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Interviewer: Robina Mapstone
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MAPSTONE:

Today is February the [laugh] seventh? It's approximately February the seventh ... and this is Bobbi Mapstone and I'm talking to Mr. Bernie Horwitz. ... Let's start off by you talking about your education, joining Continental, and the circumstances that led up to your going to Cal Tech.

HORWITZ:

I graduated from the University of Texas in, I believe, 1948, with a bachelor's degree in Electrical Engineering.

MAPSTONE:

Are you from Texas?

HORWITZ:

I was born and raised in Texas. I went from Texas to Stanford University where I did my graduate work and got a master's degree in electrical engineering. From there, I joined Continental Oil.

MAPSTONE:

What date was this?

HORWITZ:

1949. At that time Continental Oil had its corporate headquarters in a small town in Oklahoma that very few people have ever heard of, known as Ponka City, Oklahoma.

MAPSTONE:

1949 [Laughter.]

HORWITZ:

You had the characteristic reaction. Most people laugh. It turns out that the city, as are most Oklahoma towns, was named after the Ponka Indians, a very well known and old tribe. All Continental's research and development laboratories, geophysical laboratories and the central computing laboratory are presently located in Ponka City.

MAPSTONE:

Are there any Ponka Indians left?

HORWITZ:

There are some Ponka Indians there, but like others they are becoming more and more sparsely spread. Oklahoma was one of the areas of the United States in which the Indians were, if I may use the term, exported from their homes. Many Indians from Florida and the southern parts of the United States, and even some from this part of the world, were shipped off to reservations in Oklahoma. As a result, Oklahoma has large reservations, and they still have significant Indian cultures surviving.

MAPSTONE:

But are they mixed up?

HORWITZ:

Oh yes, they are very mixed, there are dozens and dozens of tribes there. But that is another subject.   In any event I left school and joined Continental's Research and Development Laboratories in Ponka City, which was a most comfortable arrangement. Before I got done I had established the Electronics Laboratory in the Research and Development Department and it's still there. At this time most oil companies' electric activities were predominantly geophysical, which was the case at Continental Oil. However, the director of Continental's research organization was a far-sighted man, and he believed that electronics was one of the coming things that would be applied to refinery and pipe line control and advanced forms of instrumentation for petrochemical work and so on.

MAPSTONE:

Was he ahead of the whole field at that point?

HORWITZ:

Not that far ahead. My initial assignment there was an outgrowth, an off-cropping if you would, of what would today be analog computing. The company was very interested in techniques for simulating oil field performance and they had acquired some equipment. We also developed our own and I devised a very elaborate piece of simulation equipment

that could measure the pressure and flow distribution in the subterranean reservoir as oil was taken out. That is very interesting to an oil company since volumes of oil buried in the earth are so large in individual reservoirs that you don't have to increase the percentage of production very much to be involved in very large amounts of money. Consequently, they spent large sums trying to determine how to improve the yield out of a reservoir. That was the type of work we were doing.

MAPSTONE:

Using electronic means?

HORWITZ:

Yes, analog simulation. We actually developed some very interesting machinery to do this. Parallel to all of this, one of the staff members in the research organization knew Stan Frankel. I don't recall exactly the reason that Stan Frankel was brought in as a consultant to the company, but I'm inclined to think it was in some other field of fluid mechanics or mathematical analysis. He had developed the idea for this little computing system and I was asked to go out to California for three months to give him some engineering help to bring this little machine into being. Out of this we were going to get a little bit of knowledge on the fundamentals of computing, and although we didn't have a clear picture at the time of what we would do with this knowledge, it sounded like an interesting idea. The three months stretched out to six, and then nine, then twelve and fifteen, and I was there for about a year and a quarter. When I got there, what I found was fifteen percent of a computing system in a very crude breadboard form, which basically was a logical diode network to do the logical functions of the machine. It had no memory, no input/output mechanisms, and it didn't even have a power supply to run it. Stan was primarily a mathematician and he had Fitz Witfield at work and Jim Cass, a graduate student, doing things like hand machining what became a little drum memory for the system. That is how I got to California. Stan was on the faculty and teaching, and since you have interviewed him, you probably have a better chronology of how he emerged at Cal Tech than I could give you.

MAPSTONE:

I'd like you to talk about what you actually did on the machine, and some of your ideas.

HORWITZ:

My primary work on the machine and my primary purpose for being there was very simple. There was nobody in-house who knew anything about circuits or how to design them. Because of Stan's unusual intellect but not through any training that he had ever had, he wasn't a bad circuit designer. He had an almost intuitive feeling as to how things would work, and I remember getting into this roaring debate with him over a power supply that he created, which I said would never work. He spent about four hours convincing me that I was absolutely wrong, and he turned out to be absolutely right. I

was wrong, [laughter] because it would work although it was a strange way to approach a power supply. That is the way Stan is, he's really a very unusual person and very creative in that form. Reducing ideas to practice probably was my major role, and along the way I was getting an education. At this time I had been out of Stanford for a year and a half and, as you probably know, engineers a year and a half out of school know very little about the real world. I was learning the real world along with Stan at the time.

MAPSTONE:

He's a good man to learn it with.

HORWITZ:

Yes, he's a good man. So reducing concept to practice is what I did. He had a little diode matrix wired up in a very crude but interesting form. Did he describe the physical form of the machine at that point?

MAPSTONE:

No.

HORWITZ:

He took a block of Styrofoam about an inch thick, eighteen inches long and ten or twelve inches wide, as I recall, drilled holes in it, and stuck in all of his diodes. These were not vacuum tube diodes, but early solid state diodes; it was even before anybody knew that the life of the diode was very short without glass bonding and hermetic sealing, so you can imagine the problems we had. We had a tight budget at Cal Tech and they did not believe in quality so buying cheap diodes, not good ones, was the idea. Consequently, we had a fairly substantial problem in failure mode, compounded by the method of construction. All the diodes were stuck through the one inch dimension of the Styrofoam with the wire sticking out both sides, top and bottom.

The logical network was created by fastening the two elements of the diodes together in combination or in tandem, and by fastening several sets of anodes or several sets of cathodes together to form a logical AND or an OR structure. We simply twisted the wires in various combinations and soldered them at top and bottom, so what you get is a porcupine-looking thing of Styrofoam with wires twisted together and sticking out of the top. It would be a very lethal instrument if you accidentally fell against it, and you would probably be scarred for life. Of course, it took a few resistors here and there to provide the current regulating sources for the gates and they were strewn around the top and bottom. This strange looking structure is what I found, run off with a couple of laboratory supplies and they were making measurements with oscilloscopes. From this we created the computing system. It is worth digressing for just a minute to comment on what really was unique about this device and how it differed from other machines that were existent at the time. A look back in history will quickly reveal that there weren't

many machines in being; there were no 650s and IBM was playing around with the SSEC, which was a big machine. Remington Rand had the UNIVAC system going, but you have to keep in mind that it would fill the lower floor of this house. We are talking about a machine so small that three of them would fit on this coffee table, so there is a slight scale of difference. The difference was that instead of using vacuum tubes, relays and what you have to perform all of the logical functions, Stan created all of it out of this diode matrix, which today has long since become the standard state of the art for any kind of matrix construction. On top of that Stan had a basic premise: "never use three diodes if you can find a way to use two." His fundamental rule in approaching the machine was to see how few parts he could use to build it. It wasn't because he was trying to build it cheaply, he was trying to create a minimal machine that was a full computational unit, and as a consequence, his machine was one of the first that used multiplicity of function per part. As it happens, it turned out to be a constant source of grief down the road, but it is used extensively in machines to this day. The early machines had unique registers, memory elements and logical structures. For instance, they would have an adder that would only add, so if you were doing something other than adding the adder sat unused. However, the adder was never unused in Stan's machine because you couldn't do a thing without it; it shifted, multiplied, divided, and subtracted, all with this same small group of diodes. Everything went in and out of that adder because that was Stan's idea. Obviously we are talking about a relatively slow machine by today's standards. We are talking about not only a serial machine, but almost a serial, serial machine in that it could only do one thing at a time. It was compact though, and there were few machines ever made that had less parts than the LGP-30, or Stan's machine, or the CONAC system, which is the one we had at Continental. As I recall, I implemented the entire Continental machine with two thousand or three thousand diodes and about twenty-five or thirty vacuum tubes, the last eight of which were a continual battle with Frankel. [Laughter.] He was consulting with us and it was his design we were implementing, so every time we wanted to do something, he spent hours trying to find ways to avoid adding another tube. It was a very, very interesting and unique approach and, looking backward, interesting for an academic approach in a research machine. However, it had an unworkable aspect in that it made the machine almost impossible to maintain. You had to have a thorough understanding of the machine to find out if anything failed, but more importantly, every circuit in the machine was uniquely tailored and, as a consequence, replacement of parts was very difficult. I solved that to a large extent by creating standard diode packages, which the Double E Company--Electronic Engineering Company of California, who, I believe, is still in business in Santa Ana (to this day I think of it as Burgess Dempster's Company)--designed for me. We paid for the dies, did all the work and put on a blue ribbon plug so the diodes would easily plug in and out; I'd had enough of soldering and unsoldering diodes. The Double E Company put this product on the market and I have no idea how many they sold but that was all right with us since Continental was not in the business of licensing these things. I also went to the Double E Company for vacuum tube packages, the flip-flops, the cathode followers and what have you. I speak in vacuum tube terms because that is what we were working with at the time. Transistors and solid state technology had not been developed sufficiently. Happily, in the year and a half that I was at Cal Tech, the glass bonded diode became well known and I had done enough work by that time to know that I wasn't about to build a machine without glass bonded

diodes. So I became a very big customer of Hughes Aircraft's diode line. I was rambling back and forth between Continental Oil's machine and the MINAC, as Stan liked to call it, and I should point out that the machine never did get completely reduced to practice, because it never really functioned.

MAPSTONE:

The MINAC?

HORWITZ:

Yes, it was a research vehicle and I don't think anybody wrote any software for it, although we did install a limited magnetic drum. You know I had better be a little bit more cautious than that, because I left Cal Tech and came back to Continental Oil Company before the machine was fully finished. You had better look at Stan's recollections since it is possible that they did continue with the machine and start doing things like writing software, but it is my recollection that they did not.

MAPSTONE:

I think you are probably right, but that will be in Stan's tape.

HORWITZ:

In any event, his machine saw the light of day twice, as we have already described: once in huge commercially successful form through the LGP-30 at Librascope, and once through the machine we did at Continental. Earlier you asked the question about why we built the machine at all. I have to put you into proper perspective at this point and to do that I have to go back a little. Before I had heard of Stan Frankel and before I went out to the West Coast, Continental Oil was approached by IBM who had this machine called the Card Programmed Calculator. As you know, this was IBM's really original venture into computation, and it wasn't even their machine, since some people at Northrop originally put the Card Programmed Calculator together. IBM, in their inimitable fashion didn't discover the idea and, initially didn't recognize it, but once they saw it they realized that it was something they would be able to market. They came to our company and said they would like us to look into acquiring one, and suggested that we send some people back to New York to learn what it was all about. Yours truly and a mathematician, both very young, went back to New York in late 1949 or early 1950 where we spent three weeks. By the way, that is where I met Cuthbert Hurd. They had already built the SSEC, one of the great steps in the annals of computer development, about which much has been written. We learned what the Card Programmed Calculator would do, we learned to use it, we learned how to program it, and we even got lessons on how to plug up the machine. We came back and proceeded to wander around Continental Oil Company, trying to develop interest in getting one of these machines. We could not find enough enthusiasm in the corporation to even lease a machine, which was rather distressing to those of us in the research business because we were so positive that the computer had a great role in

Continental. You have to smile at this when you now jump forward twenty-five years. At that same site in that small town in Oklahoma, they had to build a complete building to house the large computation center which, I think, consists of three very large IBM systems and three Xerox systems.

MAPSTONE:

Ah, that is a lovely comparison. You couldn't sell them one CPC in 1950.

HORWITZ:

Of course, you could say that we didn't understand how to sell, and I think there is something to that, but you have to remember that it was back before anybody knew that a computer could be of any value. The UNIVAC was an enormous machine scaled to do very big things, and it was being marketed only to people with very big accounting problems. The little machines such as the CRC devices hadn't yet caught on, and were being used in very unique applications. Through that entire period of vision, if you will, on the part of research people who knew what the machinery might come to, no one really understood what the field would come to. I contend to this day that nobody had the vision to know what we were doing or what we were playing with. There is a reason for that which we can talk about it you like, but it doesn't relate to history. There was a very short period when the visionaries were proven to be correct because the applicability of the machinery got around very quickly. During that period Continental saw very limited use for the machine. In that same period I came out to the West Coast, and returned to Oklahoma where we proceeded to develop the machine. The primary motivations for this depended upon to whom you spoke. We knew the company needed to become involved with computers but we couldn't see a way to come through the front door. Being a research laboratory, we thought it made sense to go in the back door because, even if we didn't complete building our own machine, sooner or later the company would be acquiring machines and we would need to know how to use them and how they work, which we would gain for the company. Or we might build a useable machine and once the company used it they would want more. The latter is the way it worked out. I would say that there was an enormous amount of naivety on all our parts, mine and the management chain's. Management didn't know what the computer field was about. They didn't understand the complexity of the problem or realize what software was all about. However, only Pres Eckert and a few people in his gang really knew what computers were all about at that time. We started with Stan Frankel and me as the two key people and we had a very limited staff. I was joined by Laymond Wann, an electronics engineer whom I hired fresh out of Oklahoma State University, and who subsequently took over my responsibilities. A unique coincidence was that he had a particularly gifted wife who had her degree in mathematics--the two of them went to school together--and they both joined the program. He helped me to develop and design the machine. I concentrated primarily on the electronic networks and he concentrated on some of the peripheral elements that went into the machine. He carried on most of the other functions of the lab that I was supposed to be doing, while I was working almost exclusively on the machine. His wife ultimately became a combination (I'll use today's terms) applications engineer

and programmer. She interfaced with Stan Frankel on the programming, and Lamond and I interfaced with Stan on the logic design and on the implementation of the circuits. We had two technicians and a draftsman, and that was our little family. The machine shop next door took care of such things as physical structures, packaging and housing and they did the whole works. The interesting part about this story is that with the exception of myself, Mrs. Wann, and the draftsman, everyone else in that group is still at Continental Oil Company and in the same laboratory. Mrs. Wann has long since retired to raise a family, as did the draftsman who was a girl, but the technicians and Lamond are still there. It took us three or four years to build the machine.

MAPSTONE:

It took that long?

HORWITZ:

Oh, absolutely.

MAPSTONE:

While the machine was being built the whole technology changed.

HORWITZ:

Everything went by us while we were doing it. Although it really didn't go by us, because I happen to think we were ahead of most of the things and that they caught up with us. During the time we were developing our machine, and before we got it going, the LGP-30 was introduced, the ALWAC machine was brought into life, the CRC machines were getting bigger and bigger, about that time NCR bought CRC, and IBM announced, and we installed, 650. Those are the major ventures that I recall.

MAPSTONE:

What about the IBM 701?

HORWITZ:

I think the 701 probably came out during this time.

MAPSTONE:

Yes. I believe it was announced in 1952.

HORWITZ:

Later generations of the UNIVAC machine came into being and ERA was merged into

Remington Rand, with those great attendant battles, so the computer world grew up while we developed this machine. You looked so startled when I said it took three or four years to build CONAC that you have put me in almost a defensive posture. This was a group of two engineers, two technicians, and a draftsman that developed a complete system. They interfaced a Flexowriter for typewriter, created a read-write system for heads, had a magnetic drum built, packaged the machine in an attractive form and created a control network system. It was dumb to do it with that limited a staff, but we were in no hurry since we weren't going to market the machine.

MAPSTONE:

You were building a research machine for your own use and, I suppose, mostly for the development of knowledge.

HORWITZ:

Yes. As a matter of fact, we were a continual source of supply for speakers at the local electronic conventions, and we met with other oil companies who were very interested in what we were doing. It was a very worthwhile experience for the company and for everybody in the laboratory. Should we have done it? My guess is no, because we could have financed the kind of knowledge that the company gained with all of this hard work. Be that as it may, the machine saw the light of day in very useable form and it went into the research laboratories and was used quite productively for about four years or maybe more.

MAPSTONE:

Can you give an approximate date for when CONAC went into use?

HORWITZ:

I would guess it would be in about 1956.

MAPSTONE:

Earlier, you mentioned the work you did with diode quality. Were there other areas where you upgraded the technology or looked ahead for ways to improve it?

HORWITZ:

Well, I think the limited use of the vacuum tube active elements was certainly one of the things we accomplished in the creation of the machine. We also built an extremely limited number of parts, an extremely compact read-write selection matrix for the recording heads and we had a magnetic drum. I specified a drum system, went out on bid for it and Librascope won the contract. They lost a ton of money on that contract, and if I hadn't let them off the hook by accepting the drum, they couldn't have fulfilled the

contract. It turned out that I specified the drum tighter than they were capable of building it at the time and that is not a criticism of Librascope, because nobody had ever built a drum to the type of specs that I put on the machine. I didn't try intentionally to create that problem, but I tried to make the specifications tight to give as much leeway in the circuitry as possible. Today the types of specifications are fairly reasonable and, as a matter of fact, if you created a disc or drum today you would fall into the kind of specifications we created.

MAPSTONE:

But your specs were ahead of the state of the art?

HORWITZ:

Of drums. I guess at this point there is no reason not to say it; Librascope was the only company that bid on the drum. I sent the specs out to about fifteen corporations and Librascope's was the only bid. The others recognized that they couldn't do it, although I didn't recognize that until they all came in with no bids. If our motivation had been to see how cheaply we could buy a drum we would have specified to a lesser extent and gone back out to bid, but that wasn't what we were trying to do. We knew we had a research vehicle and whether I spent a few thousand dollars too much for a drum wasn't that significant, although, at the time, it seemed like an enormous price. As I recall we spent $12,500 for the drum and my guess is that if I had asked Librascope to build a second drum, they would have asked for $50,000. We had an eight thousand word memory on this machine which was indeed unique at that time. To this day one of our fond recollections is the way the eight thousand word memory came about. In a discussion between Stan, Betty and myself, we decided that a thousand-word memory probably would be sufficient. However, we weren't certain about that and Betty suggested that we should use two. If we thought there was a possibility that we might need two, then we should double it just to be safe. At this point we were about to go to a four-thousand word drum when Stan got to thinking about how much mechanism there was and the size of the rewrite matrix, and he said, "You know, for practically nothing extra we could make that an eight-thousand word memory." We had a great debate about how silly it would be to put eight thousand words of memory in a computing system. [Laughter.] You have to smile about that at the moment because OS/360 wouldn't fit into eight hundred thousand words, but that is neither here nor there since this was back in about 1953 or thereabouts. I ordered this eight thousand word memory with several hundred recording heads, and I don't recall now how many words there were to a track, but I could look it up. It did have a unique feature which I will come to. The several hundred recording heads were connected to the computer proper with a matrix that took sixteen vacuum tubes, as I recall, and a couple hundred diodes, and that was all. I remember that I first visited UNIVAC to explain the system to Pres Eckert because it was such a simple system and required so little circuitry. I also remember the difficulty I had expressing it to him, because with his vast experience, he was capable of asking questions that were more piercing than I was easily able to respond to. That is the sort of thing that happens in this business.

MAPSTONE:

What sort of reaction did you get from him, because he was into those huge machines and you were coming up with computing ability in this relatively small machine. Was there a meeting of the minds?

HORWITZ:

Oh, I think I would describe it as sort of a polite interest. I'm tempted to use the words amused interest. When I visited at Remington Rand and saw what Pres and his people were doing, it was the first time I had ever walked inside a UNIVAC system, and I use the term "inside" literally. Have you been close to a UNIVAC machine?

MAPSTONE:

No.

HORWITZ:

The UNIVAC's main frame would just about fit into this room wall-to-wall. It was a long, elliptical machine with circuits and tubes all around, and all the tubes were inside. You serviced it by going inside and, as I recall, it was an awesome feeling. It had a control console that must have had four or five hundred switches and lights on it and it struck me as an appalling beast. Here we were working at the low end of this field, and they were working at the high end. Pres was trying to develop what became the first of the high speed line printers, and I very quickly discerned that it was eight times more complex than the CONAC, so I didn't have any trouble understanding that he wouldn't be particularly interested in what we were doing, except as an exercise. He had known Stan Frankel very well and I had first met him when he was visiting Frankel in the early days. I was going to talk about one other innovation, a Frankel innovation for which he gets credit. There were four registers as today all machines have registers, except that in most machines today, you would use registers that would be solid state registers. The early machines would use long banks of vacuum tubes as registers, and Frankel's four registers were on the drum. He put two heads side by side, spaced precisely one word width apart, and you read from one back into the other, a technique long since abandoned by the computer people. It was a variation of the UNIVAC mercury registers where you spilled data out one end and put it back in again. Of course, this maximizes the opportunity of dropping a bit, and the instant you drop a bit, you have no way of catching it and it goes back into the register improperly. By the way, we made another little mistake: there was not a single parity check in the complete computer. Stan didn't believe in them.

MAPSTONE:

I take it that he thought about it.

HORWITZ:

Oh, sure, but that took extra parts. To tell you how far this world has come, we used to estimate at XDS that at least twenty five percent of a physical IBM system was non-computational equipment for service, maintenance, and insuring reliability. In the early days people did not understand why IBM built such big machines to do all this extra stuff that you didn't need. Once again they were smarter than the rest of us because they knew that with tens of thousands of these machines out on the market, servicing would be their biggest cost. It has long since become standard that the cost of hardware is not nearly as significant to you as the cost of servicing.

MAPSTONE:

Yes, they were way ahead of the small computer companies and the individual researchers in this area since they knew their market.

HORWITZ:

That's right. In any event, those circulating registers were very unique. We used forty-bit words; a thirty-two-bit data word, a six-bit address, and a couple of special codes. If we had used vacuum tubes for those four forty-bit registers they would have been bigger than a complete computer. We used the drum surface itself as a storage medium for the registers, so everything in the machine was paced to the speed of that drum. By the way, we could unplug the machine and it would compute until the drum slowed to the point that the velocity of the magnetic surface was no longer sufficient to induce enough in the heads, and they fell below the threshold of the recording system. Because everything came off the drum, there was nothing that went on in the computer that the drum didn't pace--the clock, registers, the whole works.

MAPSTONE:

Would it be like leaving the needle on a record as you turned off the power and it slowly chug, chug, chugs to a halt?

HORWITZ:

I will tell you what it was although we didn't know it at the time; we had devised a fail safe system. We have a power fail safe feature in every DS machine so that if the power goes off all the status words are transferred out of the active registers that are going to die and into core where they are retained. In this case, had we known what we were doing, we would have known that we had enough time to make a thousand transfers in the event of a power failure. We could have taken all the data out of the active registers and put it into main memory, and then we could have flashed into a complete diagnostic routine and easily set up a program that would allow the operator to turn the machine back on, or allow it to come on by itself. Now that is backward looking perspective because nobody dreamt of those sorts of things at that time, or if they did they didn't know how to do it,

being stuck off in Oklahoma. That use of the registers was one on the innovative features of the machine. The next innovation may not have been an innovation at all, although it seemed one to us and my impression is that it was. However, I suspect that by this time it was going on in the rest of the computing world. Frankel created the first assembler and translator for our machine. It wasn't a compiler. He recognized that you couldn't get information in and out of a binary machine and he wanted to have more than a simple conversion system from binary to decimal and decimal to binary, which everybody was doing at the time. He created a language that would enable the operators to use conventional words, such as ADD for the add instructions, with the computer software package doing the interpreting the way things are done today. I will forever remember, and I will be glad to put it into historical perspective, the man who headed the mathematical analysis group, whose name I won't mention because he's now respected in another part of the world, screamed at the top of his voice at Stan Frankel: "It is absolutely stupid to ever put a program into a machine by going through another program".

MAPSTONE:

I suspect that he has lived to regret those words.

HORWITZ:

Yes, I suspect so. [Laughter.] It is fascinating when you think that today you can't get into a machine without going through about four levels. Well, that was the state of our knowledge at the time. We were babes in the wood in this field, but I suppose that is the price you pay for being somewhat pioneering. It was all enormous fun, very educational, and most rewarding because we saw our machine go into active use. It was used by the geophysical and mathematical analysis people for quite some time. The IBM 650 that the company acquired was used for accounting purposes and it was installed in another building where it was very hard for the technical people to get at it or on it. Then the geophysical use began to build up and they bought the Bendix machine.

MAPSTONE:

The G-15?

HORWITZ:

That sounds about right. It was the early little Bendix machine.

MAPSTONE:

I think that G-15 was Harry Huskey's machine.

HORWITZ:

It was a fairly successful machine.

MAPSTONE:

Huskey sold the idea for the G-15 to Bendix and it was, I guess, their first really successful general purpose computer.

HORWITZ:

That sounds right. One of the nice things about historical hindsight is that you don't care about confessing your errors unless you are a pompous person.

MAPSTONE:

It is like therapy--catharsis. [Laughter.]

HORWITZ:

Our big mistake was not telling Stan that now that he had proved his point by building a machine with a minimum number of parts, we were going to build a model that would work and be maintainable, and we should have walked off and refused to continue the discussions. However, in an effort to minimize the number of parts we took every flip-flop and every power providing element and balanced them precisely to maximize the amount of equipment we could hang on the machine. We greatly reduced the margins on which the tubes could function, and one identical circuit that would plug into one part of the machine would not necessarily function in another part. Consequently, we added great dimensions of unreliability that we never, ever should have done, just to use a few less tubes. I'd guess with fifteen more tubes I could have doubled the mean time to failure of the machine.

MAPSTONE:

Now that is a comment. I take it that Stan had quite a bit of control over the ultimate decisions on the machine.

HORWITZ:

Well, it was an intellectual control. I had a great deal of respect for Stan and we were working very closely. However, he was a consultant to Continental and if I really wanted to do something, there was nothing he could do about it and I'd simply go ahead. I put in some power boosting elements later on that he didn't like at all, and which he felt were unnecessary, but I just went ahead and did it. Later in the program when we were close to finishing up, I found a very serious error in the logical structure of the machine, which would have required an extensive modification of the logic network to correct it his way. I wasn't about to do it and, instead, I put in a special set of parts to solve the problem and had them all checked out. Stan was very upset since we had found a way to fix it that

wasn't intellectually satisfactory. Stan's control was more out of our respect for him and his ability to argue persuasively. Sometimes he won those arguments and sometimes he lost. In any event, we were talking about the Bendix computer coming into the company.

MAPSTONE:

You started to say you were into a philosophical moment.

HORWITZ:

Oh, yes, that is right. I'm not positive of the accuracy of this statement, but I think it is correct to say that none of the pioneer corporations that started out building little machines survived. The corporations may have, but the computer ventures did not, i.e., Librascope, Bendix, the J.B. Rea machine, which was a fairly small machine, or the CRC group. All of these people who foresaw that there were a lot of uses for little machines with a limited investment on the part of the users had the right idea, but for some reason they were not able to capitalize and become major suppliers of the machinery except for a limited period of time.

MAPSTONE:

Can you take the argument further and see why this happened?

HORWITZ:

I would guess, off the top of my head, that it was primarily because these machines were created by engineering people who didn't understand marketing. The machines were bought in large numbers at modest costs because they were attractive to engineering people, but, after a while, the corporations that were doing more sophisticated marketing took them over.

Of course, not all big machine companies survived either. ERA developed a very big line of machines that ultimately became the 400 and the 1108. When I knew them it was the 1101, which was a very big machine, and by the time they got to the 1103, they were acquired by UNIVAC. UNIVAC itself entered the machinery business from the top end and came down. IBM started with a little machine, if you want to call the 650 a little machine, though actually, it is a pretty good-sized machine, but they are the exception to every rule in the computing business. The fact that they exist in their form indicates they did what they did better. I think another problem was that the engineering teams that put companies together didn't have the sophistication to manage either the marketing or the financial resources at the time. I don't mean to be insulting to the management of CRC, say, because I don't know anything about their abilities as managers, but the fact remains that they should be a major corporation today. It is very sad when you think that the CRC group formed a company and built a great little machine--another not maintainable machine, by the way, which was probably its major failing--but they couldn't survive that to be an enormous supplier of computers today.

MAPSTONE:

That's right. One suspects that if they had survived, or if Northrop had hung on, they could have been a real competitor to IBM; there are so many ifs.

HORWITZ:

Conceivably. Bendix was a corporation that had the resources and the management elsewhere in the company and should have been able to do better since it got a good start. Most of these companies failed because they started to build bigger machines and they got into trouble with them. The successor to the LGP-30, a small machine, was a very big one which was a failure that wiped them out. Packard Bell, Max Palevsky's predecessor, started out with the PB-250, a very successful little machine which turned out to have severe problems. Several hundred of them were sold but never could make it.

MAPSTONE:

What happened? Did they just disappear into the gray?

HORWITZ:

I'm sure Max must have told you the story.

MAPSTONE:

I haven't seen him yet.

HORWITZ:

You will have to get the evolution of SDS from Max; it would be very presumptuous on my part. All that is well known is that the PB-250 lost a lot of money because it had serious design problems, as I understand it. Max and Bob Beck had created in their minds, and maybe on paper, a complete new line of machines, but Bob Bell, the President of Packard Bell, wouldn't finance it. For whatever reason, they left and formed SDS, and the rest is history. That was a company that started with little machines and did very, very well, but by this time, we are out of your period because SDS was formed in 1960 and 1961. I'm really talking about the period eight or ten years earlier when these product lines didn't succeed.

MAPSTONE:

When you came back to Continental from Cal Tech, how did you sell management on building this machine? Did you and Frankel sell it to them as a research tool?

HORWITZ:

Yes, we sold it as a research program, and the corporate management didn't seem to object. I recall putting on two or three different program reviews for the assembled corporate staff of Continental during the life of the program and if they ever said, "you shouldn't be doing this," they never said it in my presence. I really think they were caught up a little in the glamour of their organization developing a machine, and it wasn't an enormous investment. You couldn't develop a minicomputer today for several times the amount of money that went into this machine.

MAPSTONE:

Do you remember the total approximate cost?

HORWITZ:

No, it was too long ago.

MAPSTONE:

You talked earlier about an assembler that Stan had developed and we talked about how the machine was used by the company for their own research work. Was it Stan's program that you used or did you get into developing your own program languages?

HORWITZ:

I left the company shortly after we put it into use and our technicians were maintaining it. My recollection is that Betty Wann and possibly, by that time, another girl or two did most of the programming.

MAPSTONE:

Was it an easy machine to program?

HORWITZ:

Of course not. An easy machine to program is a pure binary machine. Back in the days when people didn't have assemblers, you used to sit there and write the silly things in binary. Now I think that Stan solved that problem. He stopped that nonsense. The early checkout of the machine and everything we did with it was written in binary, but we quickly quit that. I even remember that there used to be men wandering around the world in those days who prided themselves on how far up the numerical scale they could read binary. Computer experts used to trade on that as if it were something of value.

MAPSTONE:

Von Neumann never did his programming in anything but machine level language.

Before we move on, can you think of anything else that pertains to CONAC that we have not yet discussed?

HORWITZ:

We gave it the name not only because it was the great days of the acronym, but also it was the days of the AC at the end. So we created the name, "Continental Automatic Computer," which was in vogue at that time and which strikes me as remarkably lacking in imagination. We called her "Connie" for short and she was affectionately known all over Continental Oil Company as "Connie". A very stylish brown cabinet was designed for "Connie". The door handles formed the Continental triangular symbol and there were all sorts of similar touches.

MAPSTONE:

Stan calls her "COGNAC".

HORWITZ:

Yes, Stan would. [Laughter.]

MAPSTONE:

Where did you go from Continental?

HORWITZ:

To Beckman Instruments, where I went into the systems business and I became the Chief Engineer of their Systems Division. A few years later I became General Manager, and from thereon I left engineering completely as, unfortunately, engineers are sometimes wont to do. I spent the next several years in the scientific instruments, process instruments, and systems business. About six years after leaving Continental we found ourselves building systems, and lo and behold, we were using computers again. Suddenly I was back in the computer business. We developed one at Beckman Instruments that was a mistake in colossal proportion at this point. The engineering team that developed the machine over designed it, the research program got out of control, it wasn't the machine we had asked for, and, as a result, it was too much machine for the price and wasn't commercially viable. We purchased most of the machines that we used in our systems and once again, I ended back in history by purchasing Control Data's third and fourth 160 computer, a small machine which they introduced at about the same time as their 1604. We designed a big system to interface with the 1604 before they had delivered the second machine, and we were working with the first 1604, which was located down here at the Monterey Naval Training Center. I made a trip East to meet with the President of Digital Equipment Corporation, a little company that used to build electronic circuit boards. Their first computer was the PDP-1, and we bought the second and third machines that they ever built. I remember sitting down and explaining to the

president what a governmental priority was all about, since he had never even heard of priority systems. The reason that Beckman bought all these pioneer machines was because we were under contract to develop systems for NASA or the Intelligence communities or the Air Force and the Ballistic Missiles Command, and these people were always at the front end of computer technology. They bought all of the new machines that looked like they had promise, and since we were under contract to them, it was very common for us at Beckman to be getting some of these machines. Max Palevsky had been interested in my joining his company, so I left Beckman in 1967 and joined SDS.

MAPSTONE:

Were there any patents involving CONAC?

HORWITZ:

Do you mean patents on CONAC or patents on other machines?

MAPSTONE:

On CONAC.

HORWITZ:

I don't think we patented anything on CONAC, we just infringed. We didn't patent. I'm not sure I should let you say that; twenty years later you can say that.

MAPSTONE:

[Laughter.] Well, it's fine because you were one of a type anyway. I believe infringement would have been a problem only if you had commercialized it.

HORWITZ:

Not really.

MAPSTONE:

You mean you can't even build a machine in your basement?

HORWITZ:

You can if you can get away with it, but the inventor is protected against anybody using his invention, no matter what the purpose. I think you could argue very intelligently that we used the machine for commercial purposes. However, the question is what did we infringe? You have to remember that the UNIVAC patent hadn't been issued so the major infringements probably involved logical networks and structures. The UNIVAC

patent was finally issued and it is a fascinating document. Have you seen it?

MAPSTONE:

No, I haven't.

HORWITZ:

If you interpret the UNIVAC I patent literally, it covers a wide multitude of things. It turns out that the entire electronic counter and timer industry and test instruments are covered in the UNIVAC patent, because Pres Eckert and John Mauchly had put these functions in the machine. I've lost the patent situation. We did not patent the machine although we had any discussions on the subject with the patent attorneys. Our view was that we were doing state of the art engineering that were not patentable features. They may have pursued some patents after I left, but I certainly didn't pursue them very aggressively on the machine.

MAPSTONE:

Your comment that you were doing state of the art technology and not patentable advanced technology is probably the key.

HORWITZ:

As a matter of fact, if anybody had the right to patent it would have been Cal Tech since they financed all of the original work.

MAPSTONE:

That's right, because when Cal Tech and Frankel made the arrangement with Librascope there was some kind of situation where, if Librascope wanted to patent, then there would have to be some financial arrangement. They never picked up the patents, Stan didn't patent and Cal Tech didn't patent so the whole thing was left wide open.

HORWITZ:

I recall that when the UNIVAC patent was issued, Sperry Rand set up a separate corporation attempting to capitalize on their patent position. I really don't know how that ever came out since I just quit paying any attention to it, but they were no longer in the development end. I do know that there were memory patents that were processed aggressively, particularly by RCA and by MIT. MIT very aggressively pursued memory patents as a result of the Whirlwind system.

MAPSTONE:

Right, the core memory.

HORWITZ:

Of course, Rajchman's early work at RCA produced many of the basic patents in core memory, but I believe that the early and truly basic patents came out of MIT. Even at Beckman we had grief with the MIT people because we had purchased core memories to install in our systems. The MIT people came around and wanted a complete list of every memory we had bought and they tried to negotiate with us on a per bit basis. We just sent them to talk to the manufacturers of the memories. I don't know whether Beckman ever paid anything, but the computer industry is one mass of infringement.

MAPSTONE:

That is why the courts are so busy with the computer people. How about talking about some of the people who you feel have had influence on the industry?

HORWITZ:

Well, I mentioned a goodly number of them over the telephone. There is Paul Morton at the University of California at Berkeley; you mentioned Harry Huskey, and you already know the CRC group. We talked briefly over the telephone about Hagen and ALWAC. There was an outfit out here that tried to build a computer company that fell apart and collapsed, and I thought it was Ney, not J.B. Rea.

MAPSTONE:

Was that the one you mentioned to me on the telephone?

HORWITZ:

Yes, but I probably did say Rea at the time. I've made that mistake before. Then there is Robbie Robinson, who was one of the forerunners of the Burroughs System back in the ElectroData days. You should definitely chase down Robinson, because he was one of the early, early leaders in the computer field. By the way, the ElectroData Corporation didn't last, and they started with a very big machine.

MAPSTONE:

Yes, they approached it quite differently from the Stan Frankel or CRC Boolean logic approach. They built an eastern oriented decimal machine that was designed by Ernst Selmer.

HORWITZ:

I'm trying to think of the name of the man who for years headed the Bureau of Standards on the East Coast. I think he may have died, but he was one of the major names in the

field.

MAPSTONE:

Sam Alexander.

HORWITZ:

Thats it, Sam Alexander; he's the one.

MAPSTONE:

Yes, he was in it all the way.

HORWITZ:

I think what you need to do is to look under some of these big name rocks because while we think of people like Harry Huskey or Morton, I suspect that there were a lot of people who were responsible for the innovative work that went on in those labs. We have an unfortunate tendency to think in terms of people running laboratories and programs, instead of the people who are doing the work and the creating.

MAPSTONE:

That's right. I've tried to start with the figurehead because he is the one who can point me, and then, from that point, I branch out.

HORWITZ:

Who put the Card Programmed Calculator together for Northrop? Those people are real pioneers, and the rest of us are amateurs.

MAPSTONE:

Woodbury and Toben.

HORWITZ:

Then there are the people who created the SSEC for IBM, and, of course, you have the gang at Harvard with the Mark series.

MAPSTONE:

Oh, yes, we have to talk to more than one person.

HORWITZ:

Did you ever chase down Gene Amdahl?

MAPSTONE:

No, but I will. Earlier, we mentioned peripheral equipment, and, I think, it would be a good idea to discuss some of the highlights in the peripheral equipment advances.

HORWITZ:

Unfortunately I can't be very people oriented here because I have a complete lack of knowledge as to the genius behind the peripheral equipment.

MAPSTONE:

Okay, but at least we can talk about the equipment and the technology.

HORWITZ:

As I have commented to you before, the peripheral equipment line has been a combination of functional requirement and economic compromise. I have a heretic view of peripheral equipment, because I think that it is a disaster, but I believe that fairly soon, enough money will start being forwarded to this area to solve this problem. For instance, the early tape transport was one of the major technological contributions to computation. It solved a problem and it was a very nifty economical compromise. Originally it was second level storage because the drum and disc active memories were not large enough to store all the data. When core memories came in, drums became secondary and tapes tertiary. Today tapes are about fifth level and have only one reason for existence, they are cheap per bit. I have said in the past that the tape transport, with that moving thin strip of Mylar that you pack data on at a few thousand bits per inch, is a Rube Goldberg mechanism that nobody in their wildest imagination would have ever thought up. It is a clumsy, expensive device to produce but it does the job and it filled an economic need. My other candidate for that category is the moving head file. It was a superb piece of engineering, for which IBM gets all the credit. The first random access memory, originally called the RAM, was developed and built at San Jose. This system, called RAMAC, was the forerunner of the moving head file. It is an engineering marvel, but an absolutely awkward way in this day of solid state technology to do what they are doing. I'm not saying that it shouldn't be done because it is an inexpensive way of doing what they are doing.

However, in the last few years, we have seen two very major changes come into the world of technology that are going to have a dramatic effect on the whole world of computation. One is the solid state memory, which is just beginning, and the other is real very large scale integration in circuitry. Ten years from today when you are interviewing somebody, active memory cost per bit will be one and a half to two orders of magnitude cheaper than they are now, and electronic circuits will be another order of magnitude

cheaper or more. What that says is that a very, very powerful machine in computational ability will cost one to two thousand dollars. All of the cost, by today's terms, will be in software and peripheral equipment. We are going to get to the point rapidly where IBM and other firms will not be putting their money into building cheaper main frames, they will be putting it all into software and peripherals. My great hope is that, at long last, a sufficient concentration of technological energies will be applied against those peripherals to get rid of these miserable mechanical devices. You gather I don't like mechanisms. I believe that with fully electronic systems, we can have a mean free time to failure that exceeds the life of the machinery i.e., that they will run for tens of thousands of hours. Maybe that is a little ambitious, but we are getting there. However, before we do, we have to get rid of bearings, sliding frictional devices, air lifted heads, whirling platters, all these creative things that people have developed because they represented an economical compromise. Otherwise you can buy a fairly small box that is the equivalent of a super computer, and surround it with big expensive things, and people are going to rebel. They are already rebelling to a certain extent.

MAPSTONE:

The little box will sit there idle half its time because of the mechanical nature of some of the peripheral equipment.

HORWITZ:

Well, I don't think that will happen, because, long before that, there will be too many people on the machine in real time from remote terminals and communications, or else the machines will be multi-purpose machines. These particular questions used to plague me a great deal and we used to spend a lot of time talking and thinking about them. Obviously the computer of the future is going to be mostly software; the more powerful that box becomes, the more difficult it is to effectively use it.

Look at the minicomputer field and its explosive growth. It relates to a comment I made very early in this discussion about the visionaries. It wasn't that nobody had any vision, they just didn't understand what the computer would become. Computer applicability spread as the cost came down. It turned out that there was not an enormous market for UNIVAC I because for what it did, it was a very expensive machine. It should have been because it was the first of its kind, a pioneer, but nobody knew what it was about. As these machines became more powerful and did more things for less cost, there were more and more uses for them. Follow the mini-machine and just think about the uses you are going to have for it when you can buy it for $50.00, and you'll be able to do that. I remember when we couldn't figure out how to build an electronic calculator. Stan Frankel and I used to talk about the electronic calculator in 1950. The Clary Multiplier Corporation tried to develop one, but it didn't have the technology to be cost competitive. The mechanical calculator sold for about $750 and cost about $250. You could not put $250 worth of electronic parts together to do that job. Technology caught up and now you buy a calculator for $100. Three years from now you will buy that calculator for $10 and you will buy a small computer for $50 to $60. When you do that, that computer is

going to be used in a million ways. Did you ever look at the schematic on your stereo system? If you translate the schematic on one of these units to vacuum tubes, you would fill half this room. It is amazing how much machinery is in it. I don't have any trouble believing that at some point in time people will think of a way to bury a computer inside a stereo system. It will be a special purpose machine that will make frequency adjustments better than by analog. It will keep you on track, it will produce filtering for you, and it will reconstruct lost sounds and all sorts of things. When all of this happens we have got to have a different form of peripheral equipment. You asked about peripheral equipment and you didn't know what kind of a door you were opening. Sorry about that.

MAPSTONE:

Don't be sorry, because this in one of the great things about this kind of an interview.

HORWITZ:

I am obviously a great believer in the remote terminal. I just think that that is an enormous step in the right direction. There are two or three peripheral devices which, when they become available, will open up a whole new world of computing. The most critically needed peripheral device, in my view, is the response unit. I will forget the economics, although IBM might argue with what I am about to say, since they want a lower cost tape unit because they sell a few million tape units. I am referring to the voice response unit that will understand what you say. When that piece of technology is at hand, you bring into being, for the first time, a range of applications that we have desired since the beginning of computers. Let me talk about the education of children. Computer assisted instruction has been a failure for a lot of reasons, one of which is the social problem, the need for children to interface with people and not machines. Another problem is obviously economic, but that is going to be solved in time. I'm not a teacher, nor knowledgeable of the education process, but I am convinced you will never use a machine to teach a child in mass form until that child can communicate with that machine in his own natural form. You cannot take a grade school child and teach that child to use a keyboard or light pen. However, when that child can ask that machine a question in his childish voice and with his bad English, and that machine will respond to him, either vocally or visually, you will revolutionize teaching, but not until then. I'm certain that all this money that has been spent has been productive and that we have built the base. The management information system, the idea of a manager using a computer, is nonsense for only one reason: you cannot get an executive to punch a keyboard. You can get him to talk into a telephone and when he can pick up a telephone and ask a machine for last month's sales distribution from the Los Angeles office divided by his four major product lines, and compared to New York's, then you are going to have a management information system. That is why I happen to believe in new devices. Another obviously required peripheral is an OCR system, an optical character reader system that does not read OCR fonts, but reads the typewritten page. I don't ask for it to read freehand scribbles on the page or handwriting or strange characters, but I ask for a machine that will read pica or elite or executive type, because this will again be a major step in expanding the use of machines.

MAPSTONE:

What really has to happen in a way, is that the machine, through the peripheral equipment, is going to have to simulate the human being in its ability to hear and see.

HORWITZ:

I never like to get into that particular phase of the discussion, because so many people misinterpret.

MAPSTONE:

I'm not misinterpreting and I'm not thinking of automata, but I am thinking of the simulation of those abilities.

HORWITZ:

In a way that is true. But let's not forget that we have got 50,000 large machines in service, and I have lost count of the little ones--probably 25,000 or 30,000 little ones in this country alone--so a lot has been done without simulating human beings. But in a way, you are correct. I decided a long time ago that we are no longer predicting the future, we are talking about the inevitable. You do not have to be a visionary anymore to extrapolate what computers are going to do. I have at long last become a believer that those early pioneers who said that the computer would have the same impact on our society as the industrial revolution were probably quite correct. That is why your work is probably worthwhile and not just another extension of the industrial revolution.

MAPSTONE:

That's right, it is its own revolution. A man like Frankel was actually a pure visionary because he saw the minicomputer twenty years ago and he envisioned machines in the schools and homes. Sure it wasn't ready to happen, but he was a visionary, and I think there were many others.

HORWITZ:

Yes, there were.

MAPSTONE:

Who would you say were some of the other visionaries?

HORWITZ:

Unfortunately, I didn't know them that well. I wasn't that deeply involved to be able to

give you a really intelligent answer to the question. I suspect they weren't in the field of computation itself, and you would probably have to go back and do some literary research and reading. It is easy to expound on beliefs in your own living room or an office as I was just doing. It's different when somebody can write a paper on that subject, have it discussed, debated and accepted. You see, the things I am talking about today are not visionary, and that is my point. They were twenty years ago. It is really a very exciting thing to think about what can and probably will happen. Now we have another hurdle that we have to get over sooner or later. Having debated this at great length and having watched the vast amounts of money expended, I have long since come to the belief, which I have expressed to many a university professor or head of department or even president, that the major contribution the university can make today in computation and computer science is to solve the software problem for us. The developers of machinery have vastly outrun our ability to program these machines, and we will soon strangle on our own inability to either find sufficient people or to utilize them sufficiently. One of the reasons for IBM's lead is that we don't have enough and we can't find enough people. One of my favorite examples of the argument is the IBM 67, their large time sharing machine, which was one of the few failures they ever had. It would have competed with the SDS 940, a very successful time sharing machine. To support it economically would have required something like 300 or 400 terminals. When the software package was written they could never get more than fifty terminals on it. It is very difficult to write a software package that will handle over 100 terminals, believe me, and people talk glowingly about it. I don't know if anybody has done it yet for 200 terminals. At IBM, all the king's horses and all the king's men couldn't put the 67 together again. The task of rewriting that software was so enormous that IBM just walked away from it, and, as I recall, wrote a software package to operate under OS 360, and then they came out with the time sharing 360. They abandoned the 67 because, even with their resources, they couldn't start over again and rewrite that software in any reasonable time. It's absurd, in this day of technology, that we have let ourselves get into this enormous imbalance. For computers to realize their full potential, the things I'm talking about are going to have to happen.

MAPSTONE:

What seems to have happened is that there was a ten year gap between the beginning of the stored program electronic computer and methods of using it, and that gap is probably still ten years or more.

HORWITZ:

I'm inclined to believe that the gap is not that big. I think that it is narrower, and that it will narrow under the impact of necessity.

MAPSTONE:

It has to; otherwise the computer will become unusable.

HORWITZ:

They'll saturate and then stop. They won't build anymore because you can't use them, and that's obviously not true. Here's a case in point: Years ago, an IBM executive made a statement to the effect that if we do not learn to design more reliable machines, we will never be able to find enough service engineers to keep them running. Now that was a very practical and true statement, but they did not give it lip service, they went to work designing machines that would run more reliably, and they were a variation of the telephone company standards. Have you heard their famous expression: "If you still had women at switchboards, you'd employ every woman over seventeen in the United States"? The computer is similar to that. Necessity is the mother of invention, but I don't think we should leave it to the vested interests of the corporations, even the big ones, to solve a problem that is almost fundamental in nature.

[End of Tape]