. We had sixty-five engineers designing the Sage FSQ-7. And in the early, the first year, fifty-six of the sixty-five

were working on magnetic memory, because if we didn't have a magnetic memory there wasn't any point in building it. So, all of my gang were on that memory.

Mertz:

Well, there...

Taylor:

Many problems. ...Dave Brown's group, for instance...(inaudible)

Mertz:

...(inaudible)...

Taylor:

Well, the big decision we made was to go for ferrite cores and use the one turn(?). Now there's one Bob Everett supported me on, and I'm not sure that he didn't invent that idea; of putting the threading wire--no, no turns. And we knew if we didn't make it with no turns there wouldn't be any great advantage. It would cost too much and be full of errors. Now, we went for two things--we went for broke on that; One was that we could get enough energy in the doughnut(?) affair with one wire through it, and the other that we could make it out of a ferrite. We were almost pressured into making it out of ribbons.

Mertz:

Of...

Taylor:

Yes. And the direction variations between those pressed ribbons were such that we--the memory wouldn't have been very reliable. Now, we couldn't buy the ferrite, of course, so Dave Brown, as another part of this activity--he also was in my group at the time--started--we started to make

ferrite cores. We were really, you know brave with them. We were wise. We didn't know anything about them, but we hired chemists, and physicists, and…and...

Mertz:

Your own...

Taylor:

Yes, we--and we hired--some German physicists came up with the idea that we squelch these things in steam from 15,000 degrees Kelvin.

Mertz:

Was this followed with the general ceramics(?)...

Taylor:

It was some relation with General Ceramics, and I don't recall whether he was there, or they had suggested we hire him. I think he was a friend of the fellow who was Chief Engineer. Anyway it seems to me we were the first ones to do this. And immediately we did that--all the cores in the batch came out even. Up until that time they were spread all over the place. Now, I don't think this squelching--we didn't invent the squelching in scheme, we just took somebody's idea and did it, you see. It's still used today.

Mertz:

Did you get any people from Hippel's group at all...

Taylor:

Yes, they were in or out--several PhD thesis students were in or out. Anyway, I imagine there were twenty-five or thirty solid state people around. And therefore, it's hard, you know, if you're really to give credit--I give a lot to Dave Brown. Now, he managed that whole thing. He didn't

know all that much about it when he started, and without that ferrite core we wouldn't have any core memory

Mertz:

Well, now, that's--you mentioned one thing, and that's the wiring of the memory itself...

Taylor:

Yes. And now one of the things--one of Bill's men in the system--I've forgotten his name--came up with the clever ways of wiring the memory, and then every other row was on the opposite wire in order to get the end effects so that all the spurious currents and cross fields cancelled out. And, for instance, Jan Rajchman, you know, claimed that RCA did this, and there's an interesting paper in the file that I wrote--he came to see me one day, and he told me--first he said, "One thing is the single wire won't work, and furthermore this one wire readout won't work." And so I spent most of the morning convincing him that--explaining to him why it worked and how it worked. He didn't understand all this clever cancellation business. He went to lunch and I paid for the lunch, and he came back in my office and he said "Would mind, Norm, if we patented that at RCA. That's the kind of thing you wouldn't be apt to patent in the university." He said, "But it's just very useful for an industrial firm to patent." And then the son-of-a-gun went to the courts and said he invented it. I really--I never quite got over that, because I had spent all morning explaining it.... But...

Mertz:

He was faced with a number of problems, actually....(inaudible).......didn't work...

Taylor:

Yes. Well, in that way he didn't understand it. I just never got over that discussion with him. I

can't really look him in the eye, now, I just....I'm so embarrassed for him, really.

Mertz:

The...

Taylor:

But you see, each one of these things was a little bit of an invention of its own, really. And therefore, it's really wrong to say Jay did it all. Conceptually, he certainly did. But all these guys contributed in these little steps along the way, until we had a practical product. And there must have been fifty people working for two years on that.

Mertz:

Now, you stayed with the project, more or less, during the time that Writer(?) came into existence...

Taylor:

Well I--well, of course, about '51 we became a part of Lincoln Laboratories. That didn't change our mode of operation at all. When we moved out to Lincoln it did change. When I worked closely with Bob Everett right up to '58--from '48 to '58. He was Director and I was Associate Director at Lincoln at the end. And we did--like we did TX-2 really, although we didn't do the details ourselves. Wes Clark did the details, and Ken Olson, who is now President of DEC. But, you know, all of those things we had done together we really--for instance I remember on TX-2 Bob and I sitting up all one night and saying, you know, we're not going to build another machine that hasn't got some new ideas in it and this was just another machine. We said "No." We got them to--we forced them to put in their -64 program registers, which are now standard scratch-pad techniques, and a few things like that.

Mertz:

What--did--well, there was another circuitry problem of sorts that came up with work length, of Whirlwind, because of its short--you had to...

Taylor:

Well, we had double and triple length instructions.

Mertz:

Well, originally, I thought you only had planned on double...

Taylor:

Then did the triple--when did they decide to go into a...

Taylor:

I don't know--we didn't change the machine instructions, but I think we had a--maybe it was a quadruple length. I think it was a software aid. You see, when we had those scratch registers, we began--people began to think, you know, I've got those--just like the PDP-6 had six accumulators, we had five extra ones right there. So, we'd store half of this and half of that...(laughter)...

Mertz:

You can do that. That overcame one of the things that--for example, the idea of a floating point arithmetic was well known at the time that you...

Taylor:

We programmed that, didn't we?

Mertz:

Eventually.

Taylor:

Yeah.

Mertz:

But the original decision was made for a fixed-point arithmetic...

Taylor:

We had to have the speed you see, in order to solve those problems in real time we couldn't fool around with floating point--we couldn't make floating point work very fast--fast enough. We just made it simple and let it go to like hell...(laughter)...

Mertz:

This is one of the things that I noticed in Charley Adams' Masters paper which he never actually put on the machine was to cope with that problem that you do have a fixed...(inaudible)....all the error...(inaudible)...

Taylor:

Yes. I think, you know, we ought to--when we designed the FSQ-7, which has a dual arithmetic unit, for the sine-cosine function mostly, which will come up all the time in radars--we argued about the floating point in there. I think IBM hesitated--we kind of designed them jointly, and as it turns out it's not much different. All our family machines, right up to the 7090, they're all the same.

Mertz:

The most significant breakthrough, of course, we touched on technologically was the ferrite.

Taylor:

Yes, the ferrite, the single turn, the fact that the one wire for the Read amplifier cancelling out all

the noises. That was really quite a breakthrough. The transformer was included because we could then use--you see, without the transformer we would never do it with anything but tubes.

Mertz:

Yes, that's something that's interesting. I'm glad you touched on it because that isn't immediately obvious...

Taylor:

You see we got that from--the problem was to get a system--a transformer whereas your pulse rate changes your average value doesn't build up.... And again we used the diode, too.

Mertz:

When was it, do you recall, roughly that you had sort of--with Sylvania and the others, decided these diodes are going to be reliable.... they'd gone through two or three models in the tubes....

Taylor:

Well, the Emporium(?) group did the tubes in Sylvania in Emporium, Pennsylvania. And the diodes came from the group over in the Boston group, initially. Then we got some Hughes diodes. But I kind of get confused because we built the five machines over there and, you know, Whirlwind and MTC, FSQ-7, TX-O, and TX-2, all the same guys, and the evolution through all the different diodes and later on all those transistors--we got some funny stories on buying those first transistors.

Mertz:

When did they first--

Taylor:

It's hard for me to say, you know, which particular diode--there's too many of them now to remember.

Mertz:T

hey never used--the MTC was expressly set up for the ferrite, for core memory...

Taylor:

Yes, we...

Mertz:

You never used the electrostatic...

Taylor:

Oh, no, no. In fact, MTC ran a year before Whirlwind got its memory. We retro-fitted one on the Whirlwind after MTC had run for a year. And the reason for that is Whirlwind was a prototype for the air defense project and we needed it as a testing device for all those procedures. So, in order to make it reliable we just put a core memory on it. Then, once they had one on it they put another one on--then they kept it going ten years. The memory really began to work.

Mertz:

Well, did they...

Taylor:

You never had to change the circuitry, that's why I wanted--you never had to do anything to the circuitry.

Mertz:

So whatever was put on the MTC was essentially...(inaudible)....

Taylor:

Well, we did change the circuitry on the MTC.

Mertz:

Yes, but you didn't have to change it on Whirlwind itself.

Taylor:

No, Whirlwind, itself, had no change to it. On MTC we used a different set of tubes--smaller tubes. Whirlwind had those great big bottles. You'd dissipate all that heat...

Mertz:

No one ever, I take it, seriously thought of trying to do anything with transistors on Whirlwind, itself--or is that...

Taylor:

You know, when the FXQ-7 was the decision point there. We really wanted to make a transistorized machine. And Gene Felder(?) at the Bell Laboratories actually pleaded with me to see if I would only make--I had to make the choice. Well, I made the choice to go magnetic core memory, because, you know, IBM had the Williams tube.... And then I had that argument with him, and I expected a fight to the death, because we had control of their money for the first model, and they had to do what we wanted to. So, I just felt that if we had--you know with a machine like that, with all the problems, we knew how reliable tubes could be made. We--we were the only ones that had ever built a computer out of transistors, and we had--before TX-O, we had a test computer at the same time as we were designing the FSQ-7 made out of point contact transistors, and the failure rate was terribly high, and the manufacturing problem was really out of control. Well, by the time--we had to make the decision to go tubes, and then we

got the prototype pretty well built, and then the junction transistors came in and we had already started TX-2, you see. We had to come--all the time we were backing ourselves up.

But they got to the point where to change over to transistors would have been really--it would have been following one of the things that wasn't the major problem, in order to say that it was transistorized. And had we started it; we might have--we might not have allowed the…(?) problem, because we would have been the guinea pigs. Everybody in the country had been selling those transistors. And we--you know, you can't do too many variables at the same time, you just can't and keep your reliability. And the cores, of course, were a big gamble at that time. So, we just had to--we had to put all our eggs--we had to do one big thing and not two or three.

Mertz:

But a lot of the push on this came from Bell. The idea of trying...

Taylor:

Well, that was a personal thing. Bell wasn't involved in the machine.

Mertz:

Yes, but they certainly were involved with transistors...

Taylor:

Yes, and we were down there a great deal. And Felker(?), you know, he said "You've got the only really big machine going right now, and if you'll go transistors the whole industry will be affected." I really--that was a hard decision. Turns out that if I had made it the other way, we'd have been alright. I think so. I think we would have bailed it out. But the point is that it would have been great for this story, but would it have been good for the rest of the air defense project, if we'd have gone all the way. Because the machine, itself, wasn't the problem. You know, we

had other kinds of problems. Certainly, the circuitry wasn't a problem.

Mertz:

Well, now that raises another question...

Taylor:

The problem was with radars, and communications, all the things that had to be integrated into that.

Mertz:

Whirlwind is--an interesting description of your involvement in this--Whirlwind was very much involved in some of the very first display--output display...

Taylor:

Oh yes. That's a whole story by itself.

Mertz:

I think it might have been possibly--correct me if you're of a different view--the result of something of like historical accident(?) because of their involvement in the ASH(?) program in terms of output display; but maybe not.

Taylor:

No, no. There was a conscious effort--we actually always had a display on Whirlwind from the very first day. We used it to actually look at the spots on the storage...(inaudible).... And that was the reason it was there. And after we got it there, we used to store everything. And I can remember when the Air Force generals came around to make the decision that Whirlwind was going to be…air defense, we were able to take--if you've ever seen Whilwind's test room--there's a whole rack of stuff and there's a little five or ten inch display tube. So that was the console.

And we actually made airplanes go in circles and trace each other on that same test console--the same one that we used to test tubes on. So, the Air Force generals got the idea of a console from that test equipment. And then we decided--then we went down a charactron and got--and put all those fancy consoles were done afterwards, realizing that, you know, we just simply couldn't--we've got to have more capability, and we had to have more writing power, and although the Air Force requirements with so many letters, and so many this, and so many that, most of which we helped them write, but still were written around the charactron, which is the only thing at that time that gave you any speed in writing terms...

Mertz:

Well, I was going to say, this was another--not a significant hang-up, certainly, in the memory core, but was output was something...

Taylor:

Yes, real time output, yes. Now the interesting thing is that most of the people that worked on the storage tubes, Pat Eudes(?), and Cordovan(?), and quite a few people--though Dodd by this time had gone off--but as soon as we went to cores one of the problems was, you know, what do we do with this talent? They don't need them anymore. And it turned out that the need for good display tubes was--needed all of those kinds of things. You need precision, electron optics, we needed accurate control of deflection systems. And those fellows really did redesign the charactron, completely. When we went down to see the first charactron, it was a shadowgraph. There was no optics at all in the tube...

Mertz:

Do you recall some of the people who worked in the optics, even going back to the--do they

trace back to the earlier problem of optical alignment...

Taylor:

Yes. Well...

Mertz:

...or was that like...

Taylor:

Yes, it was the same. Current use had a lot to do with both of those--optical alignment of the gate tube, and then later the electron optics in the charactron tube. And they had Cordoman(?), who's now down here, and Mediater(?), who was with them way back and all through those. And I got in Frank Rogers--came out of the radar part, and for some reason got interested in this and I guess he taught electron optics somewhere, and he helped. There might have been--then we hooked a storage tube on the front end of one of these guns, and it's on--to make the console storage tube, because, actually the idea of communicating--we could have done that with a typewriter, but the idea of eighty typewriters clacking away was frightening, so we thought of using the storage tube. If we'd have refreshed that as well as the other tube, we wouldn't have had enough time, so we sort of put the storage tube in there as a--kind of a, you know, here's these guys who're working on the storage tubes all of their lives, so we'll spend a few bucks and let them continue working on storage tubes as display devices. And so, we got Andy Heiff back in the act and the whole Hughes team, and they helped us with both jobs--this job and the Cromary job. So, it was quite a powerful group because Andy Heiff and his electron group at Hughes had excellent electron optics people--and still have, I think. So that was a pretty powerful team that did all that basic work. And we pretty well put the Cromary people in

business with these graphics--digigraphics things. They didn't--they invented it but they had no idea how to implement it so it would work.

Mertz:

One thing that pops up is the problem--generally the memory--the electrostatic storage tube in storing, in the late '40's and early '50's was a very frustrating...

Taylor:

Oh, it was terrible, yes. I already--see, I gave two storage tube stories and I'm not going to give anymore (laughter). It was a frustrating...

Mertz:

I've heard that one explanation when Thebian(?) starts to work on doing some tests on ferrites actually, he worked on the teletype.

Taylor:

Yes, he did most of his work on __(?)____.

Mertz:

...that this was sort of very quiet, very small-scale activity which was not built up as being a very promising thing, one consideration being that they didn't want to completely demoralize everybody close to the work on the storage tube...

Taylor:

Well, partly that was the--actually, Jay recognized that the storage tube was--you know, it almost didn't work is the best way to put it, and that if we didn't have some--the whole computer--everything about a computer was going to get into trouble. And we had had some coincident current ideas way back in our early days--was back in the Servo-Lab(?). And one guy

who claims that he ought to have the patent ahd a paper written on coincident thyratrons, using the square lip(?) of a gas discharge in the same way that you use the...

Mertz:

Square lip...

Taylor:

Square lip of a hysteresis current. But it's a two

Mertz:

Well, it's important...

Taylor:

How--have you talked to Jerry. Jerry is different than most of the other guys. He's just a different kind of guy. And one of the things that amazed me is he once said to me, Imagine me being in charge of a computer and I don't even know how to run it." Because he really had his battles to fight just keeping the money coming in. We used a lot of money.

Mertz:

And there were some serious problems--the Whirlwind almost got scratched.

Taylor:

Yes. George Valley kind of rescued it.

Mertz:

But,--well, I get the impression that they worked together, since, pretty well because some of Everett's strong points compensated for...

Taylor:

Yes, that's right. Well, Everett's a people person and Jay is not. That's not his strong point.

Everett is--generally has a tremendous amount of loyalty.

Mertz:

Among the original group, it's very interesting the people who worked during the war at Servo with Forrester, most of them stayed on and continued...

Taylor:

Yes, uh huh.

Mertz:

...(inaudible)...... You came in shortly after that?

Taylor:

Oh, yes. The war was over and...

Mertz:

And they were still thinking of analog...

Taylor:

Yeah. Well, some of them were still thinking of analog when I came. The decision had been made to go digital, and I think to go parallel. But I don't think--the day I'd come I don't think Jay had told anybody about it, because he didn't know whether they'd accept it. I think Reese(?), for instance, I don't think was going to buy that. Everett probably had already decided it. I--no, I think Everett really did a lot of thinking and Jay was just kind of the administrator to make it happen. I don't mean to detract from it because, you know, he was brilliant in many ways. There were times when he was brilliant.

Mertz:

Are there any other things that you can think of about Everett that...

Taylor:

He was the only man that I know that Von Newmann used to come to see as a consultant. I mean he really used to come to see Bob Everett. He came to see me twice and I was very flattered--once years later. But he used to come to see Bob Everett every month, or every three or four weeks. And he actually--I was sitting there sometimes and there was just no doubt about it, that there was a--there was no "I'm Von Neumann and you're Bob Everett." It was--if anything, it was "How would you suggest that we do this, Bob?" The one story that I was going to tell you about that is the first time I got that running, Gordon Brown came in and I said something about, "It's only been running three days, and it's done some many million mullifications(?) already without an error." And he says, "Oh, that's not very many." And I got kind of mad at him. That afternoon he sent over Vannevar Bush and Von Neumann came over to see it, and [Manivour Boush] put his arm around me and said, "Boy, now we're really going", and Von Neumann kissed me...(laughter)...

Mertz:

...(laughter)...(inaudible)...I'm sure he wouldn't have showed up...

Taylor:

Yes, but Von Neumann--he knew that if you could get those megacycles you could do something. And nobody else had come anywhere close to that.

Mertz:

Well, did--oh, one other person --I've heard this and it occurred to me that it's false--and that is Wiener, who had done a lot of, sort of, what you might call intellectual work on the idea of brain...(inaudible)...that he never, ever saw the Whirlwind...

Taylor:

He never came in the building, no. I used to eat lunch with him, and he knew I was designing the thing. And I don't know exactly the occasion of this, but he--I guess I belonged to one of the Ada Kappa Nu Clubs(?) that he belonged to and he used to come and tell mystery stories, and tell about computers, and he'd say, "Oh yes, you're doing the Whirlwind. Now tell me about the Whirlwind." And I'd go so rapidly, "Oh, yes it's sixteen digits." "Isn't that nice, isn't that nice. How are things going?" "Great." And he'd say, "Well, I must come and see it sometime." And he never did, he never did. I don't think he'd know a computer if he saw one.

Mertz:

It's curious because you'd think that...

Taylor:

Yes, you would, wouldn't you?

Mertz:

...that...

Taylor:

Conceptually, you know, I don't see why he gets the credit. I mean Von Neuman, that's something else. He really was something. But--and he could--he understood in great detail what you were doing, even though he wasn't an electrical engineer. He seemed to understand everything. But there was no discussion, that I ever was even able to listen to...

Mertz:

He did some--in some of his courses, he did some work on the...

Taylor:

Oh yes. And Franklin used to give us courses in numerical analysis and how to convert analog-type expressions to something you could use in the digital sense. And he'd modestly say, "Now, this is straight out of the...

Mertz:

Well, Gordon Lowen, I think...

Taylor:

Yes, Gordon Lowen. But you know, that a different--that's a part of the art that's--that I'm not really--I never have been really expert on that. And I can see that he probably did contribute there. But it always occurred to me that, you know, we have--you know, we were building these machines for the first time. You'd think he'd at least come over and look. I suppose he just assumed the engineers would make it work one of these days.

Mertz:

Well, Von Neumann didn't. I mean he...

Taylor:

No, he was there all the time.

Mertz:

...much of the...

Taylor:

Oh yeah. And he used to--I was very much flattered--he later became on the President's Science Advisory Committee, and one of the problems was should they build a lark(?), which is, you remember, that's a very large machine at Livermore. And I had had a great big discussion with

him about magnetic drums. Way, way back in '50, just when we were trying to get the architecture of the FSQ-7 to figure out, you know, could we do a machine that big. And I had told him you couldn't build--you couldn't make seven magnetic drums work. It's just too many tubes, too many switches, and it wouldn't work. It would be 160,000 tubes and that was too many. And about--oh, it must have been eight years later, he called me up from Washington and said, "Could I come and see you." And he came to my room, " This is a computer, as you know, I would like you to tell me can we build these many drums." And never forgot the argument.

Mertz:

He never forgot?

Taylor:

Yeah, that time we had invented the switching diode and we could get around the problem. But, you know, he said, "I just want you to say so because this is a lot of money." He came all the way up just to ask me that one question.

Mertz:

Did you visit--ever visit the IES Computer?

Taylor:

Yes. Once. I don't remember just what time period it was. Instead of all these transformers and correction wires, they have just wires all over the place. You know, like a physicist who would do--never worry about impedance, or transit time. And it's a wonder--I don't know if it ever worked or not. It's amazing to me.

Mertz:

Well, the Smithsonian has...(inaudible)...

Taylor:

You know what I mean, then. It's just like a rat's nest...(laughter)...in that after--you know, I think what probably happened, in order to make it--to stop it from ringing, they had to slow it down so much that it really wasn't all that interesting to Von Neumann. And I imagine that when we got this going at four megacycles, that's what he was so excited about.

Mertz:

Well, thank-you very much.

Taylor:

Is this about what you wanted?

Mertz:

Well, I thought... (Tape cut off)

Taylor:

Well one of the interesting things was the program ERA. Most of their work was quite highly classified, and they had been sent to Whirlwind, even before Whirlwind ran by members of the Defense Department to review the whole logic. So, we, of course, turned over all our papers to these people and got to know them very well. Because we realized what they'd done, they got permission to show it to--the 1101 to us before it was shipped. And I can remember them saying, you know, "Wait until you see this machine, you're going to feel a little bit jealous because we copied everything you did, and plus added a few thoughts of our own." But, and, you know, it turned out to be so close to Whirlwind that--it turns out all the 1100 machines are not very different from Whirlwind. And it was, of course, during this time--now their prime memory on those first machines was a drum memory. They didn't put a core memory on until

many years later. So, of course, after this interchange we realized that when we ran out of room and needed a drum, we of course, went back to ERA. And later Bill Narsis(?) told me he said, "You know, one thing I remember about you is you're the first guy to show off and ask me for something--a product that I didn't know there'd ever be any need, or any way to buy any of that stuff." And he said, "We built quite a few drums, but you were the first--yours was the first one we could talk about." This was quite a way back--'52 maybe, '51 or...

Mertz:

It was before...(inaudible)...

Taylor:

And, just within the last few years, I just came from Control…, but I later became Technical Assistant to Bill Norris at Control Data. Many of that group had ties just as far back, as for when Frank Mulaney...

Mertz:

This is another thing that's hard to sort out--it is because much of this work, almost all of--much of it--some of it is...(inaudible).....but most of it is.....computers--the applications.....but the machines are, themselves--that is, you have the so-called open realm of research, and then you have the classified defense world of research, which is not quite a different world, but it's an awful lot of things go on there which have--eventually crop up sometimes in..... But this is something that ...(inaudible)....continuation....

Taylor:

Yes. I think that's true.

Mertz:

And then after you left the Whirlwind--well, you might describe your own career with Whirlwind in the '50's, after it became Division 6 at Lincoln Laboratories...

Taylor:

Well, of course, by the time I left--I turned Whirlwind over as a machine to Steve Dodd and went off to become the Design Engineer on the FSQ-7 in about '52, I think. And...

Mertz:

When you say you went off, what...

Taylor:

Into another building. Went over to the Whitamore Building, and left the Barta Building, and built up a new team for--which I mentioned earlier--most people were on the magnetic core memory for a good while--and then that group grew to almost three-hundred engineers, between then and 1958 when I left the Project Lincoln. And during that period, you know, with a good deal of help we designed the MTC Computer, which was the first one that tested magnetic cores: Then the FSQ-7, which was the sage(?) Defense Computer: Then the TX-O, which was the first all solid-state computer in the MIT family: and then...

Mertz:

...(inaudible)...historically......over all transistor computers...

Taylor:

No, not in the MIT family. I think there were several there. TX-O was the first we did in that particular group, and...

Mertz:

Do you recall--there was a computer that never lead anything over CT-24...

Taylor:

Yes.

Mertz:

Which used some transistors...

Taylor:

They drove the memory with transistors. That was the one that was going to be used at the radar set, as I remember it. That was the Lincoln Lab Computer, wasn't it?

Mertz:

Yes.

Taylor:

I don't remember whether that was--I think that was a little later than TX-O. But it was before TX-2. I'm not sure of the exact dates in there. That was designed by a bunch of fellows who thought that designing computers was easy, and they went off--and they were in a different division than 6, and they lived--we all lived to help them out because it turned out--there's something about computers in those days, they're probably not so bad now, but there's an art to designing them so they don't have a lot of flaws in them--like feedback paths, and things that oscillate, unknown orders that you didn't get into the machines that aren't really orders, and the first time you do one, it's really a bloodletting, clean-up mess. And they learned that one the hard way.

Mertz:

Well, then you were describing your own career...

Taylor:

Yes. After I left Lincoln Lab in '58, I went to Itak(?), and did a lot of work in optics. I became very interested in the man-machine(?) aspects of computers, using graphics. And at that point we--Charlie Adams and I--Charlie did some work with us under contract, and we had a patent on the digi-graphic display system, which is not Control Data's digi-graphics--and that was using a lot of these old techniques, but now much faster to use sophisticated cathode ray displays of engineering drawings and things like that. And then after a few years that particular program was bought by Control Data and I went to Control Data--and became Assistant to the President of Control Data, and later I came back to Boston and worked at Arthur B. Little for a few years, in the consulting business.

Mertz:

Was this in the sixties?

Taylor:

Yeah. I went to Control Data in--I think I was--I left Lincoln in '58, and I resided with Itak(?) until about '62 or 3, and then I was the Control Data in '63 and '65 or 6, and I was with Arthur B. Little until '69. And started our own company this--last year.

Mertz:

And what are you primarily--what relationship do you presently have to computer technology...

Taylor:

We do--most everybody here has a background in computers and we do systems consulting, and

management consulting, you know, and the wide spectrum of information processing. We do...

(END OF TAPE)

(last part of Tape I, Norman Taylor, 27 Jan. 1970 (this part could not be duplicated on tape):

Taylor:...in a management sense, but in a control sense, but why, how the investment community and people who are interested in computers from a financial viewpoint can best take advantage of the ever-growing area.

Mertz: Did you get involved in any of the numerical control applications of computers?

Taylor:I didn't in the early days when Whirlwind used to do some of it.

Since I've been in the graphic computer aide design aspects of it, I've become pretty well aware of that problem. Particularly how the fast response you can give the human being in allowing them to do things graphically would be very hard to do if you have to program them all. Because they are so error prone--gemetrics are so error prone--if you have to do it by.....languages and things, then graphic patterns are pretty clear.

[END OF TAPE]

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