

**INTERVIEW  
WITH  
JIM UTTERBACK  
NOVEMBER 5, 2013  
Sloan Oral History Series**

J: Jim Utterback  
B: Bob McKersie  
G: George Roth

G: This oral history project preceded the efforts, at least on the 2014 hundredth anniversary of Course 15. We've been doing this for about two years, but probably for the last year we've shifted a bit of our focus to help out with 100<sup>th</sup> anniversary book. Many of the interviews that we have done have been useful for the people writing the book to see interviews and perhaps go back to people that we interviewed and say, "Can we use this?" But we've also recognized in the process of doing that, our intention is to have an archive that will be here 50 years from now when people say, "What happened at the turn of the century?" And many people are able to go back to the founding of the Sloan School. We can't go back to the beginning of Course 15 in 1914.

J: Ed Roberts tried to check up on an early faculty member and unfortunately the personnel files have been destroyed. The school has lost a lot of its institutional memory. It would have been very inexpensive to scan all that stuff.

B: We really start at the beginning, Jim, and ask when you came to MIT, what prompted you to come to MIT? Who were your colleagues? Where were you, and so on? And we'll move through your career at MIT.

You've had a rich career, but you've been in Engineering, you've been at Sloan, and that's clearly one of the things we're interested in, trying to understand how Sloan has been positioned vis-à-vis MIT over the years, and how that's evolved. So it's largely about you and the things you want to share for the record. We'll ask a question about things you've been particularly proud of that you've done here.

We can just get started, unless you have some questions of clarification before we start. Why don't you just tell us when you came and what was the motivation to come to MIT?

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J: I came as a doctoral student in September 1965. I had gotten a bachelors and masters degree in industrial engineering and management science at Northwestern. My master's thesis advisor was Al Rubenstein, who just died a couple months ago, in April 2013, at age 90. I think he had been here briefly on the faculty before he had a long and distinguished career at Northwestern. He was one of the very early people to think that we could study the process of idea generation, development and innovation in organizations in a disciplined way. I guess you'd have to say Al was an organization scientist. I don't know what his degree was in, but he certainly stressed sociology.

He led me to take a course from Donald Campbell at Northwestern, which was certainly an academic highlight. Campbell died a few years ago, and was one of the great thinkers in sociology. Professor Harold Guetzkow was there briefly as a visitor. Al Rubenstein had me minor in sociology, which was great because I met my wife, Peggy, in Richard Schwartz's basic social theory class. We'll be married 48 years in March. I loved the classes, especially social theory and urban sociology with the work that's been done in Chicago about the structure and dynamics of cities. Al was the one who encouraged me to consider getting a doctorate and to apply at Northwestern and Berkeley and here. I was admitted to all three places and decided that a more diverse background was better than staying at Northwestern, as much as I liked it there. Had I gone to Berkeley, I would have studied with Wes Churchman and might have been an operations researcher. It would have been a very different trajectory. But they didn't offer financial support, while MIT did.

The last letter I got was from MIT, and they offered a one-year fellowship, which was ... I thought, "What can I lose by going for a year, trying it out, see how I do?" It was a roll of the dice. I've lived in most of the states, most of the regions of the US. I've lived in over 50 places, but I'd never been in New England. I thought, "Well, that would be cool. I'd get to see a different part of the world."

G: Why had you lived in so many places?

J: My dad was in the Army Corps of Engineers in heavy construction.

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I was quite nomadic until I came here on a lark. I had no idea what a wonderful decision that would be. Al specifically urged me to meet Donald Marquis. Don had just started in 1962, I think. There's a little vagueness about when the program of research on innovation started, but NASA had given Sloan a \$4 million grant to start a program to essentially do work to help them with the moon landing. We did a number of projects about scheduling systems, communication, and contracting, which was Ed's early work.

Ed, and Tom Allen, and a number of others joined MIT at that time. Both were doctoral students, Ed in economics, and Tom the first doctoral student at Sloan. Both Ed and Tom became members of the faculty upon graduation.

G: This was 1965 when you came?

J: Yes, I finished my thesis in December 1968. I'm seen as a 1969 graduate, which is when I walked across the stage. But I've always thought of myself as finishing in 1968.

B: Who were some of the other faculty members? You mentioned Don Marquis at the time you came....

J: I was thinking about this. It's surprising that a number of people who influenced me either were visitors or came as visitors.

Bob Kahn was here. He taught a seminar on his then new book, *The Social Psychology of Organizations*. It was about the dynamics of groups. I just missed meeting Douglas McGregor and Alfred Sloan. I think they both died a year or two before I got here. And Kurt Lewin, the psychologist who did the communications experiments. His equipment was all in the basement.

G: Is it Kurt Lewin or Alex Bavelis?

J: I think they were both here. Robert Bales was at Harvard. Until we were married—this sounds quaint—I shared an apartment with three Harvard guys. One of them was

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taking seminars with Robert Bales, with Talcott Parsons and with George Homans. I thought he must have died and gone to heaven to be taking classes from all three of those people, whose books I had read and admired. Anyway, he was busily working on group interactions according to Bavelis' or Bales' recording technique. So we discussed that. I never went and sat in on any of the classes. I'm not sure why I didn't. All that ferment was going on here.

B: Was your research a piece of the NASA project then for your dissertation?

J: No, I was embedded in that research group started by NASA, but my work was initially funded by a grant to Marquis from NSF.

B: You mentioned some of the other faculty.  
Mason Haire was here as a visitor. He ultimately was on my committee. He decided to stay on the faculty. Charles Savage came from Harvard as a visitor. He was one of Rothlisberger's students, and that was a fascinating thing to hear about. He interested me in international research. We made a trip to South America together, and I made a later trip on my own, to look at the impact of technology on entrepreneurial companies in developing societies, and the role of research laboratories in transferring the technology to industry in developing countries.

That was a fascinating beginning that has always been in the background. That's something I didn't pursue as a dissertation or in any major way. But it was an eye-opening experience. Later I did considerable work in Brazil and other then developing countries, Korea and Israel.

B: What did you do your dissertation on then, Jim?

J: Don got a grant from the National Science Foundation to pursue a database that they had paid for, by Sumner Myers. But Sumner didn't know how to analyze the data. And of course that's not too good a prospect for how well constructed the database was either, but it had a lot of promise. It was a large database of 535 innovations in five different industries. Don hired me to run the computer, basically, which meant a lot of time punching cards.

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B: The cards we heard about earlier!

J: Yes, we had a little IBM 1620 in the basement, and you had to feed cards in, and it punched cards out, which you then had to stand in line to run cards through the printer, to see what you had. In punching out, it gave you intermediate results, which you then sorted out and put in other boxes to run through for further correlation or other analyses. So I did that to put food on the table for my second year here.

That report came out as an NSF report called “*Successful Industrial Innovation*” by Myers and Marquis in 1969. There’s a footnote in it that I ran the computer. It was just one step up from a card sorter. Lotte Bailyn and I, in our earliest work, had to use a card sorter to create tabulations and correlations. I remember her talking about that recently.

B: What was the final title of your thesis?

J: “*The Process of Technical Innovation in Instrument Firms.*” I tried to do the NSF work well, but none of us were happy with the quality and lack of control in the large Myers database. So Don made the suggestion that I do what Tom Allen was doing – having a carefully controlled field study with matched cases and random controls. Tom had gotten involved in NASA’s work on parallel projects. I hope you’re interviewing Tom.

B: Yes, we have interviewed Tom.

J: He probably told you about that. But that was where NASA funded pairs of contracts in early development of critical parts of the moon mission. Each firm got the same requirements, the same budget, the same schedule, and they picked the winner at the end of that phase to go on and further develop whatever piece of the equipment it was. Tom then had a longitudinal study of pairs of more and less successful projects done by firms that were contractors to NASA. Don suggested I do something in the order of a paired comparison of

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innovations. Not just focusing on successes, but a control group and a failure, three projects from each sampled firm.

So the idea was I'd go to firms and identify three projects: (1) one that was a success commercially, (2) one that was a success technically but a commercial failure, and (3) one that was randomly selected. That was a wonderful idea. Further, we decided to emulate what Tom was doing in the longitudinal study by creating a timeline with anchor points, significant events that we could construct interviews around grounded on those significant events. That turned out to be my thesis.

Tom was a graduate student at the time, and then was a newly minted assistant professor and could serve on my committee. He was the first doctoral graduate of the technology and innovation group. He has been absolutely one of the most important influences that I have experienced ever since. He's a guy I would trust my life to. Tom is absolutely a prince in every respect. Most generous person, one of the smartest people and just absolutely honest... he doesn't mince words when you're screwing up. But he gives you very direct advice. So does Ed.

So my committee was Don Marquis, Mason Haire, Tom, and then another new young professor, Jay Galbraith. When I graduated, Jay was the one who influenced me to join the faculty of his *alma mater*, Indiana University.

I went there in January 1969, joined the faculty, and was there for six years. It was a wonderful place to start teaching and start a family. We made a lot of friendships, which we still have.

B: You were probably originally from the Midwest?

J: No, I was born in Monterey, California. My accent is a blend from many places and sounds like the Midwest. I have not lived there except for Chicago while at Northwestern and Indiana while teaching at Bloomington. (I grew up in California, Oregon, Washington, Virginia, Colorado, Wyoming, Iowa, Ohio for a summer, and Germany during the occupation from 1950 through 1952)

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J: We were stationed at Fort Rucker, Alabama, my senior year in high school. When I walked in to register, they said, “What do you want, kid?”

I said, “I’d like physics, second-year German, calculus, and I guess I have to take senior English.”

They said, “We have senior English and by the way you have to take Alabama civics,” which was about why the South was right about the Civil War. This was the year of Little Rock and deep segregation. It was an experience that I won’t even say I’m glad I had it! The good part of the year was that it was a small enough school that I got to play basketball. I’d been at a school with 1,000 students, and half the aspirants, of the 100 kids who went out for basketball, were cut the first day, and another 25 the next day. So I did have a chance to play basketball a little bit. But otherwise, it was a downer.

In any event, I was 16 when I graduated from high school. I should have gone to college when I was 15, I guess, in some respects. I gained nothing from my senior year, except for the class in senior English, which was wonderful.

I was very young going off to be an undergraduate. My dad pointed out that there were five-year programs with work/study. We looked at Antioch, which was way too liberal for my dad’s taste, and Northwestern and Northeastern. I was completely naïve about going to college. I had no idea. My dad had gone to the local college near his farm, Iowa University. We didn’t really know much about applying to college, but I decided of course I was a smart kid, so wherever I applied would be fine. I applied to Northwestern and nowhere else and they accepted me! (I had had the good luck to receive a full National Merit Scholarship).

The co-op program there was a wonderful idea. That was an absolutely right decision. I was so immature when I got to college. I was two years younger than everybody else and the co-op program not only helped me to graduate not so far behind my age cohort, but also the work experience was great. I worked for a summer and winter in Skokie, Illinois, my sophomore year. Then I got a job in Denver, which was like being in heaven. I was in Denver in the summer and winter. Missed the hot summer and the freezing winter in Chicago, and I could go skiing and climbing. I loved it! I had the beautiful fall and spring with football and basketball at Northwestern. It was great!

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G: Before we go on from you coming back from Indiana, you had mentioned there were people who influenced you, whom Don Marquis brought here.

J: Tom, primarily.

G: But you said many were visitors or came as visitors.

J: Well, Mason Haire, Charlie Savage, and Robert Kahn.

G: Because he was known to bring an eclectic group together.

J: I don't know that Don brought them here, but they were here.

G: He seemed to attract them, is what others have said.

J: Warren Bennis was here. Warren ended up being my curriculum advisor during my second year. Bill Pounds was my curriculum advisor my first year. At the time, every doctoral student had to take a general exam in a social science, in economics, and in a quantitative area. It was basically economics, math, and social science. You had to take the field in each of those areas and then an applied area. Innovation was not seen as an applied area then. The doctoral program was unified. There were not so many doctoral students, and you all were pretty much required to be very broad as well as deep in the areas of preparation.

I took Bob Bishop's course in microeconomics. That was a trip! He was really an amazing guy. I'm glad I had a chance to do that. I had a course in heuristic programming from Meryl Flood, who was a visitor.

The school only had 30 or 35 faculty. It was a tiny place, and very drab. I don't know if you remember it. There was old furniture around. There were lots of empty offices. It was kind of an abandoned headquarters for Lever Brothers. They had just built the Herman Building, but we were all over in the old Lever Brothers building, E-52. The CIA had had a big operation in there. I actually wrote my dissertation in an empty office, which had a Cold War



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strategic bombing map on the wall. It was about 8 or 9 feet square, and it was a polar projection, so the North Pole was in the middle and you could see the bomber routes and SAC bases.

G: That was in the Lever Brothers building?

J: Yes. There had been a big study on societal stability and disintegration funded by the CIA for a couple of years at the Center for International Studies... Anyway, that was fascinating to have that map on the wall. And there was a huge safe in one of our offices that had been used for classified documents. So yeah, the place was really drab. There were boards covered with linoleum on top of two drawer file cabinets, even for Don Marquis' desk. It was a good time.

G: And you were in that basement?

J: No, we were on the fifth floor. But the Bavelas and Lewin projects were in the basement, the behavioral lab was in the basement with a two-way mirror. I don't know if you ever saw it.

B: I didn't get here until 1980, but there was one thing about 1980. The place was run by two staff people, Esther Merrill and Peter Gil.

J: Esther was absolutely wonderful! She ran the doctoral program, and Peter was someone you could always take things from. He was responsible for space and property. It turned out whenever a faculty member moved, the doctoral students who were there all night every night got the pick of the furniture. After a couple of years, the doctoral students had much better furniture than the incoming faculty. Peter seemed never figure it out or perhaps just ignored us with good humor. This was before bar coding. He would go in and inventory the office, and then when he came back the next day, it would be empty, and he couldn't find the furniture. We were pretty good at hiding it! (laughing) But none of us had very good furniture. It

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was all junk. (I became a great admirer of Peter when I became a member of the faculty. He was totally devoted to Sloan and its people).

B: That leads to the question: you'd been away at Indiana you said for about 8 years, was it?

J: No, I was at Indiana for 6 years, in operations management and then in management and policy. Don Marquis involved me in a World Bank study of the Spanish economy.

G: He certainly has a lot of different studies. He has an NSF study, he has the NASA study.

J: We lived by our wits back then. There wasn't any flood of money. You had to get support for what you wanted to do. I actually participated in an NSF study for Don, coming back from Indiana and commuting. That led to an article I published in *Science* on innovation. It was a review article. ("Innovation and the Diffusion of Technology," February, 1974).

I met Herb Hollomon (J. Herbert Hollomon) as part of that experience. It was Herb who was running the World Bank study in Spain. Herb is one of the founders of the National Academy of Engineering. He had been a member of John Kennedy's cabinet. He'd been VP of Research at GE, joined John Kennedy's cabinet as Assistant, and then Under Secretary of Commerce. And then went to University of Oklahoma to be President. Jerry Weisner asked him to come back here to run something in the Engineering School, rather like what we're starting again, a center on innovation and entrepreneurship, only a policy-oriented program. (The Center for Policy Alternatives) Herb was running that project in Spain, and he had met me through the earlier work for the NSF that Don had been involved in. He invited me to take part in that expert mission. I spent 3-4 weeks in Spain and then a couple of trips back here.

B: You were still at Indiana at this point?

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J: Yes, I was just coming back to work on research. We were riding in a taxicab in Madrid and going from one company interview to another, and Herb turned to me and said, “You know I like the cut of your jib. Why don’t you come to MIT and work for me in my new center?” which was part of the Engineering School. I said, “Oh no, I couldn’t possibly do that. I just got promoted to associate in Indiana. We just bought a house. We just had a kid.” (laughing) I thought about it overnight, and I asked him, “Maybe I should talk to my wife and think about this a bit.” I did, and I had a chance to talk to Bill Pounds and he crystallized it. He said, “Jim, is this a time to be avoiding uncertainty or welcoming it?”

I thought, “Gosh, if I’m going to settle in my new house, academic community, I’m dead. I’ve got to get out of there.”

So we sold the house and came here for two years. I also had a visiting appointment at the Harvard Business School at the time. That was a precondition for coming. I didn’t want to be a research associate and not a faculty member. I was visiting and teaching at Harvard while working on research here at MIT (at Richard Rosenbloom’s invitation while he was on sabbatical at the Hebrew University).

After two years of that, it was time to consider going back to Indiana. Peggy looked at me rather wanly and said, “Surely someone as clever as you can figure out something you’d love to do here in Boston.” She really didn’t want to go back. We stayed.

Harvard wanted me to stay on their faculty, and Herb promised that I could join the Sloan faculty here. The first year, the position in operations management—it’s ironic to think of this—was given to another person, Gabe Bitran, on the premise that he was a core operations management guy. He couldn’t be anything further from that today, but he was seen as preferable because he was deeper in math than I was. I was more on the periphery in this innovation, product development, product change area, which seemed not to be central to the operations management group. We were all kind of downcast and he said, “Be patient. It will work out next year.”

Next year, the Sloan School had a hiring freeze, so that didn’t work out. I said, “Herb, I really can’t stay just in this research associate position. I want to be a teacher and faculty member.” And, to quote him directly, “If you’ve got a position in the Engineering

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School, would that be OK?” I said, “Herb, that would be a dream come true. I’d love to be in the MIT Engineering School.”

And he did something extraordinary. There wasn’t and still isn’t a Department of Industrial Engineering and Management Science in Engineering. Herb put together an interdisciplinary committee. Ed Roberts was on it, Amadeo Odoni from Engineering and Jacob Goldman, who recently died at 90. Jack was the founder of Xerox Parc, and architect of the personal computer revolution. I think that’s not overstating the case. I’m not sure I know who the other several members of the committee were, but they put together a case. At the time, there were only four or five faculty across MIT who were brought onto the faculty the way I was brought on, which was both good and bad, because I was kind of a stateless person. I was a tenured associate professor, reporting directly to the Dean of Engineering, without departmental status.

B:           What year was this?

J:           That was December 1979. I will not forget that because Robert Seamans, who was the Dean of Engineering, walked over personally to give me the letter of appointment. I thought the world of Bob all the time I knew him, and I think it’s a tragedy he wasn’t alive to see the 40<sup>th</sup> anniversary of the Apollo landing celebration we had here four years ago. All the people who worked for him at NASA were there, including Buzz Aldrin and Neil Armstrong and Harrison Schmidt, three of the twelve who walked on the Moon, and Cape staff. It was one of those times at MIT that was unique and exceptional and unbelievable. The first guy to step on the moon and the last guy to leave it were there in the audience. Chris Kraft, who did the countdown, it was amazing...

But anyway, Bob walked over to give me the letter. He had directed the entire Apollo Program, while James Webb, the Director of NASA dealt with the broader political and budget issues.

G:           It’s really extraordinary, the people you’ve been connected with by being in the Engineering School, which wouldn’t have happened.

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J: It was great to walk into the Dean of Engineering's office and see the moon with the earth rising above it. He was the one who enabled that to be taken. That was great.

B: You were working then with Herb.

J: In his area.

G: In his World Bank study or something beyond that by then?

J: I worked on a number of studies. There was a big study of entrepreneurship in Brazil that I helped to lead. That led to what would be a \$2 million study today of entrepreneurship in Sweden. That ultimately led to me being named a member of the Royal Swedish Academy of Engineering and getting an honorary doctorate from Chalmers University for the work I did in Sweden. I've become kind of an honorary Swede, and not many of them realize my name is really German and not Swedish. It's Otterbach in German and Utterbeck in Sweden (meaning otter brook). So when I come to Arlanda Airport, I hear a "welcome home" at passport control.

B: On the appointment you had, did it have any teaching? Or was it pretty much research?

J: No, I was teaching all along, even as a research associate.

B: What courses and where at MIT?

J: It's been a similar course all along, with different numbers and different names. Before I was named to the faculty, I had to have a super-numerary faculty member. So I taught with Tom Sheridan in mechanical engineering. I'm sorry, I'm not going to remember all the

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titles, but it's always been about product and process development in industry and how to do that. Tom Sheridan is a pioneer in what is now called haptic robotics.

I taught a course with Bob Logcher in Civil Engineering on project management and scheduling. That was really fun. We worked together for a couple of years in that area. Bob was an expert in managing huge construction projects, and we learned a lot from each other.

The Center ended after Herb had a crippling stroke in December of 1979. It went on for about 18 months after that. I was the acting Director for six months, and then Herb came back to try to keep it running for a while, but it was a difficult story. In any event, when the Center broke up, Herb and several others went to Boston University.

I stayed to run the Industrial Liaison Program for six years, but I continued teaching all those six years. The course was on product and process innovation, and it never really attracted a very large group of students, maybe 20 or 30 or 40 students.

Then Sloan went toward a regime of bigger classes, and I was wondering what to do. Ed Roberts said, "Why don't you call it disruptive innovation? You need a catchier title." So we changed the name of it, and the student enrollment rose.

More recently, I've taken up teaching a course called Technology Strategy (based on the dynamics of technological change). I've done that on and off. I've also taught entrepreneurship for five years for undergraduates. That was a lot of fun. The undergraduates are simply a delight to teach.

B: What years were you heading ILP?

J: From July 1982 to I guess December 1989.

I participated in four different proposals to have a manufacturing center on our campus. One was a proposal to the Army. Another was a proposal to IBM. Neither was funded. And finally the LFM (Leaders for Manufacturing) program idea got going and there was a big push to raise \$40 million to get that started. Tom Magnanti and Kent Bowen were leading that from Sloan and Engineering respectively, and I helped to raise a small amount of money, about \$2 million. I helped raise \$1 million from the Eli Lilly Foundation and \$1 million from Johnson & Johnson. We were talking all the time about how important it would be for MIT, and for the

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country, to have a program in manufacturing. When it finally came true, Kent called me up and said, “OK, Jim. You promised to help us out if we got this going. So you’ve got to leave the ILP and come and help out.” So I did. You and I and Tom Lee wound up teaching the pro-seminar the next year. That was a wonderful experience. I got so much out of working with the two of you. That was just great. I don’t know if you feel that way...

B: Takes me back! I hadn’t thought about Tom Lee for a long time.

J: I loved working with Tom, and I loved meeting his buddies, who were basically the people we had come to speak to the students. (The pro-seminar guests included Prof. Russell Ackoff of Wharton and Alan Mulally, then of Boeing) That was great, starting up the Leaders program, and getting the first couple of classes together.

I’ll tell you what I remember about the school as a student and all the characters and classes.

B: You’ve done some of that, and we want to keep moving because you don’t have a lot more time. We can cycle back at any point, Jim. Your ability to put this into a chronology and providing the names is just terrific, what you’re able to recall.

J: Yes. OK, to recap. I’ve been a tenured member of the engineering faculty since 1979. When the LFM program started, I was still reporting to Jerry Wilson in my faculty position, because I didn’t have a department. As you can imagine, since I’d wound up in this administrative job, I was still reporting to Jerry for salary reviews and things, surprisingly. That was a little bit uncomfortable. He had some words with me, basically that I would never become a full professor under his command. There was no place I could fit, and until a department or school came forward with a recommendation to promote me, he was not going to consider it.

I had good relationships in Mechanical Engineering and Course 6A, which is basically the Electrical Engineering internship, the group of electrical engineers who want to go into industry. And excellent relationships in Ocean Engineering and Materials Science, but I

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really didn't fit in any of those faculties particularly. Maybe Mechanical Engineering, but that would have been a stretch.

The suggestion was basically that I seek to have Sloan appoint me as a full professor. Sloan wasn't having any of that. The view here was there was an endless supply of obsolete engineering faculty who thought they could teach management. Besides that, I'd been on the administrative track for six years. The whole proposal wasn't greeted too warmly. Essentially, I got an office for four years without status, and my name wasn't even in the Sloan directory, which confused a lot of people.

G:           Make an offer to come to Sloan for four years.

J:           No, there was an attempt to get Sloan to invite me and it collapsed. But in the meanwhile, Lester Thurow said he had an office I could have. I moved over here from the ILP with the expectation that Sloan would think about naming me to the faculty. That didn't happen for years, and finally happened in 1994 – I think with a lot of controversy, even now. I'm not privy to all the discussions, but you know the place is pretty... there's a lot of gossip that goes on. The gossip wasn't all very complimentary.

B:           But you were still teaching during this time?

J:           I've been teaching here, literally, since 1975. In Engineering and Leaders for Manufacturing. I might have still have the Course 2 course number. I don't know. I've been in a very strange place all along. I'm not really considered legitimate in engineering, although I've been a tenured professor of engineering since 1979.

I don't honestly think I'm considered legitimate in Sloan, to tell you the truth, at least in terms of the central kitchen cabinet here.

B:           But you're considered part of the teaching group, the innovation...



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J: Oh yes, I have two hats. I'm a professor of engineering systems and a professor of innovation and entrepreneurship and strategy. My classes are all with a 15 number now, but my students are from engineering and Sloan. The undergraduate entrepreneurship class was a Sloan class, but again the students were all from engineering and maybe a few Sloan majors (and usually five or more students cross-registered from Harvard). The disruptive innovation class had a minority of Sloan students and students from all across MIT in it.

And honestly, that's what attracted me to MIT. I think a sad thing is that we don't have a real mix of students in our Sloan classes any more. The bidding system has been a death knell to having a real diverse student body here. For years and years half the students in most Sloan classes were engineering and science students from across the campus. And that was just a wonderful mix. I don't think that's the case any more. We have special sections of some classes for engineering students.

G: But it misses the diversity they could have.

J: Yes. We've become a more standard, large MBA school. The thing about MIT that's always been a lot of fun has been the entrepreneurial dimension of it and the mixture of students. I really enjoy teaching students who have experience and ones who have a technical dimension. I guess if you read the introductory bio about me when I joined the School faculty, I've helped to start four masters programs, and they've all linked engineering and management.

The Leaders program was actually the second. The first one was Management of Technology, which is now part of Sloan Fellows. The third one was System Design and Management, which I still teach. The fourth one was called Biomedical Entrepreneurship. It later became Biomedical Enterprise. And that's fallen away now because of changes at the Harvard Medical School. It was joint with HMS. Well, it was with HST, which was joint between MIT and HMS. We have maybe 150 alumni who have dual master's degrees in Biomedical Enterprise. They're doing extraordinarily well. It was a wonderful, rigorous program for the years it was in effect.

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It was the LFM model applied to medicine. We literally had the students take pathology and dissect a cadaver. They took five or six courses at the HMS. They always struggled to pass them. We never were able to have more than 15 students in the cohort because of the limitations on operating room and pathology classes at the medical school. But these students have an extraordinary background. Imagine. They're unique students in the US who are designing diagnostic tests and medical instrumentation procedures and other medical applications, who have literally scrubbed up and put on green clothes and worked in the operating room. They were not allowed to touch a patient, but they were allowed to take data and organize things and observe the operations, and so forth. (I greatly enjoyed working with Prof. Martha Gray and many others in developing the program).

G: That's obviously been a very innovative and successful industry.

J: The program has ended. Some political things happened at the HMS, which I don't understand. It had nothing to do with our program, but it had to do with the relationship with MIT. There were internal politics at Harvard that led to the disruption of their half of the alliance, which then led to the disruption of HST here. I don't understand all the details, but it has been reorganized.

Our program was kind of collateral damage. But it was a very successful program. It was part of the Cambridge-MIT Alliance and it continues at Cambridge. Ekhard Salje (then President of Clare Hall, Cambridge) told me he thought it was the most successful professional program of that alliance. They still offer the degree.

G: It's interesting to me because, of course, I'm familiar with your work from the outside. Your writings, the things that have had a big impact. Obviously, the organizational implication is that innovation always takes place at the boundaries.

J: That's what I've always loved about MIT. There is plenty of boundary-spanning around here, and you don't get consumed by working in a multidisciplinary or boundary-dialed position. It's not always comfortable.

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I'm probably much more pessimistic about my role within Sloan than I should be. It certainly was wonderful to be given a chaired professorship. Sloan has treated me very, very well. But I still feel a little peripheral, and I think our group is a little peripheral as well. Most of the members of our group have engineering or science degrees and are only later management scholars. Ed's first degree I think is in electrical engineering. Fiona's is in biochemistry. Matt Marks has a couple degrees in engineering, as I do. Tom Allen is an aeronautical engineer. Eric Von Hippel founded one of the first fax companies. I think his degree is in electrical engineering. Steve Eppinger and Charlie Fine are associated with the group. Steve is from mechanical engineering, and Charlie's from operations management. But we're a little bit strange within the Sloan context...

G: One of the things we've been interested in is how MIT sees Sloan. I'm wondering what your view was when you were in ILP, because you were back looking over all of that. Although ILP is so much more engineering focused than Sloan.

J: The six years at ILP, the hot topics were always different each year. Every year the searchlight was on something else. Sloan was very popular with the companies. I made a big transformation of ILP, which isn't widely recognized. We changed the focus from memberships and visits, which of course are still important, to helping the faculty raise money from industry.

When I took over the ILP MIT got about \$15 million a year in research money from industry. When I left, it was about \$50 million, and today it's north of \$150 million. I told the staff they were to focus on introducing the faculty to company people whose research was of interest, and help them build an association. Then I went around to all the department heads and deans and asked them what they wanted us to help with in the way of conferences and symposia and visits and travel.

Mark Wrighton (then head of Chemical Engineering) was the most thoughtful of all the people I talked to. He was worried about the whole legitimacy of chemical engineering. He asked me to help introduce younger faculty to corporations and the major issues of practice in chemical engineering.

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I oriented the ILP to help the central mission of MIT, and Paul Gray agreed to revise the mission statement to say that.

B: What about the view of Sloan from Engineering?

J: We pride ourselves on being rigorous and scholarly. The people over on the other side of Ames Street don't see that at all.

I served on CAP most recently. This is a committee that deals with students who are in trouble. There are faculty there from each school and a couple student representatives. We were dealing with one student in difficulty, and one member of the committee said, "We should send this student to Sloan." I let them have it. But that's the view, that we're the place for failures.

G: It certainly goes back to the 1950s and 1960s. We've heard people say that, that this was just a remote place over here.

J: We're like a little brother trying to impress an older brother. We're not helping ourselves either. The truth is that we're an engineering and science dog with a management tail, and a school of architecture tail, and a little humanities core. All of which are very fine in an objective sense. But Sloan's research budget has been stagnant for the last decade. I think it would be kind to say our research budget has grown. We're the only place on campus that offers a graduate degree without a thesis, something I've always opposed. We're just not part of the research culture of MIT.

G: But in many ways, Sloan has grown its program by being consistent with other top MBA programs.

J: Yeah, and who at MIT cares? I said the other day, much to the ire of one of our administrators, that we're a commodities school in a saturated market. We're a very fine commodities school.

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G: We're at the top end.

J: We're at the top end. We produce some of the very best. But there's nothing distinctive about that (compared with our top competitors).

I talked to a colleague in another university who said of Sloan, "The trouble is business schools are cash cows. But you're not." We're lavishing money on overhead and services to stay on top, and I'm not sure it fits with MIT. (We are taking the surplus from the MBA program to support the doctoral program, which I strongly support, though at the rest of MIT doctoral work is largely supported through research grants).

When I was a student, there was huge excitement here about cutting-edge research: System dynamics was a new idea; Decision-making tools were a new idea; Heuristic programming; Bayesian statistics; Theory X and Theory Y; the Scanlon Plan were all new ideas. Participation of labor in thinking of all people as intelligent and creative participants in an enterprise was exciting. The whole area of organizational change was a big buzz. Artificial intelligence was an exciting area. Everybody was engaged in research. Our whole group was one of three or four pioneering groups in the world in looking at innovation and R&D. There is a lot of fine rigorous research going on here now, and some individuals are doing pioneering work that's very impressive, (but perhaps there are fewer sponsored group projects). (I would urge priority be given to expanding sponsored research and reinforcing the doctoral program which is a point of distinction).

G: I think it shifted over to the Media Lab. When you look at some of the things they're doing.

J: Oh! The Media Lab is astounding.

G: Sandy Pentland's work and new book on Social Physics.

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J: Yes, Sandy is formally a member of our group, but isn't a central participant. He will advise theses and welcome students in his classes.

B: We have to wrap this up. You have to be somewhere at 4:00.  
You've mentioned recognition for your work, but what are you proudest about?  
As you think about your career, what do you want to be remembered for?

J: Absolutely my doctoral graduates, who have gone on. The influence you can have in a position like this is astounding. Allan Afuah just got a big award from the Academy of Management. He was here as a masters student. He did a masters thesis with me, which I admired. As I was signing his papers, I said, "You know, I bet you would love doing research and being a professor." He was a supercomputer chip designer at the time for Intel or for DEC. Here's a kid who hadn't seen a machine until he was 8 years old. He grew up literally in a grass hut in Cameroon. His path to becoming a supercomputer chip designer and then coming here for his masters and then coming back for his PhD, and now being a professor at University of Michigan, is an amazing personal saga. He came back to the doctoral program three years later. I saw him at AOM in Hawaii and we sat down and had dinner. He said, "You know you changed my life!"

I said, "Allan, what did I do?" He said, "Remember when you were signing the sheet for my masters thesis, you said I might enjoy getting a doctorate?" It was what Al Rubenstein did for me. It was a sincere comment, but it wasn't a calculated comment.

He said, "Well, I thought about that for three years, and I came back, and I absolutely love teaching."

That's what makes a difference. I could tell you about each of the various doctoral students. A couple of them are deans. I think three or four of them have been deans or are deans. (Including Yar Ebadi, currently dean at Kansas State; Linsu Kim was dean at Korea University and then head of Korea's NSF; Robert Collins was dean at IMD; Robert Taylor was dean at Louisville).

B: They've come through which of the programs that you've been affiliated with?

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J: Well, more from early years at Indiana have become deans. Beyond Sloan I have advised students from engineering, and from Harvard, Chalmers University, and the University of Cambridge. As I've gotten older, I've started to refuse to chair doctoral committees because honestly they're going to need someone to be active in 6-8 years to be their champion. So I have agreed to be on their committee but suggested they should have a younger chairperson.

B: This has been terrific!

J: Thank you.

END OF INTERVIEW