

INTERVIEWEE: DR. MONTGOMERY PHISTER

INTERVIEWER: Robina Mapstone

DATE: February 21, 1973

RM: The date is February 21, 1973, this is Bobbi Mapstone and I'm talking to Dr. Monty Phister in Los Angeles.

All right, shall we start with Stanford, and go from there.

MP: Do you want me to repeat what I just said?

RM: (laughing) Would you repeat what you just said?

MP: Oh, all right. (laughter) While I was an undergraduate at Stanford I heard something about the computer field-- I don't rightly remember what just now--and decided that it might be a good kind of occupation to make a life's work, although I really wasn't very definite. I thus took a course at Stanford called Engineering Science, which enabled a guy to write his own curriculum and choose his own set of courses. I chose a combination of mathematics courses and engineering courses.

Independently of that, I got married during my first year of graduate school at Stanford, and we decided it would be fun to go abroad to study, so I applied for a Fulbright Fellowship. Since I only

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spoke English, I decided I had to go to England; and since I was an engineer, I decided I really wanted to go to Cambridge amongst the English universities. I specifically made that request in the Fulbright application, and in due course of time was accepted.

I wrote to the university telling them that I had a scholarship and asked them what kind of work was going on at Cambridge. At that time I didn't realize that there was any computer work going on. They wrote back giving me a long list of things that they thought I might be interested in, and one of the things that they mentioned was the Mathematics Laboratory under Maurice Wilkes. They also said that Wilkes would be in California that summer and suggested I get in touch with him. So I arranged to meet him and had dinner with him, and in the course of that conversation he agreed that I should come to work at the Math Lab. So before I even arrived at Cambridge, I knew what I was going to be doing.

RM: What year was this?

MP: This was 1950. It must have been the spring or early summer of 1950 that Wilkes was here in California. In September of 1950, we arrived at Cambridge. We were met in London by the Fulbright people and sort of had an introduction to British life to take away

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the cultural shock (laughter) of landing in a new country, and in such a strange country, what with a different language and everything! (laughter) Actually, my wife had a cousin in London whom we had told that we were coming. She had gone to Cambridge and found an apartment for us so when we arrived we were all settled and we moved into the apartment.

I showed up at work at the lab and met the various people that were there, including Dave Wheeler and Stan Gill. I don't remember whether Sandy Douglas was there when I first arrived, or whether he arrived later; it seems to me he arrived later. Bill Renwick was there and really became my closest friend at Cambridge. He passed away about two years ago.

The first task, as I remember, was first to kind of learn what a computer was and specifically to learn how the EDSAC was designed and how it worked. I was given a whole batch of papers and diagrams and sets of written-down instruction from which I, with help from people from time to time, taught myself about the EDSAC. At that point, of course, I didn't even know what the routine was to get a degree at Cambridge. I soon found that it's much easier to get a Ph.D. at Cambridge than it is to get a Ph.D. in the United States.

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RM: Is that so?

MP: That's an interesting side-story. I met an American in Washington some years later and we got to talking. It turned out we were both from Trinity College, Cambridge, but he was there fifteen years before I was, and he got his Ph.D. He said, to me, "Doggone, I've always wanted to talk to somebody who's been there more recently, and find out whether the situation is still the same as it was when I went to Cambridge." The situation then was that it was dead easy to get an undergraduate degree in an American university, and likewise a cinch to get an advanced degree, Ph.D., at a British university, because there are no language requirements, there are no courses you have to take; all you have to do is write a thesis. Whereas if you do things the other way around, it's dreadfully hard to get an undergraduate degree in a British university since you have to know several languages to even get in, and then the course of study is really terribly hard, and they really work your tail off. To get an advanced degree at an American university is similar in that there's all sorts of exams you have to take and a whole list of courses that have to be taken, in addition to your thesis. He asked, "Is it still the same way?" I said, "It's still exactly the same way."

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RM: That's interesting.

MP: The easy life is the way to do, and it is the way I did it!

RM: In England they presume if you've come up through the rank and file of the educational system, that by the time you get to Ph.D. level, you've got an education.

MP: Yes. The whole theory is that in doing work on an advanced degree you're supposed to work on your own and learn to do research. Really teach yourself to do research. I had very little help from anybody, and I really did the whole thing myself. I'm sure the modus operandi to get an American degree, involves a great deal more consultation and hand-holding by your major professor, than was the case at Cambridge.

There were several false starts as far as my particular degree is concerned. I remember, for example, one of the first things I did was look at the circuit design. Specifically, of course, transistors were coming up around the corner. Wilkes had plans to build a follow-up and replacement to the EDSAC, EDSAC II, and was beginning to think about what circuit technology should be used. Transistors were the obvious choice. However, they were very difficult to come by. I remember somehow we got a batch of maybe twenty

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transistors. I think what happened was that Wilkes made a trip to America and somebody gave him a batch and he brought them back. For awhile, I did some experiments and took measurements and did some trial circuits. They were pretty bad transistors. I didn't know much about them then, but I do remember that. It seemed like they only just barely exhibited the transistor amplifying action at all.

RM: You were about ready to go back to vacuum tubes?

MP: At any rate, that was a continuing problem. The problem of just getting a supply of transistors was taking so long and was such a slow process that it soon became clear that it couldn't be my thesis subject; it would have to be something else.

I finally wound up writing a thesis whose title was, "The Prevention, Detection, and Correction of Errors in Automatic Computers." It looked at several aspects of error detection and correction, including how you design to avoid problems; how you detect problems when they're in the working machine; tube marginal checking, which was something that Wilkes and company invented and put in the EDSAC; and it included diagnostic microprograms, which is an interesting subject because it's something that's finally come around to being used today. That is to say, one part of my thesis

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described a special section of the microprogram that just went through and exercised all the elements in the machine and collected information which would make it possible then to diagnose when something went wrong with the register somewhere. So much for my thesis.

In thinking back on the years at Cambridge, the kinds of things that come to mind are, for example, I think Bill Renwick is a member of the Cambridge computer community who played a more important role than is generally realized. He was kind of a dour and very quiet Scot who didn't ever say very much, but he was really the engineer. He was the electrical engineer who knew how to design circuits so they worked. Wilkes didn't have that kind of background, and of course Wheeler and Gill and company were mathematicians and programmers, so basically the hard problem of designing the hardware so that it worked was really Renwick's job, and he did a very good job of it.

I think probably the reason EDSAC was completed and in operation faster than the American machines that had been started earlier, specifically the EDVAC, which was the machine that EDSAC was copied after and had already been started when Wilkes came and visited,

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was due entirely to Bill Renwick and his very sound engineering. I think that probably the EDVAC was in check-out at the same time, or perhaps even earlier, than the EDSAC but check-out was completed a lot quicker on the EDSAC.

Bill finally left Cambridge and went to work with Plessey and was in charge, or had some kind of a senior position, in their memory technology, in terms of working on plated wire memories, for example. He passed away very suddenly, I think of a heart attack, about two years ago. I hadn't even heard about it until more than a year later, when we exchanged Christmas cards and heard from his wife. That's typical. He was such a quiet pioneer that his passing wasn't even noticed by the people that should have been aware of things like that. I'm sure that he never got the kind of recognition that he really deserved.

RM: Does Wilkes give him credit?

MP: Well, no. I guess I never heard Wilkes say what I just said, namely that it was Bill who was really responsible for the engineering design that made the machine work. But I'm not sure there was ever a time when that was appropriate. I know at a very early date one of the early papers describing the EDSAC was co-authored by Wilkes and Renwick.*

* There were three papers; two in 1949, one in 1950. RM

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RM: That's right.

MP: So it was acknowledged, Wilkes acknowledged it, in that sense. Let's see, what else do I remember from those days?

RM: Were you actually a user of EDSAC?

MP: I wrote some programs, but no, I really wasn't a user. The main part I played with EDSAC was as a maintenance man, which tied in with my thesis work which was computer maintenance and reliability. After awhile I got to the point where I would take the night shift on maintenance. The computer was almost always used every evening, and very often all night long. If something went wrong with it in the evening, the people that were usually there to help fix it weren't around, so it was convenient to have a free graduate student to help out and things like that.

There were all sorts of strange troubles that would occur; I remember one particularly. EDSAC was in a special room all by itself. There were about three or four racks of electronic equipment on the left side of the room as you entered the door. On the right side there was a wooden table that had a few cathode ray tube displays, which displayed the contents of the memory and of the various registers inside the machine, by having a scam with ones shown as bright

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dots and zeroes being just blank spaces. That was used as a tool by the programmers to see what was going on and to help in checking out their programs, and by the diagnostic engineers to try to follow something. In addition, the principal input to the machine was a paper tape-reader, a photoelectric reader as I remember, which stood up on a special pedestal by itself close to the table that held the cathode ray tubes. Typically the machine would be handed over to the user, he would have his tape already prepared, he'd load it in the paper tape reader, and he'd press a button on the computer's control panel.

The control panel was a little metal box maybe five by eight inches with something like half a dozen buttons on top. I guess I don't really remember what the buttons said nowadays, but one was certainly "CLEAR", another one "RESET" and another one was "START". Now the "START" button was interesting. You would press the button, and there was a stepping switch in the machine, which stepped through a sequence of positions, making a loud clacking noise, clack, clack, clack, clack, clack! What it was doing was loading the initial instructions into the memory. The memory was a mercury delay line, which was volatile. You know, when you pressed the clear button right it

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simply cleared everything out of the entire memory. As I remember, that's what happened.

In order to get anything into the machine you had to load in these initial instructions which were kind of a bootstrap, and they would load other instructions in as well. You'd press the button and you'd hear the click, click, click, and those initial instructions would include a routine which would read your tape on the tape reader. The tape would start to go in and would come to the end and stop. Everything would be quiet for awhile, and then you'd look on the cathode ray tube and you'd see the bit patterns changing.

One very famous program was written by Dave Wheeler and J.C.P. Miller. It's kind of funny, because I remember this so well and I've mentioned it since, and people have said, "That isn't the way it worked." But Miller and Wheeler wrote a program to find prime numbers. I'm digressing, but for some period of time Cambridge had the world's record for having discovered the largest prime number. It was held by Wheeler and Miller, both of them. The program used a technique called the sieve of Eratosthenes, something like that. The sieve works as follows and it's a famous ancient Greek way of finding prime numbers. You put down one little mark for every number in the

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range that you want to look at. First you cross out all the even numbers, which is every other number, because they're obviously not prime numbers because you can divide by two. Then you cross out every third number, which are all the numbers divisible by three. Then every fifth number, I guess you don't have to do the fourth factor, but then you can do the seventh and the eleventh and so on. When you've crossed out all those, the ones you have left, the ones that aren't crossed out, are prime numbers. I distinctly remember there'd be a pattern of "ones" on the cathode ray tube, and the cathode ray tubes showed the contents of the memory location. There'd be a pattern of "ones," and they'd all be "ones." Then the program would make a pass through, and every other "one" would become "zero". Then it would again pass through, and every third "one" would be a "zero". Now this happened very fast, and you would just sort of see a blip going through the screen and some of the "ones" would disappear. That would happen several times and then finally it stopped. The "ones" that you had left were the prime numbers and could be printed out. Anyway, the programmer's program would operate for awhile, and then the results would be typed out on a typewriter, which was also on the wooden table with the cathode ray tubes.

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One time there was one chap whose program just wouldn't work. Every time he ran it, something would go wrong, and he couldn't tell what it was. There would seem to be something different every time, it wasn't a consistent kind of trouble. Other people would run their programs and everything would go fine so, of course, that would point to a fault in his program because other people's programs were working. But he swore that he'd run the program before and it'd worked all right. Somebody called me in one evening to look at things. I couldn't find anything wrong with the machine, and so I observed the chap as he ran the program. Finally I noticed that immediately after running his program tape in, he would take the punched tape, which would be on the floor in a big mess, and he would wind it up into a neat ball again on a machine that was put there for that purpose. They have the same machines these days. It's a little spindle thing, and up put the tape on the spindle and turn the crank. There's a gearing system which winds the tape up into a nice little neat package. He'd do that each time immediately after his program had read in while the computer was working on the problem. It was while he was turning this crank that the trouble always occurred.

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I said, "Let's try it again." This time, after he'd loaded the program in and was just reaching down to pick up the tape and crank it on the cranker, I said, "Wait a minute, don't crank it. Leave it there on the floor." He left it and the program worked! It turns out somehow that the vibrations from the crank machine was causing some sort of a trouble. (laughter) What the trouble was, I never did discover, but we really proved conclusively what was the cause of the trouble because we were able then, after the program ran, to go back and have him try it again with the crank, and that caused the trouble.

RM: I take it you did not use that piece of diagnostics in your thesis. (laughter)

MP: No, I've told that story as an anecdote from time to time, but I've never written it. J. C. P. Miller was around at the time, and he's a mathematician interested in number theory and various numerical things. He was the one that always seemed to have very long programs, so he generally got assigned the night shift. We would put a program in, and it would run for hours and hours and hours.

One of the interesting things, of course, about the EDSAC was that its memory size, by today's standards,

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was ridiculous. As I remember, it had something like 128 words of memory. That seems awfully low. It was certainly not more than 512 words. By today's standards, memories like that are just impossible! (laughter)

RM: You can't even imagine doing anything with them.

MP: You couldn't even get a corner of the operating system in.

That routine that was put in by the stepping switch, which was called the initial program or something like that, was really an extraordinarily ingenious program that had all sorts of canny features packed into no more than 32 words altogether. There were all sorts of strange things. It was a David Wheeler program, I think, and David is one of the most inventive and ingenious programmers and logic designers that I've ever known. He's the kind of guy who does things which are frowned upon in proper programming society, like writing a program where sometimes when you make a pass through the program, a word is a piece of data, and another time it'll be an actual program step. It's idiosyncrasies like that which drive other programmers wild. But in early machines, where memory space was at a premium, tricks like that were very appropriate and widely used. Somebody really ought to look up and publish a paper on that initial loader.

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program, or whatever it was called, for the EDSAC, because it really was an ingenious and interesting piece of work, and I think typical of a lot of the things that were going on in those days.

RM: Yes.

MP: Which may be programming off the wrong foot. (laughter)
What else?

Stan Gill had an interesting program which was the first learning program that I'd ever heard about. Oh, I know what it did! You remember there were cathode ray tubes which displayed the memory contents. In one of the banks of memory Stan's program displayed a vertical bar with a hole in it. That is to say, picture two rectangles one above the other, with a rectangular space between them, and with a blank space on each side of the rectangles. That would be the initial display. Then a program is written so that a bar, another rectangle, would start at the left-hand side of the screen and would grow toward the right, and would go through the so-called hole between the two bars and this rectangle goes zip from left to right through the space between these two bars. The object of the game was to guess whether the bar was going to go from the left to the right, or right to left, something like that. You were supposed to guess what the machine was going to do; if you guessed one way you'd

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put your hand under the photoelectric reader, if you guessed the other way you left your hand off. After you made your guess, the computer would guess what you were doing to guess, and would make the bar go from the left to the right, whichever way it was. If it guessed properly what you were going to do, where you were going to hold your hand, it gave itself a mark, and if you guessed properly it gave you a mark. Then it kept score. Stan had some sort of an algorithm, the details of which I don't remember, for analyzing the previous moves you'd made and predicting what your next move was going to be. That became a common kind of an experiment to perform with computers, and it maybe still is, but that was the first one I'd ever heard of.

. While I was there, Wilkes wrote his famous paper entitled, I think, "The Best Way to Design an Automatic Computer", which described microprogramming.

RM: That was in 1951, wasn't it?

MP: Yes.

RM: That was the paper he gave at the dedication of the Manchester machine.

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MP: Yes, he gave it at Manchester. That paper then led the way to the designing of the EDSAC II which was built on that principle. Part of my thesis, the part I mentioned where I did some diagnostic micro-programming, followed from that invention.

RM: Did you actually work on EDSAC II?

MP: No, except for the kind of diagnostic concepts and for helping the lay-outs and thinking about some of the registers, I didn't, because when I left in the early summer of 1953, EDSAC II was still just a plan and talk. The details of the design had not yet been worked out, and there was no hardware; they really hadn't started it at all.

Another thing I did in connection with that thesis, which may be a first, was I simulated the micro diagnostics for EDSAC II on EDASC I. That is to say, I simulated the register logic, and had a way of simulating the MICROprogram so that I could put faults in the simulated hardware and see what the microdiagnostic would do. I would get a print out what the right answer was, and what answer EDSAC II would get with that particular fault installed. That was probably one of the early simulations of one machine by another machine.

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RM: How does one go about getting a copy of your dissertation?

MP: Well, I've got a copy upstairs you can borrow if you like. Of course it's at Cambridge and available to anybody.

RM: It would be just as easy if I could borrow this one.

MP: It's not a very important thesis compared to some of the things that could have been done, presumably, by somebody that was smarter and had a little more genius. I'm not particularly one that's come up with genius; I'm more of a hard worker rather than the inventor.

RM: That's a good quality. You left England in 1953 with your Ph.D?

MP: Well, that was interesting. I left England in 1953. Oh, let's see, I finished my thesis in the late spring of 1953, and then I was examined on it in London by Wilkes and by R. C. Williams, who came down from Manchester and whom I had never met before. Williams gave me a bad time, quite properly, in examining me on the thesis. He had already read it because it had been sent to him. I came away from that meeting figuring that I'd bombed out and that I was probably not going to get a degree. There was no course work, you know, there was no other exam. Wilkes didn't disabuse me-- he said nothing to me about my degree. He said, "Good-bye, Phister. It's been good to have you with us,

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and drop by again if you're ever in England." (laughter) When I came home I definitely wasn't a doctor. I hadn't been awarded the degree and I had no indication from anybody as to whether or not I would be. I guess it was at least a couple of months later, certainly while I was working at Hughes--which was my first job-- I got a letter from somebody down at the university. Maybe I didn't even get that; maybe I just got the certificate! (laughter) That's how I knew I had gotten the degree. Maybe Wilkes did write me. That's funny because I seem to remember either he wrote me that I had been awarded the degree, or he wrote me not long after. Up until that time he had always called me Phister. After I got my degree was the first and only time he had ever called me Monty. He's called me Monty ever since. (laughter) I figure if I hadn't got the degree, I'd still be Phister. He's a very formal man. I still call him Dr. Wilkes.

RM: You probably were thinking his English reserve had gone a little too far.

MP: During the last few months at Cambridge I'd written to several companies in America about a job. The companies that I remember writing to were Bendix, Hughes, and I think EECO, Electrical Engineering Company, which was a small company kind of in the digital circuit business. I got their names, as

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I remember, from magazines. I don't even know now what magazines they were. My folks were here in southern California, and my father probably went through the want ads or something and sent me the names of some companies. I had an offer from Hughes, and I think I also had an offer from Bendix. For reasons which are obscure now, I chose the Hughes offer. I accepted while I was still in England, so that when I came back I knew I had a job. We rented an apartment in Westchester. I showed up for work at Hughes where they were working on the business data-processing machine.

RM: Ah, yes. DIGITAC had been developed, right?

MP: Well, DIGITAC was an airborne computer, right?

RM: Yes, it was an airborne digital computer.

MP: I think it had been built, but that was in a completely different part of Hughes so I really didn't learn much about it.

RM: They were building this rather large business data-processing machine?

MP: That's right. A machine not too different from the IBM 650, as it turns out, only we didn't know about the 650 at the time. It was a decimal drum machine, as I remember. The guy in charge of that project

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was Bill Hebenstreit, Eldred Nelson was a kind of senior man, I think he was the associate department director, and Gene Grabbe, who's another well-known name in the business, was the section manager. A fellow by the name of Mike May had a section managership. He lives right around the corner here in Westwood, he's still at Hughes, as a matter of fact. There were other fellows: a fellow by the name of Harold Shoemaker worked for him, Bob Johnson, who's now the vice-president for engineering for the Burroughs Corporation back in Detroit, had just finished his masters and was working for his Ph. D. at Cal Tech with a Hughes Fellowship.

RM: Was Dick Tanaka there at that time?

MP: If he was, I didn't know him. He wasn't a part of that business data-processing department. I suppose Bob Johnson came out as the most famous of the characters. Dale Scarborough, who's now vice-president of Bunker-Ramo, was there. I think he transferred from the military end to the commercial end while I was there.

A lot of work was being done on logic design. There was a line printer being designed with one of these rotating drums with print hammers. Hughes already had magnetic drum technology in connection

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with the airborne machines.

After awhile, I got assigned the job of designing the interface between the computer and the controls for the peripheral equipment, which was to include, besides the printers, some magnetic tape equipment. I don't remember now where the magnetic tape equipment was coming from. I think we were going to buy it from Ampex or somebody like that but I really don't remember off-hand where it was coming from. I was there a total of about eighteen months, and after I'd been there about a year Mr. Hughes suddenly got wind of the whole project and cancelled it, wiped it out, said, "We will stop making the business data-processor."

RM: Do you have any knowledge about why?

MP: I never heard how he learned about the existence of, or even what he knew about, the project. I don't know, maybe somebody just came to him and said, "We're designing a business data-processor." Or maybe somebody sent him a series of reports on it. I'm pretty sure that it was not the case that somebody gave him a formal briefing at a meeting or anything like that. That wasn't his style. I think probably what happened was he heard it by accident and said that that isn't something Hughes should be doing. I think his judgement was that, and I think he was probably right. Hughes

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was a military contractor, was then and still is, as you probably know, although they've gotten into the satellite business, which is beginning to become kind of a commercial operation. I expect his philosophy was, "We don't know anything about business data processors and it's going to be very expensive and difficult to learn, so we should drop the whole project."

At any rate, we dropped it, and we dropped it before there was even a machine assembled to the point where it could be checked out, so it was in the very primary stages. Although by that time there must have been fifty people working on it, I suppose, and maybe more than that. Of course, the project being cancelled kind of shook everybody up. There were still plenty of projects in the company having to do with digital techniques, and no one was fired that I remember. There was a pass made at all of us by various people who needed engineers. I remember in particular a fellow by the name of Dick Barlow who had a radar processing project, a funded contract with the government, was very interested in people and was a very controversial character, so there was a lot of shenanigans and politics going on.

Several of us looked around on our own trying to decide what might be a good field to get into. One

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of the things that evolved from that was a machine tool control project, which did continue, although I think finally Hughes either sold or otherwise discontinued it. For a time--I don't know, it may still be going on--Hughes had developed, and I think was even manufacturing, digital controllers for machine tools to control drill presses and mills from punched paper tape, for example.

RM: Yes, as a matter of fact, one of the notes I have is that you were considered the father of process control.

MP: That's what I was going to tell you next. Controlling machine tools isn't really what I call process control, but in thinking about what might be interesting to do, I read some articles and papers that said an obvious thing to do would be to apply computers to the control of chemical and petroleum processes and things like that. Hughes had some basic technology, which was quite applicable. Their work with airborne computers required that the computers be real time, in today's sense, in that they go on line with machines and that they receive signals directly from transducers in the airplane that give information on pressures and temperature and so on.

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Hughes had developed the interface to the transducers, and analog to digital and digital to analog converters, and had all the digital technology, so they were in a good position to move into this process control area. A couple of us proposed to our bosses that that's what we ought to do next. Our bosses generally said, "Gee, that's an interesting idea and let's think about that along with a lot of other things." So we were thinking about it along with a lot of other things.

Meanwhile, the Ramo-Wooldridge Corporation had been founded. It had been founded in 1953, I think, which is the year I arrived at Hughes, and soon after I arrived Ramo and Wooldridge left with some of the key people and kind of shook things up.

Meanwhile, while I was told at Hughes to wait and think about it, I heard that Eldred Nelson, who had been at Hughes, had joined Ramo-Wooldridge and was trying to start a process control operation. I got in touch with him, and told him that that's what I was interested in, and asked whether it was possible for me to join RW. He said, "Fine, come along." I left Hughes feeling a bit guilty because I'd only been there eighteen months, which didn't seem like a long enough time to stay. I remember

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at the exit interview I was apologetic to the guy who was interviewing me. He said, "How long were you here? Eighteen months? That's a long time."
(laughter) I guess it was even longer, it was probably twenty months.

RM: You were up for a longevity button! (laughter)

MP: Time for my gold pin! So I joined Ramo-Wooldridge.

RM: Let's flip this over before we get into that.

(TAPE I, SIDE II)

MP: At that time their offices were on Arbor Vitae, which is just north of Century Boulevard near Westchester; in fact, so far as where one went to work is concerned, I went to work a half-a-mile from where I was working with Hughes. So it wasn't all that much of a change.

RM: All the companies were sort of in that area, weren't they?

MP: Yes. The real granddaddy of all the companies was, let me think, CRC, wasn't it? Computer Research Corporation?

RM: Yes. It began at Northrop and then a group spun off to form CRC.

MP: Yes. I think they were located somewhere in that general area, although I really didn't know too much about them.

Incidentally, some way or other while I was still at Hughes, I got involved with teaching at UCLA.

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RM: Oh, yes!

MP: I'd forgotten to mention that. Should we digress now, or shall we go ahead with RW and come back?

RM: Let's go back to UCLA and discuss it now.

MP: Sometime in 1954, the IRE group on computers, IRE being Institute of Radio Engineers, which is long before your time, had a local group which talked about computers and they met periodically near UCLA. They had talks by people, I guess once a month, which is very much like the societies have today. I began attending those meetings and joined the society, and at some point along the way volunteered to give a speech describing some of what was going on at Cambridge. I think this was early in 1954, although it might have been late in 1953. I prepared it and gave it, and as a matter of fact I even think there was a written paper that didn't ever get published, although I handed it out at the meeting. I don't even think I have a copy of it anymore.* I talked about three different things: the microprogram; the B-box, which had been invented at Manchester not at Cambridge, but

* Copy located subsequently. Ed.

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which really wasn't known here in Southern California, as I remember; and Stan Gill's learning program, the one I mentioned a while ago, which was kind of a novelty. So in the evening I discussed briefly those three things.

Later on, one of the very first Burroughs machines had a B-box, and in fact, some of the literature tends to ascribe the B-box to Burroughs'. At least it certainly says, and I think properly, that the first commercial machine to have a B-box was that Burroughs machine.* I think that's right. That may have come about by the Burroughs guys being at that UCLA lecture where I described the B-box to them.

RM: I think you're very right about Burroughs being the first machine, because I just recently talked to some Burroughs Electro-Data People and they said that the B-box came about through Harry Huskey.

MP: Oh, yes.

RM: Huskey was familiar with both the English work and had been at UCLA.

MP: He was at NPL, National Physical Laboratory, wasn't he?

* The machine was Consolidated Engineering Corporation's 30-201. CEC became Electro-Data and was later acquired by Burroughs. RM

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RM: I was thinking of the Institute of Numerical Analysis, wasn't it?

MP: No, no, in England.

RM: Oh, in England! Yes, he was at NPL. I think he probably had a two-sided introduction to it. I heard a very funny story. One of the Consolidated guys said, "Huskey was talking about this B-box, and we realized it was important, but we couldn't get the concept into our heads." Apparently Ernst Selmer, who was the actual designer on the CEC machines, caught it and immediately wrote it in.

MP: The Burroughs machine wasn't the first machine to have it, the Manchester machine was the first, but the Burroughs machine was the first commercial machine to have it, which should rest the case. But I think the places where I'd seen it described that statement is made very properly. However, the implication of it is that it was invented at Burroughs, and that's really wrong.

RM: That's a bad implication.

MP: As a result of that lecture at the computer society meeting at UCLA, I met some of the other people, like Willis Ware. Willis was already giving a computer course up at UCLA, and somehow I got invited to a meeting with Willis and John Salzer, and another guy whom I think had been giving a circuits course, or

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planning a circuits course. I guess I don't remember.

RM: Jerry Mendelsohn?

MP: No, it wasn't Jerry. At any rate, several of us met, and the object was to establish a reasonable set of courses for computer technology at UCLA to add onto Willis's introductory course. As I remember, we decided there should be three courses to complement and follow the introductory course: one course on logic, one course on circuits, and one course on systems. Somehow I got assigned the job of teaching the logic course. I guess I don't really remember who got assigned the other two courses, but they were taught from time to time at UCLA, generally with different people at different times. As a matter of fact, a textbook came out on the circuits course written by an old friend of mine, Joe Hawkins, whom I encouraged to write the book on the grounds that it was a good subject. It was a good idea, and I think he did a good job, the problem was that it took him so long to write the book that by the time he finished it integrated circuits were upon us. It was published by Wiley, but I don't believe it was very successful.

RM: Do you recall the name of the book?

MP: I think it's something like, Circuit Design in Digital Computers. I've got it upstairs if you'd like to have it and check its name. I was assigned the job of

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teaching logic design and I was scheduled to teach it starting in the fall semester of 1954 at UCLA University Extension. All during the spring and summer of 1954 I worked hard on some notes for that course. By the time the term came around, the notes were probably two thirds through. I started teaching with one hand, and with the other hand I kept working on the notes. By the time the semester was over, we were on the semester system at that time, not a quarter system, as I remember, it coincided with the last lecture I finished and I handed out the last set of notes. Then I immediately started to give the course the next semester. I gave that course at UCLA for ten years, from 1954 to 1964.

The notes were noticed by a couple of publishers, Wiley and McGraw-Hill, and they came around and suggested I write a book. I said, "Okay." I signed up with Wiley and finished the book during the next four years. It was published in February of 1958, and is still in print, for heaven's sake. It's been a very successful book. It's sold over forty thousand copies, which is good enough for a textbook.

RM: It sure is. What is the title of it?

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MP: Logical Design of Digital Computers. It kind of established me as a figure in the field and gave me a reputation of sorts, and it was a profitable book. It's paid me a lot of income over the last fifteen years. I taught out of the book until 1964, and as I remember it was in 1964 that some representative of the UCLA faculty came to me and said, "Phister, that textbook you're using is really getting to be old-hat and you're going to have to teach the course out of a different, new, more modern, up-to-date textbook." So I smiled and said, "Mhmmm, okay, what textbook." They said, "Bartee, Reed, and Lebow." Or something like that.* So I studied the book and taught the course out of the new textbook for one semester, and then I resigned. (laughter) Except for an occasional odd lecture, summer courses and things like that, I haven't taught since until now, when I'm hoping to teach from, and am working on a new book on an entirely different subject. Okay, That's UCLA, now let's go back to the RW.

RM: Just before we come back to Ramo-Woolridge, what did you use as your source to get your notes gathered?

* Bartee, TC; Lebow, IL; and Reed, IS, Theory and Design of Digital Machines, NY, McGraw-Hill, 1962.

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MP: I read everything I could find about logic design, including notes locally published here by people in Los Angeles, internal memos at Hughes and other places. I had some experience doing logic design in the way it was done at Hughes, and the way it was beginning to be done in Southern California generally.

RM: The Boolean approach?

MP: Yes, with extensive use of algebra. The machine really was completely described in Boolean algebra. I think that's where the Western guys were pioneers. Shannon was not a westerner, obviously, and he had pointed the way, but the people in the East at that time certainly, and perhaps still today I don't know, really tended to use Boolean algebra as a tool to help solve unusual problems. In other words, they do all the design with little diagrams with gates drawn in and lines drawn all over, and then having the design they'd say, "Here's a very complex series of gates. Let's see whether we can apply Boolean algebra to simplify these gates." So they would copy down those gates in algebraic notation, simplify and rearrange them, and then translate the result back to diagrams again. Their basic description of the machine was in terms of a series of diagrams.

We in the West held that was illogical, if you'll excuse the pun, (laughter) and that there were a great

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many advantages to translating the design into algebra right from the first, partly because simplifications would arise, but also, and principally, because it was a concise way of describing precisely the way the machine worked, and of seeing and understanding better how things worked. If you have to trace lines on a very complex diagram, you waste a lot of time running along a line with your finger and going all around the page. Sometimes you get off and get on the wrong line, and then you are really confused.

As it turns out, although we didn't make much of that point at the time because it was so early, we also used Boolean algebra to describe a machine because that makes it possible to have the description in computer language, and then to process that description using a computer to do the processing and create from that documents which are usable both for manufacturing and for maintenance. That's probably the biggest and best reason for having the information in machine language. The Gardner-Denver machines that do all the wiring automatically these days, which of course didn't exist in those days, are driven from punched paper tapes, punched cards or magnetic tapes, which are created by computer programs that process this machine description of the Boolean algebra description of the computer.

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Similarly, the field engineer has got a big book of literature that describes the machine. That book is largely, generally speaking, composed and printed by a computer based on the information that was created by design automation system out of a Boolean algebra description of the computer. Sometimes, very often I think, today's computer takes that Boolean algebra description and prints some logic diagrams, some gates with wires and names on the wires and what not, because there are certainly advantages for maintenance guy to see what gates physically look like. Some people have a hard time envisioning things that are in Boolean algebra anyway, so there is reason to have both kinds of description in the machine. The fundamental way most machines these days are constructed, I think, is through the use of algebra.

Your question was how did I learn what I put in the book, and the answer is: pretty much from reading and from observing how practices were carried out here in Southern California.

RM: By this time most of the Southern California computer groups were using the Boolean notation anyway.

MP: I think so, yes. I think that's right.

RM: Were you familiar with Floyd Steele?

PHISTER

MP: I knew about him. I don't think I ever met him.

He's one of the real inventors in the field.

RM: Yes.

MP: I guess some of the digital differential analyzers that he figured out were just extraordinarily ingenious logic combinations. But there are a lot of very ingenious logic designers. One of my closest friends is one of the most inventive and, like Bill Renwick, a veritably unknown guy. His name is John Blankenbaker. He lives not far away. He was at Hughes at the same time. As a matter of fact, he wrote an early paper which is called, "Logically Microprogrammed Computers," or something like that. He invented an interesting concept. He observed that you could design an extremely simple machine which consisted of a magnetic drum, maybe one flip flop and two or three gates, and with that machine could simulate any machine.

RM: A truly universal machine.

MP: It was a truly universal machine. It was very slow, of course, when it was simulating a machine, but in effect you would store both the logical structure in Boolean equations on the drum, you'd store the current states of the flip flops on the drum, then you'd use the very simple logic just to process that data. I think every drum revolution did one clock pulse of

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the computer, or maybe it wasn't even that fast. It was really a very ingenious idea.

John recently developed and organized a company which developed and sold a small general purpose storage program computer which he sold for \$850.00. It was more sophisticated and faster than the EDSAC-- it had 256 bytes of memory, which I think was bigger than the EDSAC memory. Maybe it wasn't; maybe I was just making that up.

RM: Maybe it's the same.

MP: Roughly the same, yes. It has indirect addressing, index registers, and was intended to teach high school and grammar school students and college students, too, about computers. Although it was an extraordinarily ingenious machine and potentially was a profitable enterprise, as a business it was unfinanced and it's in the process of failure right at the moment.

RM: That's too bad. What's the name of the company?

MP: It's called Kenbak, which are the middle six letters of John Blankenbaker's last name.

RM: Is that paper available do you know?

MP: It was published in the transactions of the IRE.

RM: Approximately when? Do you know?

PHISTER

MP: It was before 1960. It was probably 1958 or something like that. As a matter of fact, there's a reference in my book.* I can give you that reference.

RM: Oh, good. Did your book get used in the East?

MP: Oh, yes, it was used all over the United States. It was very widely used as a text. That's really why it was so successful, because it was so successful as a textbook. Amazingly enough, it's still getting used as a textbook in some more backwards universities and colleges around the country! (laughter) It must be, I don't know how else to account for a couple of hundred sales every year.

RM: All right, now I will let you go forward again.

MP: Okay. So we're back to Ramo-Wooldridge. All right. There were maybe a couple of other guys, besides Eldred Nelson, already there--the only one whose name I remember now is Ralph Conn, who's still around in the computer business and gave a paper at the Fall Computer Conference in Anaheim this year. The only thing that was going on was that people were studying the problem

* I was in error--couldn't find it in the book. MP

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and trying to decide on an approach on how to get into the business of selling computers for control processes. What was happening was that we electrical and computer engineers were trying to learn what the process industry was, and learn something about chemistry and petroleum. Ramo and Wooldridge were very interested in this project, and through their connection with Thompson Products, which has supplied a good portion of the financing for the Ramo-Wooldridge Corporation, they had a lead into practically any company in the United States that they wanted to talk to.

At some point we became convinced that we could make a sensible pitch to an oil company, and through the presence of Thompson Products we set up an appointment with somebody at Standard Oil of New Jersey. We prepared a presentation--we being Eldred Nelson, Ralph Conn, and myself--to try to sell Standard Oil on the idea that they should engage with us in a joint study of some petroleum process with the idea of designing a computer control system for that process. Well, we put our heads together on what this sales pitch should consist of, and came up with a plan. Ramo and Wooldridge said, "Fine. You're supposed to go to Standard Oil next week, come around on Thursday afternoon, we'll have a dry run and you can tell us what you're going to tell Standard Oil."

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On the appointed day when we showed up and we went through our pitch, there was generally a feeling that we could sense which said we weren't going over too well. The meeting broke up and afterwards we were told that the meeting had been cancelled and we weren't going over to Standard Oil of New Jersey. Obviously, our approach wasn't nearly professional enough in the view of Ramo and Wooldridge.

In fact, Eldred Nelson, although he's a great fellow, very charming, and a very bright guy with a lot of good ideas who was a real leader in logic technology and the application of Boolean algebra to computers, was anything but a public speaker. He just did a terrible job. He could hardly put a sentence together, and I'm not much better. Between the two of us, I think we must have been really dreadful. I am better than Eldred, but not a hell of a lot better. So I think that's what really shot the project down.

However, we continued because all it did was to shoot down that one presentation, and although I don't remember exactly how it happened, somehow our next connection was with a company called the Riverside Cement Company, which was a local cement-manufacturing

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operation which had the plant in Riverside although the headquarters were then in downtown Los Angeles. The president of the Riverside Cement Company was an ex-Harvard Business School professor, a young fellow with a very modern outlook on the world, who could see that computers were interesting and that they might be applied to process control. He was curious to see whether they might be applied to manufacturing cement, because cement-manufacturing, no doubt to your surprise, is a rather technical process.

RM: That is to my surprise.

MP: He had an ex-student who was working for him in kind of a staff capacity, and he assigned that ex-student to work closely with us. For a period of several months, Ralph Conn and I did a detailed study of the cement-manufacturing process, in order to learn all that anybody knew about what makes it tick. At the end of that time, which was maybe sometime in 1956 or 1957, I guess probably 1957, we made a proposal to the Riverside Cement Company that they buy a computer to control part of their process.

Now in the meantime we had improved our pitch to process companies, and had persuaded the Texas Company, Texaco, to conduct a study of one of their processes at their refinery in Port Arthur, Texas, down by the

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Gulf Stream and the Gulf of Mexico. In order to handle that study and the cement study, we began to have to build up an organization and staff. We hired some engineers, some guys with control experience and with process experience, and began to build up a group.

Also, obviously, we were going to have to have a computer, but it was decided at some early stage on a level far higher than me that the computer development would be handled by a different part of the organization. Basically, the group that I was working with would be involved in process studies and the application work as opposed to the hardware design. The computer which evolved, turned out to be the RW-300, and was designed by this other group. It was a magnetic drum machine with eight thousand words of 17-bit memory, I think.

RM: It was transistorized?

MP: It was a transistor machine. As a matter of fact, it was one of the very early transistor machines, it was first displayed in 1958, I think. I think it was first shipped in 1958. The first one went finally to Texas. So that development was going on kind of parallel and, to an uncomfortable degree, independent of the work that we were doing in process control,

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although the computer, as it turns out, was fine and did the job without any trouble, and it was a good match with the set of applications that we wound up with.

We proposed that the Riverside Company buy a computer. We did a study, I think a reforming unit was the name of that Port Arthur, Texaco, unit. It used a catalyst and high temperature to change one hydrocarbon into another. I forgot what they called it.

RM: Is that what they call a cracking process?

MP: No, it isn't catalytic cracking. That's a much bigger process and a much more important process in the manufacture of gasoline. This was a much less important and smaller process. We did that study and proposed a control system for that unit in the refinery, which was just a small part of the total refinery. Texaco agreed, and said that they'd buy, and we subsequently installed the first of the RW-300s at that refinery. That was the first digital computer used for process control anywhere, and, as a matter of fact, it made the cover of Business Week magazine in something like April of 1958. Sometime like that. It was a real turning-point.

PHISTER

Meanwhile, we had several other studies going. We had persuaded Union Carbide to study one of their chemical plants, and Monsanto to study an ammonia plant, so the staff had to grow. The whole operation was big enough that it really had to be formalized to some extent, and at that point somebody upstairs made the decision about how it should be organized. A division of Ramo-Wooldridge was created called Thompson Ramo-Wooldridge Products Company. It had Thompson at the beginning and products at the end, with Ramo and Wooldridge in the middle. I think that was before it became TRW. It was a separate division. A fellow named Joe Manildi was brought in to be in charge of it. He's a guy that had been in RW before, but hadn't been associated with the process control operation at all. Eldred Nelson was kind of quietly removed off it and put to work on some other thing. That was really an appropriate decision, because what obviously was needed for the Thompson Ramo Wooldridge Products Company was a salesman, and Eldred really wasn't a salesman. Joe Manildi was a dynamic guy, articulate, he was really an ideal choice.

PHISTER

RM: Did the computer company come with this, too?

MP: No, the computer development operation was still part of RW and they kind of sold us hardware.

RM: I see.

MP: Within TRWP two section were set up. I was put in charge of one, which I think was called Engineering, and a fellow named Ray Jacobson was hired to head up the other side, which was Marketing. They did the selling, prepared sales materials and brochures, and all that sort of thing. The Engineering side evolved into I can remember three sub-sections: one was programming; one was process studies; and the third was systems engineering, which was really installations.

The sequence of events was that the sales operation would sell a process study to a process company. Usually that sale was simplified by the fact that we did the study for free and it would be conducted by our study group in engineering. Marketing would then make a proposal to the customer. If the customer bought it, then he would agree to pay money for the computer system. The project would then begin to develop from a process study group to the programming and system design people who would configure the hardware, write the programs, and form the project

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team which would get the equipment installed and put it in operation, using the mathematical model of the process that had been developed during the study phase by the process study group.

Dr. Thomas M. Stout, who is now president of a company called Profimatics over in Woodland Hills and who is still doing process studies, was in charge of the process study group. A fellow named Bill Aiken was in charge of system engineering, and one Peter Friedlander, who became a close friend and now works back East, was in charge of programming. Probably the most notable figure that came out of that whole operation was a chap named Daniel McGurk, who was hired by Ray Jacobson, and came out of West Point. He'd been in the Army and didn't know anything about computers. He was the quickest learner I ever saw. A really dynamic guy. He learned what a computer is and how to sell it, and he moved up rapidly in that organization. He took Ray Jacobson's job when Ray left, and then later when Joe Manildi committed suicide Dan was promoted to be in charge of the whole TRWP.

We installed the first system for Texaco. We installed a system for Union Carbide and the Goodrich Chemical Company installed a system in Kentucky.

RM: Do you recall when the first one went in?

MP: I think it was 1958, although I'm not certain positive.

PHISTER

The copy of Business Week would confirm that.

One thing we did in addition to installing the first machines was that we really invented all the reasons why you should use a computer to control process; reasons which are still used today. When you read articles today on why computers are used in process control, they're just word for word the things we wrote back between 1958 and 1960.

As a matter of fact, we had an in-joke. There was a story that you've probably heard about the fellow who was going home on a railroad in the South. He got into his upper berth, getting ready for bed, and as he slipped down between the sheets he felt something down under the sheets. So he pulled his feet out and looked underneath and there was a big black cockroach crawling between the sheets. He got out of bed and rang for the porter. The porter came, and clicked his tongue, and took the cockroach away. The fellow gingerly slipped back between the sheets. But he didn't sleep at all that night. It was just a terrible night. He kept thinking that any moment there'd be another cockroach.

When he got to his office the next day, he wrote a blistering letter to the president of the railroad telling him the story, and how terrible it was and that

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he would never travel on their railroad again, ever in his life! The very next week he got a very nice letter from the vice-president of the railroad saying how sorry and distressed and amazed they were that anything like that had happened to him, and they were taking immediate steps to be sure that nothing like that could ever happen again to any of their patrons. They were taking down the number of the car he was on and they were taking special steps to get it all fumigated and cleaned up. They were reviewing the whole technique for fumigating cars, and they were adding new people, and they were just doing all sorts of things, and they hoped that he'd appreciate that they realized that things could be improved, and they were taking steps to prove it, and they hoped he would travel again.

He was somewhat mollified by this very nice letter and was feeling that he would travel on the railroad again; then he noticed down on the corner of the envelope there was a little scrappy slip of paper, and he pulled it out, and there written in very scribbled handwriting were the words, "Send this bird the cock-roach letter." (laughter)

PHISTER

When we heard this joke, we realized that we had been writing a paper which we immediately dubbed, "The Cockroach Paper," which said all these great things about what computers could do to process control, and every paper we wrote said exactly the same thing. It was being published in all sorts of different magazines and given different names, but it was basically the "Cockroach Paper."

RM: Oh, that's funny! Which reminds me, do you have a copy of the "Cockroach Paper?"

MP: There is no one "Cockroach Paper." There are a whole bunch. I published one in Control Engineering with Gene Grabbe which was a good example of the "Cockroach Paper."* I've got lots of them upstairs.

RM: Okay, good.

MP: I guess we've come to the point where I began to become a little discouraged about my work at TRW. I was discouraged in part because the hardware operation was separate from ours; we were really a sales organization doing process studies and writing proposals and things like that. The computers that we were getting from Ramo-Wooldridge to sell to the customers were pretty expensive. It was obvious that they were built using military technology with which they were familiar,

* "Fitting the Digital Computer Into Process Control", by M. Phister, Jr., and E. M. Grabbe, Control Engineering, 1957.

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and that they were in effect gold-plated machines. Ramo-Wooldridge didn't know how to make any other kind of machine. When you went to talk to them about how expensive everything was, your comments fell on deaf ears because they knew only one way of doing things. For that reason, I was becoming disenchanted, and I had an opportunity to go to work for another company, and so I decided to leave TRW and try Scantlin Electronics.

It turns out TRW never was financially successful, profitably successful, with the process control field, and they finally sold the operation to Bunker-Ramo and ultimately, I guess, most of it wound up with the General Electric Company. For a long time, until really maybe three or five years ago, if you looked at the list of installed process controls systems in the United States, there were more TRW and Bunker-Ramo computers than there were any other kind.

RM: You really should have been the leaders in that.

MP: We were the leaders in that sense, but we were really too early. We were pioneers and it's very hard to make money when you're a pioneer. I think now that the ground is broken and the machines are cheaper and the customer is sold that it is a good idea, people, and specifically IBM, are beginning to make

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money in process control. I see IBM people writing "Cockroach Papers" these days, and it's really the same deal but they are trying to make them sound as if they are new in 1973! (laughter)

Okay, that's my pioneering story in that sense.

RM: Did you not get involved at any time with Max Palevsky and SDS?

MP: Yes, when I left Scantlin, I joined SDS, and worked for Max. Actually what happened was I worked for Scantlin for six years. It's a small company that had developed another pioneering operation before I joined them; it was a stockbroker quotation system that enabled a stockbroker to look up the last prices on his stock, trading on the NY or American stock exchange. Jack Scantlin invited me to come there with the idea that if everything worked out all right I'd become chief engineer. That was really a chance to get back into hardware, which I wanted to do, and commercial hardware with a vengeance, because he really was doing work. At that time the Ramo-Wooldridge transistor flip flop manufacturing cost was something like \$100; a Scantlin transistor flip flop was manufactured for about \$3.

RM: Oh, wow.

PHISTER

MP: It was really obvious that Jack knew something that TRW didn't know. I joined Scantlin in 1960, learned all about that, became chief engineer and became vice-president and a director of the corporation. I finally got disenchanted there because I became convinced that my boss wasn't really going to make a success of the operation. He was doing all the wrong things.

I persuaded Dan McGurk to join Scantlin too. He was there eighteen months or two years and he was finally fired by a new executive vice-president they hired in. Dan had been at Scantlin, and he wasn't altogether happy with it. He was in a staff job, and that really isn't the kind of job Dan should have. So he wasn't altogether happy with the work that he was doing. Max Palevsky and company heard about him, and came around and made a pitch for him to join the SDS. Well, at that time it was clear that the then-head of Scantlin was really bad news, and the Board of Directors was probing in and it was pretty clear to me that they were going to make a change. I thought there was a good chance that Dan would wind up the new president of Scantlin. So I persuaded Dan to tell Max to go away because he would wind up in a good position. Well, the directors did decide to get a new president, but they didn't decide on Dan. They decided on another guy, and

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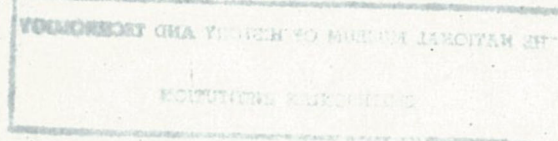
that guy came in and it didn't take more than a couple of months for Dan and the new president to be at loggerheads, and Dan got sacked. He got fired! (laughter) So there he was going back to Max saying, "Have you got a job for me?" Three or four months before he was in a very strong negotiating position where he could write the terms.

RM: Are you two still friends?

MP: Just barely.

RM: Thank you very much.

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