INTERVIEW WITH
JAY FORRESTER
SLOAN ORAL HISTORY
DECEMBER 6, 2012

J: Jay Forrester
G: George Roth
B: Bob (Robert) McKersie
F: Janice Forrester

B These interviews are going to be valuable material for the archives, for people to talk about their own careers. In your case, there's a lot of information already in the archives. You had two special interviews with the 150th anniversary of MIT.

J: Yes, and there are two more somewhere and some for the Lincoln Laboratory also.

B: George has identified, as he did some digging, other times that you've been interviewed.

G: As Bob said, when we talked about the number of interviews we've done, there are very few people who really can recall—who are still available to us—the founding of the Sloan School. We know that you even preceded that in your time at MIT. So we're very interested, as part of this oral history project, partly for the book, but also I think partly for the oral history archives that we're establishing, which will live in perpetuity. If you could help us with before Sloan started, any recollections you have of Course XV at MIT; Erwin Schell, who was a dean or department head. I'm not even sure what the term was for what started out as engineering administration and it became management later. We can start there. I have a number of questions that we'll ask as to when you officially joined the Sloan School in that process. Then you were there, you have memories of its early days. Maybe we could start even before you were affiliated with it and other parts of MIT and what you know about Course XV.

J: Well, it started because in 1939 Cornell turned me down for graduate study and MIT offered me a position as research assistant with tuition and $100/month, enough to live on.

So I came here in 1939 for one year of graduate study and haven't gotten away yet! You want to skip the things before we get to the Sloan School era?

B: Yes, I think we can because you've got those quite well in place.

J: They're covered elsewhere. I had not had any real contact with the business course because I was entirely in the research side of MIT, not in any department. The department that sponsored research was where my appointment was; my work was affiliated with the Electrical Department, but not really a part of it. Gordon Brown was head of the Servomechanisms Laboratory where I had most of my earlier experience. Then the Digital Equipment Laboratory started, and I was head of that and we built the first digital computer at MIT. We tried somewhat unsuccessfully to convince the electrical engineering professors that it was both possible and desirable to use binary arithmetic for computing.

I moved on into the creation of Lincoln Laboratory where I was head of Division 6, the largest division, which was designing computers for the North American Air Defense system. By 1956, that design was frozen. The manufacturing was getting started. The first installation was under way, and I felt that the pioneering days of computers were over, and I realized that some sort of change was desirable. In addition, there were some administrative changes in the Lincoln Laboratory that I didn't mesh with to my satisfaction. Then Jim Killian showed some important visitors around Lincoln Laboratory.

G: This would be in Bedford, the laboratory? You left campus at some point to work at Lincoln Laboratories in Bedford, and you were there for a number of years?

J: Yes. Lincoln Laboratory was started largely to carry on what we'd already demonstrated on the campus with the Whirlwind computer. I was the head of the computer division. Robert Everett was my associate head, had been for several years. I was walking down the hall beside Killian and he said, "You know, we're starting a new management school. It might be something that would interest you." Well, that was an interesting comment. I have had,

I think, a history of when a door opens, looking through it, and probably going through it to see what's on the other side.

B: You knew Killian from other interactions?

J: Yes, to some extent. That led to my meeting over the next year with Eli Shapiro, who was then Associate Dean of the School, and with Edward Bowles, a "consulting professor" who had come from the Electrical Department but was associated with the Sloan School and with Eli Shapiro at that time. We met in various places, at his house, and here and there, and discussed possibilities.

It was unclear what I would do, but as I understand it, one of Alfred Sloan's reasons for giving $10 million to start a management school was to see what one in a technical environment would do as distinguished from ones in liberal arts environments like Harvard, Columbia, and Chicago.

B: Did you meet with the dean at that time as well? Penn Brooks?

J: Yes, Penn Brooks was Dean. Penn had come from being a Vice President of Sears Roebuck. I think he was looked down upon by faculty members as not having an academic background. On the other hand, I think he was an extremely good judge of people. I think he made a major contribution. But Eli Shapiro was the real academician and had a good reputation there. He's the one I dealt with primarily.

G: Did you talk to other people at MIT? I'm guessing Sloan gave his gift about 1952? So the building was bought, the people were in the building, by 1956 the School had been around for three or four, maybe even five years already.

J: No, the school had had a Senior Executive Program, which I guess is no longer operating. I talked to every Senior Executive program there was, between the first one in 1952 and the last one whenever it was.

G: So you were a guest in that program?

J: Yes. I talked about computers, at that stage.

B: Interesting. So even before Killian propositioned you, you had a connection with the Senior Executive Program?

J: Yes, I think it would have been before.

G: Did you talk to them in the old Unilever building, now the Sloan building?

J: I think they were meeting at Endicott House, as they did in later years.

G: I know they had some things originally on campus, and then eventually it all moved to Endicott.

J: My recollection is that even the first one in 1952 was at Endicott House. I could be wrong about that. In any case, Sloan's vision of a business school in a technical environment did not have any meat on its bones because up until the time I came I don't think there was anyone there with a science or engineering or technical background. I could be wrong when I say "no one."

G: Was it Killian who told you about Sloan's vision? Did you get a chance to meet Sloan? How did you know about his grand experiment?

J: I believe I did not meet Sloan. I'm not sure where I picked up that viewpoint. Killian may have mentioned it in the beginning, or certainly it would have come out in a conversation with Eli Shapiro.

B: In terms of other faculty who were already on board. Doug McGregor was there, wasn't he, at the time?

J: Yes, McGregor. My first academic appointment at MIT was as full professor at the Sloan School. I had had no academic appointment before that.

B: That's great!

J: I'm not sure anybody can do it again, but I had one published paper in a second-level physics journal. I did not have an earned doctorate, and I was given a full professorship. Of course, that one paper had launched the computer revolution. So I went there having no idea what I was doing. I don't think anybody else knew. I basically had the first year to decide why I was there.

G: But one of the things about the Senior Program, it has been taught since the late 1980s, a lot of times faculties bump up against one another and interact as they hand the class over. You had some interaction with some of the other faculty at Sloan, at least teaching the Senior Execs? Do you remember any of that?

J: Not in teaching the Senior Execs because those were independent lectures that people would go out and give, and other faculty would not be there. Once I had my appointment in 1956, then there was active interaction because there were faculty group meetings with less than ten people.

G: Do you remember who they were and who organized them?

J: Eli Shapiro would have certainly been active. Doug McGregor was a key person. Elting Morison was a very interesting person and was a member. Warren Bennis may have come later. He was in there early on. Who else was there…?

B: Had some of the folks come over from Economics, like Doug Brown? Had he come over to the School?

J: He seemed to be associated, yes, but he was probably also in Economics. I'm having a problem at the moment visualizing…

B: Elting Morison, we just interviewed Peter Timen, who was there in the 1960s and then moved over to Economics. He told us about a course that Elting Morison ran, which Abe Seigal later taught, and I'm currently teaching, where you use great stories in literature to get people to talk about leadership issues. Elting Morison was the one who developed that course.

J: I had a high regard for Morison. He was forceful in insisting on good writing and people liked it, it was so popular that his courses were over- subscribed.

I remember one faculty meeting in the fall before the fall got started, where he asked for volunteers from among the other faculty for each faculty member to take on one of his students to critique papers that the student would write. There were two faculty members who derided the whole thing, said it wasn't necessary, and basically were negative to the idea. I volunteered to take on one student. I went back to my office, and my secretary was beside herself. She was typing up a manuscript from one of those people who said it was not necessary, and she couldn't tell where the sentences were in it!

G: So typing in those days was more than just transcribing handwritten notes!

J: That's right.

G: How many students were there in the class? When they said, "Take on students to do their writing," this would have been the Master's students that were in the programs?

J: It would have been, I think, masters' students. I do not know how many he had but probably getting up toward thirty or so; up to where he couldn't handle each individual

paper. So I signed up for one and it was a rather interesting experience. The student came in with his first paper. I couldn't tell from the title what it was about. I could not tell from the first paragraph what it was about, and I could not tell from reading it what it was about! And he thought he was an excellent writer. He'd been on his high school newspaper or something. I quizzed him until I found out that what he was writing about was how the consequences had been different from what Congress expected when they passed a law around 1923.

I said, "What do you know about what they expected?"

Well, he didn't know anything.

I said, "Well, you've got to read the *Congressional Record* for that time period and see what was going on."

The only place we could find it was in the Boston Public Library, so he had to go over there and read up on this. His first week's assignment was to get a title, and the next week a first paragraph; he worked on that paper for about six weeks. He claims he was working 30 hours a week, but I think it was a challenge to him. Eventually he came up with what I considered a good paper.

Then he brought in his next paper. I read it, and I said, "What's going on here? Am I falling under his spell or what?" I didn't see anything wrong with it.

B: You must have given him very good advice.

J: I said, "Excuse me." I went to Elting Morison and handed him the paper and said, "What's going on? I don't see anything wrong here?"

He read through it and said, "Well, there's a sentence here that should have a period in the middle and another one over here." That's all he had to say.

I have developed, not only from that experience but from other individuals who write well – I've asked various people whom I recognize as writing well, and I say, "How come?" Very often they know exactly. They know it was a paper they wrote for a particular person who made them write it, made them bring it up to high standards. Once they've done it, it always stays with them. Anyway, that was one of my experiences with Morison which I always cherished because it was very interesting.

G: I think the time you joined the School was also a time when they were hiring people and growing the School, yourself included. There was some tension in the School between hiring people who had been good managers and understood management from experience, and hiring people. Ed Schein would be one who was hired early on, who came from a particular discipline: in Ed's case, social psychology. I'm wondering in that earlier faculty meeting, whether the reason people didn't raise their hands was because maybe they were business people who didn't develop that writing skill, versus academics who maybe had to write more. I don't know. It just made me think about that.

J: I know one of the two who objected to the whole idea was an academic. Holder Huggens was one of those who lies behind your comment. Holder Huggens had been a VP of Montgomery Ward. He was looked down upon by students and faculty as just chit-chatting with students about his experiences and not really a true professor. But, you talk to students ten years later, and he's the only one they remember having relevance.

B: Probably Penn Brooks or Alfred P. Sloan, I'm trying to remember. I think his name comes up in the interview we did with Eli Shapiro. There was this effort between Penn Brooks and Alfred P. Sloan who knew people in the business community, to bring them into the School. I think that's how Huggens got there.

J: But most of the main threads of the School were started in that early era and the School has perpetuated those threads, expanded and refined them. There hasn't been much in the way of really new threads.

G: One of the other people who get mentioned in Eli's comments is Erwin Schell, who helped establish the curriculum early on, even before the School was just a course. He apparently did an annual lecture that was heard by all the students.

B: One of them was entitled "The Million Dollar Man." Did you know Erwin Schell at all? You might not have because he was worrying about the undergraduate program.

J: Undergraduate program. When did he die? I don't remember?

B: I don't know that.

J: I don't think I had any close interaction with him. I don't recall his being in these meetings of faculty members I was referring to. I don't think he was.

G: I was thinking about him because you were mentioning early threads. I'm curious also, when you think about early threads, what are the early threads that have been perpetuated and largely not changed, as you said?

J: Operations Research. Probably Finance. System Dynamics, which was started by me. The psychology things with McGregor and Bennis have continued in one way or another. When I went there, I think people expected—and maybe I even did myself—that I would do something in the area of operations research or the use of computers in business.

A lot of people asked me, "How come you made this break from science to management?" Well, it wasn't a break at all because I had managed a probable $2 billion operation. I wrote the contract between IBM and the Air Force for the computers for the SAGE air defense systems. We defended the Air Force's budget because they didn't have enough computer knowledge to do it. I really had control of IBM, the Air Materiel Command, and AT&T. We were the people at the center of that system, and we had complete control of it. The Air Force needed the pretense or the appearance of having a general in charge, so I picked the general we wanted in New York and set up an office the way we wanted it set up. We had a power that would have been disastrous had we been wrong.

G: So the principles you used for your management thinking and guidance didn't come from an education from a management school or from other managers, they came from technical, scientific logic.

J: And from the real world. This is one of the problems in academia. It does not really tie itself like it should to the real world. Economics, in particular, is deficient in that regard; to a lesser degree, probably management.

B: We ought to give you a chance to eat. We can talk a little bit.

One thing I want to leave with you, Jay, is the latest issue of *Business Week*. It has an article about Ronald Coase, the economist who won the Nobel Prize in economics. He has a devastating critique of modern-day economics.

J: Good! I'd like to read it because I can write one too.

B: Right. It parallels your thinking, that's why I'm going to leave the clipping with you. He says, "They just spend too much time manipulating numbers, and they don't know what goes on within companies. They don't really know what goes on within the economy." He's in the process of establishing a new magazine. You'll be impressed at how old Mr. Coase is. I didn't know him very well, but when I was at the University of Chicago, he was given an appointment both in the law school and in the business school. He's 101 years old. This new journal is going to be a journal of economic thought, not just data. Here's a wonderful picture of him.

J: There was another devastating article, another citation of progress. There's a brief quote from a paper I wrote. There were two economics professors, one from Middlebury and the other from somewhere else, who had surveyed American doctoral students on what they thought and what was important. One question was, "What's necessary to put you on the fast track?" or a question of that sort. "What's going to really get you going forward?" I'm just guessing these numbers a little bit; I actually saw them last night. But about 85% said mathematics.

"How important is economic history?" Maybe 15%.

"How important is knowing something about the economy?" 3%.

Those were the attitudes of graduate students in the five or so major universities in the US, which I think says a lot about the problem.

My assertion is that economics has been badly set back by considering itself a science. It's considered a social science, and often they refer to it as the "queen" of social sciences, the important social science. It should not be a science. It should be a systems profession like medicine and engineering, dealing with how the pieces of the real world interact, what's going on.

B: You and Ronald Coase think 100% alike.

J: Right. But we don't have any effect because even though he's 102, he'll die sometime and at that age he's looked upon as over-the-hill and don't need to pay attention within the profession. But my strategy is to outlive them!

G: And we support your strategy!

B: He has a similar quote there about outliving those skeptics.

So, Janice, have you been exposed? What subject areas have you…?

F: I've studied statistics. My daughter started her undergraduate studies at the University of Chicago, and she's interested in majoring in economics, believe it or not!

G: Heresy!

F: Jay is taking it very well!

B: Yeah, because they have quite a star-studded economics department.

One of the questions that interest us, Jay, is in those early years, 1956 and the years just after that. What was the atmosphere of the School in terms of where your office was, how people interacted. Did they hang out together at the end of the day? Were they in the office all the time? Some of that is a contrast to the way people behave today. So what was the collegial setup in terms of who was nearby, who you interacted with?

J: I think the main geography was on the fourth floor of the Sloan building. Economics may have had the third floor even then, I'm not sure. I think there were a lot of one-on-one discussions of things. I don't remember any after-hours meetings or get-togethers. Might have been. Not a clear part of my recollection of it.

Carroll Wilson was in there early on; I don't know quite when. Carroll Wilson was a very interesting person, and I don't know for sure when he came. He probably was not there among the first, but he was a person with business experience, as well as probably other kinds of experience. He had been involved in the atomic energy area. He was head of a company that made metals for some sort of atomic use, I don't know exactly what. Then he joined the School.

Quite early on in system dynamics we had two different, similar groups. Doug McGregor was involved with me and some other people in looking at the – we were going to do a system dynamics modeling of what happened in the so-called T-groups, and that was getting well started in the year before McGregor died in the summer. Then that did not progress. System dynamics should deal with anything that changes through time, and the psychological changes in a group like that are a perfectly valid basis for looking at.

The other group that got started was with Carroll Wilson because he was becoming interested in the African countries at that stage. He had several years of putting Master's Degree graduates of the Harvard Business School and MIT Sloan School and Harvard Law School into African countries to help the countries start a government, write the constitution, and things of that sort. These students went to Africa for two years. He had a conference every year, a big meeting and the ones who were completing their work were part of

it. Those who were going to go in were part of it, and those that were halfway through were part of it. Then various faculty members invited to go along. Carroll Wilson had a knack for picking the best places in the world to have a meeting. Susan and I went to one of them on the top of Mount Parnes, overlooking Athens.

B: That's a very important piece of history.

One of the things that are coming out of this effort to celebrate our 100th anniversary is a series of themes, and one of them is the international work of the School. Allan White has been given the task of writing the first rough draft, and this topic of what the School was doing in Africa needs to be constructed. I don't think there's a lot of information around. So what Carroll Wilson was doing is a very important piece.

J: I think there is a lot of information because there is an ongoing group of those former African fellows that meets every year in Wilson's memory. I've gone to several of those meetings, but I probably will not go to the one this winter. One of those meetings is coming up, so if you want to know about the African program, you should go to that meeting and talk to the people who are there.

B: That's a great resource!

G: That would be wonderful for Alan to know that.

J: I ought to be able to find for you who is organizing it and who to get in touch with. Let me see. Rosemary is Wilson's with his daughter. She's a lawyer in Boston and is always active in that program. I have the law firm name upstairs.

B: Okay.

G: Were there other international ventures that you remember?

I think there was also quite a bit going on with schools in India, where there was a group that helped start up management schools in India that we've heard about.

J: I don't remember it that early. I would have thought that kind of movement came along much later, but I'm not sure.

B: One person who did go off to India was Charlie Myers. You probably knew Charlie.

J: Yes.

B: At some point Charlie came over from Economics into the Sloan School and brought with him this thing called the Industrial Relations Section. Or maybe McGregor brought it over. Anyway, Charlie went to India and he wrote a book about economic development and the labor question in India.

J: Did he go over in order to write a book, or did he go over to change things?

B: I think it was to change things, and then he realized he had enough material for a book. Your mention of Doug McGregor, talking about getting in touch with the real world. As you know, he went off to Antioch to be president. You were around to watch that transformation of someone who had these theories, tried to put them into practice as a college president, and came back and said, "Boy, it's a lot tougher to be there than to theorize about it."

J: Well, I was quite fascinated by the T-Group movement that Bennis and McGregor were in. That's why I undertook to work with them on an actual dynamic modeling of what happens. That could have been an interesting outcome if it had continued. McGregor was very open-minded. He was right in there, ready to discuss it. I think the early School brought in people with very different backgrounds into a school where there were no traditions. Now people are brought in to fit into an existing group. Somebody who has a totally different view of what to do in a management school wouldn't have any home and probably could not get appointed.

I say that a lot of the present School, to a considerable extent, is just an extension of things that were started in those first four years.

G: It's interesting. In some of the other interviews, when we've talked about innovative things and how they've come in, often they've been connected with Sloan Fellows who came in and really had an influence on the faculty, based on their companies and their experience. I don't know if you saw that.

J: Yes, I did. Because as I was developing, my field was called Industrial Dynamics, to start with. We thought of it as being corporate policy-oriented. The Sloan Fellows Masters theses were important building blocks in developing industrial dynamics. They're cited in my *Industrial Dynamics* book, ones that took an interesting problem and then dug into the dynamics of what was happening. Sometimes they came up with a model that was much more elaborate than it needed to be for the subject. There was one done by maybe a Chrysler Sloan Fellow, dealing with the conflict between quality and timeliness in design. If you want to get the latest design out, the latest tail fins, or whatever is of interest at the moment, you have to do it quickly, be ahead of the game, at the expense of running all of your tests and making sure that the thing works. I used that in a meeting with some corporate executives of one of the companies I was going to in New York. I simplified it down to what I could put on one page, just to show them the issues.

Another Sloan Fellow had a thesis on government contracting for technology with an emphasis on the integrity of the company as it affected the way it bid. That had an interesting outcome because I was getting ready for the first summer session in Industrial Dynamics. I committed myself to two weeks of material for people who would come from industry. A two-week seminar, and I didn't have two weeks' worth of material. I had engaged Ed Roberts to come in the fall to be a research assistant. He was graduating from electrical engineering and working that summer for General Electric. I gave him a copy of that thesis and said, "Would you read it and come back and give a 40-minute presentation of it to the summer session?" That was one 40-minute block. He did it. I did not know him; I did not know anything about his background. So he started to present this and the effects of integrity on bidding. The group of 16

people had two interesting blocks. One was Vincent Learson who later became President of IBM, and the other bloc was three contracting colonels from Wright-Patterson Air Field. Learson got on his high horse and said he wasn't going to let any young whippersnapper suggest that American industry lacked integrity. The three colonels began to shred Vincent, and Ed just stood there and played them off against each other in a remarkable way. It was so interesting, I think that 40-minute session ran for two hours or more. I later learned that he had been on the MIT debating team and at that time was debate coach at Brandeis.

J: So that was a little vignette about the early history.

B: I wanted to ask a question about the Sloan Fellows. Did you have a regular course slot in the Sloan Fellows' program?

J: Yes, I had a two-term sequence. They took that, then they did a thesis. I mean, some of them did, those who chose to do a thesis had to have had those two terms. You can do that in a two-year program, I think. Aren't the Sloan Fellows one year now?

B: It's just one year.

J: Well, you lose a lot because you can't get preparation for doing something in a new area and have time to do it.

B: But the advantage to the Sloan Fellows is at that time they're all sponsored, so they had great access back to their companies for interesting information, problems, and so on. I really enjoy teaching the Sloan Fellows, and for many years the thesis became a big challenge for them, but they were able to pull it off because they were able to go back to their companies and work on interesting problems. Then you had people like Arnoldo Hax who would do what they called a "structured thesis seminar." Very close to what you were doing. Maybe you didn't call it structured thesis seminar, where you get a group of people together to work on something

that they're interested and you're interested in. They do the thesis, and they become building blocks for putting together a new framework, a new book, in your case, *Industrial Dynamics*.

G: So the students' influence in bringing some innovative ideas into the school has been strong. The quality of the students has continued to drive the quality of Sloan overall.

J: Right. They brought a lot of real world into system dynamics or what was then industrial dynamics.

G: Did you teach undergraduates as well?

J: Later on, certainly, but early I think it was mostly Masters students.

G: I think somebody mentioned that you had a senior thesis group that did system dynamics models? You had a small cohort every year and amongst them would have been some MIT undergraduates who did their undergraduate thesis work with you. I don't know if you…

J: It wasn't thesis work, it was the UROP program. I had several at once, and I had as many as 12 in one or two years, probably the largest UROP program in the Institute, I expect. Later on it became a group for developing the educational aspect of system dynamics for other people, self-study programs with special emphasis on K-12 programs. And the set of papers, 30 or so, written by those students, is available on the Internet. I think they're in the Sloan School Internet archives somewhere. They're in the group of courses that are listed. The course name was required, and it was given a course name for putting it in the Internet program, but it was not a course at the time. And they got a pretty good education in writing.

Bob Metcalf in an interview, some journalist talking to him, asked what he remembered about MIT. He said he remembered learning to write in my program. What that amounted to was that we discussed a subject, or a student would suggest a subject. Then the group would debate the subject, its relevance. Then the student would present an outline and the group would discuss that. In due course the student would do a first draft. The practice was to

have two students be the critics. So A wrote the paper and B and C would give him feedback on it. He would do that back and forth until the critics were reasonably satisfied.

G: And the rest of the class or the group listened into that?

J: Yes. I didn't look at those earlier ones. Then it would come to me, and I would pretty much cover it with red pencil and send it back, not only to the author but to the critics who had missed these points. Then it would be rewritten, maybe a couple more times after that. Then it became something we would put out. I think those papers are, on the whole, pretty well-written considering they were done by undergraduates.

There are two series. There is a guided study program, which is a self-study series. I think there are in the vicinity of 25 papers. If you do them as specified, those papers will take you about 200 hours to work through, and that's considered an introduction to system dynamics. Then there's another set, a group of four students organized for remote electronic teaching of any professional who wanted to sign up—and there were professionals who did, for I think as much as $5,000 to carry through the program. The participant was given one of these papers, and the next week had to send back his responses to it. Then the students would write up a critique of the response and send it back to the participant. There are 30 of those papers along with the 30 suggested responses that are available on the Sloan School's Internet site somewhere and are also available on the Creative Learning Exchange site, which is a small foundation devoted to system dynamics in K-12.

B: This is all very pertinent for the big thing that's brought together Harvard and MIT and Berkeley and now Wellesley, called EdX. Have you been reading about this? I think some of the things you pioneered would map right over to the challenge that they're trying to figure out. If you get 100,000 students signing up for a course, how do you decentralize the process of evaluation and feedback and so on? But this student-to-student is one way to think about how you can put it into manageable parcels.

J: I think we were into the remote teaching ahead of most people. Of course, I've done some pioneering ahead of most people. There's a lot of interest in wind power now. I was into wind power 75 years ago.

B: Back where you grew up?

J: We had no electricity on our ranch until I built it. We set up the system when I was a senior in high school.

B: Janice, have you seen any of these sites?

F: I have. We visited the ranch, yes, several times. We brought the children before it was sold, so they could see it. Jay sold the ranch in 1997.

J: Perhaps 12 or 13 years ago, I think. Up until probably some time in the 1960s, my father personally ran it. Had 600 head of cattle and ran it mostly by himself. Then he got to a point where he didn't want to do that any more and decided to run for the state Senate, which he succeeded in doing. He began to rent the ranch out in separate parcels, to four or five different people. They would rent it for summer grazing, and they would be responsible for their cattle. So his role was to see that the wells and fences and things were kept up properly.

He died in 1975, and I continued that process for 10 to 12 years. Then our daughter, Judy, who had expressed strong negative interest in the ranch and didn't really care what happened to it, decided she would drive to Billings, Montana, looking for an honest person. She got to Nebraska and heard that the passes in Montana were full of snow, so she called and asked if it would be all right if she stayed at the ranch for three or four days. There was nobody living there, a fully equipped house, and of course it was. She stayed 11 years!

F: I guess she found an honest person!

J: She got a lot of experience there. 45 miles from where she bought groceries. Closest neighbor was a mile and a half to the south, the next one six miles to the north, somebody 12 miles to the east. And west, I don't know how far you would go.

G: When you came to the Sloan School were you living out in Lincoln or Concord? I forget where your home was, and commuting in? And what were other people doing as the school got started? Did people live by or commute?

J: We lived in Concord until we moved into Newbury Court. We came to Concord, in the location where we stayed, in 1952. So for four years I commuted from there to Lincoln Lab and then into Cambridge.

F: Where Jay and Susan bought, many, many MIT faculty members lived in that neighborhood, in a development called Conantum. It has many MIT faculty members.

J: There were about 100 houses there. That economics professor, Rupert Maclaurin, founded it. This was a man who might have been interested in industrial history. He was out of the mainstream Economics Department, and I certainly should recall his name. Anyway, he had the idea of a different kind of community. He got himself, and Karl Koch as the architect, and the builder, the three of them working together to develop this community. About 100 people, most of them from MIT and Harvard, signed up to be part of it.

B: Did it involve putting together a housing community, and did it go forward?

J: Yes.

F: It still exists. The first such large development in Concord. Post-war, Baby boom, families looking to start a young life. They had common land.

J: Common land running down through the middle of it. Tennis courts, boat landing, we were very unpopular in Concord when we came in; we were the people on the "other side of the tracks," so to speak. Pretty soon our children were showing great success over their children at school.

F: Probably one of the drivers of that school system.

J: Well, there was a certain tradition in town. Somebody asked one of the older women in town if she had been shopping for clothes and she said, "We have our clothes."

B: It's somewhat reminiscent of Paul Gray and Bob Simoni. You would know Bob Simoni, and of course you know Paul Gray. They tried to put together a group to live together, but in this case it was at Kendall Square.

J: And what happened to it?

B: Well, it fell on its face because the developer got nervous about how many people were going to sign up, and he converted the building over to rental and then they had to go to court to make it possible for those who had put money down and intended to go in, to buy their units, their apartments, to go there. I think Paul, he's still at 100 Memorial Drive and Bob Simoni may have moved in. Only about seven or eight families have moved in on the original concept. The rest, it's all rental. It's on Third Avenue just off of Kendall Square. But you talk to Paul and Bob, they're very angry because they felt that MIT should have stepped in and bought a number of units for visiting faculty and so on, which MIT was never willing to do. That was one reason why the developer got nervous as to whether the thing was going to come together financially.

J: A developer is often a problem. In Conantum, the builder was the problem. Karl Koch and the economist friend, Rupert Maclaurin had set up the enterprise went bankrupt and then got sorted out, and for the most part people were all right because they had been paying in

as things actually got done. So no one had half-finished houses. We had a main floor that was finished and a basement that was open to the weather.

B: Right. My mention of Paul Gray leads to a question. One of the thoughts in bringing you to the Sloan School was that you would be positioned to connect engineering to Sloan. So a question would be, did you see yourself doing that? Or another way to ask the question is: as the Sloan School evolved, how do you see its connection to engineering?

J: Well, I think I did see my role somewhat like that. I started to say a while ago, the assumption probably by others and maybe by me, was that it would be pushing forward the area of Operations Research or the use of computers in management. I looked at both of those and decided they really didn't fit my concept of what was important in the outside world, within our scope of what we could do.

Operations Research was well established. It had been successful in WW II. It was already swinging toward being an academic subject rather than a real-world one. And as far as computers in business, computers were being manufactured by that time, banks were buying them, insurance companies were buying them, and there were a lot of people working on how to use them. I didn't think that one or two or three people in the Sloan School were going to affect that very much. So I laid those two aside.

This goes to when industrial dynamics began to take root out of conversations with some people from GE. They were puzzled by why their household appliance factories in Kentucky were working three shifts, seven days a week one year, and then half the people were laid off three years later. The general assumption was that was the business cycle and that demand was rising and falling. I had had, throughout the 1940s, a deep immersion in feedback systems because we were developing control systems for the military for pointing Army guns or stabilizing radar antenna on top of Navy ships. That part of my background led to the field. Anyway, I talked to them about their policies, when did they hire people, under what pressures, how did they manage inventories. Then I set up one page in a notebook with columns for employment, production, inventory, and backlog, and each line across was a week. Based on last week, here's the information you have. Now are you going to increase employment or not? So

you figure out what the next week is going to be based on your policies and where we are now. If you compute that down the page, I found that this was an unstable system even if demand was constant, that you develop this kind of extreme instability internally. Well that was very interesting. And that really became the first what I call system dynamics model.

For that first summer session, we developed what is now called "the beer game." It was the refrigerator game at that stage and then in the K-12 schools it's the soda game. Must have been played all over the world, in every kind of society and any group that plays it behaves essentially the same way because they are sufficiently contaminated by the industrial world that they behave in a way that makes that unstable.

G: Second guessing their orders.

J: Well, trying to act quickly. A lot of people think "Act quickly and you'll be better off." Well, that's the worst thing you can do with that kind of a system. Anyway, the beer game came out of that. That was played in summer sessions from there on and still played for incoming students. It's had a 60-year life and these boards for playing it are sold by the System Dynamics Society, and I think other people sell them also.

We have developed one or two other games out of studies that we've done with corporations. I don't think they've had the traction that the beer game does. The ordering ahead phenomenon. If delivery delays are rising, people order ahead because now it's going to take 8 weeks to get it instead of 7, so we order more. That's very unstabilizing to the system. We had a game based on that at one stage. There was a time in electronics where one type of electronics part, the delivery delay was rising at a rate of one week per week. Every week it would be an extra week before you could get it. And that went on through the fall, and by January they were flooding the market and it was available everywhere.

G: So, if you had an opportunity to come back to the Sloan School, and a year to figure out what to do. The School is preeminent. It has achieved being in the top 5-10 schools in

the country if not the world. What do you think you would do today, given the situation you have, with the knowledge you have?

J: I would suggest a totally different kind of management school. In universities, management schools have been criticized as being trade schools. Take the matter of an airplane. Who are the two most important people in the successful flight of an airplane? I would say it's the designer who made an airplane you could fly, and the pilot who is flying it. Which of those two roles would the manager play? The pilot, isn't it? Pilots are trained in the trade schools. Airplane designers are in the university engineering departments. I have a paper titled, "Designing the Future." What we need is a management school that designs corporations. It's not for running them, it's for designing them so that ordinary people can successfully run them. There are companies that fail year after year. They fire the president, they hire somebody else, he fails, and it keeps on going on. It's because the institution is unmanageable because of its design.

The way the structure and the information flows, traditions, all conspire to create something—I wouldn't say nobody could change, but nobody without knowledge of what's happening could change it. If they aren't really designers, they won't see the reason for it not working.

So the new kind of management school will be very, very different, and more like an engineering school. It will take as much time, there will be an undergraduate program, a graduate program, there will be an internship where you succeed at that.

There is a depth to system dynamics that is not visible to most systems dynamists. The starting point to thinking about it, there are a number of classic structures. In a way they were popularized by Senge's *Fifth Discipline*, except that that book didn't do them justice because he skimmed off the result without ever revealing that those structures he had all came from other people's very intensive work in developing the theories of how those worked, how those structures operated. The problem with just taking the structure and working from it, you only get one mode of behavior from that structure and there are many.

Anyway, this is in my proposition, not been done yet, that probably 20 of those structures, if fully understood, would cover 95% of what a manager ever encounters. But to

understand one of those structures, each one of them justifies a book, it justifies at least an academic term of study. It justifies the many ways that you would study it as such. You would study what leads to it, you would study how to recognize it in the real world.

Some of those structures, if you go out and know what you're looking for, they're right there. For example, a lot of companies have died off because of not being able to deliver the product. They bring out an innovation and the delivery delays rise because they like that; it's a safe thing. So you advertise in your annual report, "We have a backlog twice as high as last year." What that means is you're making customers wait twice as long as last year.

When I was on the board of Digital Equipment Corp, one of the things that was powerfully influential was, "Build it whether there are customers or not. There will be customers"—and there always were. You keep prices up and you keep delivery date down. A lot of companies trade it off the other way. They lower prices in order to raise the backlog and the delivery delay. That's unprofitable.

That structure is the subject of my "Market Growth and Capital Investment" paper, which is a classic in the field. When I was teaching, I assigned that for a couple of class sessions, and then began to realize what the students were missing in it. I had not really thought about how much was there that they should see. I think we ran three to four weeks on that, uncovering things that they really should understand in that structure.

One foundation for such a career might be an undergraduate program, substantially dedicated to understanding those structures, plus understanding the past literature, studying the way things were formulated in different models. I recently saw a paper where they were debating, "Should you formulate it the way I did it in thermodynamics or some other way?" They could have said, "Or the way I did it in world dynamics" because I've done quite different kinds of formulations from time to time.

G: You've shared these thoughts with me before in the past, and I find them very compelling. But as I hear them this time, you raise a really important question so me and that is the age at which we expect people to learn these things. The challenge, of course, is in being responsible for the architecture of major institutions, just like the architecture of a plane. You wouldn't have that be at the undergraduate, or even probably the recent graduate school thing.

J: Why not? No, it would require an undergraduate study. Well, the head of Boston University…

[Recording stops mid-sentence.]

G: ….the editor of the press to actually accept it, and he finally got him to take it home and he said, "Yes, absolutely, we must publish this." That was his first book.

Janice was saying that you had a Norbert Weiner story that was perhaps not ever recorded.

J: This was on a day that C.P. Snow was visiting MIT, at the time of his books on the two cultures—the science culture and the humanities culture—and the gap between them. Background here. Weiner liked to engage in conversations, always his own subject. You could not engage him, as far as I know, in your subject. So C.P. Snow was spending the afternoon meeting with different people in the administration, top people around MIT. Weiner would discover somehow where they were, he would barge in, and start one of his own conversations. Then they would spirit Snow away, to get him away from this to somewhere else, and Weiner would do it again. Another faculty member and I were early for the reception, the evening dinner in honor of C.P. Snow in the faculty club. A member of the administration took the two of us aside and asked if we would try to maneuver so that Weiner did not sit beside Snow during dinner. I was standing beside Snow when Weiner joined the reception, and Snow said, "He's a punishing conversationalist, isn't he?" Anyway, through no effort of ours, the table was set up in a big U and everybody was seated except for one empty seat at the far end of one of the legs of the U. Weiner came in, stood up behind the only empty chair in the room and said, "Where am I supposed to sit?" From that corner he could only dominate about a quarter or a third of the conversation, So he would get up and go out, be gone a while, come back and sit down in his chair. Once he came back while there was an active conversation right around Snow at the head

table. He walked around, put his hands on the back of the chair and said, "What's the question? I know the answer, but what's the question?"

G: You need one of these recorders to capture some of these stories.

J: Well, I think he was very insecure.

G: His manuscript was written in pencil for his book, the tech.

F: Cybernetics?

G: Yeah.

J: Well, he was known earlier for the Yellow Peril, which was a classified paper. I'm not sure I can exactly describe it, but it was a way of smoothing or analyzing data for gun mounts and computers for military equipment. He oscillated back and forth between being helpful to the military and being against anything of that sort.

For a time he was described as "the father of the modern computer." That is, I think, completely inaccurate. He was, in my opinion, nowhere near the stature of John Von Neumann. Von Neumann would engage in any subject you wanted to discuss and within five minutes be right at the heart of the issue, even when he started off by saying, "I can discuss that not prejudiced by any facts." (laughing)

One more. In the early days of computing, you know computers were young women at a desk calculating machine. This was the environment of a computer conference at MIT, and lunch was at the old Smith House down the river. I happened to be sitting at the end of a long table and Norbert Weiner and Von Neumann were next to me, across from each other, and of course the subject was Weiner's. The subject was: how do you translate modern tabloid headlines into Renaissance English words?

G: He chose that subject, obviously.

J: Oh yeah, he chose that subject. (laughing)

END OF INTERVIEW