INTERVIEW WITH
ED SCHEIN
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Sloan Oral History Project

B: Bob McKersie
E: Ed Schein
A: Alan White

B: We are talking with Ed Schein. The place to start is, what year did you come to MIT?

E: My first exposure to MIT was as a graduate student in 1949-50, when I took the course, which at that time Alex Bavelas was teaching in the basement of E-52. We had a big laboratory with all kinds of Ampex tape recorders and whatnot, and Hal Leavitt had his cubicles set up with what became the first classic study of organizational design. All of Hal Leavitt's work was done in the basement of E-52. At that time, I wasn't aware of Leo Moore's production lab, but after I got my degree and spent time in the Army and decided to come back to Sloan in 1956, Bavelas had left so that part of the basement was no longer involved with the group dynamics research. But Leo Moore's production lab was very active. Leo, and his colleague (whose name has slipped my mind)…

A: Yeah, they wrote a book together. Goodwin??

E: Yes, Herb Goodwin. The reason I think it's very important to emphasize that is because Leo talked at length when I knew him, about how both Deming and Juran had been around and were influenced by that lab. A lot of the original ideas around lean manufacturing probably had some part of their origin right here in the 1950s. We never evolved them, as you know, until after Deming had his success in Japan. But it's important to realize, because I think it's part of the Sloan culture to be incredibly innovative. We were ahead of the game on a lot of these things.

B: Organizationally, where did Bavelas and Moore have their appointment? There was no Sloan School then. Were they in the Economics Department?

E: Course 14 was economics and social science, I think.

B: So it was much broader then.

E: It was much broader. It's where McGregor had his first appointment. And it was under that department that Kurt Lewin was brought here in 1947. And it's the department that gave Warren Bennis and Herb Shepard and whole slew of group dynamicists their Ph.Ds. So you have this anomaly that all these famous social psychologists had MIT Ph.Ds, which didn't make sense. Except that Lewin had this year or two before he died, of great activity, where most of the group dynamics club, or band of people who became the THE field, came through here in 1947, 1948, 1949.

A: Just a little side track, but here's a question I'd like to ask. You mentioned how innovative things became here. And I think they continued to be over the years. But why all this ambition, and why this lack of ability to publicize these things? We bemoan that fact to this day. We have a dean who's now very concerned about this, and yet it's almost as if he's going uphill all the time trying to get publicity for the things that have been done here. What is it culturally about this place that works against that?

E: Innovators are not necessarily developers. I don't think we're developers; I think we're inventors. And I think there is an exchange between John Van Maanen and Jeffrey Pfeffer, which was published and is worth pulling out, because we had this problem with Organization Studies, of feeling that with Beckhard and McGregor, Nevis, Bennis, and Schein, we were the preeminent department in the world. And yet, we got very little visibility or credit for that. And Jeff Pfeffer said, in print, "It's because you guys don't have a single paradigm that you keep pushing. You keep inventing new things and not really sticking to one paradigm." He kept emphasizing, "You don't have a paradigm. You have new ideas and new approaches, but you don't settle on one thing."
        John Van Maanen countered with a very strong article saying that sticking to a paradigm reduces innovation, and it's our desire to innovate that is more important than to latch onto one

theory and just push it and do all the research, and all the graduate students would be told to research on it. It could have been Theory Y, we could have had a whole army of people doing research on Theory Y. But I think the kind of people who Penn Brooks and Eli and others hired weren't constituted that way. They were hired for their innovative capacity, for their desire to do new and different things. And that's what you then get. If you hire innovators, you get innovation. And you have a hard time buckling down to any one thing.

Now, I always thought that the ILR group might be a little different. But I'd have to have Bob comment on that.

E: When I first came to Sloan, I noted that in the upper, or the southwest corner of the 4th floor, were three major figures in the field: Doug McGregor, Jay Forrester, and Doug Brown. They had offices adjacent to each other and were in constant communication with each other. Charlie Myers was there, Paul Pigors was there. So at that stage, I saw the human side of enterprise being pretty well integrated. You had McGregor pushing the psychology of it; Doug Brown more the labor economics; Paul Pigors experiential learning, one of the early proponents of the incident process stuff and some personnel psychology. It all fitted together, and it's a bit of a mystery to me why it couldn't stay together – except for the explanation that both Charlie and Doug Brown had come up through economics, and Doug and I and subsequently the people who were hired, came up through psychology. And I think to this day, those are different paradigms and that leads to some different ways of thinking, which I'm observing now, very interestingly, in the IWER seminar. When I asked myself, "What is different about Paul Osterman and Tom Kochan in the way they approach the seminar?", it's this real deep commitment to "we have to figure this out on an institutional economics level. That's the driving force.' And I'm sitting there saying "I've got to figure this out on a micro level." So my comments always are, "how is this actually working down in the trenches," and Paul Osterman is trying to figure out how these broad institutional forces are changing the world.

So that's probably, over the years, I'd be curious how you see that, Bob….

B: No, I think that's really a very accurate characterization. And even though we've moved away from unions as the primary focus, much more to the employment relationship and a lot of

other topics under that umbrella, the orientation is still institutional. Policy is another way to define it.

Well, when you came here as a faculty member in 1956, when you arrived some of the things you had mentioned were going on in the late 1940s had been disbanded. So what was here then in the broadly defined behavioral area in 1956?

E: When Kurt Lewin died, I think in 1948, the Research Center for Group Dynamics disappeared. It had done a lot of its best work. The Warren Bennises had gotten their Ph.Ds. But Course 14 existed and was quite healthy. There were students who were taking industrial managerial related courses under the Course 14 label until probably 1952 when the Sloan School was founded. So you had in the 1950s, Warren Bennis and Herb Shepard, who co-taught a T-group course. Students could take a T-group at MIT, which is very unusual and suitably controversial. But I think Course 14 was the big transition thing. I think when you looked at some of the earlier leaders like McLauren, I think he came in under Course 14 originally. Or Schell, or the original Sloan Fellows Program was a Course 14 program, I believe.

So all that really happened is that you put a graduate school on top of Course 14 and then split off Course 15 as a subordinate thing of the graduate school. But I don't know that Course 14 and 15 were that different at that time. It would be interesting to know, historically, why you needed Course 15. Why Course 14 couldn't have carried the undergraduate load as it had before the Sloan School was designed?

Then the other point that bears on Alan's question: McGregor and Shapiro and Penn Brooks, the new dean, really were committed to doing something different than the HBS. So this message that I got personally, "Do what you want to do, so long as it doesn't resemble what they do up the river. We want a new approach." And underlying that new approach was the theory: "We should build management education on disciplines rather than on practice. DBAs are not welcome. It has to be Ph.Ds in math, psychology, economics. We don't want practitioners, we want to see what happens if you build a management curriculum out of basic disciplines." Which led to people like me coming in with literally zero knowledge of what management even was. I didn't know the word, you know? I had been in graduate school and in the Army, so I had experienced organizations, but the concept of management was a complete black box. And I think they wanted that. They wanted people like me to say, If someone with a different approach

gets involved in management, let's see what that does." In my case, it produced right away this interesting concern about whether companies brainwash their people. I had done work on the POWs, so one of my first articles was "Management Development as a Process of Influence." In Crotonville we had the "GE Indoctrination Center." That time in the 1950s people were not apologetic about it. They said, "Sure, to be an IBM-er you have to learn to sing songs, etc." For me, that was an easy transition. Oh yeah, management is brainwashing.

B: And on this model of emphasizing disciplines, there had been that Ford Foundation report – was it the Gordon Howell Report? I don't remember the exact date, but it certainly influenced a number of business schools to move away from just talking about practice, to the undergirding disciplines. Was there any talk here about that report as a driver for – or was it just more in the MIT tradition that we're heavy in the sciences… and…

E: I think that report was very important, but whatever impact it had, had already happened. So I heard many references to it as something that we needed to pay attention to, but something in the past. We were past it. I think it must have been part of the discussion when they hired Penn Brooks. What Eli, and Doug, and Penn would have been talking about probably would have been a report like that. But it didn't filter down to my level. By the time I got here in 1956, the course was set. Later on I got involved with the Ford Foundation program on higher education and did a volume on Professional Education: Some New Directions stimulated by Clark Kerr.

My colleagues were: another social psychologist Marvin Shaw, and a very quantitative statistician Warren Torgerson. So you could see in the hiring pattern where they were going.

Psychology was migrating, in Course 14, toward more traditional perception, linguistics, physiological. MIT had an incredible number of very big name psychologist: Roger Brown, who became a major figure in the understanding of bilingualism; Licklider, who was a major psychologist in the development of computing; John Swetts, who went to Bolt, Beranek and Newman. There was a very close connection to the Harvard psychology department, and I joined that group because I was a psychologist. They were really my buddies, even though they were into more perception/cognition stuff. They had something called Pretzel Twist, which was a monthly meeting of all us MIT psychologists, where we got together and just had open seminars. That eventually migrated into the Psychology department, which became much more

physiological. When they hired Lucas Taueber, they said, "We have to study the brain, We have enough of this behavioral stuff." They had Dick Held, who was a close friend, who did all sorts of seminal research on vision and cognition. So there was always a kind of early linkage to psychology. But the more they got physiological, the more I lost that linkage, as did Warren and others.

Psychology went off on its own track, and I began to push more toward sociology and anthropology. I don't know when John Van Maanen came, but by the time he was hired, I was really concerned that psychology was dominating the field, and that so much of what was really going on in organizations was better understood through the sociological/anthropological lens.

B: John was probably hired in the 1960s, would you say? Or later?

 E: I have it somewhere, I don't recall.

A: I think later, because I don't think he was here.

B: I guess the more important question is how Organizational Studies began to cohere a group?

S;   Right away, under McGregor. Right away. He was one of the founders of the school, so he and Forrester and Doug Brown and Myers were emperors who could build their empires. And Eli. So you saw the building of economics and finance around Eli; you saw the growth of system dynamics around Jay; you saw the beginning of the IR group around Doug and Charlie and Pigors; and you saw Organization stuff around McGregor. I say "organization stuff" because at that time all the schools were calling it OB, Organization Behavior, and I'm the one who said "No, that just doesn't make sense. Organizations don't behave." So I convinced people to call it Organization Studies. I just couldn't stand the idea of being identified with an OB department. So Organization Studies it became.

And… very… psychological because of whom we had hired. There was a transition period in the 1960s that I'm not clear about. Warren Bennis came back from BU, so there was this real flowering of organization development. Doug had brought in Dick Beckhard part-time,

so when you had Doug, and Dick Beckhard, and Warren and me the next generation, and we were all going in the summer to do training groups the label "organization development" began to evolve. This would have all been in the 1960s. Warren and Dave Berlew and Fritz Steele and I wrote this book called *Interpersonal Dynamics*, which went through three editions. It was, in its time, one of the greatest texts of all time, because we pulled together stuff from literature rather than having all formal research. Experiential learning really took hold, with Kolb, Rubin and McIntyre creating the basic social psych course as a self-administering course without a lot of TAs. They produced a book called *Organizational Behavior: The Experiential Approach,* which would be handed to a class and they would say, "Here, you can do this all yourself with the exercises." I think we now take experiential learning for granted, but there was a period when that was really a new idea, to have students have experiences before the concepts. That's the key. You don't illustrate a concept with an experience; you have the experience and then you let people figure out conceptually what was going on.

A: And see, I met Dave Kolb at a Peace Corp training conference, and then invited – I was director of that training center in Hawaii – and invited him out to go over that model, his learning model. And then we used it at the training center. That was back in the 1960s. I was very disappointed when he wasn't awarded tenure at Sloan. He's at Case now.

E: But recently we've been talking about action learning around here, like it's a new thing, which (laughing) I said, "Hey you guys, you better go back and get some history because it's been around a long time." Of course, it was around a long time before that too.

B: You mentioned Doug McGregor and Theory Y, which became kind of a sensation across the country. That was really – but did that just represent Doug's contribution to how we think? Or was it kind of an organic part of the group?

E: The thought that you've triggered is that content innovations get more visibility than process innovations. Psychoanalysis is a perfect example. People argue about the validity of the psychosexual development or whatever, but everybody takes it for granted that a certain kind of interviewing will release momentous forces. So you could say Freud totally revolutionized the

way we talk to each other in terms of the whole notion of how you get at people's anxieties, etc. And Karl Rogers and others are latecomers. It's the early psychoanalysts who created a vehicle for the study of the mind.

So you could say OSG created experiential learning as a concept, it was deeply involved with process stuff, how you teach more than what you teach. And that gets taken for granted immediately. People say, "Oh, yeah, that's good." But you don't give Nobel prizes for process innovations. This was a point that Bob Thomas made in his book *What Machines Can't Do*, that the US gives all sorts of prizes for content innovations, new products etc., but there aren't very many prizes for the engineering processes that the US has invented and taken for granted. And that might be another key to MIT. That MIT is more in the process business than in the content business because of this *Mens et Manus*. We want things that are practical, which immediately pushes you into process stuff.

B: And why would you think – because Theory Y had some of the trappings of content?

A: Right. A paradigm of sorts, right?

E: Right. But then we didn't do a lot of research to document and develop scales and show how – Doug McGregor knew from observation, the theory why managers perform better. But I don't think he or anybody else was interested in studying 10,000 managers, giving them a scale, rating them somehow. It was so obvious. And that's the cross I bear. Some of the things I'm interested in are so damn obvious that if you take somebody else and say, "Look, this is what culture looks like," they say, "Oh yeah, yeah, I don't need a survey." But if you don't have a survey, and if you don't put numbers on it, it doesn't seem to count… It's that clinical stage of science where we're still at that early stage of learning what to observe. Instead, we're going around measuring a lot of stuff that isn't worth observing.

B: At Sloan, there is a certain ambivalence about… we're *at* MIT, but are we *of* MIT? We certainly have solved that now in terms of our brand, because we refer to ourselves as MIT Sloan. So we are clearly embracing that. But I can remember when I was Deputy Dean and Lester was Dean, we were wrestling with this issue. You mentioned the group of psychologists

from across MIT that met together? How would you characterize how people in Sloan, back in the 1950s, thought of themselves vis-à-vis the MIT umbrella?

E: I think when we were smaller, we felt much more integrated and had more interaction. I knew these psychologists; I knew there were people over in Urban Planning, Don Schoen , for example. I think, also, we were called the School of Industrial Management, and then became the School of Management in order to allow medical schools and other kinds of institutions to feel that they were part of it. I don't know exactly when and how we dropped the "Industrial"….

A: I think Bill Pounds did that.

E: But that was important, because we did have a broader purview. My hunch is that less and less contact arose, maybe less from our sense of isolation, and more from MIT's indifference to what we were up to. I never got the feeling that anybody over there cared much about the fact that we had some pretty good social psychologists and organization people.

        I remember one year, I was working with the Information Technology folks. They were trying to do something about organizing IT differently. I was there because of process consultation. I had gotten enough publicity around the book that even though I'm an "insider", I was involved somehow. But it struck me like a ton of bricks that they did not use Jack Rockart, who was at that time a world authority on information technology. Either they didn't know about him, or he was an insider…? I think there was always a problem. I wonder whether the administration used any of our finance people in helping them with their investments. Maybe they did, but I had the feeling that….

A: Well, they with Glen Strehle, as a graduate, anyway.

E: Yeah, I guess Glen… But the theme seemed to be that we were lower status, we were not "real" scientists or "real" engineers. And therefore we don't get consulted, and therefore that feeds the isolation.

B: But we've been working very hard at this. You have Jake Jacoby working heavily on global warming. Leaders For Manufacturing was a bridge.

E: LFM was a huge bridge, yes.

A: And the new energy programs, a lot of engineering involvement.

B: Isn't Dick Schmalensee heavily involved?

A: Oh yes, and Don Lessard. He's played a big role with the new curriculum and graduate curriculum. So there are some bridges, but they've been tough to accomplish.

E: And the ones you've mentioned are all in Economics.

A: Yes. I don't know of any in IT. A little bit with the Media Lab, I guess. Gabe Bitran's been doing some work over there. And we tried forever to get the president to at least say something about – not just say "science and technology", but say something about "management". But it doesn't roll easily off the tongues of the …. Never has. But I think there's a growing recognition of what the school can contribute, at least in terms of areas like leadership in the Institute. John Reed's the new chair of the Corporation. And Tom Kochan and Lotte were both chair of the faculty. I think there's some growing recognition in those areas. But you've got some hardcore scientists over there who don't want to recognize the management school. They always say "Sloan is different," you know?

B: Yes, and Solow has gotten heavily involved in the HR side of MIT.

A: Yeah, and Tom and you have been involved in that.

E: Here's an example of "not invented here," or "you can't be a prophet in your own land." Quite by chance, I was visiting with a colleague at Stanford's Robotics Lab, which is an offshoot of some of the MIT work in the Media Lab and our robotics thing. His name is Ken Salisbury.

He not only knew about me and organizational culture stuff, but he said when I was out there he would love to have me come into his robotics group because he's got these 20+ graduate students and he would love to have someone to talk to about how to get that place organized. [laughs] For all I know, there are people at MIT using consultants on precisely the kinds of things that faculty could help with. But insiders don't have the same credibility.

B: Oh, I think when we fell on our face here with the reengineering initiative, boy – if we had to do that over again, there should have been a flow from our shop.

A: There was some, but not much. And also that thing, I don't know whether it's gone anywhere.

B: I think we spent millions of dollars, and I don't know what we got for it. Because it was just the traditional consulting.

E: And Howard did something, both for me and for MIT. I guess in 1968 or 1969, he pulled me out to be the undergraduate planning professor. There were many reasons why that might have happened, but it happened. That put me in with Paul Gray into the main part of MIT, and I had the instructions to help MIT become more innovative in its curriculum in general. That was the time when we were really re-thinking whether we were too tough. That's when IAP was invented, when we created the McVicker Chair, George Valley's new physics curriculum, Jerry Weisner had his thing over in Building 20. And I was attached to the Committee on Education that Walter Rosenblith was running; that was my main job, to be on that committee and help it be a good group. But I found myself really enjoying going to all these different groups and being a sort of process consultant. Very low-key, but help them run better meetings, etc. It struck me how valuable that was, but institutions don't use it. They don't use free-floating change agents to just smooth things, because they're not interested in the groups and the communication rather than the content. So I had 3 wonderful years feeling helpful in all these different settings. And at the end of that time, Mason Hair decided to move on to strategy, and that required having someone at Org. Studies. So I became the reluctant chair of Organization Studies because I wasn't really a manager type. But I could do that job, and I did it for 10 years.

B: About when did that shift take place, when Mason Hair went to …?

E: 1970, 1971.

A: I didn't quite follow what you said there. When you said that they, across campus, were more interested in the content? It seems to me that what they're not cognizant of is the process.

E: That's what I meant to say.

A: Oh, OK. They are so content-focused, they missed the process.

E: They missed the possibility. One of the funniest examples was when Walter Rosenblith, in one of these years said, "One of these all-day meetings we're going to have, I want you, Ed, to take charge and use some of your group improvement tools for this committee." I didn't have a clue what to do. Here's a bunch of engineering and science professors, and I was supposed to do something. So there was this little survey of measuring a group's communication influence, how much they were committed to the decisions – very simple rating scale for about 10 questions. So I decided to try that. I handed it out, asked people to fill it in, not exactly knowing where I was going or what I would do with this. But I did have a flipchart up and I put the scales down with the 1 to 10, and then said, "OK, let's just see where people are at. Where did you put Item 1?"

       So I checked off where everyone was at and by the end of that process, I didn't get another word in edgewise. They now had a dataset to analyze. It was amazing! They did a fabulous job of reorganizing the committee based on the data.

B: Well. Going to the Ed Roberts/Tom Allen…

E: Both Ed Roberts and Tom Allen were undergraduates here, and they got their graduate training at Sloan. What we need to find out is which year Don Marquis retired from Michigan, therefore became available, and I think Doug McGregor decided that it would be good to have someone like Don, an eminent psychologist spend his post-retirement years at MIT. And

somehow it was linked to the fact that NASA was going to give us a lot of money to study how you manage scientists. So the whole idea of creating an R&D group, Management of Science and Technology, was created by this pot of money and Don Marquis being brought in to head up that program. We called it the NASA program, I guess, because that's what it was.

Don's philosophy was not clinical. He was a more traditional psychologist. He said, "If we're going to study this process, the first thing we have to do is measure productivity." So he started right away developing – and it shows up most clearly in Tom Allen's book. I think two of the first hires were Ed Roberts and Tom Allen, and they began to study systematically what kind of management worked best in the R&D field. That means you had to have a measure of effectiveness, which was generally done through peer rating, etc. But it became quantitative. That created, I think, a center of gravity and style of research that was very different from the style that Organization Development was evolving. And I sort of fell in the middle because I was doing quantitative research as well as my T-Group organization development stuff.

E: A perpetual issue was who has the power? The research centers and the research professors? Or the curriculum heads and the more teaching-oriented professors? Because when you have the power more on the curriculum side, you have to bring in outsiders who represent the transition to the occupation. And they will often not have the credibility with the research professors. So you get this anomaly. This happened with Peter Senge and the Society for Organizational Learning. The projects required consultants and field workers and all sorts of workers out in the real world, some of whom had to be paid real-world salaries. That conflicted with the MIT policies on what you can pay consultants and field workers, etc. That made it, therefore, impossible to do some of the programmatic teaching work that SOL wanted to do. And therefore, *de facto,* forced SOL out of the school into its independent arena. Because certain kinds of applied research can't be done in the university research model, is the problem.

And this is why Jack Rockart was never recognized, as well. But in the future evolution of the school, I think that tension ought to be recognized as intrinsic and something should be done to solve that problem, because without an applied wing, the school will always have a too-limited research agenda. The research agenda itself has to be broader.

A: Yes, otherwise we're irrelevant.

E: To some degree, I think that's the case for bringing in more John Van Maanens. Ethnography has taken hold here. There are a lot of graduate students now who want to do their research in an ethnographic vein. That would not have happened if you hadn't had a senior faculty member like John gradually building some credibility for the ethnographic approach, attracting good students. I think we need more of that.

A: Yes, that hasn't been an easy row to hoe, either.

E: No, I'm sure it's not. I think it would be very important to get more alumni involved to see how alumni view the research output of the school, and see whether maybe the school should do more – well, the R&D area does all right. The entrepreneurial research….?

A: Ohhh, I don't think so. They have plenty of work, but there's something missing there. Research on entrepreneurship is not really respected that much, I don't believe. But also, Ed, you think about how alumni view research, I think you have to go a long way to find any alumni who see much coming out of this school.

B: Well, they would see with Andy Lo and some of his work.

A: Yes, there's some in Finance, certainly. Some, but it's pretty exotic.

B: Yes, and it's narrowly connected to Wall Street.

A: Yes, they're kind of specialized and exotic. It's not really broadly recognized. If anything is broad, it's heavily criticized.

B: Yes, that's right. Let me insert on this point something that Bill Pounds said when we interviewed him. He feels that our faculty should be on the same funding basis as many of the engineers and scientists are, where they have to get grants to support their salary, which means you have to relate to some of the topics that funding agencies find are important. That could be

argued whether that makes you more applied or not. But I think in some cases it makes you more applied than academic. And he feels—because he looks at a lot of the research that goes on in the school—that because we pay people their salary, they're free to go off and do things that don't capture any attention of our alumni. Where, if people HAD to go for grants and contracts, or get support themselves, but I don't know about you, but maybe we are, in a sense, too comfortable in terms of how we're financed.

E: Before making that decision, I'd really want to look at our current faculty and see what they ARE doing. I would be a proponent of the other model. Find the very best people you can, and then cut them loose. For a university, that's the better model. Otherwise, you really become beholden to the people who have the research money to give you. That would scare me more, to tell you the truth. But if we were to review our faculty and discover they are doing boring and useless stuff, I'd say then it's our hiring policies that are at fault, not the research-framed models. We must be hiring the wrong kind of people.

I think this culture of innovation is deep, and it would be a mistake to try to thwart it by limiting what people do through the outside funding solution, especially with the government cutting back. I didn't HAVE to get outside sources, but for me it was an incentive to get research money to buy down teaching etc. But at that time we had the Office of Naval Research, US Air Force, US Army, NIH – they were pouring money into the universities and therefore letting us do what we needed to do. The whole field of Group Dynamics and Leadership was entirely funded by the Dept. of Navy and Air Force as well.

And if you go to industry for money, they don't know how to do it. So they give you little bits. The Navy knew enough to be broad to people, give them a fair amount of freedom, and that worked. I really loved getting grants for our career development research that didn't have to justify every detail – I had to write proposals but I knew that they were mostly gambling that Lotte, John, and I would produce something useful in this whole career area. They didn't literally ask us to write down what we were going to do and how we were going to spend the money and that worked very well. And each of us eventually produced very relevant research that is widely used.

B: The question really is, "How to assess the relevance of the research work we do?" In your case, you're called upon by organizations to come in and help. So there are people out there who see Ed's work as relevant to the problems they are facing. And we probably have some faculty who are doing work that nobody sees as relevant, but yet maybe down the road, it's going to make a theoretical breakthrough. There needs to be some way to establish how that research work….

A: The volume of research continues to go down. Research centers, it's very hard to sustain research centers these days. Remember there used to be lots of them? Not any more. Most of that is because you can't get external funding for research centers. Part of which is, industry doesn't have the patience to wait for it.

B: And that was a good meeting ground between the kinds of problems that the sponsor companies had. They didn't have to work on all of them, and then the faculty would decide which ones they wanted to work on. That was a good model.

A: I think it would be good if we could have a follow-up meeting.

E: I'd love to do some more of this.

A: I think a couple of the other interviews we're going to have will lead to … we'll need to go back.

B: My last question is, "Ed, you mentioned earlier the distinctive label we had for our school being Management, not Business. I can remember when I came here in 1980, there was something in the hospital area, we had a program for all the medical school deans in the U.S.   So I guess the question is, as we think about the school from its early inception, when management was an opportunity to talk about non-profits, hospitals, whatever – what's happened over the time period since the early 1950s? Are we essentially a *de facto* business school, even though we carry the label "Management"?"

E: Yes, I think we are. And when and how this happened is a bit mysterious to me. Just to refresh us on the medical stuff, it was Jack Rockart, Dick Beckhard, and I and somebody else, who had a summer course that we put every dean of every medical school through. There was one generation of deans who all had a solid week of training at MIT. And Ed Roberts had other medical stuff going.

A: Yes, he did. And there was a guy from Tufts who was involved in that, remember? Connected with the medical people. But then we got some deans back as Sloan Fellows because of exposure. We even had one or two nuns who came, they were our first women.

E: Real characters!

A: Yes, that one we talk about. Somebody asked her what's under that dress you wear, and she said, "Roller Skates!"

E: But why and how did it happen? One critical factor was giving up the Master's degree thesis was a symbolic way of becoming a business school.

A: Which I thought was a huge mistake.

E: From MA to MBA. We let the B in there, and that cut the research down because I used Sloan Fellows very creatively to enhance my research…

A: Yes, the career anchors stuff…

E: …yes, done with Sloan Fellows.

B: Yes, the faculty would use an orchestrated topic and have people work on that.

A: Yes, it was all part of making – if Bill Pounds talked about it getting too "cushy" around here, it was all part of making things cushy. The faculty didn't like to work. Not all faculty. And

there are some faculty still today who go after students who do theses. Michael Cusumano and others. The option is still there.

E: So that was one force. But I would love to know which deanly decisions made us less of a broad school of administration. Because we also didn't link well enough to political science. I remember there were a lot of linkages to Political Science in the Center for International Studies. That's where Rick Locke came from.

A: And now he's a head of the department of Political Science.

E: So I think there's been an unhealthy drift away from management toward business. If I were Schmittlein, I'd worry about that. Because I think all this criticism of the business schools is partly because it's too much about business and not enough about management in the broader sense– why does Ron Heifetz get so much publicity? Because he's talking about leadership in a much broader context. Why do people want to go to the School of Government? Because it's broader, more interesting stuff. And we should at least be competing with that. And I don't see Political Science offering it, I don't see us offering it. We haven't hired anybody like that.