History

Of the

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Interviewee:	**Leroy W. Schroeder, Ph.D.**
Interviewer:	**Mr. Robert A. Tucker** **John P. Swann, Ph.D.**
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Place:	**Clarksburg, MD**

Interview with Leroy W. Schroeder, Ph.D.

February 21, 2006

TAPE 1, SIDE A

RT: This is another in the series of FDA oral history interviews. Today, our interview is with Dr. Leroy W. Schroeder, Ph.D. The interview is taking place at his home in Clarksburg, Maryland, and the date is February 21, 2006.

Participating in the interview is Dr. John Swann and Robert Tucker of the FDA History Office.

Dr. Schroeder, if you could give us a little personal history and educational background, we'd like to start the interview in that way.

LS: Sure.

I was born in Watertown, Wisconsin. I have a younger brother. And I grew up on a little farm, which was quite an interesting experience. My dad had to do some work outside, but we learned to be pretty self-reliant and to solve problems in a workable way. They didn't necessarily have to be elegant solutions. But I grew up in a way that it now seems you can hardly grow up that way anymore.

I just finished reading *The Last Child In the Woods*, about the estrangement of the modern kids from nature. But when I was growing up, we were outside a lot. We had a tree house, we had forts, we played games, such as kickball. Soccer hadn't come here yet; we played baseball probably every afternoon after school when the weather was

good, until suppertime. And then, of course, we had farm chores that were meaningful chores.

I'm also a product of the mythical one-room school. I still went to a one-room school, and I was in the last graduating class. And then I had to go from there to Watertown High School, which I thought was pretty big because in the one-room school, I probably was in a class of three, and in the high school, it was 200, which I guess is rather small by today's standards, but it seemed very big to me.

RT: So you went through the first eight grades in the local school.

LS: Yes, in the little school. It's still standing, by the way. It's a shed now.

Anyway, so that was quite an experience.

Well, my dad had a repair shop for farm equipment and things, and I was always kind of interested in that. I think I at one time thought I was going to be an engineer, but I had a chemistry teacher in high school who was pretty entertaining and influential, and we got to do special projects and things. I remember one of my special projects was I made scale models of molecules, simple ones, water, sugar, and things like that. I think that was what got me interested in molecular structure.

My mother had been a teacher, and she was big on education. She was determined that her sons were going to go to college. I ended up going to a little liberal arts school called Wartburg College in Waverly, Iowa. I wanted to go to Lawrence, but I didn't quite have a good enough record for scholarships, which weren't as plentiful then, and my folks weren't financially up to it.

So I went to Wartburg, and they had a little chemistry department there where we got a lot of attention. I remember the senior prof helped me solve a problem three times. The first time I didn't think it was quite right, but I was just a student, so I didn't question it too much. Then he called me back in his office. We went over it again and he still wasn't satisfied, and he called me back a third time and I finally got it right the third time.

There were five chemistry majors the year I graduated. Four out of five got Ph.D.'s. They're pretty good chemists.

RT: Did I understand you to say Wartburg College is at Liberty, Iowa?

LS: Waverly.

RT: Oh, Waverly. Thank you.

JS: So you had an opportunity in college to be working with the faculty on some problems, which in a larger college, that might not have been possible.

LS: Yes. And then I was told about the NSF [National Science Foundation] undergraduate fellowship program, and I was fortunate enough to get one of those in my junior year, so I went to Purdue University to do undergraduate research on magnetic resonance. By the end of the summer, I could tune the machine just as well as the graduate students. So that was my, those were my first tastes of research.

I decided to go on to graduate school, and I chose Northwestern University in

Chicago.

JS: Why Northwestern?

LS: Well, I think, let's see, I had applications quite a few places: Berkeley, I think, and Illinois, and some of the well-known others -- strangely, not the University of Wisconsin, my home state.

JS: A pretty good chemistry department there, too.

LS: Yes!.

So anyway, NU had smaller classes, and I was kind of impressed with them, I guess.

RT: Well, when you graduated from Purdue . . .

LS: I didn't graduate from Purdue.

RT: Oh, I'm sorry.

LS: From Wartburg.

RT: Right. I thought you said you'd gone to Purdue.

LS: No. I went there one summer to do undergraduate research.

RT: Thank you for that clarification.

LS: Then, after I graduated from Wartburg, I applied and I eventually decided to go to Northwestern University.

I was kind of interested in polymers, and there was a prof there named Dole, who had the idea that he wanted to develop polymer, large-molecule mass spectroscopy, which came to be, eventually. Fenn won a Nobel Prize for a couple of years ago. But Dole was older and I wasn't sure about that.

I was kind of interested in quantum theory, too, but then I finally ended up with Professor Ibers, who was a structural guy, a crystallographer. Crystallography, of course, has produced lots of Nobel Prizes too. And I worked on unusual hydrogen bonds and, well, it was kind of, I guess, in the lines of Linus Pauling tradition. My advisor had worked for a student of Pauling's, so I'd say Pauling was my scientific great-grandfather. But, of course, a lot of chemists could say that.

RT: What year were you at Northwestern?

LS: I started there in '65 and finished in '69.

Then I decided to do a post-doc. Well, crystallography was developing rapidly, and for small molecules, it wasn't so much the analysis anymore, it was more the type of

problem one was dealing with.

I thought I wanted to try something new, so I got a National Research Council NSF postdoctoral fellowship at the NBS to do some work on neutron scattering, which I was interested in because if you're working on hydrogen bonding, neutrons were better for that.

JS: That was known as the National Bureau of Standards at the time?

LS: Yes, it was.

JS: NBS had, I think, moved to Gaithersburg not too long before. I got there about '69, I think. It was relatively new. The nuclear reactor had just started operating.

I spent two years doing neutron scattering, and then it was kind of, well, I needed to go somewhere. My advisor was suggesting to go to Washington State University at Pullman, and they wanted somebody to do chemical physics, but I wasn't so sure about their program.

I heard about a position at the American Dental Association Health Foundation, which was a cooperative venture with NIST [National Institute of Standards and Technology]. I guess that dates back to Paffenbarger and the dental amalgams. They were looking for help -- at that time, a fellow by the name of Walter Brown was the director of the Foundation, and he was doing work on the caries mechanism, regarding why you get tooth decay, the chemical action on the enamel, and also the structure of the various calcium phosphates. And he had established this little crystallography program,

and they wanted some more help, and so I decided to go with that. And so that was my first introduction to -- well, it wasn't called biomaterials then; it was called dental materials, which is actually older than biomaterials, but they were materials intended for use in the human body, in the mouth at least.

So we worked on calcium materials, calcium phosphate structures. It was pretty much basic research, and I would say that after about five years, we pretty much mapped out all the common ones and even a few of the extraordinary ones, and I could see that that was kind of finished; I was going to have to go on to something else. Although I must say, that work did seem to be pretty influential. I still see quite a few citations to my work on calcium phosphates in various journals. A couple of my colleagues at the Foundation developed a dental cement from doing that. I believe they got a patent from it and FDA approval.

So anyway, after about five years, I thought that, well, I'm going to have to switch to something else, and I thought about going to big-molecule crystallography, but that was almost like starting over. And I had acquaintances, you know, it was kind of like networking. I had a previous friend who had been at NBS as a post-doc also, and he had gone on to FDA. And when I talked to him, he said, "Well, why don't you come work for us?" I think this was about the same time I had sent an application in to, what was it called, the Bureau of Devices.

JS: The Bureau of Radiological Health. I know you were involved in that bureau. But you're thinking of . . .

LS: I sent in an application, and I think because I knew Robert Stromberg a little bit because he had been in medical materials at NBS. I think I was rejected for being overqualified.

So anyway, I went along with my friend's suggestion and I interviewed at the Bureau of Foods and took a job there. And then I kind of went back to polymers again, sort of interesting because, while I started out in direct additives but pretty soon moved over to the indirect, and that had to do with the type of food packaging we have, like bread wrappers and things like that, and plastic containers for . . .

RT: Just to date it, do you recall who was in charge of the Bureau of Foods at that time? Was that before it became a Center?

LS: Yes. It wasn't a Center, because this would have been, this was 1977. Yeah. I'm sorry, I don't think I remember who directed Foods.

JS: Before you got into the foods work, which obviously we want to hear about, I have one question about when you were at the National Bureau of Standards. You had mentioned that there was a collaborative program with NIH at the time.

LS: It was with the American Dental Association.

JS: I see.

LS: That was the one. They did have some grants from the National Institute of Dental Research, which was -- have I got that right, NIDR? Yeah, that's part of NIH. Maybe the name has changed.

JS: They all go through name changes, but that would be the one.

LS: We did have some grants, because when I first came there, I was on the so-called soft money from grant money, and later that changed. So that was sort of the connection with NIH. But that was pretty loose because we gave NIDR a yearly report, and I saw the project manager once a year. We did what we thought was best, research-wise.

JS: So the tie-in was primarily through extramural support your work and others'. That was a standardized thing.

LS: Yes, that was it.

JS: I see. Well, I know you were talking about the work. Once you hooked up with the Bureau of Foods, then you worked on migration of particles from food packaging into the foods, which was of longstanding interest, I gather. Right?

LS: Right. It still goes on. It was very interesting. Well, it often was the monomer migration. We had two big issues with polymer monomers. One had to do with the polyvinyl chloride because the vinyl chloride is a carcinogenic, and there was [sic]

questions about the residuals and how much migrated. And that was very significant at the time because of the Delaney clause. There wasn't [sic] supposed to be any carcinogens, and that was before we realized that there were natural carcinogens. So there was a big issue -- I don't remember who the firm was, but I do remember there were a lot of meetings about it, and they kept saying that, "We're getting the residual lower and lower, and it's not going to come out, it's not going to come out."

Michael Flood and I wrote a memo about it. Congressman Ashley (Ohio) wanted a copy of the memo where we suggested that they needed a new approach because we felt that analytical chemistry kept advancing, and what was not found one year, a couple of years down the road will be found. So that was one issue.

The other one that I remember, it was along the same lines and quite important. It was a proposal to have an acrylonitrile polymer in soda bottles, and that's another case where the monomer is a carcinogen. And the same arguments were trotted out claiming that it was extremely low, it was locked in there, it would never come out, and all that kind of stuff. Scientifically, this didn't really make sense because it really didn't fit with the laws of chemistry and physics.

JS: Well, what we're talking about here is migration under sort of passive circumstances rather than having the packaging treated warm or something. This is just migration from just plain food contact.

LS: Yeah, pretty much passive. We hadn't reached the stage of the boil-in bags. That came later. I left before we got into that. So that was a big deal.

Well, as you know, the acrylonitrile, the polyacrylonitrile soda bottle never made it commercially; the polyester bottle captured the market. Appeals court supported FDA. I guess you can say that FDA influenced the product development there.

And we had research on migration. We had a contract with NIST, and I was the project monitor on it for several years, and so I participated in research vicariously, but it was mostly a review division. I felt, I guess, because I had worked in research probably eight or 10 years before I got there, I felt kind of unhappy. I couldn't really see myself only doing the paper stuff all the time.

So, again, a friend of mine said, "Well, the Bureau of Rad Health is looking for somebody in their Biophysics Division," kind of a jump. This friend of mine interviewed, but he didn't want to make the jump. I said, "I'd be interested." So I went up and interviewed. The fellow's name was Harry Youmans, the Bureau of Rad Health biophysics branch chief; we hit it off pretty good. So I decided to go there.

I wanted to get back into research at the bench, and I sort of succeeded because when I talked with my former Food colleagues, they said, "Well, what are you doing?" and I'd tell them. They said, "Are you sure you work for FDA?" Rad Health was very research oriented because, as you well know, they had a mandate to actually do research. It's actually in the Radiation Control Act.

RT: Do you recall the year or approximate time when you moved over there?

LS: Yes. I went to Rad Health in 1980. And that turned out to be sort of a challenge because I guess they were in the midst of their own difficulties. Now, I'm sure you've

heard this from higher management and some other people about how that was reorganized and things that were involved.

JS: Any insights you have to share into any of these changes, please, don't hesitate.

LS: When I first got there, the biophysics section was in the Division of Biological Effects, and they were supposed to study the effects of radiation on tissue. The ionizing was fairly well known, but electromagnetic and light and some of the sound, that was more up in the air. And the biophysics effort was more molecular or cell membrane behavior, and that was kind of its focus.

Well, I got there and we decided we were going to work on the effects of radiation on cell metabolism. I'm not terribly sure whether we had a really good idea or not, but that seemed reasonable -- at those times, it was more like the principal investigator model; there wasn't so much research into or review of proposed investigations yet.

JS: Can you talk us through a little bit about the process -- it's a pretty important research pathway that comes about. I don't think people understand how decisions like this come about, if it's done collaboratively, if it's done by a lab director, and so on.

LS: I think it was done locally. But I was sort of new, even though I had been in research quite a bit, I was sort of a newcomer there. So I wasn't really privy to all the decision-making. But most of the projects, when I said like principal, they were

proposed by the individual investigators, and then I think they were evaluated by the management. I don't know whether it got all the way to the Center director or not. Because the local management units got significant chunks of money, and then they decided at that level what to do.

JS: So they had some autonomy then.

LS: They had autonomy then, yes.

So I got into it, and I started getting going, and then after two years, there was a reorganization. It kind of took the wind out of my sails.

Well, that was a reorganization pretty much from the top. One day we heard these sections are going away and these are the new ones, and people were shifted around. So I ended up in the Electromagnetics Division. We were supposed to investigate the effects of electromagnetic radiation, low frequency and power lines; I don't think cell phones were an issue yet.

The big thing I remembered was healing the bone fractures that wouldn't grow, wouldn't re-heal. They had done a fair amount of work with direct current in clinical studies and things of that nature, and direct current looked pretty plausible in that, because you can imagine chemical action and then biological action. That seemed to be fairly well founded. But, of course, you had to mess around with electrodes, and that created some problems. And some others came up with the pulsed electromagnetic, where you just put coils around the fracture, and then these pulsed fields were supposed to do the same thing. Well, that was a case where the clinical results were some positive

and some negative.

JS: How long had this been sort of in practice in medicine in terms of bone repair and growth? Had this been going on for a long time?

LS: No, not that LONG – several years, I think, not that many years.

So we tried to look into the mechanism of healing, and there was a lot of talk about the applicants who would come in and say, "Well, this waveform, this particular variation of the field strength and time is what's important," maybe to distinguish their product from others. But anyway, then we'd have sort of a clinical outcome, but then we'd have no idea of the mechanism. So we tried to work on the mechanism, and I tried to get something going on the cellular level with a friend of mine. There were some experiments being done with cells and filters and things where they were then subjected to a field, and they'd find different responses in impedance change and it was thought this did something to cell behavior, but it was not very well nailed down. We did produce a Center-wide paper about the therapy, sort of assessing the situation, pointing out where things tend to be, where there seemed to be a consensus, and where it was pretty much up in the air.

After two years of that, there was another reorganization.

JS: But the publication -- were there firm recommendations on the legitimacy of this therapy?

LS: Well, I think it was not considered to be quack, but not probably as firmly based like the effects of ionizing radiation.

In fact, I remember helping review a product once which was called Pocket Doc. Nice name. It was about the size of the first hand calculators, probably about the size of your tape recorder, and it had buttons on it, and it was supposed to cure everything. I was supposed to review that one. It only put out a couple of millivolts.

JS: We've got a few of those in our collection in the History Office.

LS: Right. So that one we thought pretty much was a quack device. There was just a little bit of a cloud around those electromagnetic things because of earlier devices.

RT: Now, this second reorganization you mentioned a moment ago, was that when there was a consolidation of radiological health and devices, when it became CDRH?

LS: No. That's later, I'm getting . . .

JS: I think this was the one that created the national centers. They did the National Centers for Drugs and Biologics when they combined those two, and I think for foods as well.

LS: Yes. At one time the Bureau of Rad Health was a national center, but then there was some objection made to it and it was cut back to the Center for Radiological Health.

So then we got up to about, I think it was ’83 or so, ’84. Well, I think it started earlier -- I don’t know. You probably heard that from others.

So then the second reorganization, then it was going to be molecular biology.

Oh, I forgot. We had done a little research, a colleague of mine had done a little work on the effect of microwaves on DNA. It turned out the major effect was due to corrosion of electrode -- a good thing they had done controls. I guess that’s a little technical.

Well, he had DNA in a solution, and you needed an electrode to deliver the microwave field. That was a copper electrode, and they had it in contact with -- the DNA was in a physiological solution, which is like a salt solution, and it corroded. And I think they ran a control, just the setup but no field, and they still got the effect. Then somebody told them, “Well, maybe you should coat the electrode.” When they coated an electrode, that ended it. So the effect was not due to what they thought it was. That happens in science sometimes, the confounding variables.

Anyway, that provided a little basis for, because we had a little experience with handling DNA, for the Molecular Biology Branch, which I think really was supposed to deal with in-vitro diagnostics, but management still thought we needed names that sounded good and more scientific, so molecular biology sounded better.

JS: I gather that you decided to take a detail around this time with DMMS, Division of Mechanics and Material Science, rather than going with the Molecular Biology Branch. Is that right?

LS: Yes.

JS: I was curious what prompted that.

LS: Well, there were a few of us in there that thought, well, we might like to learn molecular biology because the DNA structure was a big story in crystallography, and there were a lot of physicists and chemists, physical chemists, who developed that. That wasn't developed by the biologists; but it was developed by the physical scientists. So there was some attraction to us, to some of us, that maybe there was something more we needed to learn. But we felt we needed a leader who was really a molecular biologist and who knew what the real problems were so we would have good direction. Those of us who were going to be in that division didn't really, myself included, feel that we could figure out what the problems might be, especially with respect to the regulatory problems. They did eventually hire somebody for that, but who was not known as a molecular biologist.

So they sort of trashed around, and that was about the same time that the merger with devices came about. I decided, well, you know, I don't know, it's getting pretty unstable. Maybe I'll go back to where I started, more materials than chemistry, so it sounds like I'm kind of zigzagging here. Some careers are like that, I guess.

RT: When the Device Amendments came in, there was quite a responsibility to classify devices. Did you get into any of that?

LS: No. That was a little bit before my time. I went over to Mechanics and Material Science [DMMS]. That was formed, I guess, during the merger, because before that, I believe it was the Division of Biomedical Engineering or something like that. And I think it was down in the South Agricultural Building. I never was down at that part. That was all before I got there. I got there in about, I think, ’85. Then I finally transferred over there, so it was actually ’87. Biology kept asking me, “Well, are you coming back?” I said, “No, I don’t think so.” DMMS was kind of an unusual place.

JS: There’s probably something else we should talk about. I don’t mean to interrupt the flow here, but when we get to some points where something crops up, hopefully it’s not annoying you too much with interruptions.

But you mentioned that the enabling legislation for BRH had research quite obviously stipulated there.

LS: Yes!

JS: When you see the merger, what’s your sense as a scientist in this newly formed agency? What’s your sense as far as the readiness of the authorities in the Center to accept research as a recognized function of that Center’s activity, its regulatory activity? Is this something that you’re comfortable with at the time? I know you’re maybe not making the policy decisions, but you’re certainly subject to them, so I’m sure you must be interested.

LS: Well, we saw already previously that, you know, we had two reorganizations in four years.

TAPE 1, SIDE B

LS: It's a good thing to bring this up because I did experience a change in what was expected.

When I went to the Electromagnetic Branch, they had renamed the division to the Division of Risk Assessment, so we already were starting to shift a little bit more to applied. It wasn't good enough for upper management to just find a biological effect and let it lie there, you know. You've got to decide, well, okay, if there is a biological effect, is it harmful or can you ignore it? If it is harmful, then you're into dosing, how much is allowable, and all that kind of stuff. So we were then in the Division of Risk Assessment.

When the merger occurred -- well, I think some of us thought we were in for more change when we learned of it. God, there were tons of devices out there, way bigger than radiation. I mean, we had like four branches just to cover the various types of radiation, you know, whole groups. Now suddenly we were going to have 10 times as much stuff. And while the Division of Medical Engineering was there -- I forget its history, but it wasn't as big as Rad Health -- they didn't really have the research reputation.

I remember, well, I guess you can decide whether to keep this in or not; this is one of those things at the worker level.

I remember the talking, "Those Medical Engineering guys, they can't research their way out of a paper bag. Get rid of them! Why do we want them for?" But, of

course, they were the only ones who knew anything about medical devices, so that didn't work. So, they put Rad Health in charge, but then Rad Health had to start shifting their emphasis and start learning about medical devices.

RT: Did they have to recruit personnel in the medical engineering field to staff, or did they already have some of these people?

LS: Well, they recruited a few, and people reprogrammed. For example, the ultrasonics guys did a lot of radiation work, but a lot of their exposure chambers involved fluids, so they knew something about fluid behavior, so they kind of eased themselves into problems involving blood flow and heart-valve behavior. So sometimes people would take something that was not the center of their program but they knew something about it, and then in a new situation, that effort increased, and what they did before would decrease it. They kind of reprogrammed themselves, which is kind of what I did. I went away more from the biological things back to the more classical material science more related to the Division of Mechanics and Material Science, which was pretty loosely structured when I first got there. The upper management finally told Mr. Marlowe to put some structure into it.

And, actually, I give him credit. It was a very democratic process. We went through several rounds of discussion and finally ended up -- first ended up with four groups based on scientific fields. We had a biology group, a physics group, a chemistry group, and a mechanics group, and they didn't all last very long. It eventually spiraled down to two groups, the mechanics group and chemistry group, which, it probably, in

terms of the number of personnel, should have been two groups in the first place, but that didn't happen initially.

JS: Well, at least everyone had their say in this.

LS: Everyone had their say. Compared to the previous reorganizations that I experienced, I was kind of impressed, as the previous ones were pretty tough.

JS: We find ourselves now at your work at the Center in the 1980s, and you've been doing research on stability of polymers.

LS: Right. I told you that we formed these groups in Mechanics and Material Science, and, as stated in the OST history, the medical engineering group, and, in fact, I hadn't really told you this personally, I actually got started with a pacemaker insulation problem. That's sort of a practical problem, and that's what medical engineering got started with. When I came there, they had a couple other chemists. Jim Dillon had been in the Office of Device Evaluation. We had a couple of discussions. I came over and we started to work.

We started to work on the pacemaker lead degradation problem. At first it showed up as cracks, and people had thought that it was simple, just like, you know, a breaking fracture. But then it was found to be connected to the metallic insulator corrosion, and they began to be suspicious that there was more to it than that, because the actual loading on it wasn't that great. It isn't like an orthopedic implant or something

where you really put a big load on.

I started feeling that there was some chemistry involved in this, so we started to work on the mechanism. Well, there were two things proposed. One was called metal ion oxidation, and that was also investigated to a great extent by Stokes at Medtronic. So we worked on that. And the other well-known phenomena to people in the polymers field is something called environmental stress cracking, where a polymer absorbs some kind of fluid, and the absorption of that fluid causes the polymer to undergo strain and crack, which can cause failure. This is well known in the materials world. So we started to work on those two things, and we got interrupted, kind of.

Well, I know I should say a little more, but breast implants came to the fore.

JS: I want to hear about that.

You mentioned Medtronic. I have to ask, were there contacts with the firm, with the scientists at the firm, when you were doing this work on the leads, the pacemaker leads?

LS: Well, some. When they came in with the products, there were some, and there were also scientific meetings.

Medtronic actually published quite a few research papers on this. There are other people at Case Western University who worked on it too. We worked on it, some of these others, e.g., Hiltner, worked on it; we did a literature review and report also. And then we had some regulatory aspects to the, I forget exactly when that, what year that happened, but the Agency had the required post-market studies.

I remember being part of a, well, that's when required post-market surveillance came in. I remember being part of the technical committee -- I was supposed to work on the material characteristics, and they were going to look into how many of these leads actually lasted. I think the goal was 95 percent survival after five years implantation.

There was a big post-market study on that, which turned out to be quite a task. First, it was quite a task to develop the protocol. They had lots of issues, and lots of other people were involved in that.

Well, one of the problems with the study results was that when a lead tended to look like it wasn't going to make it, it sort of quietly disappeared, and a new model would show up. So it was hard to, if the new model was then changed, you couldn't relate, it was kind of hard to sort out what really was the reason for the non-survival.

Then, of course, later on, that law was rescinded, but they did do those pacemaker, those post-market studies. Now I hear CDRH wants to reemphasize post-market again. If I look at my Center history, I can see the same themes reappear. And you know what they say. They say you're ready to retire when you've seen the same thing too many times. You get tired of it. It's a little like that.

JS: Well, you've mentioned the work on breast implants came, and that obviously started taking a lot of your time. I wonder if you would just set the context here before you go into the details of the work that you were involved in.

LS: Well, the breast implants were pre-amendment. There were some devices that were around, you know. The device amendments, those were like 1976, and when I think

about it, I think, "Man, I was out of graduate school. Till 1976, you could put, do whatever you wanted with the body; you could put anything in people's bodies without asking." It seems amazing now, doesn't it? I mean, wow!

So anyway, finally things happened on the outside, and it finally reached a point where, well, FDA had to decide whether we were going to call for PMAs [Pre-Market Approvals] on breast implants.

But before that, there was one in particular that I think I mentioned before which had the polyurethane foam cover, and that issue boiled over around 1989 or 1990. Our group had just been formed, and I was just chosen the leader. We had just started to gear up when this thing came along.

JS: Who introduced this?

LS: I guess the Center, but I think the primary person, at least what got me into it, was My Do Luu. She had dealt with this over in the Office of Device Evaluation, and she was concerned about the degradation of this foam, and it was known, at high temperatures, to degrade and form these aromatic diamines, which were animal carcinogens. But this was observed at elevated temperatures. And it was unknown whether it would be the case at body or room temperature. The sponsor didn't really want to do anything. Luu wanted to pursue it, and FDA was kind of on dead center. You know, they didn't have any good way of getting off center because there was nothing in the literature to help them out with this. They didn't know what really happens at body temperature.

JS: We're talking about something higher than body temperatures, I mean, substantially higher than body temperatures?

LS: Yes. There was some information at the high temperatures. But high temperatures accelerate reactions, and, in fact, they accelerate it tremendously. So there were some arguments made, "Well, this happens at high temperature, but this doesn't necessarily happen at body temperature."

JS: The reason I ask, obviously, is that for a layperson like myself, you'd think, well, do you study these things under the conditions of their use? Or I guess what I'm asking is, were you looking at the behavior of these products under extraordinary conditions?

LS: I'm sorry, I guess we have a little misconception here. We weren't. We looked at them under normal conditions. I'm saying there was some information at higher temperatures at other conditions, but those were extreme conditions, and it wasn't known whether those meant anything in terms of the normal use.

So we decided to look at it under normal use, and this was the first chemical-degradation study that was ever done in our Center that I know of, and it turned out to be a really important one. It got a lot of attention, and the Division director had to create a peer-review board. So here we are, we're just starting out, the first study. We don't even get this published before the on-site peer review.

RT: Did this elicit congressional committee or oversight activity?

LS: They made some inquiries a little bit later, yes. I think Luu actually testified. I never personally did.

So we had our peer review and the company had their peer review, and -- well, peers could find a few little things. They found a few little things that could be better. But basically we were vindicated, and then later on we were vindicated by third-party people who published in the outside world literature. And so, as a result of it, the firm voluntarily withdrew the product, which is commendable. So that was pretty exciting.

JS: There were certain recognitions involved here on the part of those involved in research, too. Right? Apparently, the Center recognized the value of this research.

LS: Oh, yes. This got an award.

JS: And this was about breaking research, as it said, right?

LS: Yes, because, well, it was certainly relevant, and it was a case where the agency was scientifically stuck. So that was kind of, I don't know whether you want to say it sort of fell into our laps. It came from the outside, and it turned out to be pretty important. It was quite something to have to deal with right away. Of course, I somewhat was used to this because I'd been through this a little bit before with foods with these things. Yes. I knew that this thing might mushroom, but still it was a challenge!

RT: Did that whole situation generate civil actions, suits of people who felt that they were harmed by these products?

LS: Perhaps. I'm not absolutely sure about that particular one. I know it generated a post-market study, because there was some product out there already, and so they were concerned about the people that already had it, with what their risk would be, and so a post-market risk study looking for the degradation products -- I'm trying to remember if it was multi-center -- but looking for clinical markers. So a post-market study was carried out and risk assessments were done and all that sort of effort to determine the risk.

RT: I suppose this, like in drugs, in devices, was there an adverse-reaction reporting program in the Center?

LS: Yes, I believe so, and there was a lot of activity in outside groups, and I think the women's groups were just starting to gear up. This overlapped a little bit with the silicone breast implants, which came shortly after.

JS: I wonder, too, was the Center involved in studies on stability of the silicone breast implant envelopes, things like that? Or was there any research done on that that you know of, even if you weren't directly involved in it?

LS: Well, we did end up doing some research on that. We didn't -- that question

seemed to be more about, it wasn't so much about the stability because the [unclear], the silicone polymers are pretty stable.

The polyurethane was, again, getting a little technical, was a polyester, kind of. Those are known to have some stability problems under acidic or basic conditions. But the silicones were pretty stable. So the questions more centered around breakage, like a crack in the implant, and leaking. That was a lot of silicone in some of those. They could hold several hundred milliliters. So we became pretty involved in manufacturers guidance, what should be in the PMA and this sort of thing, and what was coming out, what was the chemical nature. I mean, we kind of pushed the envelope a little bit on the material description. The AdvaMed predecessor (HIMA) was concerned about all this chemical information that was being asked for. They were not used to that because medical devices don't have biochemical action. If they did, they'd be drugs, at least by law.

And that came about the same time as the Biomaterials Assurance Act, because some of these safety problems, some of the big polymer suppliers like DuPont and Dow were pulling out because even though they'd have a little, in terms of volume, this little small-volume polymer was very profitable, but it was being used for medical applications that now had a big risk with it, too. And then there was some concern about supplies of biomaterials. Some of that came in about the same time that we were asking for this additional chemical information.

So we had some meetings with the industry trade groups about assessment of materials, like what could, might be done.

JS: You're also doing, apparently, some consulting with other parts of the Center, too, on problems, I gather, unrelated to the breast implant work, for example. Right?

LS: Yes. We still did some consulting with cardiovascular. In fact, I still worked with them until I retired, just waxed and waned, and we did some work for Compliance.

We eventually got to be known as being knowledgeable about manufacturing regarding what you had to do with certain processes, in the realm of chemistry and chemical engineering, to make products with quality control. We got involved in that a little bit. Because Compliance does it, too, but they did it more from a checkpoint standpoint. Do they have these quality-control procedures in process? They're not so much into the technical, like if there's a set point, is this a reasonable set point for the process, should it be something else?

They'd sometimes ask me to consult with them on how should a firm validate their process. Like if they want to change from solvent A to solvent B, and what do they need to show this was okay with toxicology.

RT: So your role in a way, and your responsibility was advisory to the enforcement or the investigational staff . . .

LS: Well, we did that in addition to advisory to the Office of Device Evaluation, which was the pre-market. And we did some post-market consultation, too.

Post-market maybe was a little less because, well, they had things called health hazard evaluations where we would have to kind of input our material expertise into . . .

That usually factored into, well, how likely is this adverse event going to be like if it involved fracture of a device or degradation or something to the device that would cause it to malfunction. We did some of that too.

JS: You did work on computer modeling to help predict stability of substances. Is that correct? I was curious if there's a way of explaining this in a way for people to understand how you do this, because it seems fascinating, it truly does, to predict the stability of medical devices based on technology.

LS: Right. I guess I need to preface that a little bit.

Quite often, for product behavior, we're interested in the long-term behavior, but we would like to do a short-term test. That's called accelerated testing. Like you might run the test at high temperature and then, if you have some kind of model, you extrapolate back to what you think is going to happen at room temperature, and that comes into play in, for example, shelf life. If a company wants a five-year shelf life, they want to get on the market before they've got five-year real-time data if they can, because they don't want to wait five years down the road.

So there are two approaches to that. One is to give them a temporary one-year shelf life and then extend that as the real-time data comes in. Another approach that plays into that is this accelerated testing. So we did some of that in connection with some problems with dialyzers where, this was a problem of breakdown, and we knew the mechanism, so we knew pretty much the mechanism of degradation. Rather than try to carry out the experiments over such a long period of time, we made some Monte Carlo

computer models based on the chemistry and physics, which would tell us how the breakdown proceeded with time. We had to connect a little bit with the experimental because when you do a computer simulation, that's computer time, and you've got to connect that to real time. But once you've done that and made that connection, then you can predict what's going to happen in the future. We did that on the dialyzer and predicted how much -- this was in connection with shelf life. If a dialyzer was stored and was undergoing degradation due to ozone or something, how much degradation products would accumulate after five years? Then, once you've got that and what they are, the toxicologists can take over and tell you whether this is okay or not okay. So that was one example.

JS: In this one particular example you just mentioned, do you recall if there was a particular public health problem associated with these that prompted that sort of investigation?

LS: Yes. There was an incident where there were some adverse effects where a couple of older-lot dialyzers were used, and there were more, but the investigation wasn't carried out in the best manner, and so we didn't get quite as many samples to work with as we had wanted. But, again, that turned out to, I guess you could say, influence the industry. They didn't have a shelf life, called expiration dates, when this first came about.

JS: On dialyzers?

LS: Yes. But after all this fuss with the adverse effects and shelf life and everything, the industry finally decided it would be a good idea to have an expiration date.

I wouldn't want to say this was the only influence, but it certainly was a factor, because we combined the model work and the experimental work to show the degradation and risk.

Well, you know, generally, materials science, as the years go by, has gotten more computational. The computers are more powerful now, and the theory has advanced so that they deal with bigger and bigger systems. You can do things, because of the computer power, you can do these Monte Carlo experiments that are not as limited.

JS: I have to ask you to explain, because I don't know what a Monte Carlo experiment is.

LS: It's kind of like rolling the dice, spinning the dial. You carry on all these trials and you do -- well, it depends on the problem. You do 10,000, maybe 100,000 trials, and you see the various outcomes, and then you see which ones are the most probable and that kind of thing.

The theory was known for a long time, but it didn't really do practical problems until we got sufficient computer power. That's really what makes the difference today. The computer power keeps going up and up. You know, there's talk of a molecular computer and a quantum computer, which would be billions of times faster than what we have now. Well, I don't know. If that comes to be, a lot of chemistry and physics will

turn into computational science. I don't know if I'll be around to see it or not.

JS: You shifted -- I think I understand from your background -- and by the 1990s, you went more into management than into bench work. Is that true?

LS: Yes. I was like the branch chief. They called it a group leader, but I was the branch chief. It was in management. I became the leader and hired some people. Some were great and some were not so good!

Well, we started off really good. There were five of us in a small group. We were kind of on the same wavelength, so we got off to a real good start, later we kind of wandered around a little bit.

JS: How did you look at the research of the program with different eyes in your subsequent capacity rather than working as more of a bench type scientist? I mean, I'm sure there are some obvious ways, but maybe something that you would have appreciated.

LS: Well, I think the thing that happened along the way is, I remember reading these management books about flat organizations, pushing decision-making downward, and all this kind of stuff. And I thought, it seemed like it was all going the other way there. I mean, there was more accountability over the years, more accountability and oversight from above.

Well, I have to admit that the way they dispensed research money, say, around

1980, maybe through the ‘80s, maybe even up to the early ‘90s, was still kind of the academic model. Projects were proposed at the working level, and then they kind of went up, and local management group decided. I participated in that, I think, twice. And it was a little -- it wasn’t quite the boys in the backroom, but it was not really that transparent. It wasn’t really obvious why a particular study was deemed more important than some other thing. It’s kind of hard in the regulatory market to figure that out.

Now, when I first was the group leader, we had a lot of problems that sort of came up to us, so we didn’t get into relevance so much. Then it seemed like later on, I don’t know if the problem got solved or the agency’s attention was directed elsewhere, but we kind of got away from that. Then there was a time when we dabbled a little bit with tissue engineering, the role of polymers in tissue engineering, and that seemed too vague; it wasn’t clear who was going to do what. So that was still unclear.

Then there was always -- you’ve probably heard this from the OST (Office of Science and Technology) higher managers -- OST was always sort of trying to decide what its role was in the Center, because it wasn’t so obvious like the other offices. They had a role that was pretty connected to the device law, but OST, there was not a research mandate, and even the consulting role, there was nothing that says that CDRH shall have consultants to help you with the heavy-duty technical stuff. So it was kind of, you know, they had to know their need and then you might have to help them determine their need and that kind of stuff. So there got to be more of that.

JS: How would you characterize the relationship that OST had with the rest of the Center? Was there tension? Were others looking at OST’s work and saying, why are we

doing this?

LS: Well, it varied all over the place. I think it was sort of spotty. We had some, I'll call them clients for lack of a better word, who really thought we were pretty damned helpful, and then there are other people who thought we were sort of the country club, you know. We were off in the corner. We didn't have to get in the trenches and move the freight. We could deal with the interesting nice stuff.

RT: In this area you were working in, was there any international liaison with scientists in other countries doing anything comparable, or were you directed more by our own legislative mandates here?

LS: Well, they were doing somewhat comparable, although . . . Well, you know, the European system is a totally different system than our system . . .

TAPE 2, SIDE A

JS: We were talking about, among other things, about the perceptions of OST within the Center, and I'm guessing experiences that analogous organizations within the other Centers had, maybe not. But this is something that had an impact on you as a manager in OST. Did you ever find yourself having to explain to others in the Center why you're doing the sorts of projects that you're doing?

LS: Well, yes, I think we did. Especially later on, there got to be more project reviews, and feedback was sought. Some people, I guess on a more personal level, either were more inclined to it, or more active and had more of a personal relationship with the client. After a while we got to thinking this was the way to go, that once you got that relationship established at the ground level, then the rest of it was a lot easier because your client was sitting on the other side of the table pulling for you rather than, if you hadn't done that, then what is this, and you'd better explain to them how this fit in with his needs or his problems. That still goes on today.

Just before I left, they had an informal program. They were doing exactly that. They were having clients come and listen to presentations and give their feedback. I guess I would say, in one way, the individual investigator has lost some time doing this, unless it doesn't cost anything. You can't just do something anymore. Even if you think it's pretty important, you'll have to convince somebody, other people, is the case.

RT: The agency historically has moved far from its earlier modus operandi, where we now consult with and work with industry, whereas it used to be more adversarial, and so we have progressed perhaps, particularly in the science area in this area, in this way.

LS: Yes. You know, standards development and guidance development, those are all by consensus. I've been involved in both of those activities. Those are generally done in collaboration with regulated industry. It just works out better. Some people say, well, maybe the standard or guidance isn't as rigorous as it would be if the agency did it entirely on their own. I think the medical center os a ;ott;e ;ess aitpmp,pis tjam ptjer

cemters/

I heard once that the medical amendments were the weakest ones because, by then, industry had gotten smart. They made a better negotiation than the previous ones.

Well, I think the abuses were, I mean, yes, there were some quack devices, but I don't know if it ever equated to like the quack medicines or drugs and things like that.

JS: Well, certainly our interests in FDA from the '38 Act until the 1960s, our regulatory interest primarily was quack devices. It wasn't the regular ones, the valid heart-valve devices and so on. But certainly by the late '60s, because this had come up as a possibility for consideration in the 1962 amendments, that devices were indeed left out. But certainly by the late 1960s on, clearly we're recognizing that the technology has changed so much with medical devices, and I'm sure that reflects the scientific interest in the industry.

I wanted to ask about the period, especially in the '90s, early '90s, mid-'90s. The agency is taking some considerable criticism about our role doing research, from the outside, from Congress, from critics of the agency on the outside. It might occur to some people, how is this perceived by people like yourself who are devoting their lives and careers in FDA to doing solid research? Is this something weighing on you at all?

LS: Well, I think we sort of got used to it, and we saw it in other arenas too. I mean, although maybe it's sort of a throwaway, but even now, if you apply to NSF (National Science Foundation) , which is considered the last bastion of pure research, there's a social relevance element in there. It might be pretty lightweight, but the fact it's in there

for grant applications suggests to me that there's no free lunch anywhere. So we probably got it worse than some other scientists, but I have friends in academia who have to hook up with companies. You know, funding changed; the federal government doesn't fund like it used to. A lot of profs work with companies or start their own companies and it gets convoluted. FDA runs into this problem of trying to find an independent expert with no conflict of interest. I don't know. It's getting harder nowadays.

So, yes. I mean, we found it kind of regrettable! It's kind of like the good old days. It sure was a lot of fun. I remember when I first started out with a nonprofit, it was fun to get a grant, do what you wanted, pretty much what you thought you should do for a year, and then report. Well, that's gone, and I think it's pretty much gone everywhere.

So I think we kind of got used to it and thought that, well, it comes with the territory. We're going to have to justify what we're doing.

RT: What are some of the principal organizations or associations that your discipline would relate to in the science area?

LS: Well, I guess I considered myself more or less working in the area of biomaterials, which sort of got shoved under bioengineering, at least in the beginning. And that evolved, too, because in the beginning they basically took materials off the shelf which were used for other purposes, and then they got into some troubles with compatibility and the body's rejection of it and things like that, material behavior.

And then that evolved into tinkering with these items, and they wanted to make inert materials, and it finally came to, well, that wasn't really possible, so now they want

to make biomaterials that interact positively with the body. Of course, the rise in biological knowledge feeds into that, so it's kind of evolved to smaller, more specially designed materials for implantation. Some pundits think ultimately, biomaterials will disappear when they can figure out how to tell a body how to repair itself. So it's cell behavior. I don't know whether that's possible. So the discipline has sort of evolved too.

Getting back to your questions about the discipline, I guess it's bioengineering. There's a Society of Biomaterials, some schools have biomaterials departments. Or sometimes it's mixed in with the materials science departments -- not so much with the chemistry. The chemists seem to have gone more towards molecules in the cell and cell biochemistry, and now they have departments of chemistry and chemical biology. I guess they're fascinated with it. I can understand because biology has come up with zillions of different molecules that do interesting things. Molecular motors are big now. The chemists decided they want -- biology has these molecular proteins and things which act like motors, wave the flag, do all of the chemistry or high function membranes. Chemists want to see if they can make those things, too. And this is kind of related to nanotechnology business, the latest science effort.

JS: So, have you gotten into that on a consulting basis with the agency?

LS: Not too much. We haven't really seen too many products yet.

It's kind of interesting because, rather than what happened, like in the '50s, the safety issues seem to be coming up pretty fast. So they're not waiting until we have hundreds of nanotechnology products out there and then finding out there are some

negatives. That's already being looked at.

JS: I know that, especially since you retired, you've been working with the agency on a consulting basis on drug-eluting stents. Is that right? Can you characterize what you've been doing?

LS: Well, I started to, after our various management changes, I got a little burned out, and I decided to be senior scientist. I guess it was 2003. And that's when the person who was filling at the division level had to interact with cardiovascular, and he came back and he said, "They're getting the new drug-eluting stents and they need some help, some polymer help." And I had my reputation established, so I said, "Okay, I'll help them out."

That kind of mushroomed; it really took a lot of time. But, like you were saying before, I got in on the ground floor. I worked on the very first one, the Cypher one from J&J, and I worked on Cordis, and I worked on the next one from Boston Scientific, and so on, so that was sort of ground floor, to help determine what our necessary studies were going to be for safety, and got involved in some of the standard development on the drug-eluting stent guidance.

Now, that was quite interesting because that was a combination product, so the Center for Drugs was involved too. They have their own ways of doing things, a lot of good things, but . . .

JS: How were the ways they do things different than the way the Center for Devices

does these things?

LS: Well, I think they, I personally was only knowledgeable about some of it, but overall, I think Drugs is very much, understandably, concerned about chemical stability, so stability during shelf life, stability during manufacture, quality control. They have something they call chemistry, manufacturing, and controls [CMC], which was pretty heavy-duty stuff. Of course, they probably developed it over 10 years compared to the kinds of things that we were asking for, but I thought it was pretty good. The downside was it really beefs up the submission, and a lot of it's not tremendously exciting stuff. Well, if we change the process a little bit here, it goes slightly off control, what happens, and so a company does all those variations to show that it doesn't matter, you know, it's minor changes. It needs to be done. But what I'm saying, for researchers, it's not exciting stuff.

The same way with the analytical method. What if the guy sets the instrument setting slightly different from what it's supposed to be. Does that totally wipe out the results or are the results still okay? So you look through a lot of the same data over and over again where they're proving that things are robust.

JS: Did you have much opportunity to work with people in the Center for Biologics?

LS: I didn't very much personally. A couple of my colleagues did work a little bit on, there were some problems with blood bags and some filters for blood, treatment of blood.

Some of the devices are sort of convoluted because some were, I forget how that

was divvied up, but some were done by Devices and some were done by Biologics when they involved blood. I think it had to do with the patient contact. I don't remember the details of it. So we did, my group, we did work with them a little bit on that. And the work often had to do with surface treatments of the filters.

JS: I wanted to go back, if you don't mind, just to one topic that we talked about, and I wouldn't feel bad if you need to use technical terms. People can look up what these things are if they need to -- but the research that you were involved in on food packaging. It struck me as being interesting to find out how to capture this, how you try to get a handle on a problem like this research-wise, what you look at, how you design your studies.

LS: Well, what they did was they came up with, because of the -- you're measuring a molecule moving from the packaging into the food. Food is a very complex matrix. Even though analytical chemistry has advanced tremendously, it's pretty challenging, so at least when I was there, the approach was -- and I think it probably still is -- to use what they'd call food-simulating solvents, and that involved sort of classifying foods. Like sodas, primarily water with some acid, so that's fairly straightforward. And then they'd have some things that were -- well, they didn't usually worry about dried foods too much. They'd have like fatty foods would maybe be simulated by an oil. It's a little harder on the analytical chemistry. I think there were a couple other categories also. Oh, acidic foods and non-acidic, I guess.

JS: So you were trying to find something that was like the physico-chemical nature of the food.

LS: Yes, so the solubilities are similar and so the migration rate is comparable of what you'd expect. And, of course, this is an approximation, and so there's discussions about whether it's approximately real case or worst case or tremendously worst case, and that kind of goes back and forth between the toxicology, because say it's extreme case but toxicology can live with that. Well, then it's kind of, okay, so what, you know. But when toxicology is real critical, the amount of migration really becomes a problem because knowing how much actually migrated becomes very important. There was some work there to develop some models for it, too. It became a little tougher for the larger molecules because the theory doesn't work as well for the bigger molecules, so that had some limitations.

Then, later on, they got into these higher-temperature things, and then I think they got into reusable plastics. That was another thing that came up. I was never involved in those.

I'm also a little nervous about black plastic. You just wonder what's in it. You know, black plastic should be things like garbage cans, the last line in a series, series of products.

Developing simulating solvents and the methods of analytic chemistry was a big part of food-packing research.

Generally speaking, the Center is, I would say, now pretty much methods oriented because I guess the sort of basic research questions where there's a big unknown are kind

of infrequent, maybe. At least that was my observation during my career there. We probably had maybe three or four cases where the agency was really stuck and we really needed to do the research thing, and maybe it was, circumstances were such that we couldn't really contract it out and get it done in a timely manner, or you had to deal with confidential information, which makes it tough, too, on contracting out. But the more what I call run-of-the-mill things like standards and standard test methods and fatigue testing and things like that seemed to go on.

When I was in the Division of Mechanics and Material Science, the director Marlowe, he was a big standards guy. He was an engineer, so I was kind of going upstream a little bit because I was still kind of researching and I preferred the more unknown type problems. Although there is often some unknown in the testing, too, especially if you're doing accelerated testing, because you have the question of how to extrapolate back to the real conditions. If you don't have a way of doing that, then the whole thing is kaput. Yeah. So I think my experience at Foods kind of prepared me a little bit for Medical Devices.

I realize you have a bit of a blur because of all the changes.

RT: I wonder if your time at Foods might have been during the tenure of Virgil Wodicka. Do you remember that name? I mention that because I think he was a scientist who came to the agency from the private sector.

LS: I just don't remember who the Foods director was.

RT: You didn't have to probably worry about that too much.

LS: No. I went to the Division of Chemistry and Physics, and John Howard I think was the director's name.

RT: Oh, yes.

LS: I remember when I left there, they were starting to get involved in animal residues, I think, residues of antibiotics given to the animals used for meat. But compared to Devices, they had it a little bit easier because they basically have a single exposure when you're eating or drinking.

RT: Yes. It's not a long-delayed reaction, is it, whereas some of these other things may take a while.

LS: Well, if it's a device, you know, geez, that could be in your circulatory system, digestive, maybe some brain shunt, you know, all over the place, different environments.

JS: Had you gotten involved in, before you retired from the Center, any research into reused medical devices and possible problems they might introduce into a system?

LS: Well, I wasn't personally involved too much with it. I was still in the Division of Mechanics and Material Science. That came up, and the director said, "Well, we've got

to have a polymer guy involved," so one of the other polymer guys was involved in it.

That turned out to be pretty challenging because, well, there were questions on various fronts: cleaning and then testing for any infectives or harmful agent after the cleaning, and then, what does the cleaning procedure do to the materials? Is it just surface, or are there bulk effects? You know, if you have a wide variety of things, that can be really quite challenging. So I think they focused more on the effectiveness. They did some work on materials, but material surfaces, well, unless you see things like cracking, it's not so serious, it's probably more a compatibility issue, especially if it's a blood-contacting device, and then there are some standard tests for that that can be applied. So I think reuse was sort of handled that way. I don't think they got too detailed on the chemistry on that problem. Like I say, I was more of a manager then. I wasn't so involved. But, yes, that was a pretty big deal for a couple years. Well, it was pretty important.

I think there were questions around, I think, about what some of the hospitals were doing cleaning for reuse, but I've heard now that it's pretty much gone to a third party.

RT: Through your career, you've been recognized at various points for your achievements. Are any of those ones that you want to comment on?

LS: Well, I received the FDA Commendable Service Award for some of the work I did at Foods when we worked on indirect additives and their exposure in intakes. We coupled the migration to the various food intakes so we could get a better idea of the

actual dosing, so that stands out.

Then, of course, the work we already discussed with the polyurethane degradation. We got the word out about the dialyzer degradation and some other, another dialyzer problem. We got an FDA award on that.

I guess those are the main ones. There are other ones. I have a couple other mementos, like I got from Burlington, I think, a CDRH hat, and a t-shirt for, something with management. I can't remember what the hell it was for. I think for just keeping things going through some crises time or something.

RT: All right. A noteworthy achievement, of course.

Let me ask you, if I may. As I recall in our earlier discussion, we went directly from your higher education to FDA. In the interim, were you in other agency or other government work prior to FDA?

LS: Well, I did work at NBS.

JS: We talked a little bit about the National Bureau of Standards.

LS: I went to the NBS on a two-year postdoc, and I worked there a little while, and then I went over to the American Dental Association Health Foundation at NBS, where I first encountered -- well, at that time they didn't call it biomaterials; they called it dental materials. But I was in the NBS Medical and Dental Materials Section. But that was still pretty much manmade materials.

RT: Yes, and that wasn't really government per se, was it? It was more private . . .

LS: Well, it was a lot smaller. I mean, the ADA was probably the bigger part of it. Since then, NIST has boosted up biomaterials. Biomaterials have come up, and then the industry has asked for their help on various things. So they now have quite a few biomaterials people. I think they may even have a section. But when I was there, their part of biomaterials was maybe three or four guys, I guess. And ADA was leading various aspects of it. So that was sort of my first exposure to it. And I actually did publish in the *Journal of Biomedical Materials Research* in 1980, and that was work that I had done at the ADA Health Foundation.

I went to Foods afterwards. In Foods, we went back to more classical materials because food packaging is manmade. Well, problems of polymer degradation probably came up, at that time also, no biodegradable polymers were involved, but I think it's the manmade, just like the medical products we kind of skipped over. Besides the breast implants, we had a workshop on barrier products during the AIDS crisis and we got involved in barrier products like condoms and medical gloves. I actually contributed to a chapter on gloves, and then in the second version, that was one of my last efforts. We did the chapter on glove testing and quality control.

JS: Chapters in what?

LS: In a book, dermatology, of all things. I don't know if you want the name, but

here's the book. I kept it because it's kind of far afield for a chemist.

My colleagues and I wrote a chapter in that book. It's the second edition.

JS: Oh (holding book). This is edited by Anders Bowman et al., called "Protective Gloves for Occupational Use." Interesting. It looks like you have a number of signatures in this, too.

LS: Yes, for old time's sake.

JS: This brings up another point I wanted to ask. Your publications. Now, was publication something that was encouraged, and was it something that was recognized, at least in your Center?

LS: Well, it certainly was -- when I first started in the Bureau of Radiological Health, it certainly was recognized and encouraged. That was a measure of performance. When I went to the Division of Mechanics and Material Science, I don't think it was quite as much emphasized. They had things like internal reports, but the Rad Health guys weren't that impressed with them, although the reports were sometimes useful. They'd be on some technical problem that maybe wasn't publishable.

Well, a lot of the studies sort of don't fit into standard hoppers so easily because if you're doing product performance on something, then you've got to find a journal that deals with that, and sometimes that's hard to find because there may be user journals that

have some articles on it, but they're not really centered around that. But, yes, I'd say it was encouraged. I don't think, however, not quite as much in Mechanics and Material Science.

But that sort of didn't fit in with the OST and now the OSEL (Office of Science & Engineering Laboratories) mission because if you're supposed to be the scientific technical heavyweights, the last-word internal consultants, you do need a certain recognition and reputation, and a good way of showing that is that you published something in the outside world.

RT: Now, during the past years, we've gone through several performance appraisal systems, one of which was Management By Objective. Was that a difficult descriptor process for a research person? In other words, can you define, or can you anticipate and properly define criteria by which your performance will be evaluated?

LS: Well, sometimes that was a little bit of a forced fit. You'd have to be creative and come up with something . . .

TAPE 2, SIDE B

LS: It was interpreted fairly loosely. If it sounded plausible, at least when I was involved in it, that more or less worked.

Now, things that were countable tended to be more easily used. I mean, I would certainly argue, well, nobody can really determine for FDA's purpose what quality is, so

counting is easier. And, of course, that fits right in with the Office of Device Evaluation, where I remember years ago they had an economist who was the head of it, I think, and he used to say, "So many approvals this month." And I used to think, "Boy, this is just like corporate PR work, so many units produced this month," which would fit right into their scheme of things. You know, it still fits. With the researchers, papers, talks, and reports are pretty much the countable items. If you don't have them, then it's kind of hard. What the heck are you counting, how are you envisioning performance? For researchers, it kind of backfired and now numbers became primary, so now if you can't count it, you kind of have problems.

RT: That's what prompted my question. Widget counting would be perhaps a little more elusive, I would think, in a research arena than in regulatory inspections and so on.

LS: Yes. The trouble is timing, I think -- I asked my thesis advisor about this once, you know, I asked him sort of a question along the lines, "Well, who's going to be prominent?" and he said, "Well, that'll come out in 20 years." That's true. After 20 years, you can sort of say who the best performers are.

But the interesting thing about it, at least in my graduate class, that the relative ranking, pecking order, is pretty similar now to what it was then. The top guys did the most and then, you know, second rank, like me, did some more, and then you go on down. So it sort of followed early indications. There are probably some exceptions.

But, yes, sometimes we had to be a little creative about trying to interface with the management system.

RT: There's been some discussion, at least I think, by Congress and others about whether the Food and Drug Administration is really a scientific-based agency. What's your impression as to the trend or the accuracy of that criterion?

LS: Well, that's pretty interesting because when I first came to the FDA, there was plain old science. They just called it science. Then as the years went by, I guess there was a need, or maybe because plain old science wasn't distinctive enough or descriptive enough, this term regulatory science came up, which I don't think we ever really got a handle on what that was. I guess it was sort of science in support of regulation, so it's probably some kind of applied science. Then we progressed from there to science basis, which to me smacked of a little bit of, well, we're going to make a decision for some political reason and we need to try to find some scientific rationalization for it so we can sell it to the public. So I don't know what's next.

RT: Thank you.

LS: So we went from science to science based, through regulatory science to science based.

JS: Well, one could argue that they are indeed political descriptions of the same thing you've been doing, but it's a way of defending our role in doing science to an appropriating body that might not necessarily recognize that we need to understand what

we're working with to do our jobs.

LS: Well, I think for the more pure scientists, we keep getting reminders of this, like the morning-after-pill incident. I think was another reminder that, for us scientific types, it looks to me like the science was there, but the politics was not, so the science got trumped over. And, of course, Susan Wood got really bent out of shape about it. She left FDA and spoke out at several times. So we hear, you know, FDA is a science-based agency, but then we have such events. And some scientists have more trouble with it than others.

Well, it is kind of discouraging. If you worked pretty hard on something and you gave it your best scientific shot, and then this goes poof because something else totally trumps it over, it's . . .

JS: It's got to be discouraging.

LS: It's discouraging. Fortunately, that didn't happen too often. But then, you have the other side sometimes where if you're fortunate and something is kind of on a tipping balance, it may go this way or that way and it needs a little push, and you happen to do the right sort of study to do the little push, and then maybe down the road you can see, "Well, I managed to tilt that one," it's sort of satisfying.

JS: You've worked under a number of directors of the Center, I suppose beginning in the Bureau with John Villforth. Was John Villforth the director at that time?

LS: Yes.

JS: And so you've worked with a number of directors in the Center for Devices since then. Now, I don't know to what extent you had contact with these folks, but to the extent you did, can you kind of characterize your perceptions of the Center under these individuals, what their impact was, in your eyes, on the work of the Center?

LS: Well, you know, that changed over the years, too. I played volleyball with John Villforth, and the last Center director, I think I barely saw once in a while.

I sort of think that John Villforth was pretty good. He seemed to know how to handle things. He seemed pretty definite, decisive, and maybe it was the times or something. Some of the others don't . . . Also, I think his tenure might have been considerably longer than some of the subsequent ones.

Some of the others, like Jim Benson, I don't remember too much about him. And then we had -- who else did we have? We had Bruce Burlington for a number of years. I didn't interact with him too much. I had heard that he was sort of gruff. My limited interaction with him was okay, although I sort of had the impression that you wouldn't want to bullshit or you'd be in trouble. And David Feigal.

JS: You overlapped with him, I think, did you not?

LS: Yes. It seemed to me like he liked gadgets. His talks always had a lot of slides

with devices and gadgets. And he had one slide that we liked. He called it "Small Things Matter," and he had all these problem cases where, well, several cases, probably about six or seven of them. I remember one was where a company changed a process. They had a big recall. And there was another one where a company was doing a washing step, and some advisor told them this didn't do anything, but it turned out that it -- it didn't do what it was intended to do, but it removed some other impurity, and then they got into trouble over that. So he had all these little cases. So we kind of jumped on that and said, "Well, this is the kind of overlooked materials problems we address. So we used that a couple times and he once in a while recognized it.

Well, he was, I guess he's remembered -- and I remember, too, for the TPLC concept [total product lifetime cycle]. That was his big thing, total product lifetime -- I forget what the "C" is, oh, Cycle. We had all these cycles all the time.

JS: What was this all about?

LS: Well, that a product is envisioned. There's a design stage, it goes through development, clinical trials, used in the market, finally becomes obsolete, and then maybe needs to be disposed of. The environmentalists call it dirt to dirt. That's the ideal situation. You start with dirt and you end up with dirt. So it was kind of like that.

And then he had the different interactions, where the Center played into the different aspects. I guess the idea was that it sort of counteracted the idea that, well, I'm not saying that the agency was wrong, but it used to be that approval was kind of a very definite point. A company got approval, and then, if nothing ever happened at first, they

kind of were home free. Right? That was it. There was never any more interaction.

Well, now it's a little different because a lot of times they have conditions of approval. They're supposed to be doing studies after approval, so it drags on. And I think his TPLC concept played into that a little bit. Well, you know, you can't just approve it and then forget about it. It's got to be addressed post market. And that waxes and wanes.

Now the latest I heard, they were going to reemphasize post-market. Post-market is, well, it's very resource intensive, too, doing those things to monitor products. Problems always have a convolution with the user. Is it due to the device itself, or is the user -- I mean, sometimes the medical community gets to misusing it somehow or started using it for some purpose other than the labeled purpose, that kind of stuff.

So, yes, I think that the interaction with the Center director is, I guess I would say it probably diminished, maybe it's necessary because of the size of the Center.

RT: Well, are there any other areas that we want to touch on before we conclude? We've covered quite an expansive and varied career experience which you've had with the agency. Do you have, in your retirement, any science pursuits in mind?

LS: Well, I might do a little consulting. I don't know. We're thinking about moving to North Carolina, near Hendersonville, near the mountains. My wife has a little idea that she wants to try. It's more like a plant and crafts. So I may do -- there's already a firm down there which does some device consulting, so I might try to hook up with them. I don't know. We have to see. There are lots of things up in the air right now. Maybe.

I had a bite from another fellow who does safety and forensic engineering. He said, "Oh, you're a materials guy. I could use some materials help." So I'm kind of open to that now just to think about something else for a change, because he doesn't work with medical materials, but structures, buildings, things like that, which might be interesting.

JS: It would be a nice change.

LS: Yes. Well, I've changed direction, as you can probably ascertain. I've done it a couple times before. When I got to the point where I was going to have to make a change, I kind of liked to make it a significant change rather than just tinker. And that's kind of the way I felt about OSEL, and I think the research game is really getting pretty hard now; reduced to looking for outside money, which is pretty tough.

CDRH instituted a new, great big review mechanism for all the projects. It seems like a lot of overhead to me, and I don't know if history is going to prove out that what they do under that system is really, in the long run, going to be better than the local control or not. Maybe.

JS: Do you know what prompted that change?

LS: Well, I think it was partially industry. It's partially, I guess, connected with MDUFMA [Medical Device User Fee and Modernization Act] because I guess it was Biologics had a somewhat similar innovation, and industry was paying the fees and then the Center for Biologics was kind of rearranging the money, and some of it was going to

the researchers. Industry absolutely did not like that! When they're putting up money and paying these fees, these review fees, now, that better be going towards the review process. So I think that may be a part of it.

Like you said, there's some outside criticism about, why does FDA need to do research? They're regulatory. Well, we didn't get into it, but that's sort of related.

The way the European system works is they have accrediting bodies. Those third parties look at stuff. I don't know if they get into it deeply. I don't know. I'm not that familiar with it. I sort of doubt that they get into the same amount of detail that FDA does, but I'm certainly under the impression that they could take things off the market a little faster, too. It's a little more like easier on and easier off the market.

I think those things all played into this business about FDA also prompted creation of a mechanism for determining what research they were going to do. First they were going to do this every year, and, of course, the scientists crowed. "My gosh, yearly proposals and funding, what I'm going to do." Well, now they backed off, and maybe they can stagger it and do like a three-year cycle, which I think is more reasonable. Plus, the funny funding cycle; and this isn't really the agency's fault, but with Congress mucking around, you don't get research funding now until January, so one quarter's gone, and then they keep pushing the cutoff, like August or September, you're supposed to be done. So you have almost a six-month window for spending -- that's a heck of a way to run a research shop. So it's getting pretty tough.

JS: Well, do you think this is costing the agency the ability to attract good, young scientists?

LS: Well, I think that has a lot to do with what goes on in the outside market. Five years ago, when I was trying to hire, I had quite a few people tell me, "No, I think I can do better." OSEL isn't that researchy, and there's too many other sides to it, you know, a consulting side. Well, then OSEL had another hiring round about a year ago, and they got some pretty good people. Well, some people like the FDA mission and I can understand that. It does have a good feeling -- like the mantra of FDA, you know; public health is a nice-sounding thing. I've read quite a few books on careers, and I agree myself that you'd rather be doing that than working on some widget that nobody really needs. But then, of course, the FDA execution is not as maybe as nice and high as the goal.

JS: Well, but as you yourself stated, when you realized at one point that what you were doing, the impact it had on real life, that was pretty impressive.

LS: That was pretty impressive. That was pretty satisfying. I also sort of think it's kind of rare, too. There aren't too many situations like that. I mean, we did quite a number of things, but for a lot of them, the impact was more diluted or was one of many factors, that kind of thing. The same way with standards. You have your input, industry has their input; through some give-and-take, you get some sort of consensus that's acceptable to everybody, but maybe not what each individual really would like. So we have more of that.

I don't know. I think it's really pretty much an applied-research shop now, except

perhaps if you've got something, like in some of the newer types of ultrasound, high-intensity or something, where you've got some unknown possible biological effect. Maybe nanotechnology would fall under that now, too. That seems to sell a little better lately.

Just as a humorous aside, when I first got to Foods, I listened to a talk by the microbiologist, and he was talking about infection, and he kept referring to an epidemic all the time. He kept saying, "Why aren't you preparing for the worst?" Afterwards, I talked to him. I said, "Why did you harp so hard on the potential epidemic of this?" because it wasn't obvious there would be one. He said, "Well, nothing opens pocketbooks like fear."

So, you know, I think to a certain extent, when the agency is faced with some big unknown, and they have no good way of getting a handle on it, if you can plug your research into one of those situations, you probably have a better chance. And for some people, that's pretty challenging. They go back to a more methodologies approach. So I think, in my view, it's probably less research and more technology. It's a very slow, long decline, but there are other evidences.

I remember one time our office put out the number of papers published per year (declining) as a function of time (years). Well, it's pretty hard. I managed to do it at first, but, of course, I was a manager also. I think I averaged about one or two a year, probably one a year. Compared to the academics, it isn't much, but . . .

Well, I figured that out once from some data published on this, what it really amounts to is if you count all the people who are working with the author, it really comes out to one paper per person per year. That's the real output.

JS: Some post-docs, some technicians.

LS: Yes. If you factor in everybody who's working on it, this guy that has a hundreds of publications, that's really what it came to.

Don Marlowe at one time said, "Well, we were supposed to consult 50 percent of our time." And so then I was sort of, when I was brand new, I sort of thought that, "Well, we should sort of average a half paper a year, on average." I don't know if we really held to that because it would fluctuate. We thought that was pretty doable. I don't know if they're achieving that now.

Well, they have a lot of consultants available for help.

Well, you know, you were talking about the relationship between them. It's kind of sometimes love-hate. I guess what they like is, they like having researchers sort of on the shelf, and when they need to tap this expertise, when they really need it, it's great for them to go there. It's readily available, so tap it, as long as it doesn't cost anything. Otherwise, they sort of ignore it. You know, what are those researchers doing anyway?

But I didn't experience that too much on a personal level, because I guess I was one of those deemed helpful. Most of my clients are pretty satisfied. Of course, that didn't necessarily translate into increased funding for me, but at least I got recognition. I could bask in that.

RT: Dr. Schroeder, we really appreciate the interview you've given us, and we look forward to getting a transcript to you for editorial review.

LS: Well, I enjoyed doing it.

JS: Thank you so much. It really adds substantially and fills in many gaps in our oral history program. It gives, I think, considerable insight into the work of a scientist in the agency, and the Center for Devices in particular. So thank you so much.

LS: Sure. You're welcome.

END OF INTERVIEW