INTERVIEW WITH JAY W. FORRESTER

INTERVIEWER JAMIE PEARSON, DIGITAL

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INT: Perhaps you could describe for me the environment at MIT when you were leading the Whirlwind engineering project there.

JF: The MIT environment at the time of the Whirlwind computer had, of course, drawn out of the research laboratories of World War II. These had been rather free-wheeling operations in which the laboratories had a vision of what they could do. They had considerable freedom to carry out that vision. I would say that more than you found in other institutions, and more than you find at MIT now, it was a free enterprise society in which anyone could do about anything they wanted to do as long as it was honorable and they could raise the money for it.

INT: [LAUGHS] Fair enough.

JF: I had started this under Gordon Brown who figures very heavily in everything that I've done. Joining with him in 1940, I believe to be one of the four founders of the Servomechanisms Laboratory which, during World War II, developed feedback control systems for military equipment.

INT: Where were you prior to 1940?

JF: I graduated from the University of Nebraska in 1939. I came to MIT for one year of graduate study and have not gotten away yet.

INT: A professional student. Excellent.

JF: A lot of the atmosphere at that time was in an environment of national emergency. Beginning to develop at a time when there was little or no bureacracy and very little sponsored research at MIT. And where the research laboratories came under the general guidance of a man named Nathaniel Sage, Senior. [There's been a] Nathaniel Sage, Junior since, but not Nathaniel Sage, Senior. To illustrate the lack of bureaucratic organization, when I was a research assistant just out of college beginning work in the Servomechanisms Laboratory, if I wanted to buy something for our work I would call up the supplier, order it, take an MIT purchase order out of my desk, write it up and mail it to them. And whatever was needed would arrive promptly. It was also the beginning of classified military work for which MIT had no guard system, so I had a Cambridge police badge and a pistol permit, and doubled as guard. I still have my Cambridge police badge somewhere. [BOTH LAUGH]

The point of this is that we had experience in every aspect of what went on from the underlying theory to the execution. And since we were developing equipment very unprecedented for the military, there of course were problems all along the way in manufacturing and out in the field, and the people who did the development would follow into the factories in Pittsburgh and places, and from time to time out in the military field to see what was going on or to find out why some problem was arising. In 1943 there were technical electronic problems on the Carrier Lexington, which had a piece of equipment on it that we had developed at the Servomechanisms Laboratory. They wanted to know who would like to go out and fix it and I volunteered for that, went to Pearl Harbor to work on it for a couple of weeks, wasn't quite finished when the executive officer came around and said would I like to go with them and finish while they went to sea. So I said yes, and that turned out to be the invasion of ________ where the Carrier stood offshore during that invasion, and then afterward it made a turn up to the north and came right down through the middle of the Marianas with the Japanese air bases on each side which they were bombing, and which the Japanese did not think very much of, so they kept sending in torpedo planes. And ended up, in fact, hitting the Lexington at about 11:00 that night as it was beginning to pull away from the island. It cut off one of the four propellers and jammed the rudder, but it was able to come back to Pearl Harbor in sort of a wallowing, slightly uncontrolled way. [Laughs] But the importance of that was that we had a group of people: Bob Everett was one of those throughout that period who had been together as a team, and had carried several projects through from theory to actually working, and being there if they didn't work. So, when we came to the computer program, we had several people who understood each other. Understood each others strengths and weaknesses, had learned to work together, and who had had this joint experience that drove home absolutely to them that Murphy was right, Murphy's law. Anything that can go wrong, will. We knew that time after time. In many ways, that overriding belief was part of the atmosphere of the computer work that led to, for example, the high reliability of the SAGE air defense system which was the successor to the Whirlwind computer. Those centers were forced to raise high, had 60,000 or more vacuum tubes in them. Do you have the statistics on the reliability of those centers?

INT: Yes, somewhere. It's 99.9, isn't it?

JF: Well, 99.8 percent uptime for those centers. If you ask people these days what they would expect, their estimates tend to run from five percent to twenty-five percent. They cannot imagine, even in today's technology, that something would run 99.8 percent of the year. But that was primarily because we intended to make a reliable system and we dug to the bottom of anything that seemed to stand in the way of reliability, and in general, just remove the causes of unreliability.

Going back, quite a bit of the atmosphere of the Servo Lab and the Digital Computer Laboratory, I attribute to Nat Sage, and of course even more so to Gordon Brown. Nat Sage is a rather unusual person. I think he'd been trained in civil engineering. He had grown up as a son of an army officer and therefore had lived in army camps around the world, and somehow had come to develop a very keen judgement of people, and a very considerable confidence in his own judgement. So at MIT there were people that he would trust almost anything under any circumstances. Gordon Brown and Charles Draper were in that category, and I believe I was. And then there were others that he wouldn't trust any further than he could _______ somebody's office doors.

INT: Charles Draper, as in Draper Laboratories, you were students together?

JF: No, he was older than I was. He was a faculty in the aeronautics department while I was a research assistant in electrical engineering. But we had some interaction, because we brought some of our feedback control systems in to be a link between his gyro computing sites in some of the gun mounts (?)

INT: Did you know at the time that you were surrounded by a bunch of great men?

JF: I suppose we just assumed the world was like that.

INT: Or that you were on the cutting edge of the discovery of...

JF: Well we knew that because, certainly, because one had to battle the conservative opposition to the new ideas, the continuous ongoing process because if you were developing new and different ideas, they are of course unfamiliar to other people. They don't trust them, and a great deal of convincing is necessary unless they are already in a state of desperation and know that what they're doing isn't going to work. And in some ways that was the environmnent for the introduction of computers into the air defense system. One could never have done it a few years earlier when they thought the manual systems were adequate. But then they had already arrived at the knowledge that what they had wasn't going to work, then they had to do something else.

INT: And the need was driving the technology.

JF: And then, the even very radical ideas had a much better chance. And I think as of the late '40s, putting digital computers in to analyze radar data and run fighter planes was very radical at a time when no highspeed general purpose computer had yet operated.

You see it was a battle through the late '40s to get enough money to carry on the research. On Whirlwind, we spent huge extravagant amounts of money in the eyes of our critics. The whole Whirlwind development, entirely new ideas about electronic circuits, magnetic random access coincident current magnetic memory, the building of the Whirlwind computer, the use of it to demonstrate aircraft interceptions, totalled about four and a half million dollars which is less than the production price of a lot of computers today. This was at a time when a lot of people thought you should build a computer for $100,000 or less. And of course that was out of scale with anything that had been done at the time. The whole radar development at MIT, radar being a relatively simpler undertaking, had of course, cost many fold more than that.

INT: What kind of a management team did you and Bob Everett make? Were your styles different?

JF: Bob Everett and I were probably, I would say, the most compatible pair of leaders that you're likely to find. We had a deep respect for one another. I sometimes describe Bob Everett as the only person I know where if we differ, I will automatically give him more than fifty percent odds at being right.

INT: You continue to spend time together; you just got the presidential medal.

JF: Yes, and we have lunch together a couple of times a year. I think we were very good complements to one another. Equally good technically. Everett was more low-key than I was and probably much easier for the staff to deal with. And I'm probably more abrupt. So he would help to smooth out things that I would sometimes stir up. I tended to focus somewhat more on the outside politics and the outside selling, if you like. Promotion of the new ideas. He'd focus somewhat more on the inside and getting things done. But by no means an exclusive division, just a little bit of emphasis in that regard. But we worked together I think very well.

The atmosphere of the laboratory was much like the atmosphere of the outer MIT environment that I started to mention with regard to Nat Sage. It was an atmosphere in which bad news flowed uphill. In a lot of organizations, people are afraid to take up difficulties, failures, shortcomings, mistakes, problems with superiors for fear of some kind of retribution. But the environment at MIT was more that the higher administration were there to help if there was a problem. And otherwise they didn't interfere. There was very little that had to be asked of them in the way of permissions. We would simply go ahead and do our job. Also, there was a very definite hierarchy-skipping system. By that I mean that if somebody wanted to know what was going on, you might go right down to the person doing it even though there might be two or three levels in between. If some lower level person had a problem and took it two levels above his immediate supervisor, that was considered normal and reasonable.

INT: It seems this attitude prevails at Digital still to this day.

JF: It could be part of the legacy because it had been pretty well established. And the founders of Digital, quite a number of them, had been through that particular environment. Another part of that environment that I understand was important in the launching of Digital was our rather meticulous time and cost estimating that we did. We had charts showing who would do what, when, when things would come together, timescale, what it would cost, and these would run out one or two years in advance, quite unusual for a research or academic laboratory. I was told without being present or having any firsthand information that when Ken Olsen and [Harlan] Anderson went to AR&D with a plan for the company they followed essentially the same process and structure. And the ARD people said it was the most carefully prepared plan that had ever come in. That again was part of this background of administrative orderliness at the same time that there was a freewheeling technology.

INT: And very careful documentation.

JF: Very careful documentation all the way through. It's the best documented project there is certainly in the early days of computers. And maybe anywhere in those documents still exist. And they were at many levels. There were internal memoranda on essentially any subject of importance. There were biweekly internal reports from everyone, every person wrote up one or a few paragraphs on alternate Fridays. These were simply put together, reproduced as they came in and then passed back to everyone. So that everyone had a document each two weeks telling what everyone else was doing, where they stood, what they had accomplished. That was one of the internal communications processes. Then for a few years we prepared rather formal quarterly reports: glossy paper, halftone pictures, for the outside world. Two or three hundred of those each time were distributed among military. For the most part I think they were classified. They went to a fairly broad section of people interested in computers. Two or three hundred copies would pretty well cover the interest in computers at that time.

INT: Did you know that Digital had biweekly reports from the day that they were founded?

JF: No.

INT: I was going through some papers in our archives or records group. And there they were. and styles that carried through. Because there they were. And you get a sense of the engineering perspective, the sales perspective, the public relations and so on.

JF: Well, we had that running for a long time in the computer lab as a means of helping to hold it together and keep it focused. It was, I think, a very high efficiency development. I remember as we were beginning to get into the SAGE air defense system and had not yet finally settled on who would build the computers, Tom Watson, Jr. and maybe eight or so of the top officers of IBM came up to spend a couple of days with us to see what we were doing. After they had been there all of the first day and we were on our way to dinner that evening, Tom Watson turned to me and said, "How do you accomplish so much with so few people?" It was a different scale of ratio to accomplishment than you will usually find in the industrial settings. I think this was primarily because of the clarity of the vision of what we were doing, and a practice of setting down very early all the things that had to be done so you knew exactly what was important and worth doing. So there was very little done that did not converge into the final result, and that's in contrast to some studies that have been made where, well, one study many years ago looking at what fraction of the number of projects that go into the research lab come out as successful products in the market. That study showed that the ratio ranged from one success out of two hundred starts, to one out of two. Whereas at the the time that study came out, Digital had been in operation a number of years, and at that time their ratio was about nine successes out of ten. I think comes from that clarity of what it's for at that time at least. Ken would have been very able to put himself in the customer's shoes. He would know what they needed, what they would do with it. You had from this group of people that had been through a program from the idea to the final working result that people have to visualize the rest of the process, which you do not ordinarily find in a functionally separated company where you have somebody doing research, then somebody to do development, then somebody designs it for production and somebody produces it and everywhere along the steps they say, "Well, this is a bad job because they didn't do it right." Then they try to re-do it, and it finally gets to the sales department and they say "It doesn't fit our product line, we can't really sell it." Or, it gets to the customer and the customer says it doesn't fit what they need. Rather than having someone who in charge of it, who's had that experience and has personally moved through all the stages, then comes back and starts over. I think this is a much underappreciated characteristic of how people learn. I was talking to a primary school teacher the other day who teaches in some system, I've forgotten the name of the system. Founded, initiated by a European. Anyway, the pattern is that the teacher starts with a first grade group, and that teacher stays with them through eighth grade and then starts over. So they have this one teacher for the eight years, who gets to know the families and the students. The teacher of course learns what happens every step of the way. Then goes back and starts over. It's this idea of looking at the timescale of where it's going, of where one is going with what one is doing now.

INT: When you were selecting people to work with you at the Digital Computer Laboratory, could you see that characteristic in a lot of the people that worked there? Norm Taylor, for instance. Ken, we talked about...

JF: First of all we had this high degree of entrepreneurship and independence at MIT. If I wanted to higher a mathemetician, I would do so. The mathematics department did not have any opportunity to pass on their credentials or decide if they approve. Because of course, our goals and purposes might be very different. I placed a high premium on initiative, courage. Hard to judge these. Most of our people, Taylor did not, but most of our people came in as masters degree students in electrical engineering. The procedure was for the applications to be stacked up on a long table and then people who would like to have a research assistant and had the funds to engage them would go through these and pick out the ones they wanted to try to attract. So there were letters of recommendation which probably were more informative then than now. People have become cautious about letters of recommendation in this freedom of information atmosphere. But I usually went several steps further than most people in picking applicants. I would telephone their references and talk to them and find out what the weak spots were as well as the strong ones. I would, I think, usually telephone the applicant and talk to the applicant. We may sometimes have had them come in but I don't recall that we really did pay their travel to come to see us. I placed a reasonably high ranking on their military experience. A lot of people in academia considered that a lost cause and didn't really take it into account in applications for the graduate school. It seemed to me that it had given them a lot of background in personal relationships, leadership, finding out what the real world is like. And so quite a number of our people came in [from the military]. Of course this, you know in the late '40s and the early '50s, these were people coming back from the military from World War II. Some of them hadn't even finished their undergraduate college before they went into the army. They came back and finished, then went on to graduate school. So at that time, we were picking up people who had had military experience. I believe Ken had.

INT: Ken did, yes.

JF: Quite a number of the others had. And this I considered a valuable experience. So beyond that it was of course subjective and not always successful.

INT: Ken managed the MTC [Memory Test Computer] computer project. How was he selected to do that?

JF: The history was that we had started the magnetic coincident current random access memory work in 1949. That's when I first came to the ideas of how to do it. Actually this was based on work two years earlier in 1947 using random access switching for a memory that would have low discharge tubes like little neon tubes at the intersections. The logic had been worked out at that time. But the mechanisms, the low discharge tubes were clearly unreliable, changed with time, depended on secondary emission which is a treacherous process. And we did not go very far in trying to implement it because it just didn't seem as if it would work. I doubt that it would. But the logic around it was fairly elegant, and basically what eventually went into the magnetic core memory. In the intervening two years then, the idea was dormant until I was attracted to an advertisement for a rectangular hysteresis loop of magnetic material, and began to think about how that could be incorporated instead of the low discharge tube. And it was a gradual development then up until about 1952, I would say. First testing a single core and then arrays of two by two. And then gradually bigger ones and using, and moving from metallic tape cores to ferrite cores. And we were getting then to the point where it was time to build a full scale array and to try to test it before commiting ourselves to put it into the Whirlwind computer. So I had wanted a tester made for the magnetic memory. Somewhere between Norman Taylor and Ken Olsen, they came back with the idea that they would build a computer to test it in. Now part of that background is important I think to the launching of Digital. Because the proposal was to make a computer out of our so-called standard test equipment. Has that work crossed your history?

INT: Tell me some more.

JF: Well, the normal practice had been that you would develop an electronic circuit. Then in order to test it, you had to have probably even more electronics than in the circuit to run the tests on. That was expensive, time consuming, usually totally special purpose, so that when the test was over then you threw it away. This wasted time, and as I say, was expensive. So among us, I think maybe I suggested this, that we build standard, logical building blocks that were sufficiently, robustly designed. Sufficiently insensitive to small disturbances in their inputs so that you could plug them together, helter skelter, anyway you wanted to according to the logic and they would work because they wouldn't be sensitive to how many things you plugged into the outset.

INT: Just like Digital's first product.

JF: It became Digital's first product. The history of that was that we designed them, and because we needed a lot of them, we had them built for us by the Burroughs Company. These were all fairly big, on panels, sixteen inches long. Five or so inches, four to six inches high, and would have one flipflop, one or two logic units to a panel. Burroughs made these, and Burroughs made their first Digital computer out of them. The proposal was to build us a computer out of a new test of the magnetic core memory. We had spent four years building Whirlwind I. We needed this tester in less than a year. I think it seemed rather preposterous that we would now undertake to build another computer for this test vehicle. But Norman Taylor and Ken Olsen insisted. And I said well, okay go ahead and try it and see if you can. And in about nine months they had in fact built the Memory Test Computer which was used for testing the first full-size bank of magnetic storage.

INT: A successful attempt?

JF: A successful attempt. It was tested, I think in the spring of 1953. And by the summer we decided to move it into the Whirlwind Computer. I think that was a 32 by 32 array. Eventually I think we put a 64 by 64 in Whirlwind. I know we had to modify the logics to get switching access to more memory. But I've forgotten the details of just what happened there. Then the Memory Test Computer was used for a number of years as a vehicle for testing other kinds of things. Drums, probably, but other things that came along in connection with the SAGE air defense system market was a powerful general purpose piece of test equipment. And then to finish the thread of that test equipment, Burroughs, who had the center stage in making it, lost interest, because among other things, it was mundane, unglamorous material, very hard to get an engineering group to be excited about and so they just dropped it. Then in the Lincoln Laboratory, significantly later, the idea was revived in terms of transistor circuits. I think the TX-0 was built I think out of pluggable units. I didn't pay too much attention to the TX-0. I think it was built out of these pluggable units essentially logic blocks. Those, then, were redesigned to become Digital's first product.

Digital did not go into the computer business. They went into the logic business and test equipment, and then began to push forward from that in the various niches in the marketplace coming relatively soon to a true computer.

INT: Was the idea to start Digital percolating in the laboratory between Ken and Harlan? Were they speaking with you, asking your advice before they went to AR&D for funds?

JF: I don't think so. I left Lincoln Laboratory in the summer of '56 and came to the Sloan School. I believe they left after that. I don't think I had any particular knowledge or any role in their leaving the lab or deciding on setting up a company, or they're going to AR&D. I came back into it when I was asked I think by Horace Ford, who I believe was treasurer of AR&D at that time. In any case, he was treasurer of MIT. I think it was Horace Ford who asked me if I would be on the Digital board. I do not know what discussions went on before he made that request.

INT: But Ken or Andy, [Harlan Anderson] never asked you?

JF: I don't believe that they directly asked me as a start. We might have talked about it later but I think the first approach, as I recall it anyway, was Horace Ford, coming to me on behalf of AR&D.

INT: Can you talk a little bit about Harlan Anderson and your relationship with him? Was he also a student of yours?

JF: He came in, I think, as one of the masters students in the same way. Very often these masters theses were for all practical purposes supervised by other people that had been in the lab for longer. I do not recall the details of Anderson's graduate work, or thesis, or just what.

INT: You weren't as close to him or as an advisor as you were to Ken?

JF: I wasn't close in the sense you may mean it to either because there was one layer of the laboratory between. For example, Norman Taylor would have been kind of the chief engineer. Ken would mostly have reported to [him] and possibly Harlan, also. We had four or five. We had a mathematics group and a programming group. Just mathematics and programming. We had the electrostatic storage tube group until magnetic core memory took over. You might be interested somewhere down the line about the history of the field of computer memory.

Adequate, low cost, high speed memory was the bottleneck to the early development of computers. The kind of memory that people selected then dominated the design of their computer. And there were several kinds of memory being tried at that time. The so-called Williams tube, which was a cathode ray tube, in which you would put an aluminum foil electrode on the outside and deflect the beam and pick up some very small signals through the glass, was used by a number of people. Not very reliable. The EDVAC computer used mercury delay lines which were about a meter long tube filled with mercury with a crystal at one end, and a crystal at the other. You would send a shockwave down the tube, pick it up at the other end. It could store perhaps a thousand of these shockwaves in the length of the tube, but of course you'd have to wait til they got to the other end in order to detect what they were and transmit time was probably about one millisecond which is a long time in the computer world. We had gone the road of designing our own electrostatic storage tubes, quite different from the Williams tube, ones that did not have to be refreshed. The Williams tube you had to keep scanning it and reestablishing the plus and minus charges because they would leak off and disappear and get dissipated by the secondary electrons in the tube. We built one, it had two electron guns. One for selecting the spots, the other for flooding the whole surface with electrons in an arrangement where if the spot was positive, the incoming electrons would drive off more electrons than came in, keeping it positive. If they were negative the incoming electrons would be slowed down to come in gently and build up the negative charge. So that this flood gun would maintain whatever the charge was on the surface without rescanning. However, it depended on secondary emission. It had two cathodes in it which give off vapors and change the effect. On the whole, a very treacherous system. These tubes would store a thousand binary bits. Not bytes, bits. They cost about a thousand dollars to make and they lasted about a month. We were spending one dollar per binary bit per month to sustain the memory. If you had an eight megabyte computer, which means about 64 million bits, you would be spending $64 million dollars a months to sustain the memory! That was the incentive to do something else. That led to the magnetic core memory..

INT: That's a very clear and useful way of describing the need.

JF: Of necessity being the mother of invention.

INT: Absolutely. I've been through your notebooks in the core memory collection here at the archives. And also Bill Papian's and Ken's. That whole collection is very, very well documented.

JF: That collection you see was brought together because of the patent suits. We had the interesting situation of taking about seven years to convince industry that it was a good idea and would work. And then another seven years to convince them they had not all thought of it first.

Digital and RCA and General Electic all started in the computer business at about the same time with rather different philosophies of how to go about it. RCA got out after losing about $400 million dollars. GE got out after losing about $600 million. And Digital never had a lost year, not even the first year.

INT: Why do you think that happened?

JF: I think there were several reasons. Initially at least, the clarity and practicality of what they decided to do, namely the logical test equipment line where there had been two generations developed already. And the new generation was, I suppose, largely a repackaging. Another view that Ken had of not following the frontier technology but the second or third behind that, namely the technology that you knew how to make work. Another, their prior knowledge that there were people out there who needed this equipment since they themselves, he and Harlan Anderson and the others, had been using it for years in various ways. And the design, the effort to develop it consistent with the size of the organization. And I think another important part of it, a niche that other people didn't see as important, or carrying prestige, or satisfying their engineering and technical desire to be doing something new, meant that -- and also a product that was worth a great deal more to the customer than it cost to make it, in other words the cost of a customer going and doing it for himself for a special test you see was many fold above buying the test equipment so that it could be priced to pay for the development -- those things I believe -- and of course the technical and engineering skill...do you know when Dick Best joined the company?

JP: I think he joined in '59.

JF: Well, he was one of the outstanding electronics designers. And yet he must not have been directly involved in the first test equipment. I would assume that that had been done before he joined them.

INT: He probably was one of the first ten hired. Because I know he did a lot of work on the early circuits.

JF: Well, you see they carried over the reliability, the ruggedness, the performance independent of how you plug them together that made them truly a logical system rather than a temperamental marginal electronic system. So there was a good match between initial funding, initial available time, the state of the technology, the aspiration of where they were going to sell it. We very often do not see this and and also a small niche for a small company. If you see someone who has a product, a proposed product, wants to start a company, and says every household in the country is going to have to have one of these immediately, then you can be fairly sure that once...

[END OF SIDE A]

JF: ...a small company won't be able to grow fast enough to fill it and it will become immediately attractive to some big operator and they will find a way to take over the business. Digital, for a very long time, was in businesses that nobody saw as big enough or important enough to get into. So their strategy was a little bit like the German and Japanese automobile companies relative to Detroit. Detroit didn't worry as long as the Germans and Japanese were just snipping off the extreme high-priced end of the market, and the extreme low-priced end. But then, of course, they kept coming toward the center.

INT: Very fast.

JF: Very fast. And you see that in American industry everywhere you turn. With the present emphasis on return on investment, companies are methodically dropping their products that sell less well, even though the customers may turn around and be rather desperate for them among the customers that want the product. And so the customers will start turning around and asking for competition: why doesn't someone else go into the business, it's not available anymore. And then those competitors have an opportunity to move toward the more profitable part of the market. Or, as a matter of fact, they can make the part that was abandoned profitable by raising the price and selling to those people that in fact really need it.

INT: You talked earlier about the system model as it was applied to Digital. Can you give me some background about that?

JF: Yes. I came to the management school from Lincoln Laboratory in 1956. There had been a course fifteen, business course at MIT for a long time since the mid-thirties, but not a management school. Alfred Sloan, the primary force in making the modern General Motors Corporation, had the vision that a management school in a technical environment would develop differently from one in a liberal arts environment like Harvard or Columbia, or Chicago. Maybe better, maybe more interesting, but in any case different. And it was worth ten million dollars to run the experiment. So he gave MIT $10 million dollars around 1950 or shortly thereafter to set up a management school. The school was officially organized in 1952. Four years later I came and joined it, primarily with the mission of seeing what this vision of Sloan's -- the connection of a management school to the engineering technical sight of MIT -- might mean because at that time nothing had really been done to bring anything from the technological sight of MIT into the management school.

Others, I think, assumed, and I probably did myself, that this would mean either one of two things. To work on how business should use computers for the processing of management information, or pushing forward the field of operations research which then existed in a small way. It was about the same as the field is today. I had my first year at the management school with nothing to do except trying to figure out why I was there; in the process of which, I laid aside both of those possible objectives. It seemed to me that there was so much momentum by then in the computer field with the manufacturers, the banks, the insurance companies, all beginning to get into the act and trying to work on how they would use these machines, that a small number of us here probably wouldn't have any major impact on the direction that it was taking. You see, I had left the computer field from Lincoln in '56 because I felt the pioneering days in the field were over. And, in a way, they were because the percentage increase in performance, from 1946 to '56, the multiple by which all the dimensions of computers increased, was bigger in that decade than any decade since -- although it has been big in every decade since. Anyway, I decided that we probably weren't going to have much impact on the management use of computers. Then, as I looked at operations research, it was interesting, probably doing useful work, but it clearly was not dealing with the things that made the differences between corporate success and failure. If you look at the companies in a field and some are succeeding, and some have great difficulties, in general these were issues that the operations research people had not latched onto. So it was out of discussions with people in industry, and rather largely at that time some people in General Electric, about their problems that I began to move into what became system dynamics. They were concerned about why in their household appliance factories in Kentucky, they would be working overtime one year and three years later half the people would be laid off. It was kind of easy to say, "Well, this is the business cycle," but it didn't quite hang together. And against my background of feedback systems which I had brought with me since the 1940s, I began to look at the process of decision making that would just take inventories and order backlogs, and present production rate in employees, and if the inventories are low and the backlogs are high, what would they do about hiring people? And how would they adjust production? And as one set up that system on a piece of paper, just a notebook page, and then line by line compute what would be the condition of production and inventory, and employment and backlogs, given what seemed to be their policies for how they would expand and contract, one found that you had a control system, a feedback system

-- which even with absolutely constant demand would produce huge fluctuations of inventories and employment -- that these cyclic behaviors were primarily internally generated. People don't really understand that, even to this day. An absolutely standard part of presenting system dynamics to any group that you're going to spend more than two days with, is to run something we call now the beer game (it used to be the refrigerator game.) You take a team of five or six people, line them up down the table; one of them is the retailer, one is the wholesaler, one is the factory warehouse manager, one is the production manager. The game board is laid out so that if one stage wants to order from the next stage, he writes his order on a piece of paper, turns it face down on a square that represents a one or two week shipping and handling delay, and then the person upstream can pick it up and look at it and fill the order if he has inventory, and he puts the inventory in a shipping delay and the next person can get it in the next time period. And this game board is laid out. Everyone can see it. The inventories are poker chips that you can, in fact, look up and down the table and see what everybody has. You can't see the orders, but you see the physical set up. At the far end, the retailer end, there is a deck of cards face down that represents what the customers are buying. In each time period, each week, say, represented by two or three minutes of realtime, the retailer turns this over, pushes the chips off into the market that calls for it, decides what he will reorder. So it goes on upstream. Well, you may have ten or fifteen of these groups in a room all doing this simultaneously. And in general, every one of them will come out with a huge fluctuation of production at the factory, with peaks maybe several times to the height of valleys. Then in the debriefing, when they're asked to explain what this is, well it's clear to them that there was a highly fluctuating cyclic demand in the deck of cards and they were responding to that. Well, in fact, the deck of cards were absolutely constant: every week was the same sales. And this whole phenomenon was created by the interactions of these people, one with the other.

INT: Did you develop this?

JF: Yes, the first appearance of it is in Chapter Two of my industrial dynamics book which actually was an article that appeared in 1958 in the Harvard Business Review. It sold more copies, I think, than any article they'd had up to that time. It was a computer simulation model that showed many interesting things about different policies that could be followed in running a distribution system. Anyway, that had been done early in the development of systems dynamics, and we had applied systems dynamics to slightly more subtle things in management. But most of the work had to do with quite tangible behavior: inventories and employees and things you can pretty well go out and see and count. Although we had begun to get into the so-called un-quantified variables, quantifying them, of course, to put them in a computer model, and had gradually been evolving more elaborate models. [By] then I had become a member of the board of directors at Digital, and I did not feel that I understood how a high-tech growth company really functioned, although I had a lot of contact with such and knew quite a bit about the inside workings, I didn't feel that I really knew the dynamics of what really went on. So I began to work on a model of a high-tech growth company which took something like two years because it was very new territory for modeling, and I wasn't working fulltime on it by any means. But the objective of the model was to explain and come to understand the different growth behaviors of such companies. Some of them grow up to a point and have a crisis and go out of business. A lot of them grow up to a certain level, $5, $10, $20 million a year, and then just stagnate. They hang on there with fluctuations but no growth; no difficulty serious enough to lead to a merciful death, but no successes that really satisfy people. Then there are some that have a strong growth trend with repeated major crises, and there are a very small number that have strong growth trends with nothing that you would call truly major crises. The question was, why these differences? Because they exist in the same market, they exist in the same product lines. You can't say it's the external environment because you will see these differences between companies that are in the same external environment. It's got to be something going on inside the organization. So I began to work with the modeling of this process. It turned out to be primarily a model of how decisions are made in the company, how decisions are made in the market, and how the company and the market interact with each other in both directions. The result of this was my coming to an understanding of why some of these big differences, and you could see the differences in the policies in the model, and you could go out around Route 128 and see that, indeed, the companies with these different patterns were in fact following policies that represented the differences we've seen in the laboratory. So I used this background to guide my own position on the board, and I think it would be fair to say that I had a more influential position on the Digital board than outside directors usually have. Partly, of course, my prior relationships to the founders, but more importantly, I think, there were two power centers on the board. There were the financial backers who had the majority of the voting power. And there were the founders who had the overwhelming majority of the know-how and technical knowledge. And quite often these didn't see things the same way. If I would take one position or the other, and could explain why, it had a fair amount of bearing.

I think there was really no acceptance on the board that this kind of modeling could be done. You can use the system dynamics modeling in a couple of extremely different ways. One is to bring people into the modeling process, have them understand it, explain it, see the point and try to learn from it. The other extreme would be to not tell them that you have a model, but be able to discuss the situation with a clarity, completeness and lack of internal contradiction. That is impossible for most people because they don't have any way to tie together what they know about the pieces and what happens when they all interact with one another. Actually, I think on the board we followed an intermediate one: I tried to interest people in the process of modeling, which on the whole was not successful. But when I used what I learned, as just a board member discussing the situation.

INT: Was it not successful because people felt that things were going along well enough? Or it wasn't necessary, or?

JF: Well, it's not entirely clear, why people even to this day rebel at trying to understand the way things work. Among technical people, engineers, who may be quite familiar with feedback processes in control systems and devices and circuits, often tend to reject this underlying set of theories when it comes to applying it to them and their environment. There's also a strong feeling on the part of practical operating people that it would not be possible to capture the essence of their experience and judgement and bring it to bear on any kind of formal modeling. And besides that, they're very busy with crises at the moment, and it takes time to do this. Its benefits tend to be long-run rather than immediate. Somewhere out of that complex probably comes the reluctance to become involved. I kept pressing them to make this a part of the thinking and a part of a process. I remember one incident in the board when I tried this once again. The response was, it is not possible to model the things that matter in a company. We agree that we have been following many of the ideas that you suggested. We agree they've been successful, but it's not because of your modeling. It's because you're a better manager than we are. Which, you see, does excuse one from having to pay attention to the methodology being used. It is my assertion that it was the modeling that gave me the insights that I needed. The insights, after a lot of complicated modeling, were relatively simple. If one looked at why so many technical companies grow to a certain level and then stagnate, they think that they've exhausted the market, or they think that the problem lies outside somewhere which, of course, is immediately contradicted by the fact that they have competitors that are continuing to develop in the market. What you find is that they have trapped themselves in a process where their own decisions are limiting their sales. They have moved into a mode where everything is in balance with everything else and there are no motivations for expansion. Everyone that now works for them has a place to work. Every order that comes in gets filled. Everything is in equilibrium with everything else. And they're rather proud, usually, of the large order backlog, the six- or eight-month order backlog that represents safety and assurance of business in the future. What they're oblivious to is that that six or eight months is about the tolerance of the customer in waiting. If the customer has ordered a little more, the backlog gets bigger and this becomes less satisfying to the next customer, so they don't order.

So you have a completely balanced system in which the delivery delay moves up and down to adjust orders to what the company is capable of doing. Then, because sales aren't growing, the company may try to reduce prices to increase sales. This may bring in a few more orders that increase the backlog another two weeks, which is enough to compensate for the lower price. Again, there's no reason to expand, except that profits are now lower. They do not have the resources to in fact expand capacity that would have made it possible to sell more. I think out of the modeling came the idea that you expand whether you have orders or not, you buy real estate whether you need it or not, you keep up the prices to pay for it, and do business only with the people that want high quality, rapid delivery and are willing to pay for it.

I would say two things came out of the studies from my viewpoint. It may not be shared by others on the board. But, from my viewpoint, the idea that you keep expanding your capabilities, and in the process probably keep prices [high]. I would say prices in Digital were probably kept twice as high as they might otherwise have been set; people don't realize how insensitive the market sometimes is to price. They think it is highly critical. But at that time in computers, if you went into the market and looked at computers that had the same nominal catalog specifications, I think you could find a price range as big as ten to one. And who had the highest prices in the field? IBM. And who had the biggest market share? IBM. In other words, you had a situation where the highest-priced products had the highest share of the market. Because, in fact, they had the resources to deliver everything in addition to the product. Reliability, which you can't tell from the catalog. Confidence: the buyer if he gets into trouble, absolutely knows IBM would have the resources to bail him out. Salesmen knew their products. Answering the telephone. Delivery when promised, things of that sort. The price needed to be kept up to where you could maintain the rest of it. So I would say keeping up prices and expanding were cornerstones coming from the modeling.

I remember just one example of this. When the company was relatively small, still occupying just part of the initial floor they were in, the occupant of one full floor in the mill buildings was going to leave. The question was whether Digital should rent the space. This was a huge amount of space. [There was] some trepidation about whether to take it on, and I encouraged doing so. The board agreed and they signed the lease. The next board meeting was held in the space. I think we walked out into a floor that I believe was as big as two football fields. I'm not sure whether there's a floor that big in the mill or not, but that's my recollection of the size of it. You stand in the middle and you could hardly see the windows on either end. Within nine months that was full of people, returning 15 percent net profit after taxes. The space was there, so you could hire the people to fill any opportunity that came along and do it quickly, and begin to sweep in the market. And of course, Digital has been a real estate company over the years, buying buildings everywhere.

INT: Those seem like reasonable projections or assumptions based on your modeling.

JF: But you would be surprised how many people in business not only don't see what I've just told you, but will argue against it when you explain it to them. Mostly they argue against the idea that they can raise price, even if they're sitting there with a backlog so big their customers are all complaining about how long it takes to get the product. A big order backlog is evidence, on the face of it, that the product is underpriced. You raise the price, you may scare some people away on account of price, but otherwise you're scaring them away on account of delivery delay. It makes a lot of difference on your profitability whether you scare them away with price, or scare them away with bad service or long deliveries.

What I've just told you can be applied to as broad a situation as why the Japanese are invading the American market. The American companies have backed out of the market. They've been cutting back their capacity while Japan has been increasing theirs, and many products that were pioneered in the U.S., you cannot find a U.S. brand to buy now.

A brief conversation with a vice president of one of our machine tool companies five or six years ago illustrates the point. It was a hallway discussion, not very long. But he just made the comment to me that for all practical purposes they were out of business in the Seattle area, and I think he meant all American machine tool manufacturers. So I said "Why?." He said, "Well, if you're in Seattle and order an American machine tool, it will be 18 months before it's delivered. It will be installed by a crew that does not know how to make it work quite right. And when you need service it will take six weeks. If you're in Seattle and order a Japanese machine tool, it can be delivered the next week out of a Seattle warehouse. It will be installed properly and work the way it should.

And, if by any chance, you need service, it's overnight." End of the story. No moral in that for him. He had the whole thing in the palm of his hand, but he didn't see what it meant he should do. Now I didn't pursue it beyond that point. I don't know whether literally he didn't see it or whether in his corporate culture he had given up trying. He probably would have turned around and said, "One of the reasons they're selling is because they've got lower costs, or underpricing." Yet here he sits with an 18-month backlog which says there's something about his product, there's something about the American product that make some people willing to wait 18 months even though they could get the Japanese one in a week. What is it? It may be higher quality but it is certainly lower price. Because if he would raise the price, he would in fact discourage some of that backlog, and he would also have the money to do a good installation job for the people that did order it. And, he would sell just as many or more because then he would have the money to expand the business. Instead, they were essentially backing out of the market by saying we can't make any money because in fact they had cut their prices quite unnecessarily and had misjudged the balance between quality, installation, service, delivery and price. All of those are very much interrelated. But partly the business school and economics background, and partly because price is an easy thing to deal with and change, there's a widespread feeling that competition is based on price, rather than all these other attributes of a product. So, to some extent, the blame for this lies on American business schools, and I sometimes make the snide remark that American business schools are rapidly driving American business out of business because they are teaching ideas that are only partially to the point; and tend, once the ideas have gotten out in the business, to be used in a counterproductive way.

INT: What kind of power did the board have in those days? Was it a sounding board body?

JF: Probably, it was a more participative board that covered a wider range of things and looked more deeply into questions than you ordinarily find in at least larger organizations. I have not been on many start-up boards. I was on the board of the Alza(?) Corporation on the West Coast which has been a very successful company in pharmaceuticals. I would say it was not a rubber stamp board.

INT: How long were you on the board?

JF: About ten years. It seemed best that I leave at the time that MIT filed a patent suit against Digital because of my magnetic core patent, being a sort of a conflict of interest situation. That led to Tom Watson resigning from the MIT Corporation and Jim Killian resigning from the board of IBM.

INT: When you look at the company now over the thirty years, can you talk about some of the internal structure and policies and how they were responsible for the company's successes and failures?

JF: I don't think that I have a good enough feeling, or at least a certainty that I really understand it to answer that question for the later years. There's been a lot of turnover in the top people. A lot of people say that it's very hard to know where authority and responsibility lies. But I think that's hard to judge on the basis of the kind of information that comes to an outsider. Clearly, the company has grown and succeeded magnificently, and on the other hand, it's not evident that there has been the building of a strong management succession for the future.

Digital has shown some reviving interest in the field of system dynamics in recent years. I'm not entirely in touch with all of it. They have been off and on sponsors of the work we're doing here on the national economy and corporate policy. They have had seminars at least in their engineering side run by Barry Richman who was one of our Ph.D. graduates a number of years ago here in systems dynamics, on the modeling of management policies. But as far as I know it's not become any broadly introduced set of ideas. In a few companies around the world I think it is very much more influential.

INT: Which ones?

JF: The ones where it's influential, in general, consider it highly proprietary. In general they will not admit to the outside world that they're even working in system dynamics with corporate policy because they consider it so important and so proprietary.

INT: Is it just known then inside corporations by a small number of people?

JF: Sometimes a very small number. One of the most exciting things going on in the field now is the introduction of system dynamics down at the junior high school level where it can become a common foundation that ties together physical science, mathematics, social studies, environmental studies, history, even literature, and therefore become something that is a thread through the whole educational process even starting as low as the fifth and sixth grades where successful experiments have been run. This produces very exciting results, especially when combined with something that in a few places is known as learner directed learning, which is that the students take on projects that draw them into computer modeling, library research, writing up reports in areas that maybe the teacher has not yet explored, and where the teacher is not a teacher in the conventional way but is a resource person to learn along with them and provide guidance but not pretend to know it all in advance. We'll find these out together. That is a very liberating kind of environment. One Tucson school, where this is particularly going well and has advanced in the hands of an eighth grade biology teacher, says it's the first time they've ever encountered a situation where in a class students say, "When will the room be free so we can come back. Can we stay after school? May we come in ahead of time at the beginning of the day to carry on our projects?"

INT: That's very unusual at that age.

JF: Another teacher teaches literature and English to juniors in a high school that is below the average socioeconomic level of the school system, where she has the bottom third of the track system, and where they have never shown any interest in literature and certainly not in Shakespeare. She went to a seminar in systems dynamics, and with the help of the instructor developed a systems dynamics model, still a model which you can display on the face of a Macintosh computer, of the psychological dynamics going on in Hamlet. When she brought this into the classroom, this lowtrack group of students became absolutely, intensely, involved in all of the characters in the play: discussing which character was like which of them, what ordinary people would have done, what would happen if his character would change a little bit. A teacher could change his character in the model and then you see who gets killed and when.

INT: But that is, in fact, the way you should read it.

JF: Yes, but you see they don't see it.

INT: They're not going to sit down and have these things occur to them.

JF: This model that we developed of high-tech growth, the one that we were talking about before, was one in which maybe only five or ten percent of the variables were tangible things like employees and inventories and orders. And 90 percent or more were very intangible things, like, what do we think that the customers think of the quality of our product. It was also heavily loaded with the leadership and other characteristics of the people involved. How did they respond to information? These are very important in that kind of a setting, because how they use information, what information they respond to, how they make decisions is, I think, crucial in distinguishing the successful growth companies from the unsuccessful ones.

INT: And although there's not a right and a wrong way to do those kinds of things, I suspect it's a somewhat threatening thing to apply to a company.

JF: It is, because you see it's easy enough for management to say, "Go off and develop this wonderful new product for me," but if it comes to developing a wonderful new decision making process that I'm going to use, then it is unsettling.

Then, of course, you see extremes in this management attitude. Detroit has been losing ground relative to the Germans and the Japanese for a long time. And even to this day they apparently do not understand what the problem is. They try to sell by price discounts rather than putting out a product that the customers want to buy. I think one of the nicest examples of this was three or four years ago, whenever it was, General Motors was being heavily criticized and recalling cars because the back wheels were coming off. In a public speech reported in the press, a vice president of General Motors defended this on the basis that it's not really dangerous to have a back wheel come off! Now with that attitude, you can easily see what you're going to get in the next automobile. The first one that came off should have been a major corporate crisis digging all the way to the bottom of how could it ever have occurred. Instead, the result is to try to hide it, or contend that it doesn't matter. The potential customers discover this very quickly and easily, long before the management does, by ten or twenty years. So in this corporate model, some of the time delays built into it from the product to the market and back to the company, can be years long; how long it takes the company to find out what the customers are really thinking. And of course it can be a short cut.

[END OF TAPE]