

INTERVIEWER: Today is June 30, 2010. I am Karen Arenson. We are speaking with Richard K. Lester, a professor of nuclear science and engineering and head of that department. He's also the director of the MIT Industrial Performance Center, an MIT-wide research unit based in the School of Engineering. Richard, thank you for talking to us. You recently joined MIT's provost, Rafael Reif, on a visit to Russia. What was the purpose of that trip and what did you do there?

LESTER: The President of Russia, President Medvedev, fairly recently announced that Russia's economy was no longer going to be as dependent as it had been on oil and gas, and that it was going to become more modern and depend more heavily on innovation as we understand that word. And having made that statement, his cabinet sort of jumped to attention and said, well we better figure out what that means. Several months ago, en masse, the Russian cabinet, or at least the economic part of it, arrived at MIT on a fact-finding mission to figure out how to do innovation.

They came almost entirely unannounced. As a matter of fact, Susan Hockfield wasn't even here. She was in Singapore for those couple of days. But a seminar, basically, was organized for them and I spoke to them and several others, too. Out of that grew a number of conversations around the possibility of establishing outside Moscow a Silicon Valley-like region in an area called Skolkovo.

The Russian government is asking a number of western institutions for help in creating such a place in Russia, including MIT. They invited a group of MIT people over, I think probably seven or eight of us went, for an extraordinary week of discussions with many of their university people and the Academy of Sciences and some of their business people. To explore whether MIT could a) do something useful and constructive and b) whether we would want to do something of that kind in Russia. So we had our own fact-finding mission over a period of a week with Rafael and a number of others.

INTERVIEWER: Has anything been decided yet?

LESTER: Well, yes an agreement was signed, as it turned out, the week after our visit. Last week, Medvedev came to Washington and met with Obama. Around that meeting MIT signed an agreement with a Russian entity called the Skolkovo Foundation, which is responsible for developing this initiative. The agreement calls for MIT to spend the next six months or so investigating or exploring whether a longer-term engagement with the Russians would be worthwhile, and would be a productive and constructive thing to do. So that agreement was signed last week.

INTERVIEWER: Do you really think one could grow a place like that? We used to have new towns in America that were kind of architectural phenomena.

LESTER: We did have new towns, and actually the Russians have made something of a practice of this in Soviet times. They created science cities of various kinds dotted around. In fact, we visited one while we were in Russia, in Novosibirsk in Siberia. There's an adjacent town called Akademgorodok, which is a science city. So they actually have done that before. And whether it's an appropriate thing to do in a global economy in which what really matters more than anything else is your connections with the rest of the world, how that will work is I think one of the big questions that we will want to think about, and I'm sure they will need to think about too as they consider this.

INTERVIEWER: If it's doable there-- if they've done it, why are they coming to the US for help?

LESTER: Well, what they've done is very different. What they've done in the past is a science city. And these science cities were literally isolated from the rest of the country. I don't know that you could easily get in and out of them, even, for security reasons. What they do understand is that, to create an innovation region it can't be like that at all. It has to be open and for that reason they identified a location just outside of Moscow. Which, traffic permitting-- and one of the things we discovered was the horrendous, horrendous traffic that was in Moscow and St. Petersburg --but the idea is to make it accessible to western innovators, western companies, western venture capitalists, and to make it something that is integrated with the global economy. Of course that's something they've never done before.

INTERVIEWER: How long do you think it would take to do it, and do you think it's truly feasible? What are the ingredients?

LESTER: You know people around the world, countries and cities and regions around the world, are all looking for the sort of magic sauce of Silicon Valley and, to some extent, here in the Boston area. A number of them have tried to do it. I don't think anyone has really pulled off a created location. These things tend to emerge organically. The closest that I think we might come to something like that is the Hsin Chu Science Park in Taiwan.

I visited 20, actually 30 years ago as a young assistant professor because they have a university there, National Tsing Hua University in Taiwan. And at that point there was nothing there, but they were beginning to create the beginnings of an industrial park around the university. And now, less than 30 years later, it is an astonishingly transformed location with both Taiwanese companies and also many western companies having located initially manufacturing, but subsequently research and development laboratories. That's actually one example I would say, maybe there are others, but one that I know a little bit of a place that has been created. Most efforts to do that haven't really worked out terribly well so far. Where the Russians are starting from is so far back that even a partially successful initiative would probably make a significant difference to the Russian economy. If it's organized well and if its links to the rest of Russia are well designed.

INTERVIEWER: And there are certainly economies that one can point to over the last 20-30 years, South Korea, Singapore, that have taken off along technological lines, I think.

LESTER: Indeed. And I think those are examples, Israel is another one, that the Russians are looking at and thinking about emulating. But of course it's an entirely different scale. I mean the geographical scale with Russia is just extraordinary.

INTERVIEWER: Well will they try to concentrate it?

LESTER: And they're trying to concentrate it. There they have the view that if they start in a physically concentrated particular location, maybe it can diffuse outwards into the rest of the economy.

INTERVIEWER: Does a research university need to be at the heart of it?

LESTER: That's part of the plan. And that's one of the things that MIT, if we were to go forward with them, would be working with them on. The question there is do you bring existing universities into the location, and of course they have many, some of them quite strong, or do you try to create something from scratch? Which might also have advantages given the somewhat encrusted nature of the Soviet research system. I should say the Russian research system, but it did look a little Soviet from time to time during our visit.

INTERVIEWER: And if you had to put a price tag, order of magnitude, on something like this?

LESTER: In terms of the total cost to the Russians of doing something like this?

INTERVIEWER: Yes.

LESTER: I would think tens, hundreds of billions of dollars. I mean I haven't seen any such numbers, but this is a huge, huge initiative.

INTERVIEWER: With payoffs in 10, 15, 20 years? Or sooner?

LESTER: I think if they were looking for payoffs in anything less than-- if they're looking for economic payoffs in anything less than 20 years I think that would be a big strategic error. If they're looking for payoffs in terms of changing ways of thinking, in changing attitudes, in changing ways of doing things, one might hope to see payoffs sooner.

INTERVIEWER: These are all topics you discussed when you were there?

LESTER: Yes.

INTERVIEWER: And who does one discuss them with?

LESTER: Well mostly ourselves.

INTERVIEWER: What level of Russian--

LESTER: We were meeting with quite senior people in government and in the business community and heads of the universities, the Academy of Sciences and so on. But I would say the most interesting and stimulating discussions were the ones that took place with just the MIT people.

INTERVIEWER: When it comes to helping other countries, and MIT is doing a fair bit of partnering with Singapore, Portugal, a real range of countries. How does MIT think about, is this a country we want to help? Or doesn't that issue arise?

LESTER: I think that we have done a fair amount of this and I don't think we have been as strategic in our judgments about who we work with as we probably should have been. That's a personal view. But the reasons for MIT to engage in these kinds of collaborations are many. Obviously there are financial reasons, and I think that's one of the things that, probably more than anything else actually, has driven the administration. Especially with its legitimate concerns about the sustainability of funding from the US government, which of course is such an important part of our financial base. So financial issues have driven a lot of these decisions, but there has to be an intellectual payoff for our faculty. More than anything else that seems to me to be the key issue.

Sometimes that intellectual payoff can take the form of-- and probably most of the time --can take the form of collaborations with counterparts, other researchers. But in many cases these partnerships that we've established don't necessarily have that kind of capability. In other cases it may be just the intrinsic interest of the country. It's going someplace that looks like it might be important, its history may have been important, or something that's happening may be an important phenomenon for MIT investigators to explore. But there has to be some kind of intellectual driver for this.

I think that in the case of Russia actually there is. In the sense that, despite 20 years of the harshest deprivation when it comes to funding of the Soviet, the Russian, I keep saying Soviet, but the Russian science and engineering structure, there are still really quite remarkable pockets of strength. And so I could see, perhaps more than in any other case, well the British maybe that's another example, but I could certainly see opportunities for real symmetric flows of knowledge and insight between investigators-- in some fields -- investigators in Russia and here in the US at MIT specifically.

INTERVIEWER: You started out as a nuclear engineer, and you're currently chair of the nuclear science and engineering department, but much of your research and writing is about innovation and productivity. Topics traditionally found in business schools, economics departments, public policy programs-- how much science and engineering do you still do?

LESTER: Well it depends how are you define engineering. in the sort of conventional, narrow sense of engineering I'm probably not doing very much and haven't done for a long time. But if one thinks about engineering as being basically about the application of technology and figuring out how to make technology produce things of value, so this is a value, I would say that I'm still in some ways engaged in engineering. Because a lot of what I'm doing has to do with the obstacles to applying technology in the world, on how to overcome those obstacles, and how to also educate people, our own students, to think about how to do that and solve those kinds of problems. So in that sense I'd say I'm still doing engineering a little bit.

INTERVIEWER: What drew you to nuclear engineering initially?

LESTER: Well I grew up in the UK, and like many of us probably in this room I'm a child of the Cold War. As I was growing up I was always, not always, but thinking like probably many, many people did at that time about the potential for harm that nuclear technology had, and how this might be controlled.

That was always an issue for me and even while I was an undergraduate, and actually before I was an undergraduate, that was an issue and an interesting problem for me to think about. You couldn't, in those days in the UK, actually study nuclear technology as an undergraduate. There wasn't really an opportunity. And there wasn't either really at graduate school. Which is sort of why I came to MIT to study nuclear engineering.

INTERVIEWER: So you knew as an undergraduate that you were interested and you tried to study things that were related, even if it wasn't called the Department of Nuclear whatever?

LESTER: Yes, and actually I managed to find a way while I was an undergraduate. We had little projects that we did in our third year. In those days, in the UK engineering was only a three-year program and I did a little experiment irradiating a little potassium iodide tablet.

So I started a little bit there. But I always knew, even at that time, that I had a reason for wanting to study nuclear engineering, and it wasn't necessarily to be the best nuclear engineer, it seemed to me because my background was science and math and engineering, that seemed to me to be the best avenue to going to where I really wanted to go, which was to think about problems of control of this very important, very powerful, and also potentially very dangerous technology.

KAREN But you didn't follow a political science or public policy armaments discussions?

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LESTER: No, and I didn't even really know that such a possibility existed, I don't think. Certainly not as an undergraduate. It was an incremental move in that direction. I knew engineering, or a little bit about engineering, so I thought well let's keep going on this path. And that's why when I came to MIT, I came into the nuclear engineering department. But when I got here I discovered this extraordinary, I mean really extraordinary, range of other related activities on the campus. And it was just an eye opener. And I got a little bit involved, as much as I could given the heavy demands of the curriculum in nuclear engineering. I couldn't spend too much time. But I learned about courses in political science, courses at Harvard--

INTERVIEWER: While you were still a graduate student?

LESTER: --while I was still a graduate student.

INTERVIEWER: What kinds of other courses, related courses or unrelated courses did you take?

LESTER: In political science there was a wonderful arms control course that used to be taught by George Rathjens and Jack Ruina, which I remember taking.

INTERVIEWER: At MIT?

LESTER: At MIT. And at Harvard there were wonderful science policy courses that were taught by Harvey Brooks, who actually became my thesis reader. A marvelous, marvelous man who really taught me a great deal about how to think about science and technology policy.

INTERVIEWER: It sounds like, as a child, to the extent you were thinking about control issues, they were more about preventing bombs, rather than nuclear waste. Is that where you headed next?

LESTER: Yes, that's true. But when I got to MIT I quickly learned that the professor in the Department of Nuclear Engineering who was sort of closest to the sorts of issues I was interested in was a man called David Rose. And David Rose, at the time, was interested in nuclear waste and he had other students working on this problem and he involved me in a project which was actually-- we're now, remember talking about 1974-75 and not too many people were really thinking about nuclear waste in those days --but he got me involved in a nuclear waste problem at West Valley in upstate New York.

That became my first publication. I wrote an article for *Technology Review*. It was the cover story on *Technology Review*, and it had bright red letters against the white background, I can't remember exactly what it said but it was something about nuclear waste. And that started me off down that road. And then I continued to do work in the nuclear waste area, although that wasn't actually the topic of my doctoral dissertation. I stayed involved in nuclear waste for quite some time.

INTERVIEWER: When and how did you begin your shift gradually into the more and more policy, sometimes even less connected with the nuclear issues?

LESTER: Well I was involved in policy issues even as a graduate student, and my doctoral dissertation had a big policy component. It concerned that a new technology at the time for enriching uranium, which has the potential of course anytime you can enrich uranium you can potentially make nuclear weapons material which is highly enriched and in the U-235 isotope. And a new technology was coming along at that time which used lasers to enrich uranium, and my thesis was to explore the feasibility of laser enrichment of uranium, and then to explore the potential implications of having a new technology that might be readily available to countries and even sub-national groups. What did that mean?

And so there was a policy component even there. When I joined the department as a faculty member in 1979 after I completed my thesis, my arrangement with the then department head, Norm Rasmussen, was that I would need to spend half of my time teaching and research time doing more or less conventional nuclear engineering work. But the other half I could work on the things that really were driving me. And they were policy related. Initially nuclear waste and other nuclear fuel cycle issues. But over time I became interested in the question of why was it-- and remember this was in the early 1980s, a few years after Three Mile Island-- why was that more or less the same nuclear power plants were being built in different places, both in the US but in other parts of the world, at vastly different costs? Why was it that these same power plants that to a lay person who couldn't really tell the difference, they all looked more or less the same and indeed they were more or less the same, why were they operating with such vastly different efficiencies?

This seemed to me to be an important question to explore and that got me a little bit into organizational questions, management questions. I reached out to colleagues in the management School and in the economics department for help on these questions and they taught me quite a bit. The agenda was sort of evolving a little bit in the direction that eventually I followed.

INTERVIEWER: When the nuclear science and engineering department hired you and let you do half-time policy, were they hiring you despite that or because of that?

LESTER: I think probably both. There were no doubt some people in the department that were willing to take me on despite the fact that they were going to have to put up with this policy stuff. But there were other people in the department, including the department head, who thought that this was an important thing for a nuclear engineering department to do. And it should be said that the department had been doing this. Particularly under the leadership of David Rose, who'd been involved in these policy issues for several years before that. In fact I would say that the nuclear department of all the engineering departments at MIT was probably by necessity engaged in thinking about policy issues before any other engineering department. So there was an orientation there, and I think when they hired me they kind of knew that I could do nuclear engineering. So it wasn't, in that sense, a huge risk.

INTERVIEWER: Your thesis topic, is that the laser use for enriching radio isotopes? Is that a theoretical topic, or does one do that experimentally?

LESTER: Well, I didn't. My contribution was to analyze the different processes that could use lasers.

INTERVIEWER: But you weren't analyzing them by trying this version and that version in a lab somewhere? **LESTER:** No, I mean these are big. At the time, and even today, these are fairly large-scale complicated systems and I was not at any point in my career really an experimentalist anyway. No these were assessments of the process and thinking about what the energy requirements would be, what other requirements would be to make it work.

INTERVIEWER: So you were an assistant professor and were promoted to an associate professor without tenure, which is the normal step. And then in 1986 you were chosen as the executive director of the MIT Commission on Industrial Productivity. Who put that commission together and why were you chosen for that?

LESTER: Well, the commission was put together-- actually it was Paul Gray and John Deutch, the then-president and provost, who made the decision to put it together and may also have been responsible for the idea. Although I think after the fact there were many others who claimed that they had been responsible for proposing this.

INTERVIEWER: And the US economy was kind of in the doldrums, or the industrial sector--

LESTER: The industrial sector was under real pressure at that time. Many people won't remember this, but the real source of the problem was understood to be Japan, which was at that time competing very effectively in electronics, automobiles, and a variety of other industries. There were questions at that time which had begun in the late 70s I suppose and really accelerated during the 1980s, about the survivability of the American manufacturing sector.

Paul and John felt that this was a subject that MIT might be able to shed some light on. What were the reasons for the problems and what could be done about them? This was a very MIT-like thing to do. It was something that really, I think at that time and maybe even still today, differentiates MIT from most other universities. The idea that we would take a big, challenging national-scale problem and bring a group of faculty together to study it and come up with some recommendations. I remember after the study was done-- the study became known as the Made in America study because that was the title of the book that we published --the then president of Cal Tech, I remember talking to him and there's always a little bit of rivalry as you know. And he said, you know I think that Cal Tech is every bit as good as MIT and better in some ways, but I have to say there's one thing that I don't think we could ever have contemplated doing and that was the Made in America project.

And it was a really-- I don't think unique in MIT's history because I think if you go back far enough you'll find other efforts that had somewhat of this character --but this was a pretty unusual thing to do. And John and Paul deserve a lot of credit for pulling it together.

INTERVIEWER: And do you have any clue about how or why you got pulled in as executive director?

LESTER: You know, maybe they were looking for a victim or something. I knew John, and Paul to a lesser degree, and actually had written a paper with John at some point earlier in the decade. They probably saw me as someone who probably had a lot of energy also had a history of working with colleagues in the social sciences and management sciences. And this was fundamentally a meeting, one might even say a collision of the engineers and the social scientists at MIT, around this topic of industrial productivity. So it needed somebody who had some degree of ambidextrous characteristics. So that may have had something to do with it.

INTERVIEWER: Do you know if you had tenure at that point? Were you concerned that it might divert you from research and publishing and interfere with your career or your promotion prospects in the field you had chosen?

LESTER: I don't actually think that I did have tenure at that point. I was probably coming close to it. I suppose it might have been considered a risky career move, but I knew some of the people who were going to be involved in this thing. And I knew how much I was going to learn from them. I knew, for example, that Bob Solow was going to be involved and Les Thurow and Paul Krugman were also from the economics side and involved in this thing, and several of the department heads in the engineering.

So I knew that this was going to be very, very interesting. I knew that it was a problem of major, major significance to the country. And it just seemed like a pretty exciting thing to do. One piece of advice that I got, and it was one of the best pieces of advice I ever had from anybody at MIT, and it was from Gerry Wilson when I was first hired as a young assistant professor. Gerry said-- and I think he probably said this to every assistant professor --he said, you have to do what you really want to do because it's pretty hard to get ahead here. And unless you really are enjoying something, you're probably not going to succeed. So choose the things that really fire you up. And that was great advice. And it was, in this case, advice that I followed.

INTERVIEWER: Would he have been dean of engineering at that time?

LESTER: He was dean of engineering. As a matter of fact, Gerry was subsequently one of the members of this Commission on Industrial Productivity, but he was at the time the dean of engineering.

INTERVIEWER: Tell us about how the Commission proceeded.

LESTER: Well the commission consisted of about 14 or 15 professors, administrators. Paul was on it, John was on it, Gerry Wilson, dean of engineering, Les Thurow I think might have been dean of Sloan at that point. And leading professors from many disciplines, some of which you might have expected others perhaps less likely. But we had the head of aeronautics and astronautics, Gene Covert, and Mert Flemings, who was head of materials. On the social science side I mentioned Paul Krugman and Bob Solow.

INTERVIEWER: The chairman was?

LESTER: The chairman was Mike Dertouzos who at the time was director of the lab for computer science, an electrical engineer. Again, on the social science side, Suzanne Berger was a member of this, Tom Kochan, now the chairman of the Faculty--

INTERVIEWER: Did you have hearings or studies?

LESTER: We met every two weeks. Fairly quickly we decided that we were going to approach the problem by choosing what became eight manufacturing industries. Some of them old, some of them new. So textiles and apparel of the old, and electronics, and computers, and telecoms, and so it was a broad range. The aerospace industry, steel, at the other end of the spectrum.

And we organized ourselves initially at least around sub groups that went out into the field and talked to practitioners, both managers but also people in the workplace. And this took place over a period of several months. There were graduate students involved, and postdocs, and my job was to kind of organize this effort. And it was a huge effort. Eventually it involved something like 80 or 90 people. Many of them faculty, but others were postdocs, because we recruited-- once we decided we were going to study, let's say the steel industry --we recruited people on the faculty who knew a lot about that industry to participate with us. And it became a pretty big activity.

INTERVIEWER: And a group that went out to investigate would include everybody from the grad students up through some committee members?

LESTER: Yes, absolutely. I don't remember exactly, but I think there were 200-300 companies that we visited during the course of this study. Not only in the US but in Japan and Europe.

INTERVIEWER: Did you go on any of those fact-finding--

LESTER: Oh, I did. These were invariably fascinating trips. And one of the things that made them fascinating was that very often we had in the group-- which might have even only consisted of two people --but almost always there was a technically knowledgeable person and a social scientist. And that interaction was really very stimulating. And became, by the way, one of the design features, the design principles for the Industrial Performance Center.

But before we got to doing these visits and studying these industries, we had a period of intense-- and I use that word deliberately --intense discussion among this group of 14 faculty about what really is the nature of this problem. And I would say that the Commission nearly broke up before it got started, before it really got started, over that issue during that period. Because you had very strong voices and it essentially broke down along economic verses engineer. Economists versus engineers. That's somewhat of an over simplification, but you know there were strong voices.

I remember Paul Krugman was very vocal on this point and he was to some extent being a bit of a provocateur as he loved to be in those days and still does today. Saying, look this isn't really a problem that we need to spend a lot of time thinking about, this is a problem of the exchange rates. If we just fix the exchange rate we can-- so he was sort of stirring up the engineers who rose to the debate. They said, look this is not anything to do with the exchange rate. This is to do with our failure to take the technology that we so brilliantly develop in the MIT labs and do something useful with it. All this technology that we're developing at MIT is being productively used by the Japanese and the problem is that Americans are not.

So that's basically where this commission started. Where it ended up was a very different place from either of those two starting positions. But we nearly broke up over that. I mean people just had no patience for the other side. And all credit goes to Mike Dertouzos who basically hammered people into shape and kept them at it. Over time, in good MIT tradition, people didn't agree with each other but they said, look let's stop arguing about this. Let's go and collect some information. Let's collect the data. And the data that we collected was actually collected in a way that was not particularly familiar to many of the technical people, which was: let's go out and talk to people and ask them questions and figure out what they think.

And in that process we were guided, and I think very importantly, by Suzanne Berger, who was already at that point a very leading figure in her discipline of political science, political economy, but who had done a great deal of interviewing as part of her own research. And she was enormously helpful, and I know in my work she really was a tremendous source of advice in helping to get this big activity organized.

INTERVIEWER: I would have thought that somebody in industrial organization would have come closest to thinking through problems like this. But you didn't--

LESTER: We didn't have an IO person. We did have an industrial relations person, Tom Kochan from labor relations, as a member of the Commission, and he was also an important voice.

INTERVIEWER: How was this all funded and what kind of price tag was it?

LESTER: It was funded, and this was I think a very sound decision, we only used foundation funding. There was no government funding and no industry funding. And we were fortunate to receive funding from the Sloan Foundation and the Hewlett Foundation. The price tag, I'm not going to remember exactly, but I think it was on the order of \$1 million dollars. Maybe a little more than that.

I think the fact that we didn't rely on government or industry funding, it probably wouldn't have affected the outcome in terms of the intellectual outcome, but I think it created an environment for this study which I think was very important. At the time there were many, I mean we heard them, we actually solicited the views in industry that said, you know the problem here is lazy American workers. We heard that a lot from leaders of American industry. And then there views on the labor side that we heard a lot. And the government had policies that the administration at the time was completely committed to. We didn't have to worry about these views. We had to worry about them intellectually, but we certainly didn't have to worry about them in terms of what people were going to think about what we said. And that was a very wise decision.

INTERVIEWER: It's interesting how different the structure sounds like it was from another study that was going on almost simultaneously, the one on the auto industry. Which was funded by industry and maybe by governments. Even though they held individual contributions to no more than 5 percent so nobody could put pressure on them, it was a very different approach.

LESTER: It was a different approach.

INTERVIEWER: Although, in the end, that too was a study of an industry and productivity--

LESTER: And in fact there were connections between the two studies. On my staff were postdocs and graduate students who were also contributing to the automobile study. And we used some of the research from the automobile study, which was one of the eight industries that we were concerned with. But I think that we avoided some of the difficulties which that study had to deal with.

INTERVIEWER: Along with Bob Solow and the late Michael Dertouzos you were named co-author of the book that grew out of the commission's work, *Made in America - Regaining the Productive Edge*. How much impact do you think the book had? Did it change American industry or the economy in any way?

LESTER: You know, it's very difficult to answer that question, how much impact does any book have. It's a hard question to answer. I do think that it had a much bigger impact than any of us, when we were writing it, expected. It became, and I think may even still be today, the best selling book in the history of MIT Press. But it had other impacts. I think that there was a lot of interest at that time in what this group had found. One of the contributions that I think it made was to change the direction of the debate. It changed a little bit the way people thought about this problem.

INTERVIEWER: From what to what?

LESTER: I think an important view, the sort of predominant view when we got into this was that this was the Japanese, unfair competition, currency manipulation-- which is not unfamiliar today when we talk about China --that they were unfair in how they competed, they were taking our intellectual property, et cetera, et cetera. That was the almost prevailing view when we got started. When we were finished, and what we said was, this is an issue that we here in the US have responsibility for.

We have a lot of problems that have to do with-- one of the things we said was under investment in research, but that was sort of what any group of university professors would be expected to say --but I think more than that, and more importantly we talked about problems in the way work was being organized on the factory floor. And problems of walls being erected, organizational walls being erected between research and development, production, marketing, and so on. We saw very rigid structures in American firms which we weren't seeing in their competitors. And I think in the end -- we also were seeing problems, importantly at that time, and of course even more importantly today, with the educational capabilities, the educational qualifications, the knowledge in the workforce.

So what we did was to change a little bit the direction of the public policy debate. And maybe even more significantly, I think there was a real impact on management in American corporations. I think that they thought about their problems in a somewhat different way. I remember Les Thurow, at the beginning of the study saying-- everybody was given the opportunity when we first sat down in one of the conference rooms to sort of tell us what they thought the problem was --and Les Thurow, I remember he said, this is death by a thousand cuts.

But by the end of the study we didn't think that. We thought that there were six or seven key problems that we had to address as a society. And I think that was a contribution. We took what seemed to be, and was, a very, very complicated problem and boiled it down to a few key issues. And I think that helped people.

INTERVIEWER: After the Commission finished its work and the book was published, the Industrial Performance Center was created here at MIT and you became head of it.

INTERVIEWER: What was involved in the decision to set it up and what did it take to get off the ground, and did you have any reservations about continuing to follow this path and stay with it?

LESTER: Well on the last point, no. I mean I had spent two years of my life really immersing myself in this problem and realized at the end of it that there was a lot more to learn. It was fascinating. And there was this wonderful group of faculty that had really been living with each other more or less for a period of a year or two. Many of them wanted to continue with this work. Not full time in most cases, but as part of their intellectual agenda. For me it just seemed like the right thing to do to continue.

How we would organize was also fairly clear in the sense that the style of work we had begun in the Commission. It was very interdisciplinary involving people from different departments committed in every case to field research, direct observation of particular industries and practices in particular industries. We'd started that in the Commission and that seemed to be a good template for a continuing research agenda. There were many questions that Made in America raised but didn't answer, so we even had an intellectual agenda. It was a natural thing to do, and for me it was a natural thing to do because of Gerry Wilson's advice-- do what you're interested in.

Setting it up was not trivial because the Commission had always been seen as a one-time event. When it was done, it was done. But this was now to be an ongoing research activity. The provost at the time, a wise man, Mark Wrighton, who is now president of Wash. U, made a very important decision. He said look, let's do this. I'm going to put it in my office for the time being until it develops momentum and then at some point I'm probably not going to want it anymore. But for the time being it can report to me. And we were able to get funding, a large amount of funding actually, from the Alfred P. Sloan Foundation to get us going. And so the organizational home in the provost's office and the funding that came from the Sloan Foundation were what was needed to get the thing going.

INTERVIEWER: Did you think about changing departments at that point, or having the dean of engineering as an advocate was sufficient to keep you in nuclear?

LESTER: The department, I have to say, my Department of Nuclear-- now Science and Engineering --just Engineering as it then was, had sort of tolerated my wayward activities for quite some time. And very generously they-- I talked to the department head and said, I'd really like to do this. What do you think? And he said, fine, you know we'd like you to keep teaching in the department. And I said, of course, and have continued to play a role even over this period where I was mainly involved with the IPC. They were generous in saying if that's what you want to do, do it. I didn't really feel that I needed a different departmental base, because my department was comfortable with it even though the benefits were really only very peripheral to them, to those colleagues.

But the issue of department was, in some sense, a secondary issue. This was a very, very interdisciplinary group from the outset. I mean, there's some of the key players, Suzanne Berger continued to be involved, Tom Kochan also. New people joined the group. Mike Piore from economics became very involved in the Center. He hadn't been a member of the Commission, although his research with Chuck Sable had been a very important influence on the Commission. From the electrical engineering department, actually Rafael became involved at some point, Charlie Sodini. So there were new people, but there was this network of alumni of the Commission who had worked together and really knew each other very well and provided the core faculty for the IPC.

INTERVIEWER: Innovation is a major focus of your center. Why?

LESTER: I think we have to go back to when the Center was formed and really the agenda that we inherited from the productivity commission, which was a lot about manufacturing, and production, and efficiency and productivity of the production process. But it was becoming already pretty clear at that point, and we're talking about the early 1990s, that the competitiveness of American industry was going to depend on more than its performance and its factories. It increasingly was going to be determined by its effectiveness in the development and introduction of new products and services. In other words, innovation.

One of the things that struck me at the time was that a lot of the business chatter about innovation was almost born as a cliché. You had to listen to the voice of the customer. You had to cooperate and break down walls and integration was important across functions. There were all of these prescriptions that people banded around, but it seemed that many of these things were actually very hard to do. And to figure out why they were so hard to do really looked like an important research project. Which became actually more than just a project, it became sort of a program.

And at that point Mike Piore was starting to get involved in the work of the Center. And he and I began a collaboration-- this was probably around '92-'93 --which was focused on this problem. Actually, initially it was focused on the problem of integration in business organizations, and why that was such a hard thing for them to do. Integration across functions, across boundaries, and so on.

We started a program of research in the style that we had inherited from the Commission. We identified some firms that were doing interesting things that seemed to have integration issues. Medical devices where the integration problem was clinicians versus science, and how to overcome that barrier. There were a series of others.

And it became clear that, as we talked to these people about the problems of integration, that integration was very much a part of the innovation task. That is, as we talked to people about why integration was so hard, we increasingly were drawn into discussions of how they were developing and introducing new products. What we learned from those interactions became the basis for a long-term investigation into how businesses actually innovate. That led to a book, it led to a whole series of other projects, and that work that began in the mid-1990s has infused a lot of what the Center has done ever since.

INTERVIEWER: Do you think innovation is something that can be taught, or cultivated, or learned? I guess that's at the heart of what you're talking about with Russia, for example maybe?

LESTER: Well I think that there are certainly techniques and practices that can be taught and learned. But one of the parts of the process that we were most interested in, and remain most interested in, is the part that doesn't really lend itself to techniques and practices. In particular, it's the creative part.

What we discovered in our research, and it's not surprising, was that that's the part that people don't really have the ability to talk about very well. They can't even really articulate what's going on at that stage of the process. You have things like people talking about brainstorming, and one of my favorite cliches is, "you have to kiss a lot of frogs before you find the prince". By the way, when I've used that cliché in France-- from time to time I give talks over there-- it doesn't always go down very well.

What was striking was that people didn't really have a way of talking about creativity. They just have these sorts of words that they use to describe it. In our research we have really tried to understand what goes on during the creative part of the innovation process. We came to think that there was a whole vocabulary that was missing to describe this. People just use these terms which tended to focus on the individual. The creative individual coming up with an idea.

But actually in our studies of companies in industries as diverse as cellular telephones, and blue jeans-- the fashion jeans, and automobiles, and all sorts of other things, we came to see that it's typically not an individual. It's more of a social process that is involved in the development of new ideas for products. Over time we developed our own vocabulary for describing this phase of the, as some people call it, the fuzzy front end of the innovation process. We came to think of this as an interpretive part of the process, as distinct from the analytical part, which is the part that most engineers are much more comfortable talking about. It's problem solving. In fact, many of the people we talked to said, Yes design is problem solving. It's a form of problem solving. But our question was always, well how do you know what problem you're supposed to be solving? Where does the problem come from?

And when that question was posed to the practitioners, particularly the engineers, they knew that it was an important question but they didn't have a way of talking about how it happened. And the focus of research, Mike Piore and myself spent a number of years following companies and talking to practitioners and so on, it was to try to develop a vocabulary and a way of talking about what happens when you don't know what the problem is that you're supposed to be solving?

And when you think about our curriculum in the engineering School at MIT, we're great at problem solving. I mean that's what engineering really is about. But we don't spend a lot of time teaching and helping our students think about the processes through which problems emerge. As I say we came to think of this as being essentially an interpretive process. Very, very different from the analytical problem solving approach.

INTERVIEWER: Partly a question of how you choose what to work on?

LESTER: How you choose what to work on. Where do the problems come from? We came to think of this as being something that happens often literally, but sometimes metaphorically, in the form of a conversation. When you start a conversation you don't typically know where it's going to head. This conversation may be a little different, you know exactly where it's going. But in many cases, conversations when they start, you really don't know where the conversation is going. But often something interesting happens and you have to develop it. We came to draw on ideas from linguistics and the development of languages as a helpful way of thinking about what goes on in these stages of the innovation process.

INTERVIEWER: But as you look at MIT, certainly people here are very good at the analysis. But one of the things that seems to make it unusual, perhaps, is that they're really good at having ideas other people don't have. And going off in directions they haven't. I think perhaps that's why there's so many Nobels and the like. That whatever it is, it has been part of the culture, perhaps?

LESTER: And one of the things that I think we don't fully appreciate about what we have here is that MIT is a kind of a public space within which these kinds of conversations, creative conversations in which new ideas emerge, take place all the time. And when people think about MIT, and in particular its interactions with industry, they tend to think about professors and students solving problems for companies. And of course that does happen. Sometimes companies come and say we've got this very particular problem and we'd like you to help us solve it.

But a lot of times what goes on in our interactions with industry and with firms isn't that at all. It's that they come here, they want to be here, because they want to be part of a conversation about the direction in which a discipline is moving. Or the direction in which a particular market may be moving. And those conversations are the raw materials in some sense from which the problems emerge that eventually get solved. Often not by our faculty and students, but by the companies working in their own labs. So you have this dual role that the Institute plays. Problem solving for firms but also serving as a public space within which people come in and out.

That's the other thing about MIT. It's very, very porous. And it's porous both within the institution, disciplines, there are constant interactions between departments.

INTERVIEWER: More so than most universities?

LESTER: Much more so than most. Maybe less so today because other universities are beginning to do this better. But MIT has always had that character. I think partly-- this is going to sound a little chauvinistic --because engineers tend to not care quite as much about departmental and disciplinary boundaries as some other branches of academia. And the Institute has always been very much engineering oriented.

So I think that we serve as a public space within which these conversations, both literal and metaphorical, can take place. I think that's one of the reasons why we have been such a prolific institution in terms of new ideas and the ability for those ideas to find a home somewhere and to be developed and exploited.

INTERVIEWER: Have you looked at what kinds of labs work? As you talk about innovation and ideas, a number of labs run through my mind. Something like the Skunkworks at Xerox. Ideas, like at the Sloan School, the \$100,000 they give awards for entrepreneurship and ideas. The Amy Smith Design Lab here. They're very different and yet all of them seem to have creativity as their focus in a funny way.

LESTER: They are very different. But I think one of the things that from a distance they perhaps have in common-- the Skunkworks at Xerox is a different-- that's a very mixed story, as you know. But some of these other things that you mentioned, I think what they may have in common is there's something about the interaction between the designer, the developer, and the user.

INTERVIEWER: Woodie Flowers and the 2.07/2.70 contest is another one.

LESTER: That's another example. I think there's something very difficult for engineers to grapple with here and I've found that as I talk about this issue with my colleagues. Because engineers like closed form problems.

They like to know what the problem is. What resources do we have to solve it? How long do we have to solve it? Who are the experts in the different areas? Let's divide the problem up into the pieces and ask each expert to deal with his or her piece. And then somehow this thing gets pulled together, and the quicker you pull it together the better. We know about that.

But there's also, and this is going back to this other, there's also this open-ended thing that happens before you even get to the problem that needs to be solved. I think that some of the things that you talk about at MIT that you mentioned, you can look at them in different ways. You can look at them as being about problem solving, but you can also look at the same activity as being part of some continuing open-ended conversation between designers, developers, and users. In which the artifact that comes out-- the design, the product, even the company --is a thing that people then talk about in some continuing conversation.

If you look at this extraordinary set of activities that we have at MIT, you can look at them as producing things, you know businesses, products. But you can also think about them as part of some ongoing engagement between MIT and the world. And that's really the key to understanding the prolific nature of MIT, is to think in terms of that ongoing engagement between MIT and the world. And the fact that we've actually come up with a whole variety of different ways of having those engagements.

INTERVIEWER: Bob Langer and Langer Lab is another. It's something about anointing creativity, and saying that this is really important and good. And giving people then the goal, perhaps, of becoming good at it. Or the freedom to feel they can do it, or something. To take risks--

LESTER: All of that, but also to signal the message that engagement with the world is good. Engagement with the world of practice in order to figure out what the problems are that need to be solved. That that's good. And we give that signal, and we create opportunities for that in many different ways.

INTERVIEWER: Coming back to your own center, would you say there's a new discipline emerging, or is the work you do mostly based on an amalgam of tools from other disciplines and a willingness to use tools from different disciplines?

LESTER: It's, I think more the latter. I've never seen the role of our center as being the creation of a new discipline. In fact, the contrary. I don't know that I've said this publicly, but the metaphor I've always thought of when I've thought about the work of the Center is it's really a kind of a crossroads between different disciplines within MIT, and between mostly firms-- because those are what we work with and on --firms and academia. So it's a sort of a crossroads in which people bring their perspectives and disciplines and methods to bear, but we don't try to capture any of our colleagues in the orbit of the Center.

What we hope happens is that people come work with colleagues from different departments on a problem, on a project, and then go back probably a little different from what they were like before they got involved. Maybe their work will move in a somewhat different direction than it would have done if they hadn't been involved with us. But that's not about creating a new discipline, that's about enriching existing disciplines with perspectives drawn from somewhere else. And I think that's a very important role for centers to play.

There is in academia always a strong pressure to develop a new discipline, develop a new way of thinking about something. We all understand where those pressures come from, but I think in this particular case, this particular center, that hasn't been the objective. It's simply been to give people a chance to learn from each other and maybe be changed a little bit as a result.

INTERVIEWER: Much of your focus has been on firms, business entities, but you've also looked explicitly at higher education and universities. How did that happen and why? Where are you on that?

LESTER: Well that actually is again this idea of the Center serving as a crossroads. One of the things that we have done over the years is to organize seminars for faculty to think about particular problems. And one of the seminars that we organized probably back in the mid 90s, I don't remember the exact date, was a seminar on the future of the research university. Which was a subject then and now, of real interest. Especially to faculty around here. So we convened a group of faculty and organized some visits from people who we knew had been thinking hard about this.

INTERVIEWER: So it wasn't just a two-hour, one-time thing?

LESTER: It was a year-long series of seminars and I had a couple of students who were actually writing theses during the year on the subject. It was a very interesting year and a number of ideas came out of it. Related to that was something that was happening increasingly around that time.

Our center is located in the same building as the Industrial Liaison Program and other kinds of entities that we have at MIT that work with industry. And because we were all in the same building, I noticed a growing flow of people coming from around the world to the ILP, or I knew they were going across the road to the Technology Licensing Office, trying to figure out what it is that MIT did and how they could emulate it.

And I spoke to many of these groups. The ILP and TLO often asked me to meet with them. And one of the things that struck me was this might not be the right thing for them to try to do, to emulate MIT. They were often coming from a very different place and a very different set of capabilities, and it wasn't obvious to me that emulating MIT was the right thing for every university. They were coming from all over the world. But I also realize that we didn't have a framework that might suggest that there are other ways in which universities could contribute to economic development and growth. Many universities are under pressure to do more and more of that, but doing it in the way that MIT does it is not necessarily the right thing.

So we initiated a research program growing out of that seminar and stimulated by these visits from people around the world to study the different ways in which universities can contribute and are contributing to economic development and innovation in their regions. We worked in 25 or so different city regions around the world, focusing in particular on this question of what are universities in these regions doing? And we deliberately selected places that had first tier universities, or second tier universities, or third tier universities, different industries driving the economy, and we really learned a great deal about the different ways in which universities contribute. And they don't all look like MIT by any means.

INTERVIEWER: And this was how recently? How long ago?

LESTER: We started this work probably around 2000-2001 and it has continued for several years and in a sense continues even today.

INTERVIEWER: Did you try to bring out any of your findings a few years ago? Before the financial markets crashed research universities were on the hot seat in Washington, and here, partly because of the size of their endowments, particularly Harvard, Princeton, Yale. And policymakers in Washington, Massachusetts, were beginning to suggest that they weren't delivering enough public benefits in exchange for the tax benefits that they enjoyed as non profits. The question of do they contribute in other ways, I think it's an interesting one. A lot of the conversation among the policymakers came down to, why isn't tuition zero, you have so much money. But the question of what do they contribute in the way of innovation, or research, or economic bases didn't seem to come up as much.

LESTER: It didn't. But I think there were two separate conversations going on. There was certainly that conversation about the rich universities and why are they benefiting from the tax laws, but for a much longer period prior to that there had been a separate conversation that wasn't directed at necessarily places like MIT and Princeton, but at the local university, the state university. Why aren't you doing more to contribute to economic development? In many cases they didn't have any endowment, or nothing significant. But we note that you are part of a family of institutions that include Stanford and MIT, we need more from you.

And of course university administrators were often willing to say well we will do more. Because they saw this maybe as a way of shoring up their faltering budgets. So there was that conversation going on, which I think is actually a longer running and probably more important conversation perhaps than the one that was directed at those few rich universities who were getting ready to be hauled over the coals. I don't know that it ever actually happened because as you say the financial crash.

INTERVIEWER: Is MIT doing anything on this front that you think is interesting or useful?

LESTER: In terms of contributing? I mean MIT does so much.

INTERVIEWER: In terms of your model or can they do more?

LESTER: Well I think what we what learned from our studies of this question was what everyone thinks MIT is doing-- discoveries, patenting, licensing often to start up companies run by faculty or students, and spin offs -- that is an important thing that we do, but it is only one of the things that we do. And among the most important things, probably in the larger sense, more important, is education. The technology transfer that happens when people graduate from here and go to work at a company. That may not involve starting a new company, but it's a very important contribution.

Another contribution that we make is what we were talking about before-- serving as a public space for industry and government people to talk with academics about important technical questions. But what has happened in this debate around the country and increasingly around the world is the policymakers have focused on the thing that is in some sense easiest to identify about MIT and Stanford, and easiest to count, which is how many patents, how many licenses, how many start ups. I think a lot of the other things that MIT does and that other universities are probably no less able to do-- maybe they can't do the start ups as well as we can --but they can certainly do some of these other things.

Unfortunately the debate has focused too much, in my judgment, on the things that are the most obvious things. What we have to do is to move this beyond the spin off phenomenon, important as it is. One of the things I like to point out is that we have a half million new businesses formed a year, every year, in the US. And the number of businesses that are formed out of university intellectual property for all American universities each year is just a few hundred. Now it's true that many of those businesses, or of some of them at least succeed and get big and they end up like Cisco or Google or something, but it's a small part of what the economy is doing.

And similarly on patenting. Every year something like 150,000 patents are approved by the PTO. Universities collectively in the US account for less than 3,000 of those 150,000. So if we were to judge the university impact on the economy just by patents and start ups you'd have to conclude that it's a very small piece. But actually it's a much larger piece. In order to see that, you have to look more broadly at the range of interactions between universities and industry.

INTERVIEWER: When you were growing up in the UK, did you have any links to academe? Were your parents in education or did you ever dream about entering and becoming a professor?

LESTER: My father was a schoolteacher. He taught what we would call here grade school. So in that sense education was very much part of our family. And my older sister became a teacher. But university wasn't something that was part of our family history. I think this is true of many families of my generation that I was not the first to go to university, I had one other sister who got there a couple years before me, but weren't connected to higher education.

INTERVIEWER: How did you choose where to go to college?

LESTER: Well in the British system you have to give a few places that you think you might want to go and it's a clearing house scheme. I ended up at the Imperial College in London. And the subject, in Britain, you actually choose your major before you go. Which I think is a very bad idea and I think the US approach is much better. It's a bad idea because kids just have no idea before they go to university about anything or most kids. And I was certainly no exception. I chose chemical engineering for my major mainly because it seemed like the major that would cut off fewest options. I don't know why I thought that. I'm not sure that it's any different from any other engineering field--

INTERVIEWER: Or how you learned about it. It's not a topic that one hears a lot about--

LESTER: I like chemistry, and I knew engineering seemed like a good thing to do and practical.

INTERVIEWER: And you had that background interest in nuclear control.

LESTER: But that wasn't an option in the UK. You couldn't major in nuclear anything in the UK. So it was an almost random thing. As it so often is.

INTERVIEWER: And where and how did you decide, oh I'm going to go to the United States and do my graduate work.

LESTER: Well it's interesting. Imperial College is quite like MIT in the sense that it's a college of science and technology. In fact, in those days it was even more science and technology oriented. But as an undergraduate you had to have one humanities subject. I took a humanities subject from a man who had previously taught at MIT. Amazingly, this was an historian.

INTERVIEWER: Do you remember his name?

LESTER: His name was Sinclair Goodlad. And I don't know what his position had been at MIT, but he said to me while we were talking as I was taking this course which I really enjoyed, you know you really ought to think about going to MIT. And I had no means of getting to MIT, financially, but there was a fellowship program called the Kennedy Scholars program which actually had been set up as the British national memorial to JFK in the 60s. It sent 12 people or 10-12 people a year to Harvard and MIT. I think that Goodlad might even have pointed me in that direction. Anyway I applied for it and was awarded this scholarship which enabled me to come to MIT as a graduate student. And that's how I ended up here.

INTERVIEWER: And you knew by then that you wanted to focus on nuclear?

LESTER: Nuclear, yes.

INTERVIEWER: What were your first impressions of MIT? You got here and thought what?

LESTER: It's hard to remember exactly. I mean I was completely taken aback by the range of activities at MIT. It was a kid in the candy store sort of impression. I was also taken aback by the pace of work, you know that was something I had to get used to. But it was really so exciting. There were so many people doing extraordinary things and it was just incredibly stimulating. That was my overwhelming impression.

INTERVIEWER: Do you have any recollection of whether your fellow graduate students shared any of this sense of wonderment and reached out as you did, or whether you were somewhat unusual in that respect?

LESTER: I don't really know, to answer that question. It's interesting now I'm department head of that same department I came into, I'm sort of reconnecting with some of these people who I was a graduate student with. And of course they've gone in many, many different directions. I do remember that this was a very internal thing for me. I don't remember talking to people about it. It was something that one held internally. There wasn't a lot of socializing as I recall in those years. At least I didn't. It wasn't a very social environment. It was in some sense all business, so to speak.

INTERVIEWER: The fact that you carved out time to do these other things is interesting, because I think there are professors who are very broad in their interests, and there are some who focus more narrowly on their labs and encourage their students to stay in the labs many hours, many days and arch an eyebrow if there is a suggestion of going off to see a concert or a play, or take a course somewhere else or in a different department.

LESTER: And I gravitated within nuclear engineering towards professors who had a broader view of their role and the role of engineers and even of students.

INTERVIEWER: How does the department today compare to the one that you entered as a grad student?

LESTER: Well it's considerably smaller. Probably starting in the late 80s or early 90s, the Institute allowed it, let's put it that way, to shrink. I don't think the department wanted to shrink, but it did. And it shrank in no small part because of what was happening in the nuclear power field for decades, really.

So the department is a smaller place. Until very recently it was an older place because the department wasn't allowed to hire young faculty for much of that period and the faculty got older. That's changed even since I became department head last fall, but was chairing the Search Committee for the previous two years. And we have hired five new junior faculty within just the last two years. And that's tremendous. I mean we have a lot of energy now coming from our young faculty.

INTERVIEWER: Is there a resurgence in student interest and funding that is permitting you to do this?

LESTER: There is a resurgence in student interest. It really dipped down quite steeply in the 1990s, the late 1990s, but since the beginning of the 2000s. I looked recently at the numbers. In fact for the last many, many years I was running the Admissions Committee in the department. The number of applications, the number of applicants to the department from the US has increased by factor of four over the last 10 years.

INTERVIEWER: From what kind of base?

LESTER: It's starting from a low base because it really was down. I don't remember what the numbers are--

INTERVIEWER: Roughly, order of magnitude--

LESTER: I mean we take roughly 25-30 new graduate students a year. Something like that. Maybe in some of those years it was probably as low as 20. At this point we're probably seeing about 200 applicants from both domestic and foreign sources, overseas sources. But if we take both domestic and international students I would guess that we were down as low as maybe 70 or 80. So there's been a lot of growth in applicants. But much of the growth, interestingly, has come from the domestic side.

Now one reason for that, of course, is the changing fortunes or expectations that the fortunes of the nuclear energy industry will improve. But the department is not just about nuclear power, or at least is not just about nuclear fission. A large section of the department's faculty and students work on nuclear fusion and plasma physics problems related to nuclear fusion. We have one of the biggest fusion labs in the country at MIT, the Plasma Science and Fusion Center on Albany Street. And then the department has always had quite a lot of activity in non-energy applications of nuclear science and technology. So from PET scanning and more recently quantum computing, uses of neutrons as scientific tools to investigate materials and things of that kind. So while the department is very closely identified with the nuclear power industry, there's always been important parts of it that are not engaged with nuclear power particularly.

INTERVIEWER: Do you have a particular agenda for the department at this point? A plan of any kind?

LESTER: One of my first tasks on becoming department head last September was to develop a strategy for the department. I felt that it needed one and in fact I knew that it needed one. Because if we were going to be allowed to grow, we had to have a compelling story for why we should grow and how we should grow. So for the last several months I've been working with the faculty and working the faculty to get a strategic plan.

We now have a direction, a pretty clear direction for growth that is something of a departure from where we have been as a department. But I think it is responsive to both the prospect of a real expansion in nuclear power worldwide, but also responsive to developments in the science and engineering of nuclear phenomena that are opening up a whole set of new opportunities that may, in the end, not have a great deal to do with nuclear power but that really are no less at the core of our department than the energy application.

So I think we're looking ahead with some real excitement and optimism, which is different from what the department has had to be dealing with over the last couple of decades.

INTERVIEWER: What do you see as the prospect for the use of nuclear energy and power in the US at this point?

LESTER: I think it's an uncertain prospect. Globally there's no doubt that we're going to see major growth in nuclear power. I mean we're seeing most spectacularly in China rampant increases in construction of nuclear power plants. It should be said, however, that however fast they're moving on the nuclear power front, they're moving much faster, unfortunately, on building coal fired power plants. That said, they're moving. I think they have at this point 25 plants under construction. They plan to have 70 or so in operation by the end of this decade. Koreans, Taiwanese--

INTERVIEWER: Nuclear plants.

LESTER: Nuclear plants. Globally there are probably 20 countries that are seriously considering getting into the nuclear power business for the first time. Some of them maybe not the ones that we would necessarily feel entirely comfortable about. But globally the picture is one of growth.

In the US I think it's still uncertain. There's no doubt at all that if we're to have any chance of meeting these very ambitious goals for carbon emission reduction that the president and some members of Congress are calling for based on the research and studies of the climate change scientists. Which despite the flurry that we've had over the last several months, continue to show that unless we achieve very deep reductions in carbon emissions, we're looking at the possibility of very serious damage. There's no question that if we're to achieve these goals that the president and the Congress are setting we need a lot more nuclear power in this country. But the conditions that will make it possible are not yet there I would say. Whether it happens or not is a question. I hope it does, but--

INTERVIEWER: Those conditions being?

LESTER: The conditions I think, or the obstacles mainly at this point have to do with the very substantial financial risks that would face any electric power company that was considering, and there are of course quite a few now, that are considering building nuclear plants. But the risks are very, very high and unless we have a way of reducing those risks and enabling them to build plants that don't cost as much and can be built more quickly. I mean it's all about reducing costs and reducing cycle times. That's the goal, or should be the goal of the nuclear power industry.

INTERVIEWER: In an op-ed article in *The Boston Globe* just before the presidential elections in 2008, you noted that both candidates for president had expressed support for nuclear power, but that even a major expansion in nuclear power would not reduce our dependence on oil imports for decades. Why would it take so long?

LESTER: Well when we're talking about oil import reductions most of the oil that we use is of course used in the transportation sector. The possibilities of displacing oil or other fluid fuels with electricity, which is the only way that nuclear power at least for the foreseeable future can contribute to reducing consumption and oil imports, that's going to take a long time.

INTERVIEWER: So it's not just a question of whether we build more nuclear power plants, it's a question of whether we shift to using electric cars to plug in.

LESTER: Exactly. Go full electric. Now the other main gain from an increase in nuclear power in this country would be to displace the coal plants that now are responsible for about 50 percent of our electricity and which are, of course a major, major contributor to our carbon emissions in this country. So that displacement, the displacement of the coal fossil fuels is something that can happen directly by building more nuclear plants.

But over the longer term if we're going to reduce our oil import dependence, we're going to have to have electricification of the automobile fleet. And that will take time. It all will take time. The problem is we don't have a lot of time. Because unless we have decoupled the very close link between economic growth and fossil or carbon dioxide emissions that has been a characteristic of our economy for 100 years, unless we've figured out a way to decouple those two things by mid century, it's almost certainly going to be too late in terms of avoiding serious climate damage.

INTERVIEWER: You talk about the risks to companies in moving this direction being one of the big obstacles. Risks that come from political shut downs, or from plants having problems? What are the real risks?

LESTER: Political risks are certainly an important part of the story. But they're not the only risks that companies have to think about. The political risks specifically have to do with the difficulties that the Federal government has in handling or managing the nuclear waste issue which the feds are responsible for and which companies need to have taken care of. And there are political risks associated with the licensing process and so on.

But the fact is these are multibillion dollar projects carried out by companies whose existing assets may be less than the costs of a new nuclear plant. These plants probably exceed the financial capabilities of the majority of electric power companies in the US. So we can either make these companies bigger, or we can make the plants smaller. And probably we're going to have to do a bit of both in order to overcome this risk problem.

INTERVIEWER: You've also noted that the technologies being used for nuclear waste disposal are 25 years old and that it's time, or long past time, I think you've said, for a broad based, high quality scientific and engineering program to develop new approaches. Is that something that MIT or your department could do? And is anybody doing that?

LESTER: I think we can certainly contribute. One of the worst things that the Congress has done, and its performance in the area of nuclear waste has been miserable-- and I don't think that's too strong a word --for decades, but one of the worst things that it did was essentially to freeze any innovative thinking on this problem back in the 1980s when it said not only that Yucca Mountain in Nevada was going to be the only place we looked at for disposal of high level nuclear waste. But it also said the Federal government was not to spend any other money on the exploration of any other alternatives for disposing of nuclear waste.

So for 25 years we've had no innovation in this area. Now Yucca Mountain has been taken off the table for political reasons. The government has established a commission to explore the future of nuclear power. One of our colleagues, Ernie Moniz, is a member of that commission. It will say something in a couple of years. But really it is time for a serious new research, serious research program, to explore alternatives. We know so much more today about the geo science of nuclear waste disposal, and the geo technical aspects of nuclear waste disposal, than we did 30 years ago when we were making these decisions.

And we can contribute in this department, but also other departments at MIT can and are collaborating with us on these problems.

INTERVIEWER: If there are countries like China that are betting in a much bigger way on nuclear power, are they also conducting research in terms of how to deliver it more safely and more efficiently? How to store nuclear wastes better? In other words, even if the US isn't doing this research, is it happening around the world?

LESTER: It's happening around the world and every country that's generating nuclear waste is having to deal with this issue. But different countries obviously have different geologic endowments, different population densities, different distributions of nuclear power plants. So while there's opportunity for international collaboration in this area, I think that we in the US certainly shouldn't and can't rely on other countries to solve this problem for us. And it would be frankly embarrassing for the US to do that. So we really need to be at the forefront of these activities. And there's really no reason why we shouldn't be.

INTERVIEWER: Do you see any other universities around the world with nuclear engineering departments, or even research centers, that are doing that kind of research that you think are particularly interesting?

LESTER: You know I think one of the important elements of our strategy in the nuclear science and engineering department at MIT is to establish new research collaborations and educational collaborations with universities around the world. Because there are growing capabilities in Asia, and also in some countries in Europe that backed out of nuclear power but are now getting more involved as they see the writing on the wall for a climate change. And if we're to remain at the forefront of research and education in nuclear science and engineering, we need to be collaborating with the best institutions around the world. And also leading firms around the world.

So yes we already have a set of collaborations. We need to build new ones, especially in China, which is going to be the center of nuclear development, nuclear energy development, over the next a couple of decades. We have relationships with Tsing Hua University in Beijing, and we will extend them. We have a number of students coming every year from Tsing Hua.

INTERVIEWER: This is your department?

LESTER: Our department. And that's been the case for 15-20 years and we need to look for ways to make the flows, as I said earlier, a little bit more symmetrical.

INTERVIEWER: Richard, thank you very much for talking with us today. Good luck on that front and others. This was fun.

LESTER: My pleasure. I enjoyed it a lot.