CHEMICAL HERITAGE FOUNDATION

JERRY R. FAUST

The Pew Scholars Program in the Biomedical Sciences

Transcript of an Interview
Conducted by

Andrea R. Maestrejuan

at

Tufts University
Boston, Massachusetts

on

18, 19, and 20 February 1997

ACKNOWLEDGEMENT

This oral history is part of a series supported by a grant from the Pew Charitable Trusts based on the Pew Scholars Program in the Biomedical Sciences. This collection is an important resource for the history of biomedicine, recording the life and careers of young, distinguished biomedical scientists and of Pew Biomedical Scholar Advisory Committee members.

From the original collection at the Center for Oral History Research, UCLA Library, UCLA.

The following oral history, originally processed at the UCLA Center for Oral History Research, has been reformatted by the Chemical Heritage Foundation. The process involved reformatting the front matter, adding a new abstract, replacing the table of contents, and replacing the index. The paragraph spacing and font of the body of the transcript were altered to conform to the standards of the Oral History Program at the Chemical Heritage Foundation. The text of the oral history remains unaltered; any inadvertent spelling or factual errors in the original manuscript have not been modified. The reformatted version and digital copies of the interview recordings are housed at the Othmer Library, Chemical Heritage Foundation. The original version and research materials remain at the Darling Library, University of California, Los Angeles and at the Bancroft Library, University of California, Berkeley.

REFORMATTING:

Holly Polish, Program Intern, Oral History, Chemical Heritage Foundation. B.A. History, American University.

David J. Caruso, Program Manager, Oral History, Chemical Heritage Foundation. B.A., History of Science, Medicine, and Technology, Johns Hopkins University; PhD., Science and Technology Studies, Cornell University.

UNIVERSITY OF CALIFORNIA, LOS ANGELES

Oral History Interview Agreement No. 970227

This Interview Agreement is made and entered into this 26th day of May, 1998 by and between THE REGENTS OF THE UNIVERSITY OF CALIFORNIA, a California corporation, on behalf of the Oral History Program at the UCLA campus, hereinafter called "University," and JERRY R. FAUST, having an address at Tufts University School of Medicine, Department of Cellular & Molecular Physiology, 136 Harrison Avenue, Boston, MA 02111, hereinafter called "Interviewee."

Interviewee agrees to participate in a series of University-conducted tape-recorded interviews, commencing on or about February 18, 1997, and tentatively entitled "Interview with Jerry R. Faust". This Agreement relates to any and all materials originating from the interviews, namely the tape recordings of the interviews and a written manuscript prepared from the tapes, hereinafter collectively called "the Work."

In consideration of the mutual covenants, conditions, and terms set forth below, the parties hereto hereby agree as follows:

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5. To insure against substantive error or misquotation, Interviewee will have the right to review the manuscript before it is put into final form. University therefore will send Interviewee a copy of the edited transcript for review and comment. Interviewee will return transcript and comments to University within 30 days of receipt of the transcript. In the event that Interviewee does not respond within 30 days, University will assume that Interviewee has given full approval of the transcript.

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If to Interviewee:

Jerry R. Faust
Tufts University School of Medicine
Department of Cellular and Molecular Physiology
136 Harrison Avenue
Boston, MA 02111

University and Interviewee have executed this Agreement on the date first written above.

INTERVIEWEE

(Signature)

Jerry R. Faust
(Typed Name)

Tufts University School of Medicine

136 Harrison Avenue
(Address)

Boston, MA 02111

Date FEB. 18, 1997

THE REGENTS OF THE UNIVERSITY OF CALIFORNIA

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(Typed Name)

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Date 5/26/98

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JERRY R. FAUST

1948 Born in Sterret, Texas, on 10 March

Education

1971 B.S., Stephen F. Austin State University
1974 M.A., University of Texas at Arlington
1991 Ph.D., Sackler School of Graduate Biomedical Sciences, Tufts University

Professional Experience

Tufts University School of Medicine, Boston, Massachusetts
1987-1991 Doctoral Student Researcher, Department of Physiology
1991-present Assistant Professor, Department of Physiology

1971-1972 Associated Dallas Laboratories, Dallas, Texas
Analytical Chemist

1972-1974 University of Texas at Arlington, Arlington, Texas
Research Assistant

University of Texas Southwestern Medical Center at Dallas, Dallas, Texas
1974-1977 Research Technician, Departments of Internal Medicine and Molecular Genetics
1977-1985 Research Associate

1985-1987 E.I. Du Pont de Nemours and Company, Experimental Station, Wilmington, Delaware
Principal Investigator, Cardiovascular Unit

Honors

1992-1994 Harcourt General Charitable Foundation New Investigator Award
1992-1996 Pew Scholar in the Biomedical Sciences
1995-2000 American Heart Association Established Investigator Award

Selected Publications

Brown, M.S. et al., 1975. Role of the low density lipoproteinreceptor in regulating the free and esterified cholesterol content of human fibroblasts. *Journal of Clinical Investigation* 55:783-93.

Faust, J.R. et al., 1977. Receptor-mediated uptake of low density lipoprotein and utilization of its cholesterol for steroid synthesis in cultured mouse adrenal cells. *Journal of Biological Chemistry* 252:4861-71.

Faust, J.R. et al., 1980. Synthesis of 2-isopentenyl tRNA from mevalonate in cultured human fibroblasts. *Journal of Biological Chemistry* 255:6546-48.

Faust, J.R. et al., 1982. Regulation of synthesis and degradation of 3-hydroxy-3-methylglutaryl coenzyme A reductase by low density lipoprotein and 25-hydroxycholesterol in UT-1 cells. *Proceedings of the National Academy of Science USA* 79:5205-9.

Faust, J.R. and M. Krieger, 1987. Expression of specific high capacity mevalonate transport in a Chinese hamster ovary cell variant. *Journal of Biological Chemistry* 262:1996-2004.

Liscum, L. and J.R. Faust, 1987. Low density lipoprotein (LDL) -mediated suppression of cholesterol synthesis and LDL uptake is defective in Niemann-Pick type C fibro-blasts. *Journal of Biological Chemistry* 262:17002-8.

Liscum, L. and J.R. Faust, 1989. The intracellular transport of low density lipoprotein-derived cholesterol is inhibited in Chinese hamster ovary cells cultured with 3-β- [2-(diethylamino)ethoxy]androst-5-en-17-one. *Journal of Biological Chemistry* 264:11796-806.

Faust, J.R. and J.F. Dice, 1991. Evidence for isopentenyladenine modification on a cell cycle-regulated protein. *Journal of Biological Chemistry* 266:9961-70.

Faust, J.R. et al., 1994. Two related proteolipids and dolichol-linked oligosaccharides accumulate in motor neuron degeneration mice (*mnd/mnd),* a model for neuronal ceroid lipofuscinosis. *Journal of Biological Chemistry* 269:10150-55.

ABSTRACT

Jerry R. Faust began his childhood on his father's farm in rural Texas. When his parents divorced he moved with his mother, a nurse, and his brother to Dallas, Texas, where he attended junior high school and high school. When he was in eighth grade he took an advanced biology class in which the newly-discovered ATP was discussed at length, but in high school he "left biology" for chemistry. He loved chemistry, a field that was really taking off at the time. A high school chemistry teacher proved an important role model, and an influential school trip to a research laboratory confirmed his desire to become a scientist.

Faust's chemistry teacher was also the basketball coach, and Faust played well enough to be offered a basketball scholarship to Stephen F. Austin State University. As he says, he went to college to play basketball, not to learn, so he rejected an offer from Rice University, as studying might have gotten in the way of basketball. At Austin State he declared a major in chemistry and minored in biology, soon developing an interest in biochemistry. He considered working in biochemistry to be a way to make a contribution to society.

After graduation Faust took a position as a chemist. He spent a boring year testing materials before deciding to go to graduate school. He took a biochemistry course taught by Edward Bellion, and entered his lab at University of Texas at Arlington. There he continued to develop his interest in biochemistry. He felt he had certain advantages coming to biochemistry as a chemist rather than a biologist. After finishing a master's degree, Faust accepted a position as research associate in the Michael S. Brown and Joseph L. Goldstein lab at the University of Texas Southwestern Medical Center in Dallas. Faust describes Brown's and Goldstein's backgrounds; his role in the lab's work on cholesterol metabolism; and learning opportunities in the lab. He also explains their Nobel Prize for research into LDL. After being there for eleven years he went to E.I. DuPont de Nemours and Company as a principal investigator in the cardiology unit. Faust describes the structure and research resources of the Du Pont Experimental Station and his projects there. He explains his professional satisfaction in designing and implementing research per se, irrespective of clinical applications.

Faust's preference for following tangents rather than pursuing a strictly linear line of research led him next to the decision to pursue a PhD in the physiology department at Tufts University, where he entered James Fred Dice's lab. Being a student again was different and strange. Here he discusses how the need to meet funding requirements affects the direction of research; the value of funding sources that allow for creative research; and the advantages of increasing cooperation between labs. He continues with a discussion of Dice as a mentor; his own mentoring and managing style; influence on his research of the Pew Scholars Program in the Biomedical Sciences award; grant writing; and competition with Peter Pentchev's lab over work on cholesterol transport in Niemann-Pick type C disease.

He has more to say about the competition with the Pentchev lab; differences between the grant review process at the National Science Foundation and that at the National Institutes of Health; science funding in general; and his lab's work on neuronal ceroid lipfuscinosis. Collaboration with foreign labs leads to foreign students, difficult to fund and difficult to place after graduation, especially since principal investigator positions are so scarce. He finishes with a description of how he and his partner, also a scientist at Tufts with whom he collaborates on projects, balance their work life with their home life.

UCLA INTERVIEW HISTORY

INTERVIEWER:

Andrea R. Maestrejuan, Interviewer, UCLA Oral History Program; B.A., History, University of California, Irvine, 1988; B.S., Biological Sciences, University of California, Irvine, 1988; C.Phil., History, University of California, Riverside.

TIME AND SETTING OF INTERVIEW:

Place: Faust's office, Tufts University.

Dates, length of sessions: February 18, 1997 (117 minutes); February 19, 1997 (147); February 20, 1997 (146).

Total number of recorded hours: 6.85

Persons present during interview: Faust and Maestrejuan.

CONDUCT OF INTERVIEW:

This interview is one in a series with Pew Scholars Program in the Biomedical sciences conducted by the UCLA Oral History Program in conjunction with the Pew Charitable Trusts's Pew Scholars Program in the Biomedical Sciences Oral History and Archives Project. The project has been designed to document the backgrounds, education, and research of biomedical scientists awarded four-year Pew scholarships since 1988.

To provide an overall framework for project interviews, the director of the UCLA Oral History Program and three UCLA faculty project consultants developed a topic outline. In preparing for this interview, Maestrejuan held a telephone preinterview conversation with Faust to obtain written background information (curriculum vitae, copies of published articles, etc.) and to agree on an interviewing schedule. She also reviewed prior Pew scholars' interviews and the documentation in Faust's file at the Pew Scholars Program office in San Francisco, including his proposal application, letters of recommendation, and reviews by Pew Scholars Program national advisory committee members.

For general background on the recent history of the biological sciences, Maestrejuan consulted J.D. Watson et al., *Molecular Biology of the Gene.* 4th ed. Menlo Park, CA: Benjamin/Cummings, 1987, and Bruce Alberts et al., *Molecular Biology of the Cell.* 3rd ed. New York: Garland, 1994.

The interview is organized chronologically, beginning with Faust's childhood in rural Texas and continuing through his education at the University of Texas at Arlington, his work as a lab technician, his doctoral work at Tufts University, and the establishment of his lab there. Major topics discussed include the advantages of learning biochemistry in the context of chemistry, Faust's role in the Michael S. Brown and Joseph L. Goldstein lab's work on cholesterol metabolism, Faust's own work on cholesterol mobility and transport, and funding in the sciences.

ORIGINAL EDITING:

Jane Collings, senior editor, edited the interview. She checked the verbatim transcript of the interview against the original tape recordings, edited for punctuation, paragraphing, and spelling, and verified proper names. Words and phrases inserted by the editor have been bracketed.

Faust reviewed the transcript. He verified proper names and made minor corrections and additions.

Collings prepared the table of contents and biographical summary.

Gregory M.D. Beyrer, editorial assistant, assembled the interview history.

Kathleen McAlister, editorial assistant, compiled the index.

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INTERVIEWEE: **Jerry R. Faust**

INTERVIEWER: **Andrea R. Maestrejuan**

LOCATION: **Tufts University**
Medford, Massachusetts

DATE: **18 February 1997**

MAESTREJUAN: To begin with, I'd like to ask when and where you were born, and perhaps you could tell us a little bit about your family background as well.

FAUST: Okay. I was born in 1948 in Sterret, Texas. It's a small town about halfway between Dallas and Waco. My mother [Mary Elizabeth Pigg Faust] was a registered nurse, and my father [Jack Rudolph Faust] was a farmer at that time. However, my parents separated and finally divorced about the time that I started junior high school. At that time, my brother [John Cairy Faust] and I and my mother moved to Dallas, where I went to junior high school and high school. I guess my background is pretty nondescript for someone who started out on a farm and had some of the typical experiences, mostly unrelated to sophisticated science, but then when I look back on it, there was some interesting biology going on, as most people would witness from a farm.

But I think the first type of exposure to, quote, "modern-day science" appeared in the eighth grade. I was fortunate to be in a science and math honors program. I had good grades in those particular courses and not so good in other courses. Then in eighth grade, I took ninth-grade biology, which was just an advanced level course. And there was an article that had appeared in *Look* or *Life* about the discovery of a new molecule within cells that had enormous ramifications on cell metabolism, and that molecule was ATP, adenosine triphosphate. Actually, I think it was a series of articles.

So my biology teacher made copies of those articles and we spent about three or four days talking about ATP. But this was a little bit hard to fathom, because at that point, biology was animals and plants and occasionally an organ, like a heart or some part of a salamander that we'd dissected, or a leaf.

To all of a sudden begin to think about what was inside of the organ or the cells was kind of a new experience. That was really about all I saw to the inside of cells for a long time, because I left biology, so to speak. When I went on to high school, then, still being part of a science honors program, I took chemistry and really kind of fastened myself to the physical sciences like chemistry and physics more so than biology. I guess as we get older and gain more knowledge, then it's easier to grasp on to the new stuff that amazes you than it is to remember some of our old fondnesses or remembrances about that excitement in the eighth grade, versus

that excitement in the tenth grade.

But I liked chemistry. You know, it had all that magic to go with things that happened in changing colors and producing gases and explaining a lot of what we were seeing as we moved closer to the twentieth century. [E.I.] Du Pont [de Nemours and Company] 's motto was "Better living through chemistry." It was certainly, at that time, true-- All the new things that were being produced as a result of synthetic chemistry.

And then I was fortunate then that-- I forgot now. What did I take in the ninth grade if I took biology in the eighth grade? Did I take any science in the ninth grade? Because it wasn't until I got to high school that I went into chemistry. My high school chemistry teacher, at the end of the sophomore year, asked me to go with him to a symposium that was being held at the University of Texas, in Austin, specifically for high school students to-- I guess to get them acquainted with advances in science. And although I hadn't had any physics at the time, this particular symposium was geared towards high school students, and it dealt with discussions and distribution of the technology and knowledge associated with fusion versus fission as a means of making energy.

So we heard talks from eminent physicists who I didn't know, and then we saw laboratories, and then we spent three days and two nights wandering around on this enormous educational complex, which was a far cry from W.H. Adamson High School and Sterret, Texas. And then we even went into the physics buildings and walked through the laboratories where graduate students, I suspect, and postdocs and faculty were doing experiments.

Even though I had read about people who were working in that kind of environment and had a mind's eye view of what went on in research laboratories--what did people do and how did they react and what did they look like and did they all wear lab coats--I'd never been in one, or a building that did that stuff on every floor, in every room. So that really amazed me. It set me to thinking that these things really exist and that there are whole groups of people that do this kind of stuff for a living. And the opportunity to just walk through there for a few hours in my life I think had a significant impact on saying, "Well, these opportunities exist for people, research, whether it's nuclear physics or chemistry or biology or what have you."

Then I think the chemistry teacher [Kenneth Brashear] was a role model for me. At that time, I was not associated with my father, so I didn't have--and other than my mother and my brother--any male role models. And he was a good guy, and he was also an assistant basketball coach--and I played basketball, so that elevated his status in my mind. He was a chemistry teacher who was a basketball coach. He, I think, did a lot to-- Not only taking me on that excursion to UT [University of Texas at Austin], but motivating me to look at opportunities and all sorts of endeavors, whether it's chemistry or biology or physics.

The rest of high school is just kind of nondescript. I got into being a high school student and a basketball player and all that other stuff that goes along with it, so you really don't think much-- Or I'll take that back. I'm sure a lot of people, especially in this part of the country-- Kids think about what they're going to do when they get past eighteen. What colleges are they

going to go to? What are they going to do after college, whether they're going to go to med school or vet school or going to be--? Law school? Are they going to go into business or whatever? And quite frankly, I didn't give it a single thought, up until well into my senior year. I really didn't have much aspiration to go to college, but I didn't know what else I was going to do, either.

That part of the country's a little bit different--as I've been around a little bit more now--in the expectations for even middle-class people, especially back in the sixties and late seventies. I mean, there was a lot going on in the country with youth and young adults. You know, the war had an incredible impact, certainly on men, or young men, and I suspect on women too. But there just wasn't much talk about going to college or what we wanted to do or anything like that.

MAESTREJUAN: But you did have an idea that if you said, "Oh, I want to grow up and be a scientist," you had an idea of what a scientist did?

FAUST: That's it.

MAESTREJUAN: Because of this one trip, or had you before that, your mother being an RN [registered nurse]?

FAUST: She was an RN, and I had been around hospitals and been in-- Not in operating rooms, but just as if you would go visit a sick friend, occasionally I'd go visit my mother on the wards, or in hospital rooms, but never to the inner workings of a hospital. These were smaller hospitals, so they didn't have any research labs. They had clinical labs, but I don't think I ever went in one of those.

But I think you're right, in that that one trip just showed me, physically and tangibly, that research goes on and what the people look like, not that they're any different than anybody else, but kind of had a feel for, or better focused a picture about, how their life worked in a professional setting. Whereas that's easily obtained for people every day, if you were looking at lawyers or policemen or sales clerks or insurance agents or whatnot. We encounter these people every day. Allow me to ramble on a little bit more. What I'm leading up to is that one of the problems, I think, with our profession is that we don't have the opportunity, or we don't provide ourselves with the opportunity, to let other people in different aspects of our society see us on a day-to-day basis and know what we do. I have some very educated friends who I deal with at different times of the year, lawyers and even doctors, that still think that I wear a white coat every day and I have goggles on and I go off into a laboratory and actually do experiments, which-- And all that's not even true. I don't wear a white coat. And I don't do many experiments these days. And I don't wear goggles. You know, they just don't have an idea about what goes on day-to-day in a research laboratory. So that experience at UT gave me that kind of

opportunity to see and to realize and to think about placing myself possibly in that situation, later down the line.

MAESTREJUAN: And why do you think--this is jumping ahead a bit--but why do you think that there is not this public conception of a scientist?

FAUST: Yeah, I thought about that for a while, too, every time I bring this up in my mind. And I suspect maybe a little bit it's got to do with old fiction about scientists and Dr. Frankenstein, and maybe science fiction, as it was created, has allowed society to say, "Okay, it ' s acceptable that we don't know what these people do because it may not all be good or it may be too confusing. We may not understand it. It's okay that we don't try to understand, whereas we need to know, to a certain extent, what a lawyer does, because he may become a politician." [laughs] Or a doctor, because we deal with these people every day. So we need to have kind of a conceptual framework about what their thinking is and what they're saying and how they work, in order for us to understand what they're trying to tell us.

But there is no real need, I guess, for society to understand what we do, because they don't really communicate with us. It's a poor excuse, but that's about the only thing I can come up with as to why we don't portray ourselves in a more user-friendly manner. Well, it's not that we're looked down upon. It's just that I think people would appreciate us much more-- Not, once again, that science and biomedical science is not appreciated as it is, but if you become familiar with a topic or a discipline or an individual in his profession, then you can understand what he's trying to do much better and you can criticize him in a more critical fashion and appreciate his contributions and sometimes his pitfalls.

MAESTREJUAN: And one last question here is where, then, should the burden lie? Should it be the public's initiative to try and build a better understanding or bridge to scientists, or should the scientists take up the initiative? Where does this middle ground come from, to have a more public understanding of something more of the science?

FAUST: Well, I think clearly it has to come from our end of the field. We have to take measures to make ourselves more public. The public's not going to do that, at least at this time in my life. Who knows, things may change, but the onus is on the scientific community to try and make itself more public friendly. And make the public more aware of what we're doing and why we're doing it and what the goals are. So how we go about doing that is certainly open for discussion, but I think there's always some constant movement in that direction.

MAESTREJUAN: Okay. Well, to go back to some things you said about expectations--and expectations in your part of the world-- But to go back to that, how big was Sterret, the town you grew up in? How do you spell that? Is that one of those Texas German-named towns?

FAUST: Well, yes. There was a large immigration of Germans that came to Texas in the middle of the nineteenth century, of which my father's ancestry were part of. And the bulk of them settled in the Texas hill country, which is west of San Antonio and Austin, in Fredericksburg and Boerne and New Braunfels. That's where his people came. Sterret is a little bit north of there. It's in central Texas. But a good bit of Germans settled there also, as well as some other immigrants from Czechoslovakia and Yugoslavia, not in the numbers like the Deutchmen, but there were quite a bit. And so there's Flatonia and Hasak or other small towns in that same area.

But Sterret is population about two hundred and fifty or three hundred, I guess, now. It never has grown. Most of the people left the Blackland, you know, when farming kind of got out of vogue. Actually, there was an opportunity to revive the town when the supercollider was going to be built. This was actually in Waxahachie, which is south of Sterret. It would provide a tremendous economic boom to that part of central Texas south of Dallas and Fort Worth. That project folded, of course. So people haven't moved back to Sterret. I think that the numbers have just stayed the same. So to answer your question, it's about two hundred and fifty or three hundred at this time, a very, very small town.

And then when we moved into Dallas, Dallas had that small town feel to it, even in the sixties and seventies. It was only about 500,000. We lived in a part of Dallas called Oak Cliff. It was on a cliff that overlooked the Trinity River. And that was an even more countrified part of Dallas, compared to across the river in north Dallas, which was more city-like. Well, I don't know the suburbs of Los Angeles, but-- I'm trying to make a comparison here. Let's just say that it was less cosmopolitan in Oak Cliff than it was in Dallas, so it wasn't that much of a step towards real big city living to go from Sterret to Oak Cliff. There were only two high schools in Oak Cliff and they were still pretty small.

MAESTREJUAN: Was there a grammar school in Sterret?

FAUST: Yes. There was a grammar school.

MAESTREJUAN: How many kids were in this school?

FAUST: Well, there was one class. My brother's two years older than I am, and I think classes were roughly about twenty, twenty-two, something like that. The same class went all the way through to-- See, I left in the sixth grade, or by the end of the sixth grade, I moved to Dallas, and then I went to junior high. And there, there were three seventh-grade classes, and I was in one of them.

MAESTREJUAN: So, in relative terms, that is probably kind of moving; your horizons were changing.

FAUST: Yes, yes. Very much so.

MAESTREJUAN: Well, what kind of farming did your dad do?

FAUST: He was a Blackland farmer, cotton, and then a little bit of corn, and then, like all farmers then, we also had a couple of horses that provided more or less entertainment than anything else. I mean, no one pulled a plow by horse at that time, even in Sterret. And there were always a couple of pigs running around and chickens and that kind of stuff, that animal life, that taught us a few things about biology.

MAESTREJUAN: So I imagine your chores, your daily chores, involved a little bit more than just taking out the trash.

FAUST: Right. I was pretty much in charge of small animals, like chickens and pigs, and my brother handled the cows. And as we both grew older, then we shared responsibilities for horses and taking care of them: grooming them, feeding them, watering.

MAESTREJUAN: Farming isn't an easy profession, and how successful was your father at farming?

FAUST: Not very successful. It's hard to do on small acreage. That's, I'm told still, the explanation for the collapse of the family farm-- It's these large corporate farms that can buy at lower cost and then take care of their labor responsibilities with less outlay that are successful. And the other important thing is, in farming, like most other things connected with nature, there are good years and bad years, and so it's important to be able to either hedge your bets through futures trading or insurance or the like to even out the peaks and the valleys, and that's just impossible for a small-scale farmer to do, that kind of stuff.

I think as I go back and visit Texas now that most of the people who live in the agricultural part of Texas that are not part of large aggregates are not even working the land. They're employed at some other profession, or jobs that are not directly related to agriculture, whether it's driving a school bus or running a mechanic shop or being insurance agents or what, and maybe they're just kind of piddling on the side as a hobby, you know, running some horses or running some cows or the like.

MAESTREJUAN: Had your dad gone to college?

FAUST: No.

MAESTREJUAN: And I take it your mother, though, with her nursing degree, had gone to college.

FAUST: Right. When nursing school was not really college. It was a professional school, so I think after she graduated from high school--my father didn't graduate from high school--but after she graduated from high school, I think she went-- She's originally from Indiana-- She went to nursing school for three years and became a registered nurse, a degree--well, I don't know if it's a degree--but just a certified registered nurse and has a nursing degree. So maybe that's equivalent to dental hygienist or something? I think schools do offer bachelor's degrees in nursing now--and they may have at that time--but she didn't have one. She went to Indiana University [School of Nursing], I think.

MAESTREJUAN: And how did she find Sterret, Texas?

FAUST: Well, after she graduated from nursing school she wanted to travel. So she actually spent a winter here in Boston and then a year in New York City, and then I think she spent some time in the Midwest again and then finally ended up in Los Angeles. And my father was vacationing out there, and they met and fell in love. They must have stayed in Los Angeles a good bit of time. Either that or they had a short courtship, because they moved back-- She moved with him back to Texas, and he was living in Ellis County at that time.

MAESTREJUAN: And is your dad a Texan from a long ways back?

FAUST: Yes. Let's see, I think his great-grandfather came from a region in Germany that my brother actually visited when he was in the army. But I can't remember where it is. Those people settled down in the hill country there. In fact, I still have some distant third, fourth, fifth cousins or something that live in the Comfort area. And there's a landmark there, an old mid-nineteenth-century hotel called the Faust Hotel. So we can trace our name back to—

MAESTREJUAN: I remember driving from San Antonio to Austin and seeing-- Well, I would have pronounced it "Grune" [Gruen] but they pronounce it "Green" in Texas. But it's spelled like you would say "Green" in German.

FAUST: Right. That's it. The Germans would say "Grune."

MAESTREJUAN: Exactly. And was your dad born in the same town?

FAUST: No. He was born in-- I think it was outside of Comfort, once again, a little farm between Comfort and Kerrville. But it didn't have a name associated with it. And had just moved on to Sterret to better farming lands, potentially, you know. There's not a whole lot of farming in the hill country. It's pretty rocky. All it's good for is running goats and hunting--there are a lot of deer down there--and fishing on the Guadalupe.

MAESTREJUAN: I floated down the Guadalupe on an inner tube.

FAUST: Yeah. At New Braunfels?

MAESTREJUAN: A little bit further up.

FAUST: Okay.

MAESTREJUAN: I think. But it was on the Guadalupe.

FAUST: Yeah. So there wasn't much farming activity there, and then usually when there's not much farming and you have a large family-- He had two sisters and three brothers. Then as the family grows, somebody's going to have to leave in order to maintain the same amount of survival for the ones that are remaining.

MAESTREJUAN: And they were all farmers as well, or married to farmers?

FAUST: Well, he had one sister, and she actually moved to Washington--I can't remember how she married my uncle--in the Seattle area and lived most of her life up there. And now she's still living, but has retired to Arizona. And then another sister stayed there in the area and finally moved into San Antonio and married and is no longer alive. And let's see. The brothers-- One moved to Louisiana and into the Alexandria area: worked as a salesman and is no longer alive. And the other was a pharmacist in Arkansas-- went to pharmacy school. Actually, I don't think

even he went to pharmacy school. I think in Arkansas you could, at that time-- Let's just say in the 1930s or 1940s you could just hang up a shingle in Arkansas and say that you can dispense drugs. It's probably not much different nowadays. He was in the Hot Springs area. He is no longer alive.

MAESTREJUAN: And was there a hospital, or--? Did your mom work when you were growing up?

FAUST: Yes. She actually drove into Dallas and worked at a hospital when that work was available, but most of her jobs were what's called private duty, in which you would go to someone's home during the day--I guess the shifts were seven to three or oftentimes three to eleven--and take care of patients in their homes. So that she could do in and around central Texas there, just driving around and doing that.

And then we moved into Dallas. Then she was sort of an independent contractor, was still a private duty nurse, but would work out of certain hospitals. So a patient would contract a private duty nurse to come and be her nurse at Methodist [Medical Center] or Parkland [Memorial] Hospital. But Mother wasn't paid by the hospital. She was paid by the patients or their insurance company. And that was a little bit archaic, because she didn't have any benefits, which I didn't realize until I got older, what benefits really were. But you know, that's important. She never paid Social Security, at that time, or she had no health insurance herself, and she had to file her own income tax. And it wasn't until she later kind of gave up the private duty practice and began to take jobs in hospitals that would retain a nursing staff that she could begin to accumulate Social Security payments, that have helped her a lot in her retirement. She's still alive and drawing on those payments. So [she] was kind of a, like I said, independent contractor at that time.

MAESTREJUAN: When you were younger and your parents were still together, I would suspect that your dad didn't have benefits-- How did they get through the traditional childhood accidents and broken bones?

FAUST: Well, I guess they paid for it themselves. When I look back on it, I haven't thought much about it. But I've never had a broken bone, though. I've been pretty luckily accident free. And measles and chicken pox-- I never had mumps. My brother had mumps, but they weren't-- I guess in retrospect, there weren't a lot of medical expenses paid for our keeping. Mother was there to--if we got a big gash—to stitch us up. And if we were ill, then she took care of us rather than having to put us in a car and take [us] to an M.D., who would make some kind of diagnosis which was probably pretty routine for a childhood disease and then charge you whatever it was at that time.

So just her profession allowed us to cut costs there. And we grew a lot of our own food.

We had year-round gardens most of the time in Texas, and there was plenty of meat available. It certainly cut back on the amount of money that went into that end of the budget. But we were always-- I wouldn't say dirt poor. There was always enough food around, but we always watched what we did with the money, or my parents watched what they did with the money. And my mother, up until the time that she was not able to take care of her finances any more, still did that. She was an incredibly frugal person. Sometimes it was almost embarrassing or even pathological--to that standpoint.

But she is still living off of her funds. And she has been in a nursing care center since 1993 that costs roughly about $2500 a month. I have direct responsibility for her financial affairs, and it's just amazing to me that someone who didn't have that much earning potential--to have accumulated that kind of wealth, that she is not a burden to myself and my brother. And she's getting excellent care--damn well ought to be, at $2500 a month--much better than what he could provide or I could provide.

MAESTREJUAN: She's still in Texas?

FAUST: Yes. She's living in a nursing care center in Dallas. We moved her up. So ironically, when I left high school, she left Dallas also because my brother was in the army. She moved to a small town out in west Texas called Seymour, which is out west of Wichita Falls.

MAESTREJUAN: Yeah. Near Wichita Falls.

FAUST: All right. This was about the time she realized she wasn't accumulating retirement or Social Security benefits, and so she took the job as director of nurses at Baylor [County Hospital] in Seymour, Texas. Now, to put it in perspective, Seymour had a population of about eight hundred, but it had its own-- It was the county seat. People are kind of few and far between out in west Texas. So it was a county seat, and they had a hospital, maybe fifteen beds. But if you've got a hospital, you've got to have a director of nurses, you know. So this was a salaried position with insurance--right there at the hospital, health insurance of course--and they paid Social Security and there was a little bit of retirement.

And so she went out there and worked all the time that I was in college and even longer than that. I think she stayed there for about six years. And then she took a similar position at Kerrville, became director of nurses at the [Sid Peterson Memorial Hospital in Kerr County], which houses Kerrville as the county seat. And Kerrville-- The population kind of swells and ebbs according to the hunting season, so in the fall--and it's legal to shoot deer in Texas--it could get up as high as about eleven thousand inhabitants, and in the winter when there's not much else going on it could probably drop to about two thousand. So it's a pretty good size town in a lovely part of the state.

She worked there for a good many years, and then finally gave up her job as director and went back, did some part-time by working private duty, and then even part-time. And she retired in Kerrville. Finally she got to the point where she could no longer live by herself and in 1993 my brother and I moved her to a nursing care center in Dallas--he lives between Dallas and Fort Worth and Arlington--to where he could help look after her. She's been there since '93. She's eighty-eight.

MAESTREJUAN: Wow.

FAUST: I know.

MAESTREJUAN: And after you and your brother and mother moved to Dallas, did your dad still continue to farm?

FAUST: No, no. He moved to Dallas also. He kind of gave it all up. And then he took a job as a salesman, selling produce, fruits and vegetables, for a company in the Dallas area. It was traveling, mostly, around to grocery stores and restaurants and meeting their needs. And worked there for the rest of his life and then actually remarried, and had a very good life. [He] retired in Dallas and died about twelve years ago.

MAESTREJUAN: What kind of expectations did your mother and your father have for you and your brother in terms of what you were going to do when you grow up?

FAUST: I don't think there were any expectations in terms of what professions we should pursue, but there were expectations that we'd do our schoolwork. When we got home from school, we had to have the homework done before we went out and played. And grades were important. We'd bring home good grades. I don't remember ever being criticized for a bad grade, but there was a lot of praise for good grades. Both of them, especially my mother I think, knew the value of a good education--even if you were going to stay a farmer. So I think they just wanted us to do well in school, and that would open doors that we could possibly walk through with our own two feet.

MAESTREJUAN: And within your family, were there expectations that you might go to college?

FAUST: No. My brother went into the army, and then with the incentive of the GI Bill-- This was right around the Vietnam [War], but he's about six [feet] seven [inches] and weighs about

250. Thank God the army realized it. He would not survive long in the jungles of the Vietnamese peninsula. So they made him an MP [military police] and sent him to Germany [laughs].

MAESTREJUAN: Oh. Well, good for him.

FAUST: Yeah. And so when he got out, he, through the GI Bill, went to school for a while at Texas A & M [Univer-sity], but was not successful and never finished. I think he stayed down there on and off about three years, but how many hours he's got accumulated towards a baccalaureate degree I don't know. He's currently a loan officer for a credit union in Dallas. He got enough business hours, I guess, to qualify for that.

So there weren't any expectations about going to college. I guess we always knew that there wasn't money to pay college tuition. In Texas you didn't have to pay to go to public school. It was free. I guess that's probably true in most places. But even college tuition in Texas was relatively cheap. But I guess we just realized that that was never put into the budget. If we wanted to go to college, then we'd work it on our own. And it actually wasn't until my senior year in high school that I began to think about college. But it wasn't because I wanted to go to college to learn, it was because I wanted to go to college to play basketball. I had a reasonable amount of skill as a basketball player, which isn't saying much for Texas. Now, we're not talking about Indiana, but still the colleges have to field basketball teams in Texas, and they do it primarily from drawing upon the high schools. So I wanted to continue to play basketball, and the next logical place to do that was in college. I was fortunate to be recruited by some colleges--not major ones--so I went to college primarily to play basketball, and I would have never gone to college if it hadn't been for basketball. There's just no question about it.

I took a basketball scholarship at Stephen F. Austin State University in Nacogdoches, Texas. That's in east Texas, close to the Louisiana border. And then interestingly, because I still had pretty good high school grades--I can't remember what I made on the SATs [Scholastic Aptitude Tests] and the ACTs [American College Testing Programs]--I was also offered a basketball scholarship at Rice University, which is an excellent school, perhaps the best overall high-standard school in Texas. But my mental thoughts at that time were that it was going to require a great deal of effort for me to stay up in the classroom at Rice, and that would distract from the other things that I was interested in, like basketball and whatever else was happening to boys between the ages of seventeen and twenty-two.

So Stephen F. Austin looked like a good common ground to get a college education, albeit probably not at the same level as Rice, but yet enjoy playing basketball. And then I often thought about that-- I've often thought about that decision in retrospect. Where would I be if I'd gone to Rice and majored in--I majored in chemistry, as it turned out--versus going to Stephen F. Austin? Because I've been very fortunate in my professional career, I don't think I'd be any better off, so I don't regret making that mistake. And there was a close friend of mine in high

school who- - We played basketball together, although he also played football, and he went to Rice on a football scholarship, and we stayed in contact all the time we were in college together. And it really changed him. I would think, was I going to turn out to be like—

I won't mention this guy's name, but people who read this will probably figure out who it is anyway. There weren't many of us from our high school that went to college, much less Rice University. So I don't regret going for those two reasons.

MAESTREJUAN: Well, one thing the listener won't be able to pick up is your height. How tall are you? You must be six five, six six?

FAUST: I'm six four. And I've probably shrunk a little bit as I get older. I was closer to six five. As I said, my brother is six seven, so he dwarfs me, still. That's probably one reason why I'm a better basketball player, because I always had to play against him.

MAESTREJUAN: And he didn't think about going to college on a basketball--

FAUST: No, he didn't play basketball all through middle school and all through junior high and high school like I did. He had other thoughts. He was a little bit more clumsy and not as coordinated and as agile as I was, so—

MAESTREJUAN: When you went off to college, you did have this role model, you said, that was the chemistry instructor at your high school as well as the basketball coach. Usually history gets a bad name because the basketball coach is a history instructor too, and it usually doesn't work out that well, but how much did this influence your athletic, but also your academic, decisions when you did start off to college?

FAUST: I guess it influenced it to some extent, but not as much as it could have-- He was the assistant basketball coach, wasn't the head coach. I took chemistry when I was a sophomore, so I had this time in junior and senior year to develop other interests or maybe other role models. His influence on me wasn't as great by the time I got to college as it was, say, in the early years I was in high school. But nevertheless, when I went to college, I didn't know what I was going to major in, besides basketball, and when it came time to declare, then all I just said was science. But then I began to lean towards chemistry because I maybe subconsciously was reflecting back on his influence and his mentoring, so to speak. So I never had much contact with him after I left high school--with the exception that much later on, he, like a lot of coaches, moved into administration and became a principal at another school, and it just so happened that he was principal at a school in which my best friend in college became a teacher. So Miles, who was another basketball player--and he and I roomed together for two years in college--when he left

Stephen F. Austin with a teaching degree, moved to Dallas and took a job in the Dallas Independent School District. And lo and behold, the first school he landed was where Ken Brashear was the principal. So we had contact occasionally through there. It pleased him to see me still involved with science, I could tell. He'd always ask about that. And then when Miles left the school where Mr. Brashear was at we lost all contact. And I think my brother, several years ago, read his obituary in the newspaper, so he's no longer around.

[END OF TAPE 1, SIDE 1]

MAESTREJUAN: Okay. Before we send you off to college, I wanted to ask how you made the transition between moving from Sterret, both academically and socially, from this smaller town with a very small school, to, then, a junior high that has three different classes for one grade and an honors program and, I would assume, more choices in the courses you were able to take.

FAUST: I made that transition pretty easy, I think. I don't remember any real trauma associated with it. My brother was with me. And he was considerably more gregarious than I-- If I remember now, most of the friends that we had were his friends, or most of the friends I had were his friends. I didn't find the course work any more difficult. Now, I don't know if that speaks volumes for the Sterret Public School District or speaks down for the Dallas Independent School District, but I didn't struggle in the class.

John, my brother, didn't do as well, even starting at that point and then all the way on through high school, but I think that's because he-- Let's say he didn't apply himself intellectually, you know, began to focus on other endeavors. So he struggled quite a bit in high school, academically—and socially, too. But I don't recall having any problems with that transition. That's about the time I started playing organized basketball, which helped a lot.

MAESTREJUAN: You probably get asked this question a lot because you were living in Dallas in '63. How did you experience the JFK [John F. Kennedy] assassination?

FAUST: Truthfully, I didn't understand the impact of it. What I understood most was that it was disruptive in the community. So, if you remember-- Allegedly, Lee Harvey Oswald left the scene close to the triple underpass where Kennedy was shot and went across the Trinity River to a movie theater. And that happened to have been in Oak Cliff, on Jefferson [Boulevard]. It's called the Texas [Theater]. And that's about two blocks from my high school. And there, allegedly, he shot [J.D.] Tippit, the Dallas policeman. So there was a lot of activity around the high school that day.

And of course, this was in November, when basketball season was starting. And at that time, I was riding the bus back and forth from home to high school. And there wasn't a direct

route from the high school to our house in the Cedar Crest part of Oak Cliff; we had to go take a bus to downtown and then transfer to go out. Well, nobody was getting in and out of downtown. Very few people were leaving the Oak Cliff area where the Texas Theater and Adamson High School was. So it took forever for me to get home that evening. And then the same degree of difficulty was required the next day to go to school, not because school was-- School was canceled the next day, but because I had basketball practice. They don't cancel basketball practice.

So I guess as callous as it sounds, I don't remember much about my thinking on the impact. We watched the TV, of course, but I was more concerned with trying to just get around town. But when I look back on it and think about the people that I talked to--not only on those days and that weekend, but the months and the years to come--the impact that that event had on that large community was enormous, on Dallas. And I think they still feel that way. So I guess it was a defining moment in the history of Dallas, but it wasn't so much for me.

MAESTREJUAN: Okay. Well, at your high school, where you're able to take your science classes and go on these special field trips, how did the teachers respond or identify this interest in science or being--after this one field trip--turned on to the idea that you could work in a research institution setting?

FAUST: Well, that was the only field trip I ever took. We didn't go with Mrs.-- To the bread bakery, or—

MAESTREJUAN: You didn't go to the Alamo, or anything like that?

FAUST: Well, yeah. I might have done that, but that wasn't a field trip related to science. I can't think of any other outside--

MAESTREJUAN: Were they able to encourage an interest in--?

FAUST: Well, Ken Brashear did. There's no question about that. I think I mentioned that he-- I kind of served as a lab aide, would help prepare equipment and experiments for the next class. But from that point on, the science classes got pretty small. I think every student had to take biology in the DISD [Dallas Independent School District], but chemistry wasn't required. There was only one class of chemistry in my high school, and there were only about twenty or twenty-five students. And then nobody took physics-- Well, I took physics. But I think the class was like ten or twelve. So that was it. Then usually the physics teacher, like the chemistry teacher, only taught those once for each semester, and then they spent the rest of the time either teaching general science or health or history or whatever. So there wasn't a framework of teachers that

could set about to encourage students in a systematic manner. It was kind of secondary, elevated levels of science. Biology was where it stopped, because you could encourage students maybe to take a degree in biology and become a teacher or take a degree in biology and try to get into med school. But I don't recall anybody saying to me, "Well, listen-- You know, if you have a degree in chemistry, then you can work for Dow Chemical [Company]" or "If you have a degree in physics-- They're doing lots of neat things down here at NASA [National Aeronautics and Space Administration], so you can get a job down there." I don't remember anybody saying anything about that.

MAESTREJUAN: Did you take any field trips to NASA?

FAUST: Not at that time. No. I never liked Houston. Houston seemed so much different.

MAESTREJUAN: That's a Dallas thing.

FAUST: Yeah. [laughs]

MAESTREJUAN: Because Houston-- They don't like Dallas.

FAUST: That's right. Well, the weather down there is worse than it is up here. [laughs] It's humid and hot and there's no winter at all-- There's more of a sprawl than there is in Dallas, and I just didn't even like visiting in Houston.

MAESTREJUAN: What were the expectations on the students as to what they were going to do after high school, in your high school?

FAUST: Well, I guess the biggest expectation was to get a job and to just join the workforce. Along those lines-- At that time, a lot of the young men went in the army, either by enlistment-- So I guess the options were that you enlisted for three years and then you had some kind of say as to what training you would get--not necessarily where you would go, but whether you were going to be trained for an aircraft mechanic or a gunner or whatever. Whereas if you waited until you were drafted, then you got no say, so that was always a dilemma for young guys.

If you got a job and were waiting to be drafted, then you might start taking some courses at a recently created-- Like Dallas County Junior College, which is, like-- I think in California there are junior college districts. That thing sort of started up in Dallas in the seventies. These were a good opportunity to get your feet wet beyond the high school level and to see if you'd

really like doing it. Most of those failed, because you never put your heart into it, I think. But it kept them off the streets for a while. Mostly it was getting a job or going in the army. And for girls, speaking completely ignorant of any basic knowledge, I'd say the same sort of thing. It's getting a job and maybe taking some courses. The army was not a factor for them, but eventually maybe getting married and what have you. So there weren't many of us that went to college, and fewer of us that actually graduated too, after that.

Unfortunately, I haven't kept up with my high school class as much as I'd like to, even though I stayed in Dallas. I went back to Dallas and stayed there until '85 or so. I just never stayed in touch. I think we have a reunion coming up in a couple of years, and I'm not making an effort to go down there.

MAESTREJUAN: Well, how many of your fellow students, both parents worked? Their mothers worked out of the home--?

FAUST: I'm trying to think now--the ones that I know. I'd say about half. And it seems that it kind of fell along the lines of if there were, say, more than two or three kids in the family, mom worked. And if there were two or less, then she stayed home. And working is more of a sales-clerical-type job, clerking kind of job. I think my mother was the only close to professional kind of a person--and worked full-time.

MAESTREJUAN: I'm assuming you had to register for the draft?

FAUST: That's correct. I registered for the draft, and then if you kept your grade point average up in college, then that helps you to avoid the draft. But I guess the real measure of whether you went into the draft or not was associated with which board you registered with. There was a lot of inequity in this, as it is, I'm sure, all over the country. So if you happened to be with a board registered in, say, a very upper-middle-class neighborhood in north Dallas, then their quota was less than a black neighborhood in south Dallas. So fortunately, we were in Oak Cliff, and that was kind of in the middle ground, and I was able to keep my head above water just by keeping grades up. But then the lottery came along, and I drew 336. So that was the clinching blow that kept me out of the military.

MAESTREJUAN: Well, how much did that influence your decision to go on to college?

FAUST: The draft? Well, I think that had a lot to do with it. I mean, that coupled with basketball, because I knew if I could stay in college, then I would avoid the military. But you know, I'm not sure that at that time I really was that keen to avoid the military. I mean, I was going to avoid them, but I wasn't going to go to Canada. You know, if the time came, then I

would go. And in fact, at one time I thought to make it-- You know, there was a time when everybody thought that-- All guys thought that they were going to go in the military. It was just a matter of whether you were going to go in before you went to college, during college because you flunked out, or after college when you no longer had any kind of deferment-- because there wasn't anything, I guess, except for M.D.'s or whatever.

So to make that a little bit easier, the reserves became an avenue to lighten the load, so to speak. So people would try to get into reserve units all across the country. And they got filled up pretty quickly. And then once again, there was a lot of inequity about that because getting into the reserves is sort of like getting into the unions. It' s not what you know sometimes, it' s who you know. So although there was a list of candidates and theoretically, as places became available, the list would be filled by the people higher up on the lower numbers, that wasn' t always the case. So I applied to, and came very close to, joining the Marine Reserve and had the opportunity. It was a little bit more difficult to get in the Marine Reserve than it is in the National Guard or whatever. But an English teacher in college who I' d asked for a reference for-- I don' t even remember why I asked this woman for a reference. She talked me out of it. She said this was a big mistake to go in the Marine Reserve. I think she was speaking from--very antiwar and being older, and probably more mature, and could see that-- She was advising me to at all costs to stay as far away from any military machine as possible. And so I turned down-- I mean, I went to the Marine recruiter office and had a pen in my hand, and stopped. This was before the lottery too, by the way.

MAESTREJUAN: And why did you stop?

FAUST: I guess I just had enough trepidation about it and didn't know that it was right for me, and she was pretty emphatic about it. And things were beginning to-- Even in Texas, which was prowar for a long time, there was a lot of sentiment-- Now, we're not talking about California or Berkeley. We're talking about Nacogdoches, Texas, you know? And there was still some growing sentiment against the war, that it wasn't the best thing that we were doing, and that there were a lot of problems with it. And that we ought to be looking at it a little bit deeper and trying to figure out what to do. So questions were being asked.

MAESTREJUAN: And where did you fit in?

FAUST: Well, I kind of just watched them: watched the people ask the questions and listened to the answers. Once again, in retrospect, I'm not sure I was one way or the other.

MAESTREJUAN: And how did you see your brother--his decisions and his fate in the army? What happened to him?

FAUST: Well, I thought he was pretty lucky and I was thankful. I was grateful that the army felt that way about it, and I was glad that he wasn't going to Vietnam. But the army treated him well and he learned. I'd say he was running through some personality problems by the time he got out of high school, and the army did a lot to correct that. So I thought it was a good experience.

MAESTREJUAN: Okay. Well, when you decide to go college, one thing I guess I wasn't clear or maybe I missed is were colleges coming after you to attend them, or did you--?

FAUST: Let's just back up a minute, though. I just—

MAESTREJUAN: Okay. Certainly.

FAUST: As I became more aware of the Vietnam War and what it meant and how shallow it was, then my attitude changed to where I became against it--just probably fell in step with everybody, with the majority of the country. But the real impact of that war didn't hit me until I began to realize where most of the other men in my high school class, or boys in my high school class, had been all the time I was in college, and why some of them hadn't come back. So that was a significant influence on my current thinking about peace and war, just to realize that in another time, in another place, I would have been right there with them and seen some of the things that they saw, did some things that they certainly aren't very proud of and having to live with that.

You know, I'll never forget some stories that I don't think are lies that I heard from some friends that had been over there, some of the things that were done. So even to this day, I know-- I'm in a long-term relationship with a woman here in Boston, and her brother is my age, and he served in Vietnam, two terms, and he doesn't talk about it. That whole era, I think, defined a lot of--well, I don't know how old you are--at that time defined a lot of what we would come out to be, the people in my generation. Now, what was it--?

MAESTREJUAN: I'll continue on with it. I have just a couple more questions. Knowing what happened afterwards, how did that change your own perspective on your decisions at the time, say for instance to go to college, to not sign up with the Marine Reserves? Has that changed now that you've heard the stories from your friends who came back? Or I know from my own cousins, they were very ambivalent. They were reading in the local newspaper friends that they knew in high school that had-- The death lists, and they were very ambivalent, to see people that they knew, their names were in the paper. So they didn't know which way to come out on the war. And just because it was too close. So there are those decisions, the immediate decisions.

Now that you have this perspective of people who did come back and tell these horrible stories, how has that changed your perspective on your own decisions of, say, choosing to go to college or choosing not to join the Marine Reserves?

FAUST: Well, I 'm certainly glad I made those decisions, because I 'm not sure how I would have come out under those other influences, and it's just a lot easier to look back and say, "Well, I'm glad I didn't have to do that." You just never know. So I'm glad that I chose not to go in-- I'm glad I chose to go to college. I'm glad I stayed in college. I'm glad I chose a high lottery number. I'm glad I stayed out of the Marines. You never know, if you got in the Marine Reserve, then-- I'm not saying that they brainwash people, but there could have been an opportunity that my attitude about serving in battle could have changed, and I might have gotten gung ho, decided I wanted to take a tour of duty over in the rice fields. So without a doubt, I'm glad that I stayed away from the military as far as possible.

MAESTREJUAN: This comes just from my being a historian-- [Lyndon B.] Johnson, after JFK is assassinated in Dallas--and Dallas is immediately affected by this--and then Johnson becomes president. How do you see this, Johnson being Texan and being president of the United States, affecting Texas politics and the attitudes towards the war in Texas as opposed to other parts of the country?

FAUST: Well, I think Johnson kind of mirrored a lot of what I was thinking about the war, that initially he didn't think much about it. He didn't think it was going to amount to what it turned out to be, that it wasn't going to be the thorn in the side of America that it turned out to be, and that it wouldn't really affect his presidency. But I thought that it really didn't affect my life. You know, yeah, there are people going over there fighting and I'm going to try to stay out of there, but it didn't affect it on a day-to-day basis. But as time went on, then he and I both changed our attitudes. And it's unfortunate that he still had to make decisions concerning the war, whereas my only decision was trying to stay in school to stay out of it. But he had to make decisions about what direction the war went into, and of course he was getting a lot of pressure from both sides about what to do about that, and I suspect that it weighed pretty heavily on him. It must have been an extremely difficult, agonizing experience for a person who's more or less a gentle giant of a man. And to have to decide what direction this war is going to take, how much more sacrifice is going to be made-- Or how can we get out of this and save face? What I'm saying is, more or less, I think a reflection of most historians, that it really beat him down. And I feel that it probably shortened his life.

I saw LBJ [Lyndon B. Johnson]. He retired to his ranch outside of Johnson City, which is, once again, up in the Texas hill country. When I used to go visit Mother, then I'd turn west out of Austin up on [Highway] 290 through Johnson City, and you could drive right by his house. And oftentimes--as a lot of other people--would just duck down into the fence and come up to-- He had a little large flowing creek that ran in front of the house. You could just stop and see his house and his homestead and the like, and I stopped there one time, and he

was out there fishing. And I just kind of waved to him. It was across to the bank, from about here across to the window--or here across to the room-- Waved at him. Thought about him.

MAESTREJUAN: Did he wave back?

FAUST: Yeah. I feel sorry for him because he had to do the things he did and how difficult it must have been for him to think about that. And at the same time, how he-- You must think, as I would have thought, "How did I ever get into this?" You know, "Why wasn't I more knowledgeable early on?" and "How do I get out of this?" So I was pretty sympathetic to him. I'm probably sure that others could have done better. So I guess I felt he just kind of went the way of everybody else and a lot of people in Texas, along that same track.

MAESTREJUAN: Well, to get back to you going off to college. You said you wanted to pursue basketball. You had some opportunities to pursue in basketball, and I just wanted to make clear in my own mind, had colleges come seeking you, or did you go out to seek colleges playing basketball?

FAUST: A little of both. The ones that I wanted to go to, then I made contact with them, usually first through letters and then went on a couple of tryouts, but then a lot of them-- And that's how I went with Stephen F. Austin. But other colleges-- There weren't that many, now; we're not talking about this recruiting service here. So I think most of the outright offers I got were from junior colleges, more established junior colleges. There's a whole loop of junior colleges in Texas that-- But I was interested in a four-year school, so I made contact with Rice and Stephen F. Austin and North Texas [State University], Abilene Christian [University]. And I think that's about it.

MAESTREJUAN: Had you thought much about the academic side of life when you went off to college? How did you come to decide on majoring in chemistry?

FAUST: Like I said, I thought science because I had taken all the science courses and then I had an interest in that, so I was going to major in science. I didn't know what discipline it was going to be in. I guess it wasn't until my sophomore year in college that I began to focus on chemistry. And that was probably due just to my past interest in chemistry. The chemistry professors seemed like nice guys. Although he never had an impact, I always marveled at this one fellow, Harold Abbott, who was the chairman of the chemistry department. He wasn't a mentor. He didn't care beans about me. But then, he didn't know me. He taught one semester of my freshman chemistry class, but it was a relatively large class. It was around sixty people, and so I didn't stand out.

But he was also the timer for the basketball games. So you know, whenever I'd check in or something, he would be there. I don't think at that time he connected me with chemistry, you know. And he was this relatively short fellow that was always dressed in a white shirt and a bow tie. And he was in his late sixties at this time. And he was formerly a professor at Columbia University in New York City. And I often wondered, just how in the hell did this guy go from Columbia-- Now, I knew what Columbia was and what it stood for. Now, how did this guy go from Columbia University in New York City to Stephen F. Austin State in Nacogdoches, Texas? And you know, to this day I wonder about that. And I guess because he was kind of mysterious, that that kind of helped solidify my bond with the chemistry department, you know, that this was something unusual. Not that I wanted to be like him and wear a bow tie, but it kind of set him apart from the biology teachers.

Yeah, I took physics, but I wasn't interested in that. Then there was a newly acquired faculty member in the chemistry department that came on board about the time I got there. A guy named Jim [James] Garrett, who's still there. That's an organic chemistry teacher. And he was enthusiastic. You can see once again that--doing a little bit of teaching myself--that the newer faculty are always a little bit more enthusiastic about conveying information and excitement than older faculty that are kind of in the humdrum, so to speak. So I think that enthusiasm came across in organic chemistry [more] from Dr. Garrett than from others, and then that kind of helped facilitate this bond with chemistry. So these small occurrences, or feelings, I think, added up to help me by the time I got to sophomore year to decide--and that was about the time you had to declare a major, too--that it was going to be chemistry.

You know, a lot of basketball players and athletes would choose physical education, but let's face it, we all knew deep down inside that the only job you're going to get being a P.E. [physical education] major was being a coach, and that wasn't going to make a whole lot of money. Not that I was greedy or so, but I think I wanted something a little bit more out of it besides being a high school teacher and a coach.

So apart from education, or-- Now, Stephen F. Austin had a forestry program. It was one of the five forestry schools in the country at that time. And I used to always admire these guys that were going to actually go off on real field trips. These guys went out and camped in the woods, which was fun. And they could look at a hundred different plants or insects and tell you what they are and what they were good for and how you controlled them. So that was neat biology; it was applicable. So I always thought that if I had to do it over again, I think I'd want to major in forestry, but it was too late. So chemistry was just kind of the way to go.

MAESTREJUAN: Well, how much were you influenced by the stereotype of "the jock can't do academic kind of things"?

FAUST: Well, I didn't fall into that mode, because I knew that that stereotype existed and other people felt that way, but fortunately I kept my grades up good enough that I didn't fall into that. So I didn't get put into that class, or that group of jocks. I was a little bit apart from them. I

didn't want to be in that group. I knew that I wasn't going to play basketball beyond college. Some of those guys thought that they were going to continue on.

Actually, one of our basketball players played for a little bit in the NBA [National Basketball Association], but that was about it. But once again-- When we graduated from high school, it was hard to see what we were going to do past nineteen. Oftentimes, when we graduated from college at twenty-three, or twenty-two, [it was] difficult to see what you're going to do past twenty-four. But I kind of felt like by the time I got to be a junior that I was going to have to figure out something to do, at least for the next five or ten years. And it better be something that I could get a job in. And I didn't like teaching, and so chemistry seemed like a place that I could get a job.

MAESTREJUAN: What did you think you could be doing?

FAUST: Well, there was a lot of chemical industry along the Gulf Coast. Granted, I didn't like that, but there was a lot of it going on. And then as I said, "better world through chemistry" was going on, whether it's Du Pont or Mallinckrodt [Chemical Works] or Monsanto [Company]. I wasn't wedded to Texas. Well, I was prepared to leave, and I applied for those jobs. I actually applied to those companies when I graduated. Didn't get any offers, but-- So I just thought that there was a better opportunity to have a stimulating career and make a decent stable salary as a chemist, compared to other things that I had any kind of interest in. You weren't going to make a lot of money being a forestry person. I mean, schoolteachers, I think they have a good job. They probably make more money now than they did before, but they weren't making a whole lot of money then.

MAESTREJUAN: Was there the dream back there somewhere that you would play professional basketball?

FAUST: Never. Never. I knew I wouldn't. I didn't have those levels of-- In fact, I didn't even finish up in college. After my sophomore year, then I began to have some problems physically with my knees. We all get—basketball players--with knees or so-- Once again, in retrospect, the treatment I got wasn't as good as it could have been. I'm not sure how good it could have been in that era. But the corrective measures probably did more harm than good.

So my skill level fell quite a bit between my sophomore and junior year, and my playing time and proficiency, along the same lines, waned. So by the time I was a senior I wasn't even playing very much, and as my interest and ability in basketball began to decline, then my interest in trying to figure out something to do with the rest of my life, and what meaningful job I was going to get, was increasing. So there was this shift in outlook.

MAESTREJUAN: When did you decide to pick up a minor in biology? Where was biology in your decisions there?

FAUST: It just kind of fell into place, that you had to have a minor in something. So I think in those first couple of years, when I was kind of undecided, I was taking biology and chemistry, so--I think this was my reasoning now--that I accumulated enough credits in biology that all I had to do was just take one or two more, then I had a minor. So is that kind of like a default pathway? Yeah. But I did take one biology course at Stephen F. [Austin State University] that I remember. I don't remember anything about any other biology courses except a physiology course, and it was in summer term. And this was organ physiology and I thought that was pretty interesting, because it taught me about kind of a reductionist view. If you talk about an organism, whether it 's a tree or a cow or a human being, but inside that organism there are separate integrated units that are working together to maintain that life form-- And also, we touched a little bit on cell biology. So then I can see that the same sort of scenario exists within an organ. And at that time, I didn't know about subcellular organelles, but all that refines down to this other thing. So that was neat.

But one thing that I did-- I think when I was a senior-- The chemistry department taught biochemistry, also. And there was one semester of biochemistry taught by one of the more esteemed faculty members. And I took it because I didn't want to take an advanced physical chemistry course. And it was mostly intermediary metabolism. We memorized a lot of pathways about how cells metabolize glucose to convert it to something else or how cells synthesize lipids or process nucleic acids or synthesize bases or whatever. Now, that's all chemistry; those are chemical reactions that the cell does. Granted, it's facilitated by little enzymes that are nothing more than catalytic components, but it's nothing but pure chemistry. You're moving carbons around and you're shifting bonds, just like organic chemistry. And that was neat, because it applied a familiar perspective to drive my interest, that the same sort of things that I liked about chemistry, mixing molecules together and formulating new substances and trying to figure out what they do or whatever, was really going on all the time in a much more exquisite fashion and scenario.

So I think that was the first time I got interested in, really, molecules and what goes on--or cells and what goes on inside of cells. Because of that, when I started this master's program, [I] kind of leaned towards biochemistry. And once again, it was another chemistry program, but then I was concentrating on biochemistry because of that.

MAESTREJUAN: Well, that's interesting, because I was a bio[logical] sci[ences] major, and all the biochemistry was in the biology department, probably the School of Biological Sciences.

FAUST: I don't know why, but some schools had biochem-istry taught by the chemistry department. I think yours is in the majority, perhaps.

MAESTREJUAN: And was that part of the biology curriculum, would you know? As a major, that they would have had to take biochemistry--?

FAUST: No.

MAESTREJUAN: Or was it clearly seen as the realm of chemistry?

FAUST: Biology students certainly could take it. It was open to them. And I suspect [it] applied towards a minor in chemistry, but they wouldn't be able to apply it towards their major in biochemistry. And that seems kind of stupid, doesn't it, when you think about it. I'm not sure that biochemistry should be in one versus the other. You could make an argument either way. But certainly, kids clearly, in a biology department--unless it's some kind of animal biology or zoology or something like that, and you could argue the other way around--ought to be exposed to biochemistry. Molecular biology, for that matter, too.

The textbook that this professor used, even at that time, was an old, outdated biochemistry textbook. But because he'd lectured from it for the past ten or fifteen years, he was familiar with it. But it was dotted with excerpts bringing into attention disease processes, and how it led to abnormalities in biochemistry, principally like inborn errors in metabolism, that if an individual is missing this enzyme involved in, say, amino acid breakdown--phenylalanine breakdown--then they developed a severe neurological syndrome. And that led to tests for PKUs [phenylketonuria], all right? And then diabetes is a result of elevated glucose, and the reason it ' s elevated is because insulin doesn't work so glucose doesn't get metabolized. Now, at that time, nobody knew how glucose led to all the other secondary effects. So in it, dispersed in these studies, were some real relevant examples about the importance of biochemistry towards medicine or health.

I think at that time I began to think about the idea-- With some knowledge, especially in biochemistry--or even chemistry, at that time--you can do things that really have an impact. Now, I kind of had that feeling with chemistry, but it was more making new fibers or developing a better process for petroleum products or maybe synthesizing a new derivative on penicillin that would make it have a longer half-life or something like that. But not in the context of actually doing something fundamental to helping health and combating disease. And that's a pretty altruistic thought to have. It feels good to think that way, you know? It's "Yeah, that's something I'd like to do. I like that feeling."

MAESTREJUAN: Well, where was this need for you to do something altruistic or socially valuable?

FAUST: Well, there wasn't a lot of motivation or need there. It just crossed my mind then that there's an opportunity and these are neat things because they have direct application towards suffering and whatever, but I just didn't jump on it and say, "I want to go to med school or go to graduate school." Just this "Hmm, this is a nice avenue, a potential way to really make contributions." These guys Banting and Best discovered insulin, you know. A lot of-- The Krebs cycle. You know, recognition, notoriety. I mean, just like in chemical reactions, whether it's a Friedell Kraft or an Erlenmeyer or whatever.

[END OF TAPE 1, SIDE 2]

MAESTREJUAN: Well, to start back up again-- You know, it's interesting now how Pew scholars define themselves. Some see themselves as biochemists, although they may not have any kind of formal training in biochemistry, or they're molecular biologists-- Who knows what that means. When you were in college, how aware were you of molecular techniques and the contribution biochemistry was making to the revolution in molecular biology?

FAUST: I was aware of the contributions that biochemists were making because I read them and could understand, but I really didn't know about the concept of cell biology or molecular biology. We were dealing with molecules, whether it was macromolecules of DNA and RNA and protein synthesis-- But I guess I just didn't piece it together as molecular biology. And cell biology I really didn't have much of a handle on at all because I didn't know much about the inside of the cell, except what was going on in these chemical reactions, but [I didn't know] the structure of the inside of the cell and how it was organized. So I really didn't have much information about molecular biology and cell biology at that time. It was all biochemistry.

MAESTREJUAN: As your basketball career is waning and the chemistry is taking its place, what did you start seeing for yourself after college? What were you going to do after college or what were you going to do even with your chemistry?

FAUST: I always wanted to get a job as a chemist, maybe a biochemist. But I wasn't going to limit myself to applying to only jobs in pharmaceutical houses or whatever. And I would just get a job and start to work and see what happens then. I thought, well, after working for two or three years, maybe I'd get into a master's program. But I didn't apply to anything right away. I wanted to go out and get a job. I had a high draft number, and there wasn't any reason for me to have to worry.

MAESTREJUAN: How much independent lab research were you able to do as an undergraduate at Stephen F. Austin [State University]?

FAUST: I didn't do any. I taught a summer's quantitative analysis lab in the chemistry department. And then that gave me an opportunity to kind of play around with the tests that the students were going to do, whether it was titrating one base--a base with an acid to an end point with an indicator--or determining the copper content in a solution of cupric sulfate. As a result of that playing around, I can see the value in optimizing assays to establish the right set of variables or the right concentrations to give the maximum absorbance or the right volume to do something. So once again, in retrospect, I can see that that's kind of research in itself, just trying to set up an assay or make it better: What variables do you twitch around to get something to work better? I still have a tendency to do that even with assays today, to optimize them. Like I said, that's kind of research, just playing around. That's the closest thing I had to any kind of research activity.

MAESTREJUAN: Well, teaching this class, did you gain a concept of how do you make other people interested in chemistry?

FAUST: Like I said, it was a lab course in quantitative analysis that was held during the summer, and being a summer class it met, I think, four times a week. This was part of my work-study scholarship money. So it was continuous, and it was easy to generate enthusiasm for me as well as the students. There wasn't this week's lag in between to the next class. So it was pretty easy to get caught up and try to do a good job on and get enthusiastic about it. Conveying enthusiasm for science and seeing what it can do and how it can motivate you I've gleaned from that.

MAESTREJUAN: Well, you always hear about [how] some of the most memorable chemistry professors are the ones that can cause flashes and booms. Is that a good technique to use for college students or even high school students to get them to understand chemistry?

FAUST: Gee, I don't know what motivates [laughs] college and high school students these days.

MAESTREJUAN: What motivated you?

FAUST: I think just my own realization that if I understand concepts related to chemistry and in biochemistry, then that can, number one, provide me with a profession, and number two, it might give me an opportunity to do something important someday. They weren't simple concepts, but there was a general trend in chemistry and biochemistry that cells behave and do things the same-- It's the same sort of processes, you know. I think biochemical reactions can be summarized in about a dozen different types that, more or less, they all fall into, whether it's

condensations or hydrolysis or Schiff bases or things I've already forgotten. So if you just could understand those and think about them and apply them, then you could have a rewarding career.

MAESTREJUAN: Okay. And the work-study scholarship-- You had to play basketball. Did that require you to play basketball for the four years you--?

FAUST: Well, no. They required me to, but I didn't the last year. But you know, I still got paid for it. I didn't get as much, though. I mean, I had to supplement it. I remember I took a job in a feed mill working about ten [A.M.] to three [P.M.] to cover expenses, because I wasn't living in the athletic dorm at that time. I had to live off campus and pay my own room and board. I could have stayed there, but that was my choice to move out.

But then I paid-- I mean, I had these tuition waivers. I cleared out my desk several years ago, and tuition at Texas in a state-supported college was $50 a semester. This was back around 1970. That is really cheap. That was tuition, and then there were a few fees tacked onto it. I don't know what it would be, books and all. But we're talking about $400 a semester, max, for books and everything. Do you know what kids pay up at Tufts [University] for tuition? Twenty-eight thousand dollars a year.

MAESTREJUAN: It's amazing.

FAUST: I know.

MAESTREJUAN: I went to all public universities, so I really don't have a good-- But I do think public universities are getting more expensive. But that's really a lot of money.

FAUST: Well yeah, they're a lot more in Texas [now]. But gosh, what a bargain, in terms of—

MAESTREJUAN: And that doesn't include books, because I know most of the biology and chemistry course books are $100. They last two semesters, but they are incredibly expensive.

FAUST: Do kids still resell them? Or sell them and then buy used books like they used to?

MAESTREJUAN: Yeah. They can. It just depends on the bookstore. The school I went to as an undergraduate had a lot of used books. I ended up keeping mine, but at the university that I go to as a graduate student, it's rare that their biology or chemistry texts are used. You have to

buy them new.

FAUST: I think when you get to upper-level you end up keeping them, so they don't turn over, and that really gets expensive. Boy, but that's really expensive. That was really a bargain. The money wasn't spent on me, but it was the best money that ever was spent on me.

MAESTREJUAN: Well, how did you balance course work with the amount of time you needed to put into basketball: into practice and traveling? And then working?

FAUST: Traveling was a real pain in the ass. I mean, that was just no- - Because basketball season overlapped two semesters. God, it was cramped. You know, we rode around in these little vans-- Well, these vans, at that time, were little. And there were big distances in Texas. We played in the Lone Star Conference, and so we would-- For others to play, it would be Abilene, which was about six hundred miles or San Marcos or Brownwood, which was about another three or four hundred miles respectively. And traveled a lot at night. And it was hard to study in these vans. You know, I don't read well-- And some of the guys would get sick because of the motion sickness.

But I also stayed around, I think, two summers and worked then and also went to school, which gave me a little bit lighter load during this semester. I must admit, the stringency of the classes at Stephen F. Austin weren't on the same level as--although grade inflation, it's hard to factor that in these days--but weren't on the same level as Rice University. So it was easier to get by on a B average, which is about what I had in college. I think I-- Overall, I had a 2.8 GPA [grade point average] and then like a 3.5 in chemistry and a 3.0 in science. And so I wasn't a top student, you know. But it wasn't difficult for me to maintain that level and still play basketball. And I didn't do a whole lot of other stuff besides that. I think if I had a more challenging academic environment, then it would have been more difficult. And I probably wouldn't have done as well in either court.

MAESTREJUAN: Okay. And were any of your instructors encouraging you to continue your education in chemistry?

FAUST: No.

MAESTREJUAN: Did they have any kind of graduate programs there?

FAUST: They had a master's degree in chemistry. And I think they had a master's in biology, but at that time there were no Ph.D. programs at Stephen F. Austin. I think that's true. I think

there are a couple of them now in education, which would be an Ed.D, I guess, wouldn't it? Or something. But no- - They had a master's program, but there were only a handful of graduate students. I think what I remember most about it was that they worked late hours. And, once again, they were in the chemistry department and they were just always doing things in the laboratory, either in terms of synthesis or analysis or construction, which I thought was kind of neat-- I guess the neat thing about it was that they were doing this at night, so it seemed like that was special, that if you saw somebody working at night rather than sitting in front of the TV or playing catch or going to the movies, then it must be special to them. Maybe because they have to, for whatever reason or not, but at least it's important. It just wasn't something that I saw normal people do, work at night. So maybe that placed research on a little bit higher level than selling cars, which sometimes people did at night.

MAESTREJUAN: Well, you do graduate and get a job in industry. And what were you doing in this job? Was it biochemical work or was it more straight chemistry or organic chemistry?

FAUST: It was a little bit of chemistry and a lot of material science testing. This was with an independent analytical lab in Dallas called Associated Dallas Laboratories. And although I was hired as a chemist, the only chemically related task I had was to do spectrographic analysis on metals that came from foundries in the Dallas area mostly, in all of Texas for that matter. So if a foundry wants to supply an aircraft manufacturer with some aluminum, then they have to mold that aluminum themselves and then send samples of the finished product to an independent testing lab to confirm that the composition of all the components is within the range for that aircraft. Magnesium has to be a certain percentage and chromium and iron and etc. The way to do that at that time was what's called spectrographic analysis, in which you just put an electrical discharge onto a plate, and then it would emit energy according to the elements that were there. And then the intensity of that energy would be an indication of how much each one of those are, and so you just take a picture and then look at the spectrograph. So that's what I did three days a week. And that gave me an opportunity to optimize that, which was just something to break the boredom, you know: changing the wavelengths or the amount of current that goes in the ARC or the development of the film or whatever. But the most-- It wasn't three days a week. I think it kind of came in loads at a time.

But the other part was that this lab also had a contract with the city of Dallas to measure things like components in asphalt that went on their streets or the composition of the cement that went on the streets or the chlorine content in their swimming pool water in all Dallas parks and recreation swimming pools or soil testing for percolation, if they're going to build a building or a house or a street. There was this whole world of materials testing which was completely unrelated to anything science or chemistry. It was just all physical kind of testing. Breaking bricks to see at what tension or at what force they would break at. So I did a lot of that stuff. Once again, the only thing that broke that boredom was optimizing those assays. [laughs]

But I could see that that wasn't really what I wanted to do, and so then the idea came, well, maybe it was time to--after about a year--to look into a graduate school. The University of

Texas at Arlington had these part-time programs. I think I went to my boss and said that I wanted to take one course, and he said, "Sure." I think at that time it was a night course, and then I took another one. And the second one happened to be a biochemistry course, an advanced biochemistry course. So after I took that course, then the instructor, Ed [Edward] Bellion, who's still a good friend of mine, just more or less sight unseen, because he had some money and he had to spend it for a graduate student, to support research for a graduate student in a small fellowship--he had a [M.W.] Welch [Foundation] fellowship--he offered me the opportunity to come to his lab. And I quit my job. It was time to quit my job and go into a lab then and to see what research was really like, on a small scale.

It was mostly a bacterial metabolism project. But it was pure biochemistry and intermediary metabolism. It gave me an opportunity to understand the experimental progress of pursuing an unknown and developing a hypo-thesis that can be testable and doing tests and answering questions, on a small scale--and working in a lab. Just began to piddle around, working at nights. And that was kind of fun. It was a different experience. You know, you could get in a different kind of schedule. I don't think you work any harder. It's just instead of going nine [A.M.] to five [P.M.] you went from noon to eight [P.M.]. You slept later or whatever. But a little different lifestyle. And then talking to people that had common goals, whatever they were at that time.

But my next goal was going to [be to] get a job again. You know, I wasn't going to pursue that. So I guess working in the independent laboratory was unsatisfying in that it didn't fulfill my desire to use some of the skills that I had learned in college, or thought I'd learned, and wanted to see applied, do the things I wanted to do, whether it was "better living through chemistry" or what. So then that said, okay, I've got to do something else. And the easiest thing to do was just to start to school again, and fortunately Ed had this money that was available, so the next easiest thing to do was to take him up on it. I kind of wanted to do that anyway, although it meant a cut in pay, but because he had a Welch fellowship I got paid more than the other graduate students at UTA [University of Texas at Arlington], which had their biochemistry department program in the chemistry department. But they had two biochemistry teachers in the chemistry department at that time.

MAESTREJUAN: So were you hired into this lab as a research assistant or more as a full-time graduate student?

FAUST: As a full-time graduate student. In other words, I quit work and I became a full-time student and took classes and then got paid to do this research. Didn't have any teaching responsibility. But it was only towards the master's degree. At that time, University of Texas at Arlington didn't offer a Ph.D. I think they do now, but that was a dead-end degree.

MAESTREJUAN: And UT Arlington was chosen because of proximity to the laboratory, the independent laboratory?

FAUST: Yeah. The only other known schools in the Dallas area that had chemistry and biochemistry were SMU [Southern Methodist University]--and I think it was more costly, because it was a private school--and then North Texas [State University]-- Now, why did I not think about that? Well, I guess because I was living south of Dallas. I'd kind of fallen back into the more rural mode, and so I was living on a farm--and living with a woman and we had some acreage and she was into horses and so we were raising horses. We were south of Dallas, whereas North Texas is up at Denton, north of Dallas, and UTA was just a little bit closer.

MAESTREJUAN: And where was the interest in biochemistry--? You said that the pseudochemistry work you were doing in the industry was driving you to go back to school. And then this position opens up in a biochemistry lab. But where is the biochemistry in terms of driving this interest? Or where is your interest in biochemistry at this point? I mean, if a position came up and opened in another lab that wasn't—

FAUST: I guess if a position came open in an organic chemistry lab or a physical lab, then I probably would have hesitated a little longer, because I didn't have that keen an interest in physical chemistry, for sure. Maybe if it was a quant [itative] lab I might have taken it. Now, I had had a course in organic chemistry. That's the other graduate course I took the semester before. I don't know, but because I had an interest in biochemistry and that was solidified by the second course I took with Ed—

You know, I thought it was pretty neat that somebody was going to pay me--sort of like being on a basketball scholarship--to go to school and do research. I think it was like $3,600 a year. There was a living support in there, and I could do something that I wanted to experience, to have some actual laboratory wandering, some research besides measuring metal content in iron ore, you know? So by and large, I guess just [that] the right offer came along was a real influence.

And then Ed was a really nice guy. He was young. He'd never been very enthusiastic, but he was relatively young-- And I never looked at him as a mentor. We were pretty close. He was British and he'd gotten into the British system relatively young and gotten his Ph.D. at [University of] Leeds and then came and postdoc 'd for a couple of years in Minnesota. So he wasn't a whole lot older than I was, really. But we were quick friends and I enjoyed his company, and he knew a lot more than I did, obviously, about what was going on, so it was easy to learn from him. And we had a good relationship, so that certainly helped a lot. We worked side by side and I was impressed with the skills that he had, apart from knowledge in textbooks, but just in manipulating experiments. I could see that it would be good to learn from him just the intangibles about doing research. And not that he's a great experimental biologist or biochemist. He's not. He's a good one, but, you know, he's still at UTA. He's full professor.

MAESTREJUAN: And were you able to initiate your own projects?

FAUST: No. He had a project and he gave it to me and said, "This is what I need to have done." And it involved deciphering this metabolic pathway in the bacteria *Salmonella*. But that didn't bother me. I mean, I thought it was interesting. It didn't bother me that he gave me the project and told me what to do. I would have probably been just as happy or maybe flattered more if he said, "Look, you just define a project and we'll do it together." So being led around didn't bother me.

MAESTREJUAN: And when did this happen, that you became a full-time graduate student? Committed?

FAUST: Well, it's probably on my CV someplace. When was it? 'Seventy-four?

MAESTREJUAN: So that was the last year of your master's?

FAUST: Right. Well, I just worked at it a year. It was just thesis research.

MAESTREJUAN: So the first year of '72 and '73 you were taking the part-time classes and working full-time. Okay.

FAUST: The academic requirements weren't that many. I think I only took about four courses, and then did some research and then wrote a thesis. So it was only like thirty or thirty-six hours for a master's.

MAESTREJUAN: And where did you see this M.A. leading you?

FAUST: Well, once again, a job. But it was a job that I thought I had more enthusiasm-- Well, I was enthusiastic about coming out of undergraduate and "We're going to work in the nice chemical company," but I didn't get the kind of job that I wanted. So I thought maybe this master's could get me greater likelihood of having a meaningful job. Now, granted, it was going to kind of narrow the choices--or the companies or the positions I could apply to, because it was-- You know, I had focused on biochemistry and I no longer was part of the big broad category of chemistry.

But I kind of liked biochemistry. It looked like something I wanted to continue to do.

Once again, I became more fascinated with this idea about chemical reactions occurring in cells and how wonderful that is, the cells being a reactor--and then the implications of perturbations in a chemical reaction in the form of defects or too much of it, what effect it has on physiology and the like. So those were really interesting.

MAESTREJUAN: Well, now that you had experience both in industry and then more experience in an academic setting, where did you see the distinctions between a chemist who works in an industrial environment versus a chemist who works in an academic environment?

FAUST: I don't think I really experienced the academic environment as it should be at the University of Texas at Arlington. This wasn't a real high-strung research environment. It was still more like a job. In large measure, research still is. You know, I didn't have a whole lot of freedom to wander off and do my own experiments. Ed was leading me around pretty good. I was doing experiments and getting answers, and I was solving problems or questions, but they were more or less the questions that he was asking. The essence of research is asking your own questions and solving your own problems, and I don't think that I had a full gulp of academic research at the University of Texas at Arlington. To me, it was still more a job-oriented thing.

MAESTREJUAN: Okay. Why then do you choose to stay in Dallas and take the position at the [University of Texas Southwestern Medical Center at Dallas]--?

FAUST: Well, I like Dallas. It's a good place and I grew up there and I had this relationship with this woman that-- We had a good life, and-- Although I realized that there weren't many places in Dallas that a master's in bio-chemistry could fit into, as compared to, say, a master's in chemistry, because there weren't any pharmaceutical companies in Dallas and there weren't any other--at that time--what we might call biotech companies, or even biomanufacturing companies. The closest things around were, say, like the Miller [Brewing Company]. They were guys concerned with yeast and trying to optimize those sort of processes. Nevertheless, there was a major medical school in Dallas, and I was aware of that, and there were other biomedical support type of institutions that hired either biochemists or—

So one of these courses I took at UTA was a cell biology course, a real cell biology course, which opened up my eyes to what was going on inside of cells from the standpoint of structures. That, coupled with one eye being opened to all this nice chemistry that's going on inside cells, and now seeing this nice architecture that functions to do some of this chemistry, I think really amazed me: the wonderment of what is inside of cells and what they do and how they do it and, once again, when they screw up what's the ramifications of it. So this whole integrated system of biochemistry and-- Molecular biology wasn't part of my vocabulary then, but I knew that there were molecules and their places in cells and how it was important that they get from one location to the other in order to act properly. And then maybe a little bit of communication between cells, which was still part of cell biology.

I didn't think at all that would ever apply to an industrial setting. I thought that was just pure academic knowledge. But it still was very interesting and it helped motivate me to maintain a high level of enthusiasm for what I was doing and what I wanted to do, or whatever. So I guess just the convenience of staying with Kathy and enjoying-- You know, I had friends and a good lifestyle and then the hopes that the medical community in Dallas, which was pretty strong, could provide a place to work just led me to stay there. At that time, I don't think I applied to anywhere else. I just stayed right in the Dallas area.

MAESTREJUAN: Well, I'm probably going to be talking about this a little bit more because I'm interested in these distinctions-- How much do you think learning about biochemistry in chemistry departments influenced the kinds of options you thought you had for the kinds of jobs that you would be kind of qualified for or what you could pursue?

FAUST: That's a good question. I have never thought about that. So I guess what you're saying is, if I had been associated with biochemists in a biology department, would I have different training, and would I have different perceptions about what I could do?

MAESTREJUAN: Yes. Where biology types go looking for jobs-- Or what kind of career paths that-- Just the opportunities of the different kinds of things that one can do.

FAUST: Well, in all honesty, I always thought that biochemistry should be in a chemistry department because it was just an extension: so physical chemistry or inorganic chemistry [and] there's biochemistry. So I guess, maybe being a little bit prejudiced, I think that I might be better qualified, having learned biochemistry in a chemical environment, than a biologist in the biology department. Now, I don't know if that's true or not. Although, now that I recall-- So we'll get on to Mike [Michael S.] Brown in a bit. But one of his famous quotes is that "It's much easier to teach a chemist about biology than it is to teach a biologist about chemistry."

Now, I've used that quote before and biologists I know say, "Well, why does a biologist need to know chemistry?" You know, that's a good argument. But he needs to know some, and if you can understand chemical reactions, and especially now it's-- In our age of enlightenment in which structural biology is really coming to the forefront--it's the next frontier in this revolution, so to speak--then chemical interactions, especially at the atomic level, are-- Just understanding those is key towards the progress of this discipline. So if biologists want to be in that area, they're going to have to know chemistry, and a lot of it. Now, maybe not ten years ago like when I started out. Biochemistry is more easily approached from a chemical environment, and then perhaps you learn a little bit more, learn a little bit better. You understand a lot more about physical reactions, too--the concepts involved in enzyme mechanisms and rates--because if you know anything about thermodynamics and kinetics, which I've forgotten [laughs]--

MAESTREJUAN: Okay. Well, I think that we're at a good stopping point, if that's good.

FAUST: Okay.

MAESTREJUAN: Okay. Thank you.

[END OF TAPE 2, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Jerry R. Faust**

INTERVIEWER: **Andrea R. Maestrejuan**

LOCATION: **Tufts University**
Medford, Massachusetts

DATE: **19 February 1997**

MAESTREJUAN: I wanted to start off today with asking a question in one area that we didn't bring up, but is pretty important in the development of a person, and that is did you have any religious training or traditional religious values in your home growing up?

FAUST: Not a whole lot. I think the values were there, but there wasn't any formal training. I don't really recall my mother and father going to church themselves. We were Christians. My brother and I, prompted more by my grandmother, paternal grandmother-- [She] saw to it that we started to church when we were very young and went until we were in our midteens, and then [we] got out of the habit. I haven't been back since. I don't think that had a real impact on, certainly, my professional career. I hope that the values that I learned there had an impact on my personality and any good features that I have. But nevertheless, there were good moral lessons that we learned at home from my parents, and not just necessarily in a religious context.

MAESTREJUAN: And have any of these more, what we consider, religious values carried on today in your life?

FAUST: Well, once again, I'd like to say that there were good moral values that I think that have carried on and I'm sure they're probably related to important lessons or training in the scriptures, but I couldn't quote you—

MAESTREJUAN: Right, right. So nothing formal, organized.

FAUST: Right. And I don't attend the church regularly now: regularly would be a little bit [laughs] of an exaggeration, too. The only time I go to church is weddings, and funerals are becoming a little frequent.

MAESTREJUAN: Okay. Well, also I wanted to follow up on-- We didn't discuss it too much, but you mentioned that you were in the honors program in science and math in high school.

How would you describe the preparation you had in high school, particularly as you're obviously in the top, top groups of your classes if you were in the honors program. And your instructors are trying to teach you about things that are happening with the ATP [adenosine triphosphate] articles, taking articles out of journals and reading them. Well, *Look* or *Life*, whatever it was.

FAUST: That's a good point.

MAESTREJUAN: And then how did this compare to the kind of instruction that you had in college?

FAUST: Well, I think it was a real good program, for me, because it did give me an opportunity to have a more meaningful experience with early science education and a leg up on students that weren't in the honors science program or math. I'm sure that that helped me in college, although remember I wasn't all that great of a student, so maybe if I hadn't been in the honors program, I would have been an even worse student.

But when I think about the early experiences that prodded me to go into science, then it was clear that that eighth-grade biology class and then my chemistry teacher were right up there at the top of the list. And those were all due to the fact that I was in an honors program and was in that class. So if nothing else, it just continued the smoldering interest that I had in science so that by the time I was midway through my college days it seemed appropriate to make the decision to go in that direction as opposed to business or athletics or whatever.

I don't know that it helped me to better prepare for science at the college level, because that just seemed like different course work altogether, just different material that was covered. For instance, the first biology course I took was a botany course, in college, and we never had anything to do with botany in high school. It was all more animal-type biology. So that was kind of unusual, I thought. But that was a school that was down there. It had a strong agricultural program and forestry program, so botany was a big thing.

MAESTREJUAN: In terms of levels of effort in high school, how did you come to be in these honors programs?

FAUST: Well, evidently, some information came to the Dallas Independent School District on me from Sterret. The answer is, I don't know. I have thought about it, how it was that I got placed in that program. It may be a mistake. Maybe they got me mixed up with somebody else. But when I went into Oliver Wendell Holmes Junior High School, I remember that I was placed into one class, and I only stayed there about three days and then got moved into this other class

that was centered around honors. And I just came home one day and told Mother that. I was no longer in Mrs. So-and-so's--who was kind of homeroom; that's where you started the day, I guess--I was in Mrs. So-and-so's. And she says, "Well, what's the difference?" And I said, "Well"--so this was in the seventh grade--I says, "Well, I'm in the eighth grade. I'll take biology instead of waiting until the ninth grade." You stepped up the math, too, so I guess I took algebra in maybe the eighth grade and then geometry in the ninth grade, which was a step ahead. So I don't know how it happened. I just assume that there was some kind of communication there--and then maybe as a result of a test that I took in elementary school, or what have you, that standard score?

MAESTREJUAN: So are you a natural, then, or were you a natural for the sciences?

FAUST: Probably. Although when I was in high school, I used to like to write. And I thought I was pretty good at it, but I really wasn't. But I think that was just more or less just wanting to express myself about different things. I thought if anything I'd be a natural at that, but by the time I got to college and really started becoming burdened with composition, I could see that I wasn't very good at it. And still, I suffer in that category.

MAESTREJUAN: Had you considered, perhaps, doing something more in the lines of the humanities or the social sciences rather than the physical and biological sciences?

FAUST: Not really, because I was always just right there in the science and math departments and the only time I went over to liberal arts was to take the courses that were required.

MAESTREJUAN: Okay, and you had also mentioned that on this field trip you saw this huge campus and were introduced to physics topics like fusion and fission. You also said you were good in math. Had you thought at any time that perhaps you'd go into physics or more analytical mathematical--?

FAUST: Not so much physics, but I really enjoyed the mathematics at the level through calculus, because that was gratifying to solve an algebraic equation in the eighth grade, or to solve problems and get it right. You know, there's some feedback there. And calculus was sort of that way. But I think I only took one course beyond-- Differential equations was a good example of that, but beyond that, then I guess it became more theoretical and less yes-and-no, right-or-wrong type of feedback, and that turned me off a little bit to it, I think.

I guess the same sort of thing applies in the science. If there are no real yes and no answers, there's just gray lots of times. But for lack of a better answer, I think I did consider math and liked it and studied well in math. But I really never considered physics. Visiting the

University of Texas [at Austin] was an opportunity just to see science in action or to listen to some of the people who were doing it, and it didn't make a difference whether it was-- You know, I could see it was broad enough that-- Physics probably wasn't that much different in the day-to-day lives of a biologist or a chemist.

MAESTREJUAN: A lot of Pew scholars who were going to high school in the sixties talk about how many programs or opportunities for summer-type programs, because of the space program and just the level of funding towards scientific education in the sixties and the early seventies-- Being in Texas, was there a big push on students to perhaps develop interests in aerospace engineering or to become an astronaut? Did you eat your Quisp [cereal], or whatever, or Tang [drink], I guess it was? Were you aware of these opportunities to do summer-type research or extra programs in the sciences?

FAUST: Yes. I was aware of them. There was quite a bit of publicity. Every high school bulletin board had applications or information about applying for summer internships at NASA [National Aeronautics and Space Administration] and associated universities. University of Houston had labs that were collaborating with NASA, as [did] Rice [University] and Texas A & M [University]. And I think some of the students in my high school did go down there. I never applied and never really thought about it. I guess mainly because it was more physical. And I guess I never got turned-on to engineering, either, the practical side of it. So I did not have any summer positions that allowed me to develop an interest in science, one way or another. I worked every summer, but it was at other types of jobs.

MAESTREJUAN: Of course, NASA was in Houston, too, and there were--

FAUST: That's correct. [laughs] That's correct.

MAESTREJUAN: Okay. Well, I think where we left off yesterday was with-- You were finishing up your master's degree and moving on to the University of Texas Southwestern Medical Center [at Dallas] as a research associate. How did you find out about the job? You had mentioned that you wanted to just find a job after. The master's was a terminal kind of thing for you, and you wanted to return to industry or get a job. How did you go about getting the job at the University of Texas?

FAUST: Well, I probably went there and applied in the employment office, as I did in several other of the hospitals around there: Baylor [University], Wadley Blood [Center], the Miller [Brewing Company] in Fort Worth--I think--[laughs] I did. And so I just applied.

And I can remember two interviews that I had at the medical center. Actually, the only

person I talked to when I first interviewed with [Michael S.] Brown and [Joseph L.] Goldstein was Mike. And another one in the anesthesiology department, which was around the corner from their offices. And it was more animal-type physiology. Anesthesiologists, to me, have always kind of looked at animals in terms of model systems to study their drugs. This was one of those laboratories, and I wasn't keen into-- Not that I would be squeamish working with animals. I just didn't think that that would hold my interest as well as something-- I was beginning to come up with ideas about molecular biology and cell biology, and, I think, what I wanted to do.

And so I think I had another interview-- Actually, this is an interesting story. I had an interview at Wadley Blood too, which is a research institution. The principle responsibility is just processing blood for all the Dallas hospitals, but then I think their investigators could get extramural funding and do research, and so there was a position open there that I applied for and had an interview. And it was, I think, a project that was more immunology based. I don't remember all the details, but just expression of cell surface markers on different populations of lymphocytes to characterize lymphocyte differentiation. So that was pretty cellular and molecular. I thought that certainly would satisfy my interest.

But Mike Brown is an impressive individual from the get go, from day one. I met in his office and he asked me to describe my master's thesis, and I did. It's purely a metabolic problem dealing in bacteria. And I could tell that although he was keen on metabolic pathways, that it wasn't his forte. He was an M.D., and he knew it. But he had sufficient interest and common knowledge that he was really motivated to hear what I had to say and to make suggestions, as if it was an ongoing project, and it wasn't. And I don't remember a whole lot about what he described, but I was certainly pleasantly impressed with the interview that I had with him.

Unfortunately, the job salary was much lower at Southwestern than it was at Wadley, coupled with the fact that at that time, Wadley wasn't deducting Social Security because it was a private organization. And then the medical school was deducting Social Security as well as Texas State teacher's retirement. So the take-home difference was really enormous. So initially, I took the job at Wadley. In my mind, I took it. I never told them, "Yeah." In my mind I took it and was going to go back and tell them, when Mike called and said they wanted to have another interview. So I went back, and this time I did talk to Joe, along with Mike, and they offered me a job. And I explained, "Well, you know, there's a difference in salary and I'm motivated enough by the dollar that I'll probably take this other," and they said, "Well, if we match this salary, will you come work for us?" And I said, "Yeah," because I really thought I would be more interested in it.

Also, Joe was head of medical genetics in the Department of Medicine, and as part of that task his responsibility was to oversee a genetic counseling clinic that was fed into by a cytogenetics lab at Children's [Medical Center of Dallas], which is right next door to Parkland [Memorial Hospital] and part of the medical center complex. The woman I was involved with was a technician who worked in the cytogenetics lab, and then went to conference once a week that was supervised by Joe. So I got information about Joe from Kathy. And she was right, that Joe is a unique person--also a really intensely driven, motivated individual--but obviously was

going to be successful and that one could have an enormous opportunity to learn and grow in his environment. And then I could see that would be also the case with Mike. So it didn't take a lot of brainpower to say, "Okay, there's some real opportunity here. And I don't know what they're doing. I don't know the stuff about the LDL [low density lipoprotein] receptor or whatever, but they're in the Department of Medicine at a major medical center, they're both well funded, and they come from excellent programs. This is as good an opportunity as you're going to get to grow." So when they offered me the same money, I said, "Sure, I'll take the job."

And I went home and called Wadley and said I wouldn't take their job. And I was supposed to start to work, I don't know, in three or four days. And I remember it was a hot Saturday afternoon about three o'clock, and I was way out on the back part of this property that we had, mending some fences for these horses, when Mike called and I had to come in--and I was kind of amazed that they were working on a Saturday afternoon--but he unfortunately told me that he had checked with personnel and that due to limitations in the structure of what people can be paid based on their experience and educational history that they couldn't pay me that money and I had to start at considerably less.

I was really disappointed. Here I was going to have my cake and eat it too. I was going to learn a lot and then get paid for it. But it was hot that day and I was tired and I'd already told Wadley I didn't want to go there and I didn't want to start looking again, and I says, "I'll just show up anyway." So that's how I started.

So I think I was originally hired as a replacement for a really remarkable technician that had worked with them for two or three years up until that time, Susie--Susanne [M.] Dana--that had done a lot to get that program underway, and she was going to go off to medical school. But she didn't go off right away. We overlapped for about a year and a half and then [she] finally went off to medical school. And then coupled with the fact that Mike told me later on that they were beginning to start a project that required some skills in synthetic organic chemistry--very, very crude, minor, basic skills--and then also in chromatography, separation of like compounds by gas liquid chromatography as well as thin layer chromatography-- And neither one of them had any experience in that, but I did. The latter more of a lie than anything, because I knew what a gas chromatograph was and I'd done some simple TLC [thin layer chromatography]--I knew it was pretty easy--but he said that they were wanting to do some experiments that required this technology and that I could do it. So he said that was the motivation for offering me the position also. And I was successful at doing all those things that they wanted done initially, too, which I guess helped me along.

MAESTREJUAN: How long had they been at Southwestern by that time and had they already combined their labs when you had gotten there?

FAUST: I think they'd only been there about two or three years. Joe came first. Joe was a graduate of Southwestern. Mike was a graduate of Penn [University of Pennsylvania], and they met when they were interns and residents here at MGH [Massachusetts General Hospital] and

became friends. Then they went off and did fellowships at the NIH [National Institutes of Health]--Mike with Earl Stadtman and Joe with [Marshall] Nirenberg--and stayed together there, and I think it solidified their friendship. People speculate [that] maybe they were formulating future plans at that time; I don't know.

But then Joe went and took a position in Seattle to really begin to set the framework for studying LDL metabolism and defining the genetic inheritance of familial hypercholesteremia type II. That enabled him to come to Southwestern at a little bit higher level than Mike. I think Joe came as an associate professor and head of the Division of Medical Genetics, whereas Mike actually came as a postdoc with Marvin [D.] Siperstein.

But I guess I always felt like there was an underlying understanding that Mike would become a faculty member right away, and indeed he did. But about the time I came, although they had separate labs-- I think Siperstein had just left and Mike was setting up his own lab and they were still separate. It was clear that they were going to be fusing together on their experiments and projects. Mike's was on the floor above Joe's. So there was constantly communication back and forth. So altogether, I guess Joe had probably been there about four years and Mike maybe two.

MAESTREJUAN: And had they already started work identifying and characterizing the LDL receptor?

FAUST: About the time I came they just began to formulate the idea that this receptor exists. The preliminary data before that just pointed to the fact that patients who turned out to have a mutation in the receptors just were not responding to lipoprotein-containing serum, but you know, they didn't know what components were missing when you remove the lipoproteins--obviously, all the lipoproteins. And the response was just to measure what we know now to be a regulatory response that cells evoke when cultured in the presence of lipoproteins, measuring cholesterol synthesis HMG CoA reductase [3-hydroxy-3-methylglutaryl-CoA], which was Mike's forte. Mike knew how to measure HMG CoA reductase activity. So that helped, I think, once again, solidify that collaboration. And it was from that simple assay that they realized that the component in the serum that the cells were not responding to was indeed LDL--and that's about the time I came--and then later went on to develop a binding assay to show that the reason they don't respond is that they don't take it up. And started this whole business, which is an amazing continual story. I just gave a lecture at eleven o'clock about what I call the latest onion skin in this story about cholesterol-mediated regulations in cells, and it's work from their lab.

MAESTREJUAN: And what did you start working on? When you entered the lab, what did you start working on, and how far had--? It's in '74 that they do the fundamental work that gets them the Nobel Prize, is that correct? And where were you in this scheme?

FAUST: Well, they had an intuition that removing and adding lipoproteins to the media had something to do with cholesterol metabolism, because lipoproteins contain primarily cholesterol. And in patients who don't have this receptor, there's this enormous accumulation of cholesterol in their serum because the liver can't take it up and get rid of it.

So they wanted to have ways to quantitate cholesterol content in cells. Presumably, if a cell has a receptor and is taking up lipoproteins, then it's taking up cholesterol. But if cells don't have this receptor, don't take up the lipoproteins, then the cholesterol level in those cells might be lower. It comes down to a basic procedure to just measure the cholesterol content in cells, and that's where the gas chromatograph comes in, because you can use that to separate cholesterol away from all other interfering substances that are present in cells and then the gas chromatograph can actually quantitate that. So they wanted to be able to see if the nonresponding cells were somehow or another taking up the lipoprotein and taking up cholesterol. They purchased a gas chromatograph, and I got a book and read the book and learned how to use the gas chromatograph and learned how to measure cholesterol in cells and indeed showed that normal cells--that could respond to this lipoprotein--when cultured in the presence of lipoprotein had a higher cholesterol content versus the mutant cells that couldn't respond to the lipoprotein. Their cholesterol content did not change. So they weren't taking up the lipoprotein.

Then to substantiate that observation, a more direct way to show that they're not taking up the lipoprotein-derived cholesterol is to label the cholesterol part of the lipoprotein with, say, tritiated cholesterol and then to show that radioactivity from tritiated-cholesterol-labeled lipoproteins was transferred to normal cells and that you saw the appearance of tritiated cholesterol, whereas that transfer or that uptake of the tritiated radioactivity didn't occur in the mutant cells. So that the predominant form of cholesterol in lipoproteins is as an ester of cholesterol and fatty acids as opposed to just free cholesterol. And at that time, you couldn't buy radiolabeled tritiated cholesterol esters, so they had to be made. So a simple organic reaction was to just take fatty acids that are in the form fatty acyl chlorides and react them with tritiated cholesterol that you could buy and form labeled cholesterol esters and purify them, work it up, and then incorporate it into the lipoprotein and show indeed that, by just monitoring radioactivity, the mutant cells didn't take up the cholesterol. So that explains why their cholesterol content does not increase. And then the TLC comes in to separate cholesterol from, say, cholesterol esters. The cells also take up this cholesterol and turn right around and hydrolyze it to free cholesterol and then also re-esterify it. So there's this cycle of cholesterol esters [that] comes in, gets hydrolyzed to free cholesterol, then the cell moves it to another part of the position in the cell, then re-esterifies it. So you have to be able to separate free and esterified cholesterol. And you wouldn't want to do that on the gas chromatograph and put all this radioactivity out in the air, so the best way to do it was by thin layer chromatography.

I was fortunately successful at meeting those goals early on, which gave me a lot of confidence, because it required that I go to the library, get a book on gas chromatography [laughs], figure out a synthesis for cholesterol oleate purification, work on some TLC systems, and do this on my own. So that gave me confidence that I could do some part of a small research project, which I really didn't have from my master's work, remember, because Ed

[Bellion] kind of led me around, told me what to do. And then also I guess the small measure of success gave them confidence that I could function at this master's level, which was supposedly a step above a technician. I was still a technician, but a step above just a baccalaureate technician.

MAESTREJUAN: How many people were in their lab at this time?

FAUST: There was Gloria [Y.] Brunschede. She was Joe's technician. And then there was a tissue culture technician, Helen Bohmfalk. And then Susie [Susanne M.] Dana was Mike's technician. And then Mike had a postdoc, Patty something or another. I can't remember her last name. So between the two labs, I guess there were five of us, not counting Joe and Mike. And then-- Now, Joe had an office. So he had his own secretary who was part of this, being head of Medical Genetics. And then there was another woman that helped out with genetic counseling, Mary Jane--whose last name I can't remember. But those weren't part of the research lab. They were administrative. So I hope I haven't left anybody out. Somebody will be able to read these, you know. But people began to start coming. Things began to grow from that point.

MAESTREJUAN: And you had mentioned [that] coming into the lab you didn't know what they were working on. But as you started working in the lab, when were you aware of the significance of the work that you were doing?

FAUST: I don't think I became aware of the significance early on. I think it probably was measured in years. Now, I knew that this disease existed, that there was this genetic trait that led to early onset arteriosclerosis. I guess I didn't know its frequency, so I didn't know it was as frequent in the population as it is. So that was hidden from me in terms of adding significance. I guess I first became aware that they were doing significant work in that they began to win some prizes and got national recognition. This probably was late seventies.

I mean really, the only thing I cared about was that I was having a good time. It was fun. It was hard work, and I knew that--from reading the literature--the things that were going on in that lab were at the cutting edge of science. By that time, the idea of ligand-mediated receptor interactions was coming about, and here we were studying one and the cell biology associated with it. So I guess that was significant to me, that I was in the position to continue to grow if I wanted to.

And then they were getting recognition within the medical center community also, in terms of more space and more resources, that I know doesn't just come to everybody. They had a lot of support from the chairman of medicine at that time, Don [Donald W.] Seldin, who was a real supporter of their efforts. So other people that I also began to respect and admire and mimic there at the medical center, like Jean [D.] Wilson and Den [J. Denis] McGarry, John [M.]

Dietschy, also respected them, and you could see it, that they saw something special going on there. So when you began to realize that people at that level had a certain feeling, then--even though I couldn't feel it--it's worth staying around.

MAESTREJUAN: Well, you had experience working in material science, where you used your chemistry and biochemistry skills somewhat. And then you go into a job where you're using a lot of your biochemistry. How did moving away from this industrial, applied orientation influence how you saw your own knowledge and skills and abilities, now that you're in a very clinically driven biological environment, academic environment?

FAUST: Well, it really showed a weakness in my training, because I struggled quite a bit with some of the concepts that I was learning and manipulating on a day-to-day basis. But fortunately, Mike Brown's a great teacher. And both of them are real hands-on investigators, so they would try to set up a protocol and experiments as much as possible with technicians and with postdocs, even. I mean, you can talk to their postdocs even to this day, that usually within the first year it's a one-on-one situation between them, that they instruct these postdocs what to do and how to do it. And I interacted a lot with him in some of the basic biochemical studies that I was doing, and he was really very good at explaining the workings of the cell. I mean, I could understand the assay in what we were doing. That wasn't any problem. But why we were doing it and what the impact of a result was-- Which really helped me to, I think, understand the big picture. Later that filtered down into the finer points of the observations that we were making.

So the answer is that I struggled quite a bit because I didn't have the basic background information that fit into this job. But fortunately, I think primarily through the efforts of Mike-- He brought me up to speed to where I could-- Not work on my own, but at least understand where we were going and why we were doing and what are the implications and results and the like.

MAESTREJUAN: Well, how much of a background of biochem-istry had either one of them had? What were their bio-chemical skills?

FAUST: Well, I think pretty strong. I assume both of them have baccalaureate degrees and I expect they're in sciences of some sort, as most premeds. And they did postdocs at the NIH [National Institutes of Health] in biochemical laboratories, Earl Stadtman's and Nirenberg's. So I think they had a strong biochemical background. But then-- You know, Joe believed in the universality of genetics, that genetic experiments, though [they] sometimes may give us confusing interpretations or the results may be confusing, that they are universal. If you compare one sibling that's a twin-- A pair of twins and one is carrying a mutation, then all other things are just about equal except for that single gene. Or let's make it more simplified: On a cell line that you've made a mutant from, a mutant cell line, you've got two cells that are

identical with respect to all genes except one. Then any differences that you observe, say biochemically or molecularly or physiologically, between those two cells are due to that difference in that one gene. So that trait or that observation is secure in your mind as being related to that gene and not something else. So it's just a matter of finding that gene and then working back to say, "Okay, well, why is it that the presence or absence of this gene causes this trait?" Or as Joe thought from his study--from his ability to discern that this one disease was due to a single gene--that if he could just trace back from this trait, he could find the defective gene. So he had this other perspective on science, applying genetics to studying science and then also in the process working out genetic defects.

[END OF TAPE 3, SIDE 1]

MAESTREJUAN: As the years are going by and you are beginning to see the significance of the work and they are starting to get awards and recognition outside of their labs for the work that they're doing, and you are doing, how did this change your own perspective on the significance and the contributions of your work in that lab?

FAUST: As the years went by, the lab grew, so there were a lot of people making contributions, and I was just one of those. I realized and appreciated the fact that my contributions were moving this project forward, were helping us understand more about--whatever it was that we were studying at that time--[laughs] I mean, just the whole idea of receptor-mediated endocytosis in intra-cellular cholesterol metabolism. But I also realized that I was just a small part of it and there were other people pulling their weight and making contributions and we all kind of came together to move the wagon forward. So it wasn't that one of us was doing it. We were all doing it, and each one sort of substantiated the other or corroborated-- Each individual member of the research group was providing data that contributed to the general acceptance and the understanding of the facts that we were discovering.

MAESTREJUAN: Okay, well, you did make key contributions. You do get several first-author papers out of their lab, and if I understand it right, you develop an assay to measure cholesterol within cells. So you are functioning independently, and--given that as time is going on, their lab size is increasing, the excitement's increasing, and I'm sure the competition is increasing--why is it that you are able to make these contributions in a very productive and growing lab?

FAUST: Well, I liked it. It was really a lot of fun, learning. It was, I guess, fulfilling dreams, being part of this group that with each week or month or year became more productive, and I realized that I was just extremely lucky to be there. I could be at Wadley or Miller, whatever, and I may have been happy there. But to actually get up in the morning and enjoy going to work because of what potential opportunity or potential thing I might learn that day or result that may come up that could help us-- And it was a real motivation to get up early, get to work, walk

quickly from the garage to the lab, and stay late and come in on the weekends. And then everybody else was doing it. I mean, there weren't any exceptions. Joe and Mike were there on the weekends, all the postdocs. So it was like a train going downhill. It went faster and faster and faster, and you didn't want to get off.

Now, that wasn't always fun. There were some downsides. There were some personality conflicts and some raw times, but I think anytime you have people working together closely and have common goals, then there are going to be disputes about how to approach those goals, or egos get in the way. I was somewhat removed from that because I wasn't in any kind of competition with anybody. At that time, I was working my way up to the technician and associate ranks, so I had a stable job. I wasn't like a postdoc that was going to have to get x number of papers and so many credentials in order to go off to their next position, to the next level. So I don't think that I experienced that sort of competition and sensitivity to egos that, say, postdocs did.

And then also my interactions with Joe and Mike were a little bit different than the postdocs, which by that time had become the lion's share of the labs. There were just-- Well, Gloria [Brunschede] is still there. She's the technician; she's still there. And there are other technicians, but the numbers increased primarily, I think, due to a flux of postdocs that rotated through. I didn't have the same sort of relationship that they had with Joe and Mike. Sometimes that was good and sometimes it was bad, because they also had pressures to see to it that their postdocs had a good measure of success. Otherwise they wouldn't be getting good postdocs. You have to have this track record of bringing in postdocs and having them be successful and accomplished and productive so that they'll go off and get good jobs so that you can bring in another batch, whereas, you know, Faust, he's going to be here for a while.

But they rewarded me amply. Well, I didn't make a whole lot of money. I used to get aggravated. I mean, the University of Texas never did pay very much. I sure wish I got paid more. That was a sore thumb every time raises came up. And I think I rubbed Joe-- Because he would always be the one that would tell us what next year's salary was going to be. So I usually took that out on him.

MAESTREJUAN: I know in the humanities, University of Texas pays better than University of California now.

FAUST: Really?

MAESTREJUAN: Yes. I [don't] know if that speaks better of the University of Texas or speaks down about University of California.

FAUST: Well, I don't know what they pay now, but my starting salary, when I started in 1974,

was $748 a month. And then you have to deduct Social Security--and teacher's retirement was like 6.8 percent. That was a real letdown. And then for a long time, especially in the late seventies when oil wasn't doing so well, there were hardly any cost of living raises, so it didn't go up that much. But what are you going to do with money? I didn't have a whole lot of responsibilities. I don't have any children, so it doesn't make any difference what you make, as long as you're comfortable.

MAESTREJUAN: Well, were you still able to have your horses and everything, given all the time you spent in the lab?

FAUST: I think we left DeSoto-- We were living in DeSoto on this acreage and decided we'd buy a house, so Kathy and I moved into Garland and bought a house--Garland's a suburb, so into suburbia--and lived there for several years, and then she and I parted ways. She still lives in Dallas; I think she still lives in Garland. And I moved back into Dallas and continued to work there. That was my last experience with more of a rural setting.

MAESTREJUAN: How was it to work with--? She was working in Goldstein's lab, I think.

FAUST: No, it wasn't Goldstein's lab. She worked at a cytogenetics lab associated with Children's [Medical Center of Dallas], but they performed karyotypic analysis that was used in the genetic counseling clinics, so that was the interaction.

MAESTREJUAN: Okay. So it wasn't like you were working together.

FAUST: No, no, no, no. Actually, I think she stopped working at Children's and then began working at-- Where did she go to? She changed jobs. I can't remember, but I think she didn't work much longer at Children's.

MAESTREJUAN: Well, where did you see yourself fitting into the lab, in terms of the social hierarchies that developed, and educational hierarchies that developed?

FAUST: I thought that they treated me just like a postdoc in terms of educational opportunities, which was great, because I wasn't, but I was being treated like one. Probably getting paid more, too, I guess, by that time, because postdocs don't take home-- In the social structure, then I wasn't at the same level as a postdoc. If a visitor would come to town and they would go out to dinner, then I didn't go. I didn't go to meetings much. I think it was hard to get money to send technicians on to meetings, you know, like this, I was told. But that really didn't bother me. And

going out to eat with the seminar speaker really didn't bother-- Or not doing those things didn't bother me all that much.

So it was a good position to be in, to be treated like something that you obviously aren't qualified--or on paper, you weren't qualified--to do, but yet-- And were given the same sort of freedom as those people in that position and have the respect from not only the people who were supervising you, but the people in that position, because all the postdocs who came respected me as if I was one of their peers. It was an enviable position, as long as you didn't have a big ego, which I never had.

MAESTREJUAN: Well, how did you see your own abilities in comparison to these postdocs that perhaps went to meetings, met the important people?

FAUST: Initially I thought that my abilities were on a par, after I got up to speed with what was going on in the project and the field. But then as the-- Well, the caliber postdocs got better that came. As they became more popular, they attracted a much higher caliber postdoc, and then with each new year, then students who had finished their Ph.D.'s were much more skillful with new contemporary techniques and ideas. I guess not only is the caliber of postdocs coming [to the lab] better, but just in general, the postdoc world is much more skillful, that with time, my skills and knowledge background were inferior, especially on a lot of broadly associated fields of biology, associated with the things that we were doing in the lab. But that was okay, because that gave me an opportunity to learn from them--the postdocs that came--about what their skills were that they were bringing to the project, because Joe and Mike would try to attract postdocs, as they did for me, that could help them accomplish, initially, a specific goal or project. When they first set out to clone genes, they brought in a fellow who had worked in a very prominent cloning lab.

When they first started the Department of Molecular Genetics, a full-fledged department, there was only a faculty of two. Joe was the chairman and Mike was full professor. And then they said, "Okay, we're going to hire somebody." So who do you think they hired first? What field do you think they'd field first? They went here to Boston, to E.J. [Elias James] Corey's lab over at Harvard University, great synthetic chemist, and hired his best postdoc, because they wanted to do some really good, sophisticated chemistry, orders of magnitude above what I'd done by just synthesizing cholesterol esters. So here's Camille Falk, who--the only thing he ever used the biology book for was to set his coffee cup on--was now the first faculty hire in the Department of Medical Genetics at the University of Texas Southwestern Medical School, because that was a need that they had to have in their department, for this knowledge and these skills.

And they hired postdocs that way too. I 'm not sure that that was the only criteria. You know, their project and their knowledge and how quick-witted they were, or whatever, was important, but if they could bring skills to our group that we didn't have, that was a real plus.

MAESTREJUAN: And what did that tell you about your own skills and chemistry background, at looking at your own opportunities?

FAUST: Are you saying did I feel like I was being phased out or something?

MAESTREJUAN: No, no, just in terms of that it made an impression that the first hire was a chemist-- What did that then say to you about your own potential and opportunities, with your background and your interests in chemistry and biochemistry?

FAUST: I guess not much, because at that time I had consumed all or expended all the skills that I had that they didn't have, and used them, to where they knew what they were, and the project had progressed to the point that we were beyond the point that I was bringing anything unique to the table.

But I thought it said a lot for how they approach research. In that to try to avoid surrounding yourself with your peers--who have more or less the same skills and technology that you have--but instead, albeit at somewhat of a gamble, to bring in perhaps individuals who it might be difficult for you to, not manage, but oversee or to be critical in a positive and negative sense about their performance, if you really don't understand or have a good grasp of the experiments that they're doing and the type of work they're doing. But yet they could challenge themselves to educate, to bring themselves to that same level of this new postdoc, to grow along with the rest of us-- They had the same opportunity the rest of us did. Probably did a much better job of it. And you could see also that when new postdocs would come in and have newly acquired skills or traits or technology, then—

I could see that it was important that all of us learned from these people. And that wasn't always the case with other people. For instance, if "Frank Smith" was an electron microscopist, and he started to work in Joe's group--or in our group, because they were interested in setting up gold labeling of LDL [low density lipoprotein] and its metabolism in cells--then maybe "Mary Doe" didn't see that as an opportunity to learn about electron microscopy and what you can do with it to study a complex cell biological pathway. And that's not a bad thing, because Mary's got other things that she's worried about, but it says something about people that, I think, reach the level where-- They think that they don't want to learn anymore or want to begin to focus specifically on their own project, and not try to acquire information from the outside or on the side--or from the periphery--to bring it into their project. It's not that they're sticking their head in the sand, but they're just not keeping their eyes wide open. I thought that was a trait that I should avoid, that I should at all costs try to learn from everybody who came into the labs.

MAESTREJUAN: Was Nobel Prize ever whispered around?

FAUST: Oh, yeah.

MAESTREJUAN: At what point did it become clear that this work was of major significance?

FAUST: I think back in the early eighties, because I think the first time I heard it was that they had won a sufficient number of these prizes that they were on a track. I don't know the names. I think [Albert Lasker Medical Research Award] is one of them. National Academy [of Sciences of the United States of America]-- That they were just on the right path for a Nobel Prize. A lot of other people in the school began to joke about it, saying, "Oh, well, when they get the Nobel Prize there won't be hats big enough to fit their heads," and this kind of stuff.

MAESTREJUAN: Why do you decide to leave their lab?

FAUST: Well, I was thinking about that. I always thought about this. And I guess there are probably three reasons. One is that the politics-- My perception is that the politics in the lab began to change. Maybe it was just that set of people that were there before I left, but there seemed to be a lot more friction and a lot more pettiness. Competition between postdocs was--for attention and resources and their attention, their resources--pretty high and then that contributed to friction and a little bit of pettiness. I guess I thought, well, that's probably going to get worse before it gets better, because it may be just a normal consequence of getting to these levels in science. And I just didn't like to be a part of that. I didn't like to watch it.

And then I began to realize that it might be nice to do some of this on my own, at least give it a try. And although Joe and Mike had both encouraged me to apply to a Ph.D. program there at [University of Texas] Southwestern [Medical Center at Dallas] and to go back to school and what all, that just didn't seem like a keen idea. I guess I was just lazy. I enjoyed working in a lab, didn't want to go back in a classroom. But because the things that we were doing in cholesterol metabolism had such important implications pharmacologically, then I knew that there were pharmaceutical houses that would find a place for somebody like me, with the years of experience I had in their lab.

For instance, they traveled to Merck [and Company] quite a bit. They were consultants at Merck, and this is back when Merck was trying to develop Mevacor and Lovastatin. So they'd come back a couple of times and say, "Oh, you could go here and do these things in these labs and get paid lots of money and be a principal investigator, albeit in a pharmaceutical house, but still a research-type job. That was appealing, even though I'd leave this environment that has nurtured me and allowed me to grow. It would be a meaningful challenge to see if I could do similar things on my own, albeit at not the level or the striking force that they could, but just to determine whether I could survive. So that was nice.

Then lastly, I began to get the impression, and I'm sure that they may think otherwise, that they were beginning to see me in a more limited capacity, like towards placing me in just a specialized arena within the lab, for instance being in charge of a sequencing laboratory or a recombinant DNA laboratory, that was more routine-type work and not in pursuit of anything directly informative, a direct investigation. Now, I perceived that that was their ambitions for me, and like I said, I think that they'll say that was not true, but nevertheless, it was my perception, and I just didn't want to get into that sort of position. I still wanted to be challenged in the same fashion that I was being challenged for the past ten or eleven years and not to be challenged with, say, just technology. So I began to look for a job.

MAESTREJUAN: And were Goldstein and Brown aware of these changes in politics? What was their role in the gradual evolution of things in their lab?

FAUST: Well, I'm sure they were, but I never discussed it with them. I didn't think it was my place. I was sort of like a fly on the wall at the time, and I really was never involved in any conflicts or challenges. They certainly had to confront those and deal with them. But I don't think that they thought anything more about it. These sort of things happen in all labs--in fact, I see it all the time here--especially in big labs, and it's not anything unique to their situation or speaks poorly to their management skills or whatever. It's just that it's the natural consequence of things when you get to that large scale of operation with so many people that realize there's--as I did, so long ago--that there was a real opportunity here, that you try to want to do the best you can. Sometimes that occasionally oversteps the point at which right kind of drifts into wrong, or good sense drifts into misjudgment.

MAESTREJUAN: Where did you see the issue that you didn't have a Ph.D. fitting into what was happening in the lab, and what was driving your decisions to leave the lab?

FAUST: Well, I think not having a Ph.D. was maybe influencing Joe and Mike to think about putting me in these other types of positions, or other roles in the lab. And then also, that these roles came about. It's clear that they had to have someone who runs-- Because they were going to do a lot of protein sequencing, they needed to have somebody to do that, needed to have someone reliable to do it. And they needed to have people that could make monoclonal antibodies. They needed to have their whole lab do that, and they took up a postdoc, a very skillful postdoc, and put him in charge of just making antibodies. So they needed to have these services within their department that the rest of us would go out and pay for. So the fact that I didn't have a Ph.D. just kind of fit that mold a little bit better.

I didn't think lacking a Ph.D. was going to hurt me in looking initially for the jobs that I was interested in, like at Merck or at Upjohn [Company], so I interviewed at both those places. Because biotech didn't have to follow the same guidelines that an academic institution did with respect to doctorates. Essentially, you have to have a doctorate to get funding from any

extramural source. That's because you have to be a faculty member, and Tufts [University School of Medicine] is not going to make anybody less than a Ph.D. a faculty member, so that means that if you don't have a Ph.D., you're not going to get funding. Well, that doesn't wash in industry because you don't apply for grants. There, whoever's your supervisor, and his supervisor, is who you have to convince to give you money, and it doesn't make a difference as long as you're productive and you've got some history that you can point to and say this is what I've done and these are my ideas and this is what I want to do. There's nothing about diplomas in there. So I thought that at least at the outset, for the jobs that I was interested in, I didn't need to have to have a Ph.D. They just need to have the skills and the history and the references that I had.

MAESTREJUAN: And why industry as opposed to maybe a strictly research institute or perhaps a smaller lab where you could--?

FAUST: Well, then again, if it was a research institute, it [would be] harder for me to be completely independent, or to have some independence I'd have to get my own funding. And I couldn't do that myself without a Ph.D. Now, I could perhaps piggyback along on another principal investigator, but that didn't seem as desirable as whatever. And then identifying somebody like that could be troublesome. Getting along with somebody like that [laughs] may or may not be feasible, may or may not cause problems.

And working in a smaller lab-- I thought if I was going to make that kind of change, then it was going to be not to the same situation. If I understand you correctly, by going to a smaller lab, it would be just more or less the same sort of situation, learning another research topic, just in another environment. It wasn't anything new to me, except maybe the project. But going and being a principal investigator myself, that was new. And that was worth the change. Or to try to do that would be worth the change.

Means picking up and moving, too, which I really didn't-- And then also, I guess, I never really thought about moving away from Texas until about that time, and I just matured. It wasn't that anything was pushing me out. I guess it was just time to do something else, or time to accept the fact that moving is okay, whereas in the years prior to that, I'd say, "Well, I'd never leave." I mean, my family was all there and friends.

MAESTREJUAN: And why do you choose to then go to [E.I.] Du Pont [de Nemours and Company]?

FAUST: Well, Du Pont had a wonderful initiative started. They had made beaucoup of money in chemistry, and rightly so. There were enormous profits from their chemical technology. They were setting up a life science program to branch out into biology: more or less the groundwork for a biotechnology company, and maybe even, I don't know, a far-reaching blueprint to be a

pharmaceutical company. And this initiative's first step was to just try and set up an institution within Du Pont--similar to, say, the Roche Institute [of Molecular Biology] that Hoffman and Roche set up, that was freestanding, funded by Du Pont's money--to bring in qualified and exceptional scientists to just do research and to do this at what's called their Experimental Station outside of Wilmington, Delaware, which is really a campus atmosphere: so there are all these wonderful old buildings with fully equipped, well-equipped, modern chemical laboratories, in which the bulk of Du Pont's technological research is performed, in these laboratories. And it's been that way since the turn of the century too. So there was this environment that was already set up to do basic research--and a huge library, plenty of resources.

So they were just going to build a new building and say, "All right, we're going to do the same sort of thing with biology. And then lo and behold, I'm sure that there's going to be knowledge or information or spinoffs that's going to come out of this biology building as a result of this research that we might be able to seize upon in a more applied sense. But then we'll have a head start on it, because it was started here, in our life science programs." So this was sort of like a purely basic research institute, but not all by itself. It was part of a large complex of other supportive systems. It really seemed like an ideal situation. I can't tell you how exciting it was just to realize that they were starting this, and on paper it was going to be the paradise. You didn't have to apply for extramural funding. And for someone like me, you know, great.

They were targeting three areas: immunology, membrane biology and cholesterol metabolism, and neurosciences. So one of them happened to be something that I had a good stance in. And then secondly, the leader of the group, Jim [James L.] Gaylor, is very prominent in cholesterol metabolism. He was familiar with me and my work, although I didn't know him. So that was certainly an inside track. It was just a perfect setup, to go and begin to work there. I didn't take anything with me from Dallas, in terms of projects, but just began to study what I wanted to study in the context of other avenues in cholesterol metabolism, in a brand-new building, brand-new laboratories, plenty of money, more salary--more salary than I make now. This was, what, twelve years ago?

Even though Merck had made a job offer, it was still more or less being part of a group that was focusing on pharmaceuticals related to cholesterol metabolism. So it was drug hunting, and that was okay. About half the time it was that and the other half it would be something else. Upjohn had a comparable offer, but nothing as potentially spectacular as what Du Pont was proposing.

What they did, and later I discovered that they weren't able to follow through on this-- But on paper, it was great. I think Joe actually went-- They actually opened the building before I went, and then he was part of the opening day ceremony or something, a keynote speaker. I think that speaks for, at that time, the commitment that Du Pont had to really project this enterprise into what they hoped would be a Roche Institute within the confines of Du Pont de Nemours.

MAESTREJUAN: How committed were you when you left the lab, the Goldstein-Brown lab, to continue research in the cholesterol metabolism field?

FAUST: Absolutely. I think that I had plenty of ideas. I thought that I could do my little niche over here, that wouldn't be related directly to what they were doing and so therefore wouldn't be in competition. It was clear that you couldn't be in competition with them. I was just anxious to get started. I thought that there was a great opportunity for me to test my mettle.

MAESTREJUAN: Had the Nobel Prize been announced when you left?

FAUST: No, they waited until I left. I left in January, and then that was announced in that fall. I joke with them about, well, the Nobel committee waited for me to leave.

MAESTREJUAN: How did that change the way you saw your decisions, or did that influence how you subsequently felt about leaving the lab?

FAUST: I don't think it made me regret leaving any more or less or it made me glad I left any more or less. I understand there was a big party down there. I think I taped their acceptance speech and played it back. I didn't see it live for some reason or another. Now, I was glad they won. So I don't think you-- I think Joe pointed this out one time. Really, in the truest sense, you shouldn't say that people win these kind of prizes. That's not the right verb. They're awarded. But I mean, there is competition for it, and I just used [the verb "won"]. But I think that they deserved to be awarded the Nobel Prize, and at this rate I think they probably should get another one. Not today, but if they continue to be as productive, then they probably deserve another one for their efforts in science and medicine. So I was certainly glad, and I was glad I was part of that.

You know, they sent me a nice silver platter--beautiful thing. In order to keep it [laughs]-- I have to polish it once every six months just to keep it sparkling, but it's something that I have that I can point to and say that this was my contribution, or this shows that I contributed to history. I don't think there are other people around that have that pleasure; there are, but not as many. So I'm glad that I have that platter, and I'm glad that they gave it to me. But I don't feel, to get back to the original question, that I made a mistake in leaving or that I should have left earlier. I think I left when the time was right.

MAESTREJUAN: Did that change, do you think, the state of Texas's attitudes towards funding biomedical sciences? I think that was the first Nobel Prize for a Texas institution?

FAUST: That's correct. [There were] several Nobel laureates that were at Texas institutions--a couple of them in chemistry at [Texas] A & M [University], but they had done the work elsewhere--but that was the first homegrown prize, so to speak. And yes, that caught the attention of some prominent people in not only the government, but philanthropic circles around Dallas as well. I don't know if you're aware of it. [H.] Ross Perot is a major benefactor of Joe and Mike.

MAESTREJUAN: Really?

FAUST: Research, and I'm sure that there's been a couple of meals thrown in there too.

MAESTREJUAN: Yes, and plenty of money to do it. I didn't know that--

FAUST: But that's great for the medical center. And the medical center, I'm sure even after I've left, has benefited enormously just from their presence there. Not only in, as I said, attracting this--Ross Perot--since he set up an M.D./Ph.D. program. Funds an M.D./Ph.D. program. Just at Southwestern. I think there's twenty M.D./Ph.D. students each year.

MAESTREJUAN: Does it have his name on it?

FAUST: Probably. We could talk about the pluses and minuses of Ross Perot. I'm sure that they have their own-- But Mike made some-- I heard that at a national scientific meeting he stood up and asked the audience to support Perot in his quest for presidency in '92. If that's true, then that certainly is out of place. It was almost like-- People were kidding. It was, "Was that like an endorsement that was paid for, you know?"

MAESTREJUAN: [laughs] Qualifies as an advertisement?

FAUST: But I don't know. But their presence certainly has improved the medical school. Then the other thing too is that it has attracted this enormous caliber of faculty, I mean, Al [Alfred G.] Gilman, another Nobel laureate-- [Joseph] Sambrook was there, and I think he's leaving. Too numerous names for me to mention. Just to bring these people in just because they were there, which they view as a plus for them too. In fact, they insisted that the [Department of] Pharmacology, where Gilman went to, be right down the hall from them. So Al Gilman became a close associate and participant in our lab meetings, because he could bring some things to the table that we didn't have.

[END OF TAPE 3, SIDE 2]

MAESTREJUAN: Well, how did knowing that the work you did contributed to being granted the Nobel Prize change your attitudes or your own expectations on the quality and quantity of the work you could do, were capable of doing?

FAUST: Well, I don't think that the actual awarding of that prize changed my perspective on what I could do. I had already established my own set of goals as to what I could do--or my productivity level--and I don't think that that was changed by just the awarding of the Nobel Prize to people I had formerly worked with.

MAESTREJUAN: Well, in terms of other people's eyes, do you think it changed your worth?

FAUST: Oh, absolutely. [laughs] You know, it opened doors for me here. It's still opening doors, doors that I can't-- It opened the Pew [Scholars Program in the Biomedical Sciences] door, and that was a hard door to walk through. I'm not sure I'm all the way through it, either. But there's no question. It's still opening doors.

MAESTREJUAN: So do you think you are capable of doing Nobel Prize-attention-garnering work?

FAUST: Yes, I do: attention-garnering work. I don't think I'm capable of doing Nobel Prize-awarding work. That requires a lot more skill and discipline and time than I've got left, so to speak. But that doesn't bother me either. I think that I am making contributions. I wish it was a little less painful at times, though, but I'm enjoying it for the most part.

You know, I touched on that a little bit and maybe we could talk about it a little bit more right now-- In a lot of ways, that's been hard for me, to live up to other people's expectations. Because I worked with Joe [Joseph L. Goldstein] and Mike [Michael S. Brown] for so long and was more or less from the start all the way through-- That it's opened doors, still has continued to open doors, that I don't feel comfortable about entering. It's not that I don't want to go in there, but if you go in that room, there are some responsibilities and some expectations that are difficult to meet. So just because the door is open doesn't mean you always should walk in there. But you never know what's in there if you don't have that opportunity to look in. So being associated with them has given me the opportunity to at least look inside and go in occasionally. But I'm limiting the amount of doors that I'm walking through now.

MAESTREJUAN: Why is that?

FAUST: Because I can't meet the expectations and the responsibilities that come with being in those rooms. And you know, quite frankly, the Pew was a pretty heady room and I was not always-- We talked about this, I think, yesterday--perhaps it was off the record--but I didn't feel all that comfortable all the time, even in Cozumel on the sunny beaches. I think other people might respond differently and welcome the opportunity for these sort of challenges, but I've got a pretty good grasp of my limitations, and clearly my limitations limit me from doing some of the things that are available.

MAESTREJUAN: And what are your limitations?

FAUST: Well, scientifically-- Once again, if you think about the contributions that Joe and Mike made from defining a disease that was caused by an inherited single gene all the way to where we are now, into picking apart cholesterol regulation at the cellular level, then it has followed a discrete and linear progression. You make an observation, and you ask the next relevant question. And then you answer that question and then that leads to the next logical question along the line. And productive science follows that same logical, step-by-step process. But I've gotten out of that mode of doing science and become more fragmented in going off on tangents, I guess, and trying to just satisfy my curiosity about anything that looks like it's unique and answerable, hopefully, in biology, rather than follow logical progressions.

So fragmentation, I think, is a weak point now. I'm trying to get back into a mode-- I mean, if you look at my CV, you can see that I got off on a lot of other--although they're kind of related to cholesterol metabolism and whatever, although I'm not really dealing with cholesterol metabolism now--but these tangential projects. And part of that was just so I wouldn't be in competition with them and then could do things on my own, because I want to stay away from that. But the other part is that they just held my interest longer. So I need to come back into that.

And then secondly, I don't have the discipline that I think it takes to allocate time and to be firm in the commitment that you apply to any kind of project, whether it's in the office or in the lab or whatever. I just kind of lost this discipline that I used to have to get things done. And that's something I'm really groping with these days. It just seems like it takes forever to accomplish really simple tasks on a day-to-day basis, like writing a letter or organizing a lecture. I'm really struggling with that right now, and I think it's just from, as I pointed out, from a lack of discipline, just sitting down and doing it. You know, I said, "Okay, I'm going to write letters from eight [A.M.] to nine thirty [A.M.] or whatever, and if I don't get it done, then forget it." And I didn't think I'd ever lose that because that was something I picked up from athletics and stuff. You know, from the constant drills and performing repetitive things and understanding that it was important to be disciplined, because when the time came to do something important, then you would have the capability and the internal fortitude, or whatever it takes, to do that. So struggling with that.

MAESTREJUAN: Well, does that mean a scientist, to be productive and constantly making discoveries and innovations all the time, has to lead some cloistered life and that there's science and pretty much nothing else? I guess what I'm asking is, what does this say about science when a person must maintain rigid disciplines over their activities in order to be successful?

FAUST: Well, I think that's true in science and probably true in a lot of other fields, that those individuals who are disciplined in the time they spend at tasks and getting things done as efficiently as possible--be it mundane chores so that they can relieve time to focus attention on the really important things, like an experiment-- So for me, mundane things would be writing a letter or reviewing a manuscript or whatever so I could get back into the more important things, which would be the lab. I think that those of us in the profession who are highly disciplined stand a greater likelihood of being successful than those of us who aren't. Just like that in the funding environment that we're in--not only financial environment but also the attitude of study sections--this logical progression through an experimental tack is the way to go, rather than freelancing observations. I think that time is something of the past: that you could just get an idea and then run off and start a small project and make an observation and publish your paper. That's not going to get funding, I don't care if it does become Nobel-prize-award-winning experiments. It's just not going to survive in the thinking of study sections these days. Now, I guess if you don't have to rely upon study sections for funding, then that's okay.

But really, to be successful in funding, you have to accomplish the goals that you tell the study section in the previous granting period that you were going to do, because they're going to look at that old grant and say, "Well, here you said that you were going to clone A, you were going to make an antibody against B, and then you were going to overexpress C." So three years later, they're going to say, "So what happened when you cloned A? What did you use the B antibody for?" And it used to be that as long as you said you were going to do A, B, and C, but three years later if you didn't do A, B, and C, but you had six other publications on something else related, then that's okay. But I don't think that's true these days.

MAESTREJUAN: Well, again, I'll ask in a different way, what does that mean for science, because you have taken an unorthodox approach to at least a career in science, and yet you come up with incredible contributions to the scientific canon. And what happens to science when you can't take unorthodox approaches and freelance and do phenomenal work, but must take a safe route?

FAUST: Well, number one, it could eliminate some of the people that potentially can be successful in this profession because they're just going to realize, like I do, that fragmented science is not going to lead to funding. So number one, either they'll drop out, or number two, they'll struggle along and not be successful. But those creative, fragmented people won't have the support to potentially make really important observations. Now, that's not saying the

observations won't get made, because clearly they'll probably get made, and it won't say that they won't get made any later in time. They may come about in literature at about the same speed, but they'll come from this other process rather than freelancing. So it limits the arena in which science is done and perhaps also limits those individuals who will do it. And that's about all I can say about it. I don't know that it says it will limit the amount of observations that we make or the time that they come forth.

It's sure a lot more boring, though. [laughs] It is for me, and you can see it in people I know. Oftentimes you can really capture-- You can see imagination in colleagues' faces by talking about something that's new and exciting and unique, not connected with their field. And you can read in their mind--or you see in their mind--that they would like to be able to do something in this field to be part of it. But you know, they are in step to another dance.

And the Pew is wonderful in their lack of strings associated with their money, that they can allow people like me to be fragmented: contribute to my demise, more or less. And that's great, because I think the Pew is saying that we realize that there's a certain amount of knowledge that comes from just pure creativity and oftentimes just the pursuit of something that's not exactly part of a logical progression, and that there have been important discoveries made in a more freelance style. And that it's important to contribute to that thinking and to give investigators the opportunity to do one or two extra experiments to potentially come up with something really big that could open up whole new areas of thought and experimentation. So we want to support this, and that's great. So there is effort by agencies like the Pew Charitable Trust to keep that up.

MAESTREJUAN: Do you think your talent and abilities and productivity would be better cultivated and utilized in a different environment, in a world in which fragmented science can be funded or in ways in which funding doesn't come from study sections?

FAUST: I think I 'd probably function better. But I 'm probably in the minority. It's probably a good thing that most of the funding that goes to people, the majority of them, should be more focused on or directed towards the majority's way of doing science.

MAESTREJUAN: And what is it about you that makes you function better in this? By taking unorthodox approaches to topics and fields?

FAUST: I don't know. I don't know. Maybe it's just a different sort of curiosity. I mean, it's certainly-- You could take a lot of personal satisfaction in setting goals, specific aims, and then accomplishing those as you go along, because there's something you can point to and say, "This is what I said I was going to do and this is what I did." But to me, it's just as rewarding to kind of wander around and get an idea and think of a way to address that idea and do it and then move on. It sounds like another example of undisciplined actions.

MAESTREJUAN: Well, why do you think Goldstein supported [H. Ross] Perot? Or was it Brown that made a public announcement of Perot?

FAUST: Well, number one, I wasn't there, so I'm not sure. This is all hearsay. I guess if he did, he felt that he was the best presidential candidate at the time. I really never knew much about them personally. We didn't socialize at all. There was a holiday party every year, and oftentimes it was over at Mike's house. That's the only time I ever went to their house. So I don't know his political views. Our relationship was about 90 percent professional. I know even less about Joe. So I don't know why Mike would say that unless-- I can only assume that he felt that he was the best presidential candidate at the time.

MAESTREJUAN: Well, do you think the way the Pew funds science--or perhaps Perot's solution to funding science or even presidential campaigns--is to pour money in certain directions--? Maybe the better way is to have study sections--federally funded granting agencies through funding sections--decide who gets what. I guess what I'm leading to is, how do you think we can structure scientific funding or support for science in a way that will allow creative people to do creative things?

FAUST: Well, I think we're doing a pretty good job of it now, because there are things like the [Howard] Hughes [Medical Institute] and Perot and then other benefactors similar to Perot, perhaps not at that level, that will recognize individuals that have a greater opportunity to really move forward and to move back the frontiers of ignorance and to move forward in science. And so it's a better bet to place money in these people's hands and for society to get rewards than it would be just based on-- Once again, their past performance and credentials [indicate] that these people are a better bet to do that than others in our profession. So I think Perot does a good thing by giving a lot of money to Joe and Mike for their operation, and then also giving money to, say, [University of Texas Southwestern Medical Center at Dallas] Southwestern Medical School to try to continue to have a constant supply of potentially creative people, or people who might fit into this category that we would want to more or less give money to freely. That has to be in the works; we just can't stop.

At the same time, the rest of us will make contributions, but we're not as easily identifiable as this initial group. And because the majority of us will fall into that category--and then there's only a limited amount of money there too--there needs to be a way to give that out on a fair, equitable basis. Following directions from a study section is as good as anything, I think, because these are our skilled people in each individual field who have knowledge about accomplishing the specific gains that are being proposed and can look and see if the investigator has a certain amount of track record and credentials to do this. I guess what I'm saying is that yes, there should be a balance between more or less no-strings-attached money and strings-attached money, and we're probably doing a good job of it.

Where there could be room for improvement, I think, is in the amount of money that's available to each of these categories and then perhaps in the number of investigators in the latter category, that we would be more efficient if we had, say, more money available for everybody, and then also [would be] more efficient at spending that money if we weren't giving it to some projects that were not as feasible or the investigators were less likely to get a return on it.

So I guess what I'm saying is I think we have too many of us out there working now in laboratories and applying for grants, and that's come about because there were too many Ph.D.'s coming out and too many postdocs followed that. And it came out at a time when NIH [National Institutes of Health] funding was very, very high, and we just didn't see the downside of that when NIH funding got very, very low. But now it's back up again, so whatever. And I think unquestionably there are too many independent researchers vying for a great deal of money, but when you have to divide it up among so many laboratories, then it limits what each laboratory can do.

But with the Browns and the Goldsteins and the Hughes investigators and Perot's funding, then I think that's a pretty good arrangement. We have two Hughes investigators on this campus. They're the best investigators that we have. There's no question about it. There's a great opportunity for Ralph [R.] Isberg to make considerable contributions to understanding cell biology on a procaryotic level.

MAESTREJUAN: Given your own experience with different levels of hierarchy or different hierarchy spots you've had within a laboratory, is there a better way to organize this chain of command? You've mentioned that as a researcher's associate in a relatively small lab, you had some independence, and then by the time you left postdocs were being attracted just for the kinds of skills that they could bring into the lab, so it shifted from bringing technicians in as contributing to skills to postdocs--not even graduate students but postdocs--with these kind of skills to contribute. Can we change the way we organize research tech[nician]s, graduate students, postdocs, PI [principal investigator]s, even within the laboratory itself, to better utilize funds and personnel resources?

FAUST: I guess there's a lot of discussion here on our campus about dissolving independent labs and forming more integrated groups of laboratories now, that are fused together to accomplish common goals in research. A lot of that exists anyway. Even in my department, I have a grant to study a genetic disease, and [James Fred] "Paulo" Dice has another grant to study the same disease. We communicate and collaborate quite a bit, but we're not working in the same lab on the same project. So restructuring away from the individual investigator with his own set of graduate students and his own postdocs and his own technicians and kind of fusing areas of common interest between investigators, to where there would be a pool of postdocs and a pool of graduate students that are more or less working on a common goal, is one way I think that things could change or that is being proposed as altering the structure of laboratory science or how its personnel are handled.

Once again, drawing upon the experiences that I witnessed in Dallas, I think Brown and Goldstein, as a unit themselves, not just the two of them, are much stronger and much more productive than each one of them independently, even if they were working together. In other words, the sum of the parts is less than the aggregate, in that case. And I think that can be applied in a lot of cases to investigators. Now, I personally would like to get into that sort of relationship, and I'm trying-- And then I have a very strong collaboration with a fine investigator here, that he and I work very closely together on one particular project in my research realm. We do a good job of it, I think. And we go forward, I think, much quicker than we would if he was doing something on his own and I was doing something on my own and we just talked on the phone.

So it seems that if we had more people-- And that, once again, gets away from fusing labs together. It's just saying that you and I make a commitment that you and I are going to study this one project together and that we're going to talk about experiments together and we're going to share technicians or a technician or a postdoc and we're going to have a joint grant--not a principal investigator and a co-investigator, but really, two principal investigators on this grant--and share the expense. And then we're able to bounce ideas off each other and criticize each other's thoughts and interpretations in a meaningful fashion without egos getting into it and all this, that, and the other--which, as I realize, is pretty difficult.

But I think that we could really go if more of us did something like that, at least on a part-time basis, like Rod [Roderick T.] Bronson and I do now, that our profession will advance quicker. So that, once again, kind of breaks down those walls between individual departments, in which occasionally you'll talk to a colleague at a meeting or on the phone or whatever. It's more unifying than just calling it a collaboration in which I'm doing an experiment here and then he's doing an experiment here and then we just communicate. I mean, we're actually doing these things together.

MAESTREJUAN: Okay. Let me pause the tape for a second.

FAUST: All right. [tape recorder off]

MAESTREJUAN: Okay. Well, we'll continue more with these questions tomorrow. We may talk about your own funding career. But when we last talked about chronology, you had just started with [E.I.] Du Pont [de Nemours and Company]. You had left the Brown and Goldstein lab and started with Du Pont. And so were they making "a better tomorrow through chemistry"?

FAUST: Well, they made "a better tomorrow through chemistry," but [laughs] I'm not convinced of that. And they may make "a better tomorrow through biology and biotechnology," but they're not going to do it at the rate that they thought they were. My read is that the amount

of money that was needed to fulfill this commitment, or to project the potential for this life science program to bring rewards to Du Pont, was much greater than what they anticipated. And maybe that coupled with the fact that their resources, due to economic conditions, were projected not to be as great as they originally thought—

So the concept of having this free-standing institute began to crumble. The institute didn't crumble, but just the ideas to go forward with it-- Well, to maintain it. It was completed. The building was built. It was staffed. But to maintain it as really just a free-standing institute funded by Du Pont money without any apparent return on that money became uncomfortable, or made the board of directors uncomfortable. So there were some changes in leadership that began to shift the focus from, say, a hundred percent basic research to trying to incorporate some of the applied projects that were going on outside the walls, bring them inside the walls. And some of the last commitments that were to be made in completing this institution were dropped, financial commitments and resources.

So that became apparent to me or that was my conclusion, that it wasn't going to fulfill the expectations that were originally projected and that it became conceivable that perhaps we would just become absorbed into the rest of the mainstream of Du Pont, maybe not at the level of their chemical industry, but maybe in the form of a biotech company or whatever. That didn't sit well because I felt like that would jeopardize my ability to fulfill some of the aspirations I had for myself in terms of being able to succeed as an independent investigator and to make contributions on my own. There would be a limited amount of time and space and resources to do those things that I wanted to do. There would be plenty of time and space to do things that somebody else wanted to do, which could be interesting, but it wasn't exactly in my own aspirations at that time.

And it was about that time that I realized that the only way that I could get into a somewhat secure situation to try and fulfill these aspirations was to become a principal investigator at a real academic institution, one where I had control over what I did, albeit through study sections and the like, but that seemed like it was a hard fact of the profession at the time. That gave me the first realization that it might be a good idea to get a Ph.D., because if I was going to continue to pursue my goals-- It looked like the life science at Du Pont was going to close the door on that, and that there weren't any other Du Ponts opening up life science categories around. So the only place to do that would be at an academic institution or a basic science research institution or whatever. And in order to do that, you had to have a Ph.D. So I began to look into that.

I really didn't think about going back to Texas, not that I wouldn't want to, but just I thought that it wouldn't be appropriate to go back down to Southwestern. I did look at University of Texas at Austin. I wrote off for some information. I never did apply. And then [I] also looked into a Ph.D. program at the University of Delaware because that was close to Wilmington, and then there was some financial incentive from Du Pont to do that. But the better situation arose here at Tufts [University School of Medicine], and that came about primarily because I'd developed a relationship with a former postdoc in Dallas, Laura Liscum, who had recently become an assistant professor in the Department of Physiology here. So I was familiar

with the structure of this faculty through some social interactions, and they were familiar with me. And then the chairman, Irwin [M.] Arias, was included in that group--that circle. So when I approached them about possibly getting a Ph.D. in their department, then they were very enthusiastic and excited and glad to have me consider it even, to the point that they gave me a lot of encouragement and even lowered some of the academic requirements that had to be met, which was something that I thought would be really great, if I could get some credit for experience, on-the-job training. And so they all were very supportive of that. It was a program within the context of the [Tufts University] Sackler School [of Graduate Biomedical Sciences].

It set its own requirements, as do all the programs in the Sackler, so we all don't have the same number of didactic courses that students have to take. And then of course, a Ph.D. dissertation is as different between programs as it is within laboratories sometimes. And then I knew the people here. And then one of these little tangents that I was working on was something that Laura had picked up on. So we were scientifically collaborating on that and then it was going very well. It was appealing to come here and go off and spend time on my own Ph.D. research or whatever I wanted to do, but at the same time be able to intellectually participate in her project--and then also experimentally participate. So all the things were just right for me to come to Tufts as opposed to these other places, in spite of the fact that it was in Boston. I've grown to like it. So it was just one of those things that if I wanted to continue to follow the trail that I aspired to go on, that this was the best way to do it, was to go to the Tufts physiology [department] for a graduate degree. It was the fastest. It was the cheapest. It was potentially the most rewarding and satisfying and intellectually stimulating. So that was a no-brainer too, just like the difference between Du Pont versus Upjohn [Company] and Merck [and Company]. And once again, as I was with falling into the hands of Joe and Mike, I've been very fortunate to be in this department because they're very supportive and they've helped me enormously. The doors they open are a little bit easier to walk through. [laughs]

MAESTREJUAN: And why is that?

FAUST: Well, I fit into those rooms a little bit better. Maybe it's got something to do with the size of the doors or the people that are in the rooms, I don't know. But I've never felt uncomfortable fulfilling their expectations--or trying to do that.

[END OF TAPE 4, SIDE 1]

MAESTREJUAN: Okay, well, to go back to Du Pont a little bit, what was the expectation there in terms of the quality and quantity of work to be done? I know that you do publish several papers from your time.

FAUST: Well, I think the expectations were more or less the same as it was in an academic

setting. Jim [James L.] Gaylor was our supervisor. He was in charge of the group-- cholesterol and membrane biology--and he told us many times that "We want you just to do good work and make contributions to the fields; be interactive with the rest of the groups in life sciences--neurosciences and immunology--just learn a little bit about what each one was doing, maybe make a contribution on the side, or something like, to where we have a cohesive sort of group: kind of like three separate departments." There were only six principal investigators in the group that I was in, and I think there were eight in neuroscience and ten or eleven in immunology, so it wasn't that large. It was like three small departments. Let's put it that way.

But he was very supportive in terms of supplying money for equipment and supplies and technicians. We each had a full-time technician. We could attract a postdoc; maybe money would be available for that. Resources that were part of the Experimental Station were available to us, in terms of DNA sequencing, protein sequencing, and at that time there wasn't automated DNA sequencing, but synthesizing probes in protein in peptides, a monoclonal antibody facility. [These are] things that were mostly found at larger institutions, but we had them there at Du Pont, which helped us and our technician and other people focus attention on doing the actual lab work without having to create reagents or some of the more service-oriented work. But it was still so small-- Each laboratory was small, you know, like there was me and a technician and-- I don't know, I had half of a postdoc that I shared with another guy, but it was on a collaborative project. Then actually, I shared a technician too with that other guy. I had one and a half technicians. So it was fun.

And the level of science investigators there was good. Harry [W.] Chen was an established investigator in cholesterol metabolism, had been at Jackson [Laboratory] in Bar Harbor. He too came to Du Pont. So here was someone who at one time you'd consider more of a competitor with Joe and Mike--although there are no competitors. [laughs] I guess for some people there potentially was some animosity, but I never detected that at all with Harry. And because he and I had been in common arenas, then we got along very well. I don't think we collaborated on anything, but we were very beneficial for each other in terms of ideas and experimental results and the like. Jim [James M.] Trzaskos, an established investigator-- He's still there. Jeff [Jeffrey T.] Billheimer, another cholesterol person, working more on cholesterol esterification, was an established person in the field before he went to Du Pont, and he's still there. I think the others have left.

In the meantime, Du Pont now has merged-- Or that life science group now has become a biotech company, and then that Du Pont biotech company has merged with Merck to form this third entity called Du Pont-Merck [Pharmaceutical Company], which on paper is separate from Du Pont, although the building's right there in the Experimental Station. So there's Du Pont, then there's this building--it's called Du Pont-Merck--and then there's all of Merck over here. And I think that this arrangement was going to last for about five years and then both of the giants would figure what they wanted to do with this other one. I think that's probably being discussed right now, but it's a pharmaceutical company at this time and they're screening drugs and looking for agents and all. But it was a rewarding experience and I enjoyed it. It's just that I thought that there was limited potential to continue along that. And I was right.

MAESTREJUAN: Well, when you took the job at Southwestern, it was a job-- You wanted to go into biochemistry and here was a position. You really didn't have that much of an idea of what the research being done in the lab was. And now you're choosing to go into a very clinically oriented cardiovascular unit at Du Pont.

FAUST: Right.

MAESTREJUAN: Well, not clinically oriented, but there are clear clinical applications and this is an industrial environment. How much has the clinical side of questions factored into the kinds of decisions you make on research programs and research questions?

FAUST: First of all let me just say that "cardiovascular" might have been part of the title, but as we were told, there weren't any pushes to get us to be more clinically oriented or drug oriented or so. So to answer your question, I don't think a whole lot that I've been influenced by whether a particular topic was clinically relevant or part of an important disease process or public health issue, whatever. I think I said this yesterday. It feels good to think and say that contributions that I make from my laboratory work do help, may help, have helped people in suffering and disease. But that's about the extent of my feeling about it. It just feels good to say that. I'm glad I did it, but I don't look for projects that are focused on disease.

I have two major projects in the lab. One of them now is focused on a disease because that's just where my interest lies, and the other is completely basic. There's not anything applied connected with it. I tell study sections that just because there's not anything now doesn't mean in six months we're not going to make some discovery which will open it up and make it more applied, because that's what they want to hear. But it's just wide open, and I think that just the pursuit of knowledge is enough for me.

I can't stress how important it is, how satisfying it is, for me to know at some point in the future that I went to a laboratory and set it up and established research projects and made contributions based on my skills, and not only skills in the laboratory, but skills in interacting with the people who were working with me and in writing grants and in leading experimentation. And whether that's a project that involves studying a transfer RNA modification in bacteria or a genetic disease that causes neurodegen-eration is not that important.

MAESTREJUAN: And how did becoming a PI at Du Pont, after being a research associate, change the kinds of questions you asked and how you conducted your science, how you did the research?

FAUST: I tried to pattern a lot of what I did in the types of experiments--or the philosophy of approaching a problem--after Joe and Mike, even to the point of setting up a lab on the same sort of framework that they had set up. I think it would be stupid not to. But at the same time, I stayed away from the questions and the topics that they were interested in pursuing. They were interested in the LDL [low density lipoprotein] receptor. So I said, "Well, I'm not going to do anything with the LDL receptor." They were also interested in studying HMG CoA [3-hydroxy-3-methylglutaryl-CoA] reductase, which is the key enzyme in cholesterol biosynthesis. Well, I still had a lot of interest in that, but I tried to suppress that interest because I knew that there wasn't any reason to-- Or unless I could come up with a real foothold on something that gave me a really good head start, then I wouldn't do it. And that's in spite of the fact that there were other people in this group that were studying molecular regulation of HMG CoA reductase. It would have been easy for me to get involved with their projects, but I just kind of stayed away from it.

So the things that I did pursue were more, once again, on the circumference of the field of cholesterol metabolism, that being things like uptake of mevalonic acid in the cells or this new genetic disease that imparts impaired cholesterol movement, that was apart from the LDL receptor or LDL regulation. It was more of a cell biological dilemma. How does the cell move cholesterol around in the cell, not what does it do when it gets there or how is regulation accomplished. Just, how does it travel about? It's an insoluble molecule, so it doesn't freely diffuse like other things. So that's the project that Laura and I initiated together and that she's still working on.

So to be able to use my knowledge that I learned about cholesterol metabolism--and the experiments that could be done and the techniques that were involved in studying cholesterol metabolism from the years in Dallas, but applying it to these other projects--was really beneficial. In a lot of respects, it's just doing the same experiments, the same assays; that's kind of plugging it into a different system. So [I was] still drawing upon that core of knowledge, but yet doing something completely different rather than just calving off a part of a project that they had and start doing it. These are things that I devised on my own.

What's common is lots of times--and it doesn't happen all the time, and I don't even know at what frequency--is that postdocs, when they leave a lab, will take part of a project with that lab. We call it calving off, like an iceberg, you know? That the principal investigator will say, "Okay, well, I'm no longer interested in this part of what you were doing. You can take it all." Sometimes they don't happen that way. But I devised my own projects. I knew I was leaving for several months before, and so I began to think about things that I wanted to do.

And there were a couple-- Oftentimes--this is another tangent here--but oftentimes when we're working around, you hear about an observation or you know someone who's studying something that's made some initial observation that you could follow up on easily, that they may not have the skills to do or the technology or the manpower, but you could do it, or I could do it with what I knew, in the assays that I knew. And it's nice to have that opportunity to really do those--part of this fragmented philosophy--and that was a good time to do that.

So I had three projects that I took to Du Pont that I could fit into. One of them came from

an observation that Monty Krieger at MIT [Massachusetts Institute of Technology] had made after he left us. He was a postdoc. We worked in Dallas for a long time, a good friend of mine. And he had made this observation-- We had talked about it, and I said, "Well, that's something that I could probably follow up on. There may be something really informative there." And sure enough, I did. And a second was originally this mutation in mice that caused this phenotype that imparted impaired cholesterol transport in cells. So that was something that I could plug my skills and assays into. And that blossomed into this field Laura's working with now. And the third one is this transfer RNA that I started a long time ago--actually, if anything, that was a calving off of something that I did about two-thirds of the way through my tenure with Joe and Mike--was just to show that this modification existed in eucaryotic cells as it did in bacteria, but the function for it is still unknown. So I just began to study it from a standpoint of an antibody. I made my first antibodies to that back in Du Pont and carried them and [I am] still doing that.

MAESTREJUAN: How motivated were you while you were at Du Pont to seek collaborations, either with other Du Pont-like institutions or to make contacts with the academic community?

FAUST: I didn't seek a lot of collaborations. I thought I had some, anyway. I was starting up a collaboration with Laura-- Although Monty and I were talking quite a bit, because he's a pretty interactive fellow. His lab wasn't doing any work on this little project that I was working on, but he still was actively involved in it intellectually. I kind of stayed away from the collaborations at Du Pont because I just-- Number one, they weren't appealing, and some of them were in direct competition with Joe and Mike, which I thought was a big mistake. So I guess I had all the collaborations I wanted.

But in the first couple of years I was-- All the time I was at Du Pont, I went to Dallas about every two or three months, and I went to the medical school and talked to them just to bounce ideas off of them and tell them what I was doing. I really wasn't that keen to know what they were doing, but I wanted to have someone that I could share my experiences with and get some input on what to do next and [see] if there were any new developments in the field that might have bearing on the things that I was doing. And they were very supportive at that time, as they've always been. I don't go down there every two or three months anymore. I kind of got gradually weaned away from that. In some ways, I wish I would, but I haven't got time to go down there every two months or so.

So the only academic contacts I had-- Well, that, and then the academic contacts began with the Tufts physiology [department], but that was more social at that time. It wasn't scientific discussion. I knew what their projects were, but I didn't ask them for advice or they didn't ask me for advice. It was more just kind of cocktail party language. But back and forth to Dallas was a real benefit for me in those months at Du Pont.

MAESTREJUAN: When you entered the program at Tufts to get your Ph.D., were you already set on the research you were going to ask? How much flexibility were they going to allow you

to continue research that you'd been thinking about even before you went to Du Pont and then continued to carry out at Du Pont?

FAUST: Fortunately I identified a lab that-- Fred Dice, "Paulo" Dice, who was secure in his own position--and financially and intellectually and egotistically--that he would allow me to bring in a project that was unrelated to his work and do it as a Ph.D. dissertation. I turned down the opportunity to go to another lab that's more renowned but would have not given me that flexibility, just because of the fact that I preferred to think that I was more motivated to do the things that I was interested in than what somebody else was initially interested in. But knowing that I could get interested in something else, but initially I was motivated to do this. So Paulo was a real lucky find again. And [he] still is supported as a senior member of our department.

And it still gave me the opportunity to continue the intellectual collaboration with Laura in Niemann-Pick [type] C, which is, in a way, pursuing my own ideas, because I had a good framework of what I thought was going on in this disease and she shared the same framework, so through her experiments--or her lab's experiments--[I] fulfilled that drive.

MAESTREJUAN: How did you adapt to going from a person who had been in school for a long time and a PI at Du Pont, with, I imagine, a hefty salary, to now integrating yourself back into the hierarchy of the lab?

FAUST: Integrating in the lab wasn't a problem because I just started to work. When I was at Du Pont I worked in the lab just like I worked in the lab when I was a technician. I mean, that was my job. So it wasn't that much different to go into graduate school and take a few courses but still just continue to work in the lab. But there were a lot of limitations in the money that was available to me. I mean, I was doing this project on the side in somebody else's lab that wasn't really funded to do that, so there wasn't much money to fund it, you know. I had to make all my own reagents and get by as cheaply as possible. And that was frustrating and oftentimes discouraging. And then not having anybody to help me, i.e., a technician, which I was used to at Du Pont, or services that were available at Du Pont or in Dallas to get things done, slowed down the process and led to a discouraged attitude.

So I didn't factor those into this, I must admit, that it would take me as long to get a thesis as [it did]. But that was just a miscalculation, I guess. It's still worth it, I guess. It didn't bother me that being a student-- I thought that it would. I mean, I thought that my classroom skills were not that good when I was a full-time student, and being out of class for so long, I said, "This could be a problem," you know, studying and remembering the proper techniques. But fortunately, the course requirements were less for me than they were for other students, and then the courses I did take covered a lot of contemporary biology that I learned as it happened. [laughs] And oftentimes, we would recall that stuff better if we were part of the process. [laughs] So there were two core courses that our program teaches, and they were--membrane biology and molecular physiology--right down my line. So that was no problem at all.

Money-- You know, yeah. My graduate stipend was far less than my salary at Du Pont, but I had managed to save quite a bit of money because I was making good money, even in Dallas. By that time, I was making more money than I was spending and my expenses were low. When I got to Du Pont, then I clearly made more money than I knew what to do with, so I saved quite a bit of that. That got me a nest egg. Then I'd bought and sold some property and built up quite a bit of equity that I could take to-- Actually, I'd bought and sold twice in Texas and then once in Wilmington, so I had brought enough money even to this housing market to put a down payment on a house. And then [this] coincided with the time that Laura and I started sharing our lives together in a common domain. So she was able to make the payments after I made the down payment, and we're still in that same-- We're not in that same situation. I'm contributing a little bit more to the payments now, but we're still in that same house. But I still had to watch money quite a bit, so [I] cut back on some of the things that I was used to. But [that] probably was a good idea anyway. And now I'm back making more money than I can spend. I don't have any financial responsibilities to speak of, so if you don't have kids that you spend $30,000 a year to go to Tufts—

MAESTREJUAN: Right. So you felt like the knowledge you were picking up as you went and learned in your different jobs, careers, kept up with the textbook knowledge, then?

FAUST: I never realized that until I took that course. I never knew how universal the knowledge I got from that experience is to cell biology and cell physiology, how the concepts that emerge from that can be applied to a whole number of pathways and processes within cells. I just focused it on--with respect to cholesterol metabolism and membrane biology-- One is that ligands and receptors were-- There were other systems around too, so I guess that's not entirely true. But it's sort of like studying a cell by focusing on one particular aspect of the cell, that gradually you just learn about the whole workings of the cell.

You know, if the carburetor in a car breaks down, then you begin to fix the carburetor. Well then, to understand how it works, you've got to know what feeds into the carburetor and what the carburetor does. It provides a gas-air mixture to cylinders to be combusted. Well then, you thought, what do cylinders do, and how does combustion occur and in what--cylinders--? This motion goes up and down that causes something like this to turn on the wheels. So it's just to understand globally what's going on in a process by just beginning to focus on one particular aspect, and I just didn't realize how dimen-sionless that is until I took this course.

MAESTREJUAN: Well, what about your fellow graduate students and your interactions with them and fitting back into the graduate student lifestyle? You're a little bit older than they are. You've had a tremendous amount more experience. You've been an independent researcher. And a lot of these people are probably just out of their undergraduate. And so how did that work out?

FAUST: Well, I think that worked very well. I admired them because they were learning the same things that I knew but learning it much earlier. And I think that they admired me because of what I had accomplished, and in just going back to school. So there was a mutual admiration there—This is an urban campus. There are no dorms here, so it's like most graduate schools: there's not a whole lot of interaction between students outside of these walls. So I wasn't alienated in that respect, because everybody was sort of apart. We all went different directions when we went home. And then, we did do things as students together, well, male faculty, postdocs, and students. We used to play basketball together-- Let's just call it the "Chinese YMCA" [Young Men's Christian Association], on the other side of that building right there. It's pretty rustic, but we would go over and play basketball every Friday, and bad knees and all, I used to go over there and play that--and stopped. But I think that that helped them accept me, so it was a good relationship.

And then I think maybe that has helped-- And then, I've always been around younger people anyway mainly because of this football officiating I've done. I started in Texas and with high school games in college, and so it's the same age level, although I'm getting older, but I'm still associated with the same kind of age group of kids every year for a three-month period of time, so I can somewhat relate to them. I think that helps me to fit in with younger groups better.

It certainly has helped me in becoming adviser and counselor to these new students. That's why they made me one, because I'm closer to them, because I just went through that more recently. I kind of interact with students, as opposed to the older faculty, who are a little bit more removed or so. So all in all, I was well accepted and thoroughly enjoyed the relationship I had with my classmates, and I think they did enjoy my presence as well.

MAESTREJUAN: Okay, and to go back to what we talked about yesterday in terms of the biochemist coming from a chemical background, how did you see your--? What were the backgrounds of these graduate students coming in, in terms of the fields of their education? By this time programs are changing in terms of discipline, so you could major in molecular biology or genetics. What does genetics mean as opposed to molecular biology, biochemistry--? How did you see your background meshing with theirs? You had said that it's easier to teach a chemist biology than a biologist chemistry.

FAUST: Well, there are no chemistry majors applying to our graduate program. [laughs] No. I mean, I see every application that has come through in-- Well, ever since I've been appointed to this faculty. So there are no chemistry majors. We've had a couple of engineering-- Well, we have one engineering student now, but he was more biomedical engineering. But all of them have been biology. But you're right; it's no longer just pure biology. It's either molecular biology or genetics or biochemistry, just a focused degree in biochemistry.

I don't think they're any less qualified, because-- Well, I guess because knowledge is so specialized these days that you almost need to have a specialized degree--whether it's molecular biology--to advance. The extension of that is that-- It used to be that graduate students would,

say, do one project for their Ph.D. dissertation and then just go off and do a postdoc on a completely different field. And then maybe if they got a faculty appointment, they might take a little bit of that knowledge or a little bit of that field with them. But now the going philosophy is that graduate students who do a dissertation in, say, signal transduction want to do a postdoc in signal transduction and then--obviously when they get a faculty position--write grants on signal transduction.

But backing that up a little bit further, if they have a degree in, say, genetics-- Well, that might not fit well into a signal-transduction-type pathway, but molecular biology or cloning might be a better fit. So I don't know if students are trying to-- When I read applications, too, I get the feeling that students are deciding what areas they're really most keenly interested in studying, and deciding that they want to study that as a graduate student then as a postdoc and wherever else, rather than saying, I just want to go to school and learn and something's going to open up and the best laboratory-- Or one of those faculty members up there is going to have a really neat project that's going to light my imagination, and I'll study it for a dissertation and then I'll see what opens up after that." So that's the mind-set, I think, of most students these days. They just start tracking at a much earlier age. And [that] probably puts them in a better position, because maybe they've got this history of being part of this field and not moving about.

MAESTREJUAN: Well, I think we've covered a lot of territory today, so thanks very much.

[END OF TAPE 4, SIDE 2]

[END OF INTERVIEW]

INTERVIEWEE: **Jerry R. Faust**

INTERVIEWER: **Andrea R. Maestrejuan**

LOCATION: **Tufts University**
Medford, Massachusetts

DATE: **20 February 1997**

MAESTREJUAN: I wanted to start off with some things you said yesterday and also in the first session about your own attitudes living through the Vietnam War period. Learning and being involved in chemistry at a professional level, how did you feel and how do you feel about the role of the chemical companies during the Vietnam period? We talked of a joke yesterday that [E.I.] Du Pont [de Nemours and Company] did make "life better through chemistry," but then there was, during the time you grew up, this big problem with Dow Chemical [Company]. And had that ever entered your mind as to what you were doing and maybe that whole generation of issues?

FAUST: I guess, quite frankly, I didn't think much about napalm as being a chemical product for some reason or another until later on. But I was becoming aware of the environmental onslaught that chemical companies were participating in, or not guarding against as well as they could have, especially oil refineries. I think I was a little bit more in tune with that because I was spending a lot of time outside and enjoyed camping and hiking and the like. I must admit that I never made the connection between some of the real destructive forces that chemical companies developed that were used in wartime at the time of the Vietnam War, when I was beginning to see that, well, not all chemistry is great, because they were polluting beyond their means or beyond their control, and that wasn't so hot. But obviously, it didn't affect me to the point that I didn't apply for jobs there.

However, when I went to Du Pont-- Now, Du Pont has a long history of making explosives, and there was a lot of discussion and concern about-- They weren't doing it at that time, of course, but they made big money at that for a century, and was that money tainted or our jobs--? Are we saying that that was good because we worked there? So that was an issue that was discussed occasionally, more at social gatherings or whatever. I didn't see any demonstrations or people threatening to quit because they realized that Du Pont made gun powder and fueled both sides of the revolutionary war. [laughs] It never got beyond that.

MAESTREJUAN: And then to go on to things that we did talk about yesterday, I wanted to ask what opportunities did you have to further your education while you were in the [Michael S.] Brown and [Joseph L.] Goldstein lab? Had you thought at that time to perhaps do course work in some formal way?

FAUST: Well, I never had any formal opportunities for education. I was able to attend a couple of meetings, and I actually presented at one meeting. But I didn't take any graduate classes or courses anywhere connected with the [University of Texas Southwestern] Medical Center [at Dallas] or other schools around. And as I mentioned, I think yesterday, Joe and Mike had always encouraged me to apply to graduate school at Southwestern, and I think I talked a couple of times to the Department of Biochemistry-- I knew some people there--just to see what the requirements were and what their feeling was about my potential application.

There wasn't a whole lot of enthusiasm from them, I suspect because I wasn't at the level of the students that they were getting applications from, in terms of GRE [Graduate Record Examination] and GPA [grade point average]. I'm sure that they would have given me some benefit or some upgrade because of my experience, but it wasn't, "Oh yeah, come on, let's go." And then I wasn't that gung ho to get back in the classroom anyway. I was having a good time in the laboratory and I still didn't think I needed to have a Ph.D. to continue to have a good time in the laboratory. So I didn't give it a lot of hard thought.

MAESTREJUAN: Right. You had also mentioned that when you moved to Du Pont--and you were separating from the Brown and Goldstein lab and you were trying to create a different ground for you--you didn't want to do what the Goldstein and Brown lab was doing. What were their feelings? How did they let you separate from--? Was there-- like with graduate students or postdocs--any kind of discussion over what you could take with you? Were they advising you in any way?

FAUST: No, there were absolutely none. They didn't offer to let me take some projects, and I didn't ask to take some projects, and up or through the time that I left, I never went to them and said, "Well, these are the projects that I'm going to do. What do you think?" Now, when I went back, I told them what I had been doing. Of course, it wasn't behind their back because it was something-- And then I'd say, "What do you think?" But I just wanted to get started in those and try it out first before I sought their advice. So the answer is that we never discussed what I was going to do when I left their lab, in any form.

MAESTREJUAN: And when you left Du Pont, how did the separation take place?

FAUST: I seem to remember that when I left, Du Pont was becoming more, say, biotech-like and then therefore more proprietary. I seem to remember a discussion with one of their management people who's part of an outgoing-- An orientation about what I was going to do when I left. Not wanting to give away anything, I probably was pretty nebulous, because I already knew more or less what I was going to do. I just didn't want to give them the opportunity to say, "No, you can't take that." I can't remember the details to the discussion, but

I just didn't want to give them any more information than I could. I think Jim [James L.] Gaylor-- I'm pretty sure that he knew what I was going to do. I probably told him. I had, more or less, his approval, but then he always sided with what our feelings were, was sympathetic to my feelings. I don't think he would have stood in the way anyway.

But I guess in retrospect, Du Pont, as a biotech entity, might have had some interest in some of the stuff that Laura [Liscum] and I have done since. I'd created a cell line, this mevalonic transporter cell line, but it turns out to be a very valuable reagent for studying isoprenylation, as it emerged shortly thereafter. So we got an enormous amount of requests to get that cell line out. So I left it with them. I don't know if they ever used it for anything, but that was potentially something that could be not only profitable, but provide a significant advantage to owners.

But I didn't take any other-- I mean, that was actually-- It was a cell line that was created in Monty Krieger's lab, but it was done jointly because of my idea to select for that cell line. So it wasn't created at Du Pont and they really didn't have--although it was characterized there--I don't think they have any right to patent it. I guess I didn't think that there were many other discussions between Du Pont and myself when I left. There was a discussion about the fact they wanted to have a blood sample from me.

MAESTREJUAN: Why was that? That's something—

FAUST: They had decided that they needed to have blood samples from employees at the time they started to work as well as when they left. That way, if in the future an employee became ill and tried to say that they contracted something, that can be measured in the blood that the company would have before and after, and they could say, "Well, yes--" Or a court could say, "Yes, it did occur"--say, like a virus--"in this employee during that period of time and not before or not after." Unfortunately, they hadn't started that policy when I started, so there wasn't any sample on file, baseline, but they wanted to collect one from me at the end.

Well, as a scientist, I could see that this was an invalid experiment and put me at a disadvantage, potentially. So I refused to give the terminating blood sample, and then they refused to give me my paycheck. So that went through these management people on the "dis"-orientation, I guess, and that required about two weeks of discussions.

MAESTREJUAN: That's really strange.

FAUST: It is, isn't it? Sometimes that exemplifies the level of bureaucracy and paranoia that can occur.

MAESTREJUAN: Right, I guess so. Did you get your paycheck without having to--?

FAUST: Yeah.

MAESTREJUAN: And so is that written in the employee's contracts?

FAUST: I think so now.

MAESTREJUAN: That's amazing.

FAUST: You know, I say, well, you could have a virus that didn't leave its track in serum. It could be a liver-- Are you going to start getting liver biopsies from people before and after?

MAESTREJUAN: When you moved on to Tufts [University School of Medicine] then, and you started working with [James] Fred ["Paulo"] Dice, you had had some experience with the mentors in the Goldstein and Brown lab. How was working with Dice? How did you see your relationship with him, coming in with a lot of qualifications and yet, as it occurs, still being labeled as a graduate student?

FAUST: It was a very good relationship, and it really hasn't changed much from the time that-- The relationship we had as graduate student/thesis adviser to colleague and co-member of the faculty. I think he thought that I could do things on my own and that he didn't need to supervise me, and that if I wanted some assistance, that hopefully I'd be secure enough that I'd go to him, which I did. And so I worked in his lab and occasionally would bop into his office and tell him what I'd done or ask his advice on something and then bop back out and do some more experiments and pass him in the hall and say something. So there weren't regular meetings. It's not much different than it is now, where I just drop by his office occasionally and tell him-- Because we have a grant to work on the same project, so it's not related to what I did in the lab. He'd stop by and see me, and we'd talk about [what] we'd done, go off. So it's really the same relationship, although he is a pretty hands-off adviser to begin with. Most normal students who do their dissertation research in his lab don't have day-to-day or week-to-week guidance from him. He leaves them on their own until they seek assistance. So I was just part of that same scenario, but maybe a further extreme.

MAESTREJUAN: In terms of shaping your own role as mentor to your students, how has working with Goldstein and Brown and then working with Dice and working at Du Pont affected you in terms of how you have decided to play the role of mentor?

FAUST: I think I've benefited most from having direct interactions with Mike Brown, especially, and then also Joe Goldstein, day-to-day, daily, early on. And I think that's the best approach. When a student or a technician or a postdoc comes into my lab, then I want to see him every day because I think that gets them off to a good start, increases the likelihood that they'll be productive initially, which gives them confidence, and then increases the likelihood that the project will get started off on a positive note and will get rolling along with a minimum amount of frustration. Then as they acquire more skills and more confidence themselves, especially in students, or more competence, in terms of a technician, then gradually pull back from day-to-day, experiment-to-experiment interactions. That's my philosophy, and it's more time-consuming.

Now, that's the opposite of Paulo Dice's, but I also see--and saw--students that go into his lab that are less experienced than technicians, for that matter, that become discouraged and flounder around, and I think that's a bad scenario and I try to avoid that. But the downside of it is that it requires a lot more effort on my part to lead and direct, whereas if you just put somebody on-- I 'm pretty hands-on, especially for the first year or so. Postdocs don't often like that. But at the time that you're interviewing postdocs or attracting them, you can see which ones might not feel comfortable in that setting, that want to be a little bit more independent. But I tell them straight up, "This is my philosophy and it works. I've seen it work. So if you're uncomfortable with that, then let's hear it now." And then usually the postdocs accept it.

MAESTREJUAN: How do you treat the technicians that come into your lab, both in terms of fostering independence and encouraging them to pursue, perhaps, more education, given your own history?

FAUST: Well, obviously I want to see people grow, as I grew. I've had basically two technicians all the time-- No, I think I've had three, and I've encouraged them to become more independent, but at the same time, I was frank with them in telling them that they have a job to do and their job is to see that the specific aims of the grant that's paying them are performed, or if they're not performed, that substitute results at the same level are produced. And they're getting paid for that, just like I'm getting paid to teach as well as do research, and you're getting paid to do your job. And that's different than being a graduate student or a postdoc, because there, it's still part of a training position. Even though they're getting paid dollars, the expectation is that the bottom line is that students get trained and postdocs get additional training so that someday they can now get into a job: a goal-oriented, money-producing situation, like technicians. I guess what I'm saying is that encouraging independent research is good because technicians like that, and it gives them a chance to test themselves and to develop their scientific skills. But at the same time we have to do the experiments and to address the specific issues and specific goals, and most of the time those come from my direction, because they're the goals that I've set out in a grant, or the aims I've set out. So they can't just go wandering off and do their own research. They have to, more or less, follow a specific set path

that I lay down. And they accept that.

Now, the problem I run into with two of the technicians is they are very enthusiastic about pursuing their own ideas and doing some independent experiments. They'll come in on the weekends or occasionally come up with some [inaudible] they showed me that I didn't know they did, and that's okay. But oftentimes, that causes them to become a little bit less conscientious about the money-producing project because they're trying to do forty hours' worth of work in a thirty-five-hour week, because they want to do their other stuff.

And I did the same thing. There's just no question that I had experiments going on the side all the time I was working--not all the time, but oftentimes I did in Dallas, and I only showed the ones that turned out to be productive. But because I was doing seventy hours' worth of work in a sixty-hour week, I'd get kind of sloppy on some of the experiments that Joe and Mike were telling me to do, or the ideas.

So Ann [Dolloff] -- She wants to do so many experiments that she sometimes makes mistakes on the ones that are important. But that's a fine line that we have to walk in order to maintain the enthusiasm and the creativity, or the potential for creativity, in these people, because they have gone on-- Well, Ann's going to go to medical school in September, and then she plans to continue to have some sort of research influence in her career, perhaps in academic medicine. And before that was Andrea Brown, who's in a graduate school at Yale University. So if I'd been a little bit more hard-line and said, "All right, you've got to stop these after-hours experiments and focus on finishing this project so we can get this grant renewed," then she may not be where she is now.

So it's different than graduate students that you can let wander around, especially after they've developed some attitude and skills to demonstrate to you that they are not just grabbing things off the shelf and pouring them in the kitchen sink, so to speak, that they are doing meaningful experiments and asking meaningful questions. But the technicians-- They're getting paid, and they have to perform functions that justify that pay.

MAESTREJUAN: Okay. I wanted to talk about your work that you were doing before your Pew [Scholars Program in the Biomedical Sciences] proposal, the MVA [mevalonic acid] work and the evolution of that research program a little bit. That is not the project that you end up working on with most of your Pew money. So perhaps you can talk about how that evolved into your research program to be- - And then what happened to that to make you change directions.

FAUST: The proposal I submitted to the Pew scholar program involved cloning the mevalonic transporter, which is an activity that appeared in a cell line isolated in Monty Krieger's lab, as a result of a selection technique that I devised and conveyed to Monty. Prior to the cloning, it was just an activity.

In other words, this one CHO [Chinese hamster ovary] cell could take up twenty times

more mevalonic acid than its progenitor cell. So obviously, it had some sort of transporter that facilitated the uptake of mevalonic acid that was missing in this other cell. We realized that back when I was at Du Pont, and actually that was one project--the cloning of that transporter to actually confirm that it really is a molecular entity and not just some artifact of mutation or whatever--that I seriously considered for my dissertation. However, the other project was also a serious consideration, and I chose the latter because I guess I was just more enthusiastic about studying this modification on transfer RNA and, potentially, in proteins.

So I put the cloning and identification of the mevalonic transporter on the back burner, thinking I'd come back to it, and sure enough I did. In the meantime, I'd actually done a few experiments to give some preliminary data that it could be cloned. The most convincing was that the expression of this activity, this increased mevalonic uptake in these met-18b-2 cells--is what they were called--that that's a dominant phenotype. If you fuse these cells to just a normal cell, then the uptake is higher in the fused heterokaryons than it is in the unexpressing progenitor cell, or unfused cell. So that means that the trait got carried over, and that sort of observation is consistent with the fact that there's been a mutation in the [met-] 18b-2 cells that allows for now the gain of function of an activity, which is consistent, once again, with a dominant sort of phenotype. It's easy to clone dominant genes if you've got a selection, and then, there's kind of a standard protocol to do it. So that was, in essence, the Pew proposal.

Now, what's interesting is that at the time I wrote that proposal, I was visiting Dallas and discussed it with Mike and Joe, and said, "Oh, so I think it's time to clone mevalonate transport, and I'm writing a proposal to the Pew to do this. This is my strategy and this is the preliminary data I have." And they thought it was great. They thought it was an excellent strategy and it would work, and of course, at the same time, I asked for a recommendation from them too. And they encouraged me to go off and do it.

In the meantime, I got tied up with starting up my lab and didn't really do any more on that proposal. I wasn't planning to do any more on it, to start on it, until I heard from the Pew to see if it was funded. I was doing some other things, finishing up some loose ends and experimentation. And within about a year-- Well, actually, in a shorter period of time than that-- Within about six months, I'd heard that the mevalonate transporter had been cloned out of the met-18b-2 cells by Brown and Goldstein. So I called them and said, "How did you do it?" And they said, "Well, we-- You know, we heard-- We knew it was a dominant phenotype and we applied the standard protocol for making a cDNA library from these cells and then sorting-- And by transfecting pools into recipient cells and assaying for the activity, we could narrow the pools down until we got it." And I said, "Well, didn't you recognize that as the same strategy that I proposed to you on Saturday afternoon, sitting around your table in the office?" And both of them said, "No." So then I went down and visited and asked them more about that, and they said that they never remembered that discussion.

So now, that's not-- That hurt. It hurt real bad initially, because there have been some people in our profession that have thought that Mike and Joe were not as above table as they should be. And I never felt that way. But hearing this was not something that was reinforcing that thinking. However, I also know that they're both extremely busy and have a lot of projects

that people propose to them, and they've got a lot going on in their department, a lot of projects going on. And sometimes--and I know I'm guilty of that too--that you can lose track of where you heard something, and maybe it kind of begins to-- Not only can you forget who told you something, you might even lose track of the fact that you heard it from somebody else and didn't think it up yourself, because it is a standard sort of technology to do this.

So I've kind of accepted the fact that indeed they didn't realize that's what we talked about and that they didn't run off and clone this deliberately after I had told them to do it. But the disturbing thing is--at that time, and it still bothers me--is that I was seeking their advice and talking to them about science and they weren't paying attention, except for that thirty minutes. So it wasn't a lasting impression on them, which-- You know, I was disappointed that we could have a conversation that they didn't remember when it was really important to me.

So now, you know, twenty years from now, I may make the same mistake; I hope not. But that was a real disappointment that, like I said, we could have a meaningful scientific conversation that was keenly important for me, and they didn't remember, not that they went off and did it. I mean, they did it-- But they did it a hell of a lot faster than I would have done it and the student who did it--it was a graduate student who did it--is a really nice person. And other people who were down there are convinced that she didn't have any idea that I had made that proposal or that I was even interested in doing it. So I think it was an honest mistake, but that was a little sore-- So that's why I didn't do it, because it was done by the time I got around to doing it.

And it turned out to be a very interesting molecule that's been very valuable, because once you pluck it out of cells, now you can put it in any cell you want and get high rates of mevalonate uptake. But the utility of it emerged at a time when the discovery of isoprenylated proteins--which is a modification on proteins derived specifically from mevalonic acid--came about, so it was hot. It was important that we study isoprenylated proteins, or it was important that we label cells with tritiated mevalonic acid and look at its incorporation into things like *ras* and other GGP [gamma-glutamyl transferase] -binding proteins, really key partners in signal transduction. And the problem was that the cells don't take up mevalonic acid very well, and mevalonate is very expensive. So you had to spend a lot of money to do an experiment.

So if you had this cell line, that means you either just spent one-twentieth the cost for the tritiated mevalonate or you waited one-twentieth of the time to get an autorad[iograph], which either decreased cost or decreased time, whatever you wanted to do, and pushed things forward really quickly in studying these prenylated proteins. And then that's what they use it for. I got into studying prenylated proteins pretty heavily for a while. I'm not sure that they're studying them so much anymore, but they realized this was an important tool that would not so much save them money but speed their turnaround time. All right, so that's why I didn't do mevalonate transporter.

MAESTREJUAN: Well, do you still seek their advice now?

FAUST: No.

MAESTREJUAN: Has it changed your relationship?

FAUST: Well, after that, I have never talked with them about science, and it's not because I don't trust them or I think that they will forget or that they would go off and do something I said. I mean, they don't have to do that. That's the other reason I didn't think they did it deliberately, because they don't have to do that. They could do other things, much worse. But I guess, just over time, when I go to Texas to visit my mother or brother, I just stopped going to the [University of Texas Southwestern Medical Center at Dallas Southwestern] Medical School, and was getting advice elsewhere and was content with that.

As I said, I think it really hurt me that we could talk and [it] just didn't last in their minds any longer than ten minutes or ten days, whatever it took to conceive this other idea. They can certainly provide me with expert advice and meaningful interpretations, and I guess I'm probably the sufferer because we don't interact on a scientific level, but you've got to break away some time. You don't have to. I mean, that's stupid. That's wrong. I mean, Monty Krieger still has active discussions with them because his in-laws live in Dallas, so he goes down there quite often and visits them. I guess it was just a combination of that incident and not going as often and doing other things, what have you.

MAESTREJUAN: Did you talk to Monty Krieger about this incident?

FAUST: Yeah. He was actually the person that told me that they were doing it at the time they were doing it, because I had sought Monty's counsel on this cloning strategy, and he knew that it was around. So I guess he learned about it from one of his trips to Dallas, and then when he came back to Boston, he called me on the phone and says, "Oh, you know that they're doing this." And I says, "No." And he said, "Well, I know that you proposed to do this. How is it that they're doing it?" And [I said] "Well, I don't know." So we began to put dates together and it happened just about the time after I had been there. So he said, "Well, that's unfortunate." [laughs] I said, "Yes, it is."

Some other people have asked me about that too, mostly people who fell into the category of not thinking as highly of Joe and Mike as others have, looking for the opportunity to substantiate that feeling, that negative feeling about them. But I've never-- You know, I've always said it was unrelated. "Ah, people make mistakes, come on--" And people don't listen; I mean, I don't listen; I have that problem all the time. Laura [Liscum] 's always telling me, "Well, don't you remember somebody said this or that or the other?" So it's easy to see how that happened, but it has a lot of impact when it's done from their position. We can blow off the fact that I didn't credit a student with an idea that I forgot, but when you're at that level in our

profession, you've really got to be careful, I think.

MAESTREJUAN: Did they ever express any regret?

FAUST: Yes. They said they were sorry that I was upset and I felt that way. They wouldn't have done it if they would have remembered it. And they were sincere.

MAESTREJUAN: Well, how do you deal with the disappoint-ments and the frustrations? One, when in the natural course of the competitive world, that you get scooped, but also, second is that you aren't playing on a level playing field and there is fraud and misconduct in science, and is there any accountability? How do you deal with these periods when things aren't going right and--?

FAUST: Well, I don't think I've experienced much of that. I've never been scooped. I think that can be disappointing, but it ' s usually that if two laboratories are pursuing a common goal or discovery, and one of them elucidates it in front of the other, it ' s because they worked harder, maybe had better resources or more money, their people were better skilled. So those are all good reasons. It's not because they broke into the other laboratory and read the notebooks of their competitors or all.

But it's hard-- You can just say, "Well, okay." You lost to the best team, so to speak, tough luck; let's go on. In terms of-- I don't think I've ever knowingly encountered any fraud or unprofessional [behavior] that would have led to some disappointments. Most of the disappointments come about just from our own inadequacies. And even at the level of--or disappointments in getting funding, becoming disappointed at a study section for not giving a high enough score-- Well, after the initial frustration and disappointment, you can easily point to the fact that maybe you weren't forceful enough in the proposal--or creative enough or clear enough to point to the important issues that you were going to do and why it was important--to really focus the attention of the reader on what you had done and why and how this was good preliminary data and what you were proposing to do were really essential experiments that were unequivocally going to answer the question yes or no without maybe, and that the results that came either yes or no were important to science and would lead to more production.

So just writing the right grant is really so important. It's not so much the data that goes into it or the specific aims. Lots of times it's just presenting it. And those are usually our faults in composition or in stating the cases. As I said, the disappointments, I guess, are just along the lines of our own inadequacies, whether we have to repeat experiments because we botch them up or we don't have the proper reagents or we overinterpreted the data--we took the wrong turn at a crossroads. So those kind of disappointments I think are, once again, our own fault. It's a little bit more easy to accept those that are due to our own doing. At the same time, when we're successful-- And then we know that the credit goes to us and not to the fact that we got a hot tip

from somebody else and didn't really pursue it on our own level or whatever.

So when Laura and I started working on Niemann-Pick [type] C, which is this other lysosomal storage disease, and we realized that it was a defect in cholesterol transport in cells, then there was only one other group in the world who was working on Niemann-Pick C, and this was the group headed by Peter [G.] Pentchev at the NIH [National Institutes of Health], and I guess it was about this-- And they had published pretty extensively on it. In fact, I read all their papers and came to this-- They were postulating that it was a defect in regulation and that they didn't see the connection between transport and regulation. So I just kind of extended that to say, well, the reason it's a defect in regulation is that they don't move the cholesterol around to get to the regulatory sites.

Somebody could argue, well, that's just a fine tuning of their theory, but it still is instrumental in the thought process of how one goes about picking it apart. So I think they arrived at that about the same time we did, but we published it first with our first publication, whereas they'd had, like, six others and then finally came to the same conclusion in the seventh one. Right away, that kind of set up a competition, and a little bit of animosity. Well, I guess it was equal on my part and Laura's and Peter's, but other people would tell us that we weren't thought of very highly by the people in Washington. And interestingly-- After we put out a couple of papers that more or less said the same thing, we—to substantiate the idea that it was transport and not just a specific regulatory event-- Our papers would appear in the bibliographies of their papers. And for three papers in a row, I think, there was always some mistake in the citation.

[END OF TAPE 5, SIDE 1]

FAUST: So in subsequent papers for that by the Pentchev lab, when they obligingly cited our work, there would be a small error in the text of the citation. For instance, my last name would be misspelled or the volume of the *JBC* [*Journal of Biochemistry]* in which the article appeared would be incorrect. And then a third time I think it was the name of the postdoc that was working in Laura's lab who participated in the paper was misspelled. I'm not sure about the order or whatever it is, but there were three different mistakes in, like, three successive papers that came out of that.

MAESTREJUAN: Oh my gosh!

FAUST: Now, if I wanted to jab somebody [laughs]-- Because, I mean, if you get a paper that's in your field that you know that is related to something [you do], the first thing you do is look to-- We're all egotistical. We look to see if our papers are in the bibliography. And then there's nothing more disheartening than to see your name has been misspelled. So it's a small little kick, but it certainly didn't help improve my feeling about the relationship between what our lab--or

Laura's lab at that time--and the Pentchev lab—

Now, interestingly, Laura-- So I no longer work on Niemann-Pick C, except in my mind, and Laura still is actively pursuing it. And she has a very, very good relationship with Peter Pentchev. Laura's been down there and given a couple of seminars. She's stayed at the Pentchev house. They've played golf together out in Tucson. There's a national meeting for the [Ara] Parseghian [Medical Research] Foundation, whose family has this aberrant gene, so they have a meeting in Tucson every spring and they go there and they play golf together. I'm going in the spring and I look forward to playing golf with Peter. And you know, whenever he E-mails her, he says, "Love, Peter." All this is behind us and thankfully so--but there was some competition there. But I guess we all saw the advantage of working together, although there hasn't been a whole lot of collaboration, but at least we communicate.

MAESTREJUAN: Is your last name being spelled correctly now?

FAUST: Well, I haven't paid much attention to it. I guess you only would monitor that if you felt that it possibly would be misspelled. [laughs]

MAESTREJUAN: Well, have you ever brought it up?

FAUST: No, I haven't. No.

MAESTREJUAN: No? Laura?

FAUST: Oh, she might have. I don't know. I'll have to ask her. [laughs] It was bizarre. It was, like, three in a row. I'm not kidding.

MAESTREJUAN: That's amazing, how wars are played out in science. Okay. Well, at the time you were submitting your Pew application, what funding were you working off of when you were starting up your own laboratory? I know that you applied for NIH funding and I think an American Cancer Society grant. And how did that work out?

FAUST: The funding that I was working off when I started up was just start-up money from the department. But shortly thereafter, I wrote the same grant to the NIH as well as the American Cancer Society. It was a follow-up to the work I'd done as a graduate student, trying to characterize a novel isoprenylation. Those were--as I said--hot topics of experimentation at that time, to study these modified proteins and how they went about, or how they were formed. So

the follow-up, the extension of that Ph.D. work, was put into an NIH grant and submitted as well to the American Cancer Society grant committee. And then also to a small local foundation here, General Cinema [Corporation Charitable] Foundation, [that] was giving out awards to young investigators in the Boston area. Fortu-nately, it was funded by the General Cinema Corporation; but I think they've changed their name: it's called Harcourt [General Charitable Foundation New Investigator Award]. And that helped, also coupled with start-up funds to get me going.

Neither the NIH nor the American Cancer Society funded that proposal. And [I] had advice from people at the American Cancer Society that they probably wouldn't ever fund it; it just wasn't what they were looking for. I resubmitted it to the NIH, unsuccessfully again, after applying some more preliminary data, you know. And actually decided not to submit a third time.

I got a phone call one day from Ed [Edward] Bellion, my master's thesis mentor. And he had been serving on study sections for the National Science Foundation [NSF]. He had followed my work and, most recently, my disserta-tion work. He knew that I had submitted it to the NIH unsuccessfully twice. He thought it was just the sort of thing that the NSF would be interested in keenly, and that he had experience from serving on a study section that would review that, and that I should have submitted it to the NSF. So that's exactly what I did. I didn't add any more new data and just more or less reformatted it to the NSF guidelines and submitted it. It was funded straight off, all the money I wanted, and even the four-year time frame. So I guess this is just another example of hitting the right study section and attracting the best audience. That was my first large government grant.

MAESTREJUAN: Why do you think NSF was more interested in it than NIH?

FAUST: I don't know. I have--incorrectly, maybe--but I had the impression that the NSF does not maybe limit its funding to more applied and clinical-type projects and that they really just would fund anything that they thought was important, which includes a lot of basic, just raw knowledge, whereas I think study sections and councils want to have a little bit more assurance that information gleaned from proposals will have a sooner, direct impact on health care or treatment or the like. I don't know if that's right. That's just my perception. And then clearly this project kind of fell into the category of basic research.

Well, the study sections-- You submit a grant and there's a group of fifteen people sitting around, two of whom read it thoroughly and provide the primary review, and then there will be another two who have read it not as thoroughly, but more than just superficially, to provide sort of secondary review. Then everybody else is supposed to have read it, or at least be aware of what's going on. If you've got a hundred and fifty proposals and only fifteen people, then I think it's unrealistic to think that all fifteen have read all hundred and fifty. So it usually comes down to two, or at the most four or five people, to actually form an opinion and a score for that grant.

In the NSF, the study sections consist I think of, once again, x number of people--I don't know if it's fifteen, but let's say fifteen--and one of those is a primary reviewer and then there is another primary reviewer who's outside the study section, and often that outside reviewer is someone who's been identified on the proposal. So I submitted three names of people that I thought would be competent reviewers. Now, I didn't submit Peter Pentchev's name. [laughs] I submitted three people that I thought would be interested and who have encouraged me along the work that I had done towards this.

MAESTREJUAN: And you get your name spelled right, too.

FAUST: Yeah. [laughs] So then the NSF study section brings in the report from this outside reviewer and then listens to the inside reviewer--then comes up with a score. Now, I'm sure that they're aware that all the outside reviewers are not enemies of all the proposers, so they probably weigh all that in, but nevertheless, at least you've got someone who's going to help or who's going to reinforce the positive things you're saying about what's the potential here or that I can do this--or that the proposer can do this--and what's going to come about.

In most cases, it helps reinforce the proposal and maybe also adds some other important insights that could make it a stronger proposal from their own background or their own experiments or something. [They] say, "Well, I know this to be true. I think this is important because we've done this other and it's really neat." Whereas in the NIH you never can tell what you're going to get. So it's a different evaluation system, and maybe some grants get, in a relative sense, a more favorable review with this other sort of system, compared to just a completely blind review by anonymous members.

MAESTREJUAN: How did you respond, in terms of your research work, when there was this gap [when] you got the Pew funding and the work that you were going to do fell out and these other projects' funding were pending? What were you thinking then about how the Pew would react, that you wouldn't be spending money on the project that you proposed and how were you going to deal with this gap?

FAUST: Well, when I realized that Joe and Mike had done my Pew grant [laughs], I just—

MAESTREJUAN: You're very gracious about this whole thing.

FAUST: Well, I wasn't going to send them the money. I figured they had enough money. They didn't need another $50,000 a year. But I was concerned about how I was going to address this issue with the Pew. But with discussions with other Pew fellows-- I was fortunate in that there were actually two that came from our department here, and then there were a couple others in

the Boston area. They told me that the Pew is not so much concerned with seeing that a particular body of work is performed based on the proposal that was submitted, like the NIH would be. They judge more the ability of the applicant to formulate a meaningful proposal and whether he's in a position to do that proposal or anything else he thinks about, and then once again--in the same context of the Pew facilitating and fostering creative thinking and endeavor in their scholars and in other activities--that it wasn't a problem for them if I wanted to still take the money and do something else; they would be entirely supportive. But still, I had a lot of concern in writing that first year's progress report and thinking that that might raise some red flags. But there never has been any comment coming back from the Pew to indicate to me that they were displeased.

One of the real issues or the real concerns I'm still facing is that-- So initially, I had reservations about taking that money because that project was done. Then I said, "Well, what am I going to do with this money?" I was getting money from the NSF to do this isoprenylation project, and at the same time I had got attracted to the study of another genetic disease that, along with it, came a subcontract on an NIH grant. So I was doing some experiments. My lab was growing, or was producing, but it was being funded through its own sources and I didn't feel it appropriate to, at that time, put Pew money into stuff that was already being funded for something else. And the other thing was that I didn't know anywhere else to spend the money. I had enough money. Well, I didn't have enough, but I had enough-- But I just didn't think ahead to think, "Well, where could I spend more?" The idea of spending money just for the sake of spending it doesn't sit well. I guess that comes back from my mother.

So I didn't spend a lot of the Pew money in the first couple of years, and I have a surplus that I'm still carrying, mainly because I didn't know where to spend it justifiably. Now, some people would say, "Well, that's a pretty amiable position," but it has caused some problems. But in the meantime, I've been able to do other little projects or devise other little projects and become a little bit more freer in spending the money on things that I used to think would be frivolous to devote the Pew money to. For instance, I took on a postdoc, a real creative woman who was an M.D./Ph.D. in our department, and she'd finished the Ph.D. part, but she had to wait six months to get back into the mainstream of medical school. And she had come to me with a really neat proposal that demonstrated her creativity, and I thought that this would be something that the Pew would certainly want to consider if they had been spending money for, so I says, why not spend their money on this? And nothing really came of it, but it was a good experience for this individual and we learned a new technique that we still use in our lab. So even to this date I have this carry-forward on the Pew now that I think I'm going to be able to spend, but if I don't spend it all, I'm going to turn it back. So I guess the real problem that I faced with the mevalonic transporter is not so much getting sanction from the Pew to do something else; it's just doing something with the money that's justifiable, and that's still a little bit of a problem.

MAESTREJUAN: Yesterday you had mentioned that you do things because you think that's what they want to hear, and that sometimes-- You know, now we're living in an era where results need to lead the work that was proposed in the original application, and sometimes that makes work boring. How has getting money or making applications to these different kinds of

federal and private granting agencies changed the science that you do and the science that you propose? I guess I should put it the other way: the science that you propose and then the science that you ultimately do when you do get the money and are working at the lab bench. How does it affect--if you're going to be applying to the NIH versus the NSF versus, say, American Cancer Society or Pew [Charitable Trusts] or whatever--what you say and then what you do?

FAUST: Right. Well, I haven't really-- I come up for renewal on NSF-- Actually, I'll be writing that probably this fall. But I planned to write it based on what I've done, which hasn't been entirely what I proposed to do. And I don't think that's going to be a problem. My perception of the NSF is that they're just interested in pursuing science in maybe a more creative fashion--and especially basic understanding and fundamental knowledge--so it's less important that it follows guidelines that were set out in the original proposal. So I'll be kind of freelancing up there with the NSF. Now, the NIH-- It initially started out as a subcontract, and now I'm a co-investigator on an NIH grant with Rod [Roderick T.] Bronson, and we're up for renewal submitting this summer. And we've stuck to the specific aims pretty well, along that, because, once again, it's my perception that study sections go back and look to see what was written in the last proposal, what was proposed, and will determine if the money that was allocated to do those studies--the studies were actually done, before they give out money for something else. Now, that may not apply to investigators at other levels in our profession, for instance, Joe and Mike. I mean, I think whatever they pose, they don't have to do the NIH grant. And they didn't, for a long time.

Lots of laboratories are posing to do experiments for NIH grants that are all but done already and kind of know 90 percent what the result of the discovery is. And that's not really too fair, is it, to say you're going to do something when it's more than half done? But that kind of guarantees, number one, if it's not submitted the first time, you've got much stronger preliminary data because you can go back in and say, "Look, this is what I propose to do and this is the preliminary data I applied at that time, and now we've gotten some additional data that indeed demonstrates that we could do this and we've made a significant discovery." So it's hard for a study section to turn that down a second time, but I mean, that's a common trick. I think everybody, myself, does it. But nevertheless, it certainly tells us that we need to stay as close to the guidelines that we proposed as possible, because that's what study sections reflect on.

With the NIH I'm going to stick pretty close to what we said we'd do, and I would probably continue to do that. With the NSF, I'm not sticking to that. Although I've done other experiments on the side with my NIH grant that probably could-- And I'll put them in there as interesting background data and maybe have little small sections about how I can follow up on this apparently minute observation that may have some importance down the road and may end up devoting a lot of time to it anyway. But the key thing, I think, with NIH grants is to make clear proposals that try to answer specific questions, and then do them.

Now, I don't know about other private foundations. Apart from the Pew, I haven't applied to-- Well, no, wait a minute-- I have an Established Invest igatorship from the American Heart Association--which is just salary--and that required a proposal, and that's private. But in all

honesty, that's probably one thing that I got because of Joe and Mike. I mean, they're-- The American Heart Association's in Dallas. It's right over there on Greenville Avenue.

MAESTREJUAN: I didn't know that.

FAUST: Who's had more impact on heart disease than Brown and Goldstein in the past twenty years? If somebody came from their labs and applied for a grant from the American Heart Association, and they wrote a letter of recommendation-- I wouldn't be surprised if they didn't even read the proposal.

But it was more of an award than anything, sort of like the Pew for-- [It] wasn't based so much on what you were proposing to do and actually doing it. They were just awarding your potential and wanted to give some salary money. So I guess American Heart Association [does] offer grant-in-aids, which would be actual funding for research money. That might [en]gender-up a more in-line proposal, you know, that you wanted to follow something along a specified route. I'm not in a position to do that because I'm not doing so much with cholesterol metabolism now. Although I hear that the American Heart Association funds research outside the area of cardio-vascular arteriosclerosis, whatever. So maybe in the future I will apply.

MAESTREJUAN: What does this mean for science then, when researchers need to create strategies for their work? Especially for somebody like you who seems to me to be most productive when you can do fragmented science and for two reasons: because sometimes it may be riskier, but the payoffs in terms of advancing science can be also very big, but also just to keep the creative juices flowing in a researcher—

FAUST: I guess that the plus side is that there will be a steady stream of solid information, solid discoveries coming as a result of investigators following specific aims of their proposals as closely as possible. Because when study sections give grants high scores, then it says that in addition to the investigator who's making the proposal, there are other experts in the field saying that there's going to be meaningful information coming as a result of this experimental work. So they're concurring that it's going to be beneficial to our country, biotechnology eventually, or whatever. If you get more people who agree on that, then the likelihood of it happening is pretty good. And it's just up to the investigator to stick to that, to stick to the job at hand.

So there will be more predictable progress made in science. You can almost say we'll have the complete sequence of the human genome at some point in the predictable future. Whereas if we relied upon, hopefully, Lee Hood to develop a new sort of sequencing method or technology or machine that could sequence bases at a hundred times faster, then it might come about sooner, but that might come about just because he got creative one day and went in his garage and started tinkering around and made this new machine that he couldn't get funded for if he wrote [a proposal], because he didn't have the idea or it wasn't germinating enough in his

mind to formulate into a concrete proposal.

So with creativity, that might come about in, say, instead of the year 2010, in 2005. But it's kind of guaranteed if enough people get on the writing proposals to develop maybe new technologies along with concrete ideas, it's predictable that we could get it done in 2010, but we might not make that great leap to have it done in 2005. So we'll lose the opportunity to, say, really speed up some discoveries or new information, but at the same time, it will be a more even ride along the way instead of ups and downs.

And then I guess it will probably be more cost-effective, because let's face it, there's probably for every dollar that's spent on-- So if you had $10 and you were putting one in an NIH designated track, then you'd probably get, say, $50 return for it. And if you had another $10 and you were putting it into the Pew sort of creative, freelancing-type science, then maybe only two of those dollars would ever pay back into decent experiments. And double that, or multiply that by five, and you get your $10 return. But there may be that investigator out there who takes that $10 and parlays it into $200 worth of needed technology and new ideas and information. I mean, there's a balance, sure.

And I think that there's room for both-- There should be room for both in our profession. And who does it is up to study sections and the like. It used to be that—So there was the [United States Public Health Service Research] Career Development [Award] that the NIH gave out, that would use long grants like seven years. Are you familiar with those?

MAESTREJUAN: Well, I think--

FAUST: Are they still giving those out?

MAESTREJUAN: That I don't know.

FAUST: So that's long-term stability which would give investigators the opportunity to be more creative. But I think those have kind of waned. It would be nice to see the NIH start doing that again, and they might. They're talking about doubling the NIH budget in five years. I doubt that that will come about, and most people share that feeling too, but it could be that we could have it increase at, say, 50 percent over the next five years, of which half of that would go into just increasing the overall budget and the other half would go into some kind of new awards.

I think most of the investigators in our department are following their specific aims pretty close. If they want to deviate, then they usually get some sort of other seed money--oftentimes from biotechnology or a small foundation grant--and then take the seed money and do some experiments, and if turns out to be positive, then generate new data, either to incorporate it into their existing next NIH grant or maybe writing a new one. So their creativity's financed by,

initially, extra government funds.

MAESTREJUAN: How do you see your own funding future?

FAUST: My renewal of the NIH grant is in September, but I'm pretty confident about it at this point--not September, in July. So we're taking on a new direction, but this is expected. So as a result of my collaboration with Rod Bronson, we've identified these mutations in mice that cause the same phenotype as a human disease called NCL [neuronal ceroid lipfuscinosis]. [We're] starting out very small, you know, just doing a few experiments and doing some biochemistry, then finally writing a grant, be co-investigator doing more biochemistry. That's all we've done, is just biochemistry to characterize the phenotype of this disease in mice, but knowing all along it's probably the same genes that are defective in human conditions. But it became clear to us that if we wanted to pick up the pace or stay at the higher level, that we needed to identify those genes. So that would mean cloning them. And if you don't know anything about the structure of the proteins that the genes make or the levels of their expression or if you have antibodies against it, then the only way to do that is by positional means, positional cloning, which is identifying genes just based on their localization in chromosomes, and that's something I knew absolutely nothing about up until about six months ago.

But fortunately, we attracted this really bright Chinese postdoc who's had some experience reading a lot about positional cloning. And he looks like he's read well because he's progressing very well. That's going to be the corner piece of this grant that we're submitting, is to position and clone at least one of these mouse genes. It's easier to clone mouse genes positionally than it is human genes, and I think a study section will see that.

And we are amassing a good bit of preliminary data that we hope will convince them that even though we've never done this before, with our collaborators-- We have active collaborations with some human geneticists here in Boston as well as some mouse cloners up at Jackson [Laboratory] that indeed these two guys--one of them being a pathologist and a geneticist and the other more or less a biochemist and molecular biologist--can indeed in their laboratory clone this mouse mutation.

And then that will help a lot, because, number one, it will occur sooner than the human mutations--unless somebody knows something up there that we don't, about these human diseases that we don't--and then it will help us really go forward in understanding this disease, to identify the defective genes. But we have to be convincing about that.

But I think that we have an advantage in that Rod identified this mouse mutation up at Jackson Labs, and Jackson Labs has this policy that they don't give out those strains until they've been fully characterized in-house, so it's kind of a monopoly. So we have a leg up there that no one else has the opportunity for, and I think we're progressing pretty quickly--or are on a good pace--on identifying this gene, although it will probably be about another year and a half or two years.

And then we've also got some good biochemical preliminary data that answers or addresses the specific aims that we proposed three years ago, and there's significant data there and discoveries. So we hope that a study section will see that, okay, we accomplished the goals that we set out to three years ago, and in addition we've taken this new path and that we could do it, so we're going to ask for a little bit more money and a little bit more time, and I think we stand a good chance. So with one large NIH grant, then you can do a lot of work. And now next year, at this time, I'll, like I said, reapply for NSF, and I'm not so sure about that, how that's going to go. It's just a little bit early to tell, I guess.

Well, I can say that I think that it's foolish for me to believe that I should operate with a lot of money, because I don't think that I can operate with a lot of people, just through the philosophy I have about running labs and running the lab, so I'm not anxious to go out and try to get three NIH grants. I think two federal grants would be plenty for me. That would be plenty.

You see, I guess the more money you have, then that usually means the more people you have and vice versa. The more people you have, the more money you have to have. And then those two mores mean less time for investigators in the lab and more time for investigators attracting money and people, which is kind of like desk work, and I sorely lack desk work. It's not what I was trained to do. I don't do it best. I'm better at working in the lab. I'm still the best scientist in our lab, so when you stop and think about it-- Listen to this reasoning: So if I'm the best scientist in the lab, shouldn't I be working in the lab and the worst scientists be sitting at the desk? But it's the other way around, isn't it? Now, when I say I'm the best scientist-- I mean, there are some people gaining on me, but that's just because I don't get in the lab anymore and I'm losing skills. If I got back in, then I'd able to keep up with it, you know.

But it's not all writing grants and reviewing papers and reviewing grants and things that are associated with the profession as a whole. I mean, there are a lot of pulls from the university, apart from teaching. I'm sure that all Pew scholars begin to say the same-- All right, I'll say the same thing as all the rest of them. I mean, the university expects a lot of us in terms of teaching and administration, which you can argue, and then I will, that it's unjustified, considering what we bring in to the university in terms of not only indirect costs, which in essence runs the whole show here.

I have to really beg to have the university pay 15 percent of my salary. I mean, the university doesn't pay any of my technicians, any of my graduate students, or any of my postdocs. The university doesn't pay anything for my supplies, and in fact, they take money out to pay the light bill, which is to the tune of about 6 to 7 percent. Well, I don't think that I'm running 6 to 7 percent of the water and the lights when they pay for that.

So why is it that I get saddled with committees? You know, teaching is okay. You could easily argue that professional teachers would do a better job, and they do. Or other assignments associated with the university-- Shouldn't there be other people on those lines? Those are pulling the best scientists away from the lab--at the same time as attracting more money and better people and writing and reviewing. It's not like these are a professional type of services. So that's

annoying. You know, it's oftentimes--if I liken it to my old colloquial ways-- Have you ever ridden horses much?

MAESTREJUAN: A little bit, now and then.

FAUST: Okay, if you go horseback riding out and after you've been riding for a while the horse gets tired, it gets hungry, and it knows where the barn is-- And it wants to turn and go to the barn, and you have to fight it if you want to go over the ridge to look at the sunset. It's a lot easier just to let the horse take you to the barn than it is to do something that you want to do. So when I'm sitting in my office, it's a lot easier, when the phone rings and somebody wants me to review a paper or serve on a committee or whatever, it's just a lot easier to say yes and follow that route, but it takes me away from what I want to do and what I think are really the important things to do.

There are lots of colleagues in my department that don't even know where the laboratory is--facetiously. But they don't know where the pipette rings are in the laboratory to do an experiment with or what's in the drawers in the lab, because they haven't been in there in years, because they've just been--and freely admit--that they just went with the horse to the barn because that's just the easiest thing to do at that time. But I don't think they had the same long exposure to laboratory practice and laboratory discovery that I did, and it's difficult to forget that.

If you're a graduate student and struggling with a project and then you do a postdoc and struggle a little bit more and become successful, then get into a faculty position and get funded, then it's "Boy, I'm here. Thank God." But if you put a lot of effort in and [have] seen the things that I have and experienced discovery on the level that I have, then it's contagious. You're not ready to give it up. Like probably other Pew scholars in front of me, that they would prefer to see more of their time spent in the laboratory, although I'd-- You know, at the last couple of [Pew Scholars Program in the Biomedical Sciences annual] meetings, I've talked to some of the other guys in my class, and they freely admit they don't work in a laboratory and they don't think they ever will again. They'd like to, but they just don't see that it's possible. And getting back to what brought this out, I think the larger operation--laboratory people--you have, the less likelihood it is for the principal investigator to experience lab work. So the opposite of that is the smaller they have, the more likelihood it is. Well, I kind of lean in that direction.

[END OF TAPE 5, SIDE 2]

MAESTREJUAN: How big is your lab and how well have you been able to attract good postdocs and grad students?

FAUST: Right now I have one technician, two graduate students and two postdocs, and that's about a good size. I have one graduate student-- She's been with me a little bit over two years and she was my first student. That was a little discouraging. I thought I would attract a graduate student sooner than that, being an ex-graduate student recently and an overall nice guy and being well-liked by the people in the department. So it was a little discouraging that students just didn't flock to my lab: not that they didn't flock, but they didn't even come for rotations.

But I guess the reasoning was--or the way I reason it is--that I was sort of an unknown quantity and quality in terms of can I lead a graduate student successfully in a Ph.D. dissertation, whereas the other faculty members are experienced and were proven. So if you're a student faced with having to spend five, six, seven years in the pursuit of a Ph.D., you kind of want to make sure that it's going to get done, and best put your money on the ones that have done it in the past. But now I have two.

I've been very, very fortunate in technicians. This is a real plus for this area, that there are lots of good undergraduate universities in this area. In addition to lots of good universities in this area, there are a lot of good students who are in this limbo between undergraduate degree and figuring out what they're going to do with respect to the rest of their life, between medical school and graduate school--trying to decide. So they're going to think about it for a couple of years, and they're usually going to get a job as a technician. And they're bright, because they're obviously candidates to go to medical school and graduate school. So it's not difficult to attract a good quality technician. Now, what's difficult is to keep them for longer than about two to three years, but you just accept that. I've had three technicians that have been very good, and they've moved on. But they've been productive and helped me quite a bit.

Postdoc-- I had a disappointing incident with my first one. She came and worked for a year, and I asked for a two-year commitment, like I do with technicians. At the end of the first year, she announced that she had applied to medical school and was going to go--and just ignore the commitment that she had made to me. And she was doing very, very well, too, and had accomplished something that one other technician and myself had been unsuccessful at, separately. And then she succeeded. So she left and that was discouraging.

But I have two postdocs now, and one of them has required a little bit more attention to get started, but I kind of figured that, and I was ready to do that. He's progressing very nicely, and I think has already been productive enough that he's paid back the time that I've spent and put into him. And Xuebin [Qin], the Chinese fellow, the only time I spent with him is for him to tell me what he's done, and then to make me understand what he's done and tell me where I [can] go read about [it] so I can learn what he's doing. And I think he'll work out very, very well. I know they will be successful in cloning this gene and I hope he stays around to clone the next one, and then after that, that he finds a rewarding position, justified by [my] extolling his contributions.

So it was slow attracting graduate students, but I've got two, and that's fine. And I haven't had any problem with technicians, and an initial bad experience with a postdoc, but I'm very well pleased now. Overall, I've been very fortunate.

MAESTREJUAN: Well, we were talking about this yesterday, on tape and off tape--at the end of the last session--and we talked a lot about hierarchies in lab, and you mentioned yesterday that you think that there are so many graduate students and so many postdocs and not enough PI [principal investigator] positions. And then, you yourself were a research associate who was clearly capable of doing independent research but then hit a ceiling where there was a need for a Ph.D., not that scientifically you needed a Ph.D., but administratively you needed a Ph.D.

And then you have this postdoc, this Chinese postdoc, who's absolutely superb and is making huge contributions, but his time is limited, and if he goes back to China, the quality of his research, because of the setting in China, may be wasted, and yet is there a PI position for him?

And then what you just said about [how] you're a good scientist but you're doing desk work.

So is there some way to rethink how laboratory science is structured, in terms of who does what, so that the people who aren't good scientists--who are good administrators or grant writers or teachers or whatever do that, and the scientists do the science. I don't know if that means inflating or deflating degrees, or what the options are. How do you see this?

FAUST: Well, as we said a little bit yesterday, I think there's a real glut of postdocs and--in scientific language--the rate-limiting step is progressing past the level of postdoc into-- Whether it's faculty positions in an academic setting or principal investigator in a biotech [company] everything is getting backed up right there because there are just not that many-- The system is producing more postdocs, so the pool is getting larger and larger, which means that those that are in there are going to get less likely to get out. It just seems that we need to do something to make that pool smaller, to move these people into productive, rewarding careers, that were kind of promised them back when they started down the path towards scientific research. You know, if you do well in graduate school, you do well in a postdoc, then there's going to be a good job for you--not a great job, but you're going to be able to do things that you'd learned how to do well and that you'd be paid for them. And that's just not the case here.

So what are we going to do? Well, one thing is that I think we ought to stop making more graduate students, or reduce that flow, and we're trying to do a little bit of that here. But at the same time, are there positions that we can create to help relieve this roadblock? And it probably won't occur at the level of faculty members in universities, because they're actually getting smaller. A good place would be in biotechnology.

And I guess I don't know the economics that well, but I'm sure that it comes about as a result of just expansion of that industry. What's preventing it from expanding is, I don't know, maybe the application of some basic research--that hasn't come about--or less government control over, say, drug processing and development. Maybe creating research institutions that

are both federally and privately funded, sort of cooperatives like some of the universities have done around here, to not so much have contract-type research, but just mutual agreements to facilitate the flow of information from basic research and into perhaps an applied setting--

And then it just seems that to help the principal investigator in the academic lab--and this is actually an idea that I heard from Ed [Edward Bellion] a long time ago, and as I think more about it, I agree with him one hundred percent--that there ought to be ways to fund lieutenants in laboratories (and lieutenants could be filled by these senior-level postdocs) to where they have a stable position that's partially on hard money, as opposed to continuously being dependent upon grants for support. But that either through the university or through the federal government or some agency, just to have some of this money go into a pool that is large enough to fill these hard-money positions. So you could have a postdoc working with an established investigator--or a faculty, a professor, say--on laboratories the size of seven or greater. And part of his responsibility, this--let's call it this lieutenant--is that he can not only do laboratory bench work, but he can maybe help a little on the teaching load, maybe in the form of study groups or advanced TA-ing [being a teaching assistant].

The principal investigator can elicit his help in reviewing papers that the lieutenant has an expertise on, so it's sort of like a second level of review. [The lieutenant could] field questions from other collaborators or other people in the area that call me and ask for advice or information about my project or what's going on, just the day-to-day exchange of information. If I could direct a call to "Greg Warner," you know, and know he could answer the questions--that way take a little bit of time away from my desk work and allow me to get back in the lab, but yet give him some responsibility, and in a more stable environment, financially--then that would be something I think would be admirable and [would] help relieve this backlog. [This would be] in the academic field, now. In biotechnology, it's just I guess the expansion of the industry--

But if I was a postdoc now, I mean, I'd be pretty scared. If I was a graduate student, I'd be pretty scared. And I usually try to encourage our students that unless you're just really a hundred percent sure that you want to be a university professor, then you ought to begin to think about a postdoc in biotech. And it's not the best-case scenario, because there's a lot of creation and destruction in jobs in that industry. You know, there are companies that are bought up and sold and projects that are dissolved and people are laid off. But it might be, in the long run, for a postdoc, or whatever, more stable than this.

MAESTREJUAN: Given all these constraining factors, that have more to do with the structure of science, the infrastructure of science, how do we maintain the critical levels of creativity in the next generation, in the students and the postdocs that are going to continue to advance scientific knowledge. Take specifically this Chinese postdoc that you have, who has generated a lot of enthusiasm just because of the environment that he is in now, because of what he didn't have in China, and yet his future may not seem very certain. Given this environment, how do you still select for the creativity needed to advance science and make scientific discoveries?

FAUST: I don't know. Let me just think about it. Actually, why don't you just turn the tape off.

MAESTREJUAN: Okay. [tape recorder off] Okay.

FAUST: So I think one way that we can foster or improve or maintain creativity at a high level, if we're committed to do that, is to establish programs that stimulate creativity at the level of, say, senior postdocs, before they have to go into a principal investigator position. The reason I think that is that based on history, in the conventional professional track, that's a time when the individual is probably at his or her peak, in terms of their knowledge basis, their stimulation, their enthusiasm and, hopefully, potential creativity, because they [have] just come to the end of their own training, so to speak, or being trained, and they're getting ready to go out on their own. But yet, as soon as they get out on their own, then they have to get into lockstep with the system, which means usually writing a grant that follows a rigid set of guidelines that have to be followed--that being specific aims or whatever. And then unless they have an enormous amount of funding straight off, they're not going to have the flexibility to continue to test their creativity. So just as soon as you move into a faculty position, you lose that.

Now, I know that through agencies like the Pew [Charitable Trusts] and the Howard Hughes [Medical Institute] and the like, those individuals who receive those awards do have that opportunity, but is that enough and does it come at the right time? To me, it seems that if we might possibly consider fostering creativity, say, in those last years of postdoc, to those individuals that are deserving or have demonstrated the potential to really move forward-- I think there are some programs like this that-- Well, maybe the Pew [Latin American] Fellows Program is something similar to this. Are you familiar with that?

MAESTREJUAN: Yeah, I am.

FAUST: In which Latin American postdocs--or I think by and large they're postdocs--have the opportunity to come to the States and receive additional training. They're in somebody else's lab, and that's more or less a continuation in their training and not so much an opportunity to just really freelance. But I'm sure that there are, in large labs especially, in the Boston area or whatever, talented, bright postdocs who have demonstrated, say, in the past three years of their postdoctoral training as well as their graduate program that there's a significant potential for this individual to spontaneously develop, so to speak, some technology that would bring genome sequencing five years closer if we just give him the resources right now before he has to go out and start earning a living. So I guess a program like that might help utilize the brainpower of these people who I think are really at perhaps their peak, in terms of experimen-tation and thoughts. Now, they probably [would] be good investigators later on, but then they have to develop all these other skills like management, letter writing, reviewing and these kind of things.

But in terms of placing these--like Xuebin, the postdoc that I have--that's a much more difficult situation. Obviously, if we have a good measure of success, then we'll try to see that he gets a permanent residency in this country, which is mandatory for any sort of permanent position, whether it's in biotechnology or an academic setting. And with continued success, he might be able to get a permanent position. But it puts him into the same category as all these other postdocs and senior postdocs that already exist, and I'm sure that there are lots of Xuebins out there, each one of whom could make significant contributions well beyond the time that they're in my lab, or wherever they are. But we can't continue to pay them a postdoc salary justifiably. It's just not fair. My salary goes up every year, and theirs will go up a little bit too, but not to compensate for the amount of experience and training that they've had and the productivity that they're delivering.

Once again, I think [we] just have to try to create positions for these people in our profession; at the same time, kind of stop the input, slow it down a little bit. And then, how do we slow it down a little bit? Well, if a master's degree was more beneficial than what it is, especially coming from somebody who has one-- And I'm not saying it was a mistake to get one. It wasn't for me. I benefited from it. But I wouldn't advise anyone to get a master's degree now. If you're not going to pursue a Ph.D., then you might as well just stick with the baccalaureate. But if that was a more meaningful degree, or opened up opportunities for students, then we may see more students in those programs than there are taking the Ph.D. challenge.

Biotechnology certainly employs the lion's share of those master's students that come out now. I don't think you see any master's students in an academic setting. The salary increase for a master's over a baccalaureate--I know at Tufts [University School of Medicine]--is just not even worth the effort to go to school for the extra time, whether it's a year or eighteen months or whatever. But I think biotechnology companies pay a little bit better, so there is some incentive. But if the reward for that academic achievement was better than what it is, then maybe, like I said, it would persuade some students who normally would think about advanced training only at the level of Ph.D. to maybe just take a master's program, succeed, get into a job, and actually be more productive than they would be trying to flounder around as a principal investigator, especially after spending six, seven years [as a] postdoc. They would get into a position and start making contributions [sooner]--after two or three years of post-baccalaureate training, i.e., getting a master's, than they would after twelve years of taking a Ph.D. and [as a] postdoc--and be just as content with a different set of initials after their name.

So that doesn't address Xuebin, but I know there are lots of foreign postdocs that came in and through Dallas [University of Texas Southwestern Medical Center]. Some went back to their countries and went on to the next level, and some went back and didn't go on to the next level. Some stayed in this country and just continued to move about and postdoc longer, and they're still postdocs. And a couple actually were able to get permanent residencies and eventual appointments at universities in this country. I think those are rare, compared to other foreign nationals that come to this country to postdoc and are in other laboratories; they just end up staying here for as long as they can before they have to go back. We shouldn't stop bringing them here. I think it ought to be open and competitive. We shouldn't limit training and education

at any level or at any profession to only people that live on these shores.

MAESTREJUAN: And why is that?

FAUST: Because that just excludes an incredible mass of opportunity and mind power, which just goes against all rules of nature. We already do that, and our department does that, because we have a training grant, a large NIH [National Institutes of Health] training grant, to fund graduate fellowships, and it's restricted only to U.S. citizens or permanent residents. So right now we're trying to figure out how many graduate students we're going to take in September of 1997, and we know we have four training grant slots, but if we want to take a student who's not a permanent resident or a U.S. citizen, then that's going to have to come off of hard money from the university, which is a little bit harder to come by than a training grant.

And then that means that something's going to have to be rearranged in the budget to create that fellowship money. So as the guy who reviews all these applicants coming to our program, I've got one pile that says "U.S." and the other says "non-U.S." And they get evaluated differently. We usually end up taking one non-U.S. every year, but sometimes the pile that says "non-U.S." is a lot higher caliber than the pile that says "U.S." And if you look at the-- In the wall of our conference room, there's a picture of every student who has completed our program, and you can, in an objective fashion, rank them--with me being at the top [laughs]--and those that are in the upper echelon are from mainly China.

Out of the top ten, perhaps six of them are from China. And less than 60 percent of the students that have been through our department are from China, but they came and competed very well. So we do that anyway, but we shouldn't be doing that, and especially at the level that we're doing it. But you know, it's tempting, because most of the time you can bring in a postdoc and pay them cheaper, pay them less, because they're more willing to come. But you usually end up getting what you pay for, and it doesn't take them long to know that, if you're paying them less and they're making less money than "Joe Smith from Alabama" down the hall, because he's on a training grant or whatever. So that's not very good incentive to do well, if you know you're getting less money just because you know that they're taking advantage of you.

So I guess you could take that a step further back and say, well, maybe we ought to do things to really encourage more foreign students to apply to U.S. universities, and then that's going to produce a dilemma in bringing about more postdocs. But at the same time, if we hold high standards for admittance and high standards for matriculation and eventual degree granting, then the overall pool's going to be much better.

MAESTREJUAN: Do you have opportunities in your lab, or do you encourage opportunities, for foreign scholars at whatever level to come into your lab and work for a semester or for a summer or--?

FAUST: I have an opportunity to do that and I have done it and will probably continue to do it. I have--although it doesn't bring any money into my lab--a United States-Israel Binational Science Foundation grant, and it's a collaborative venture. The funding is collaborative, between the U.S. government and the Israeli government, although I'm told that all the money comes from probably the State Department, and then just goes to the Israeli government, who oversees the administration of these grants that are written by Israeli investigators. So it's sort of like putting money there, and then say, "Okay, bring it back here." The stipulation is that you've got to have an active collaboration with an American investigator, but they're just giving all the money to them.

So there's a colleague [Joseph Roitelman] that-- I guess we started-- But prior to this, we had never done any collaborative work. We met at meetings and discussed common intellectual pursuits in science--he's cholesterol metabolism, too--and approaches to problems and his work and my work, and we seem to feed off of each other's ideas and have enjoyed this exchange just intellectually for a long time. Then he applied for one of these grants and asked me to be the co-investigator, the American cohort to this. And I said, "Sure." The money is spent mostly for supplies and salary for him, but part of it is for me to travel there and work.

So in May of '95 I went to Israel for three weeks and worked in his lab. And then last July, his research associate came over here and worked the whole month in my lab. And I'm supposed to go back there, and I was supposed to go next month, but I canceled. So now I'm not sure when, but probably before the end of the year. And then in the following year, another person from his lab will come over for a month. I won't go for another three weeks; I'll probably go for two weeks.

And then exchanging personnel, so to speak. His people come over and learn stuff from my lab, and all I do is go over there, just to help him in his experiments. It's more of a one way-- Sort of like the money. It's all going that way and the information's all going this way, but—

MAESTREJUAN: And why do you do that?

FAUST: Well, I think he's a bright guy, and he has a good experimental system. He's trying to define genes and their factors which mediate regulated degradation of HMG CoA [3-hydroxy-3-methylglutaryl-CoA] reductase, which is the rate-limiting enzyme in cholesterol biosynthesis. The level of that protein fluctuates inversely with the level of cholesterol in the cell. When cells have a lot of cholesterol, there's no need to make it, so the level of the protein goes down. And the converse is true. Mediating the level of that protein is accomplished by either accelerating or slowing its degradation, how fast it's proteolyzed. So cholesterol stimulates the proteolysis. The lack of cholesterol slows the proteolysis or increases the stability.

That was something that Faust, [Michael S.] Brown, and [Joseph L.] Goldstein discovered [laughs] back in about 1980. But since then, we don't know anything about the

protease that does the clipping. Now, he actually came from Bob [Robert D.] Simoni's lab, which has made the other significant observation in that they defined the region in the protein itself that is required for the degradated proteolysis. They made a construct between the membrane-spanning regions of HMG CoA reductase, the N-terminal domain, and hooked it onto something else, and it mimicked that. So they kind of defined what part of the protein's involved there. And that's all that we know.

We don't know anything about the protease that does it, or the sensing mechanism that the cell uses to say, "All right, there's high cholesterol. Turn the protease on," or make the protein more susceptible to already existing proteases. Or, "There's low cholesterol. Turn the protease off," whatever it may be, or make it less susceptible to proteolysis. This is fifteen years later, and we don't know anything more about that.

So he set up a system, primarily using genetic screens, which is something I like to play with, to define factors that can slow the proteolysis or accelerate it upon command, independent of cholesterol, and then be able to pluck out these factors independently through cloning and identify what they are. That's an experimentally challenging and intellectually stimulating little project, and it's fun to keep up with it, because I kind of had a history in this, and knowing it hasn't gone forward since then, more or less. So it's working out pretty good. It works out good. I guess it could be more of an exchange, but that's the best we can do at this level.

And then I have a collaboration on my NCL [neuronal ceroid lipofuscinosis] project--not as strong a collaboration--with a guy in New Zealand [David Palmer]. And then actually, I was thinking about this-- He's got a lot of students. He's got a vet [erinary] school. Because not only are there mouse models for this, but this disease is very prominent in sheep in New Zealand-- There's a lot of inbreeding, and if your farm just happens to be in an isolated area in which you've got a--let's call it a founder effect-- When sheep first came to New Zealand, let's say a hundred and fifty years ago, if in the flock of the first original hundred--which probably came from somebody's flock that was inbred to begin with--if that particular population had a high prevalence of this mutation, then now this flock of a hundred, which in the next hundred and fifty years would be a hundred and fifty million, then you'd have a significant amount of sheep that would get this neurogenerative problem when they get up about two years old.

NCL is a significant problem in the sheep population in New Zealand. So there's a lot of interest in studying that. Unfortunately, you can't do a lot in terms of molecular genetics on sheep. You can do even less on sheep than you can on man, because of the lack of blockers and the genetic technologies not applied. But still, he has a lot of students, and so I was thinking, well, maybe one of his students ought to come into my lab and study NCL in the context of mice, or whatever, and spending some Pew [Scholars Program in the Biomedical Sciences] money to bring him over. So that's something we've discussed. But anytime you bring somebody into my lab, then it increases the numbers.

MAESTREJUAN: Well, I'm going to jump back a little bit. We're coming to the end of our time, and I wanted to cover a couple of areas, and one, to go back a little bit-- When you became

a faculty member here at Tufts, how were you able to shift identities from being a graduate student to being a faculty member and PI?

FAUST: Well, that wasn't difficult, mainly because it all occurred in the same department, and I was well-accepted. There wasn't any ill feeling from among other faculty members when I came on board because "Why are we bringing on this student to join our ranks?" I mean, nobody felt that way. And I didn't have to move anywhere, except just right across the hall. And I knew all the faculty members and that they could help me set the lab up, answer questions about where to buy things and how to go about setting up a lab. I did a little bit of that at [E.I.] Du Pont [de Nemours and Company] although it was kind of partly set up when I got there.

Physically, the transition was smooth. And even support-wise, emotionally, it was smooth because they already knew me, so they could counsel me on writing grants and the like. Well, just in talking to other people that moved, mostly from a postdoc to a faculty position, and physically moved and had to meet and become acquainted and make contacts with people in this new department, that takes time. But I just didn't have that. And I had other offers. And I wasn't thrilled about staying in Boston, because I knew if I stayed, I'd have to stay a long time. Ideally, I would have wanted to go a little bit further south. But the fact that I knew it would be a smooth transition and I wanted to get started and off quickly and in the right direction was a real strong reason to take the offer here at the Tufts physiology [department].

MAESTREJUAN: Okay. Well, another aspect of your life that we haven't touched upon is a little bit of the personal side, and I wanted to ask, given that you're in this interesting, not so uncommon situation, but definitely not common, that you share your life with a fellow scientist [Laura Liscum], and not only does she do science, she's in your department-- She does similar work. You've collaborated. And she must be undergoing the same kind of pressures and constraints that you do with funding issues and competition and teaching responsibilities-- How do you keep your lives balanced, especially when you're both scientists? Do you go home and talk about science over the dinner table? How do you maintain, or do you maintain, separate compartments in your life?

FAUST: We don't talk science over the dinner table. The only time we talk science outside of the lab is a couple of times a week, if we happen to walk to work together--which means walk to the train in Quincy, then ride it up in here--then we spend a little time talking about that. And I think that's just because we've gotten into the habit of it. The only time we talk science is when we're walking to work. Mostly at home, around the dinner table, we talk about administrative issues connected with the department and not science, like what's our feeling about maybe this new faculty hire? Or we discuss a problem student or an issue connected with spending departmental money, so sort of that discussion. And then, of course, it all gets discussed at the next faculty meeting, but at least we know how each one of us feels about it, and it's an opportunity to test our opinion.

So that's the extent of it. She takes work home and does it in the eve-- I go to work, most of the time, earlier than she does, and then stay later, because I do a lot of the work here, whereas she's inclined to take stuff home. We have a terminal there and she spends a lot of time, and does her reviewing and writing and letter compositions and whatever there, and I just do it here. So it's not that we're home all that much together. I don't get home until eight or nine o'clock, and she usually goes home about five or six [P.M.], and then I come to work about five or six [A.M.], and she comes to work about nine [A.M.].

Instead of working at home, I spend a lot of time thinking at home, sometimes in front of the TV, which is not the best-- I'm kind of a firm believer that ideas are kind of like seeds, especially in that you need to let them grow for a while, and then look away from them, forget about them for a while, and then come back to them and think a little bit more about it. Cows and horses, they have these multiple stomachs, they have this-- Ruminants?

MAESTREJUAN: Yeah.

FAUST: You just kind of--chewing on it all the time, and then you put it aside-- Don't they put that back in their stomach and then bring it back up again? All right. So to me, the best time to do that is in the evenings, with just a pencil and a piece of paper at the most, and then just write down things-- And then this pad I have at home-- And then I turn back to it and refresh my memory. That's the activity that I do at home in lieu of this paperwork, or whatever.

[END OF TAPE 6, SIDE 1]

FAUST: But she's a lot more disciplined in her approach to science. She's not fragmented in the very least, and she follows specific aims to the tee. She's had a good measure of success, but she had a funding lapse there, a couple of years ago, and struggled a little bit like everybody else. There's a threat that I will have that too. So she's been a faculty member longer than I am, because she came in '87, at the same I did, as an assistant professor, and so she's an associate professor now. So she's had funding. Not all that time, but I guess [she has] been exposed to a potential lapse in funding longer than I have. I haven't had the exposure.

But on the other hand, I admire her a lot. In fact, you could almost say that she's one of my role models, because she epitomizes some of the qualities that I'm lacking, and I know that if I don't shape up a little bit and follow her lead, then I may have some real problems. So it's good to have her image to look at, to emulate. There's a couple of other good ones too; we have some good role models for me in this department. But we don't talk much about science. We have lab meetings together; our labs have Wednesday noontime works-in-progress meetings, and that's all science. And that really is enough exchange of information.

We're not working on the same projects at all. She studies Niemann- Pick [type C]

disease, which is a problem in cholesterol transport, and I study this modified base in transfer RNA--which is not related at all to cholesterol--and then this other genetic disease which is not related to cholesterol metabolism, either. So it's not like that we can do common experiments. But we can still understand and be helpful in critically evaluating positively and negatively each other's lab's experiments. But still, because I have this long history in cholesterol metabolism, I can be more informative, or more helpful, just in drawing upon past knowledge, to the experiments that she's doing and the ideas that her work is putting forth or the directions it's telling her to go in than she can in mine, because she doesn't know as much about neurogeneration or isopentenyladenosine as I know about cholesterol metabolism. I think that helps both of us. It gives her an extra expert, so to speak, and it helps to keep me into something that I've always been very close to, and watch that field. That's stimulating.

MAESTREJUAN: Given your two schedules, when do you find time to see each other and get the laundry done and do grocery shopping and divide the--?

FAUST: It's not that difficult. I mean, we don't do anything else. Well, that sounds pretty bad, but to me, if you don't have children, then that opens up an incredible block of time. I don't understand how anybody can carry on the professional work that I'm attempting to do with a family, even with a supportive wife. You know, that building's full of those guys like that and women like that and I just don't see how they can do it. It's just impossible. But I guess they're probably better disciplined than I am, or whatever. But it's just not difficult to do the wash on the weekends. Just make sure you have enough clothes to go the whole week. [laughs]

Now, it does get a little harried-- She plays a little golf, but if I futz around with golf in the fall, then-- I get pretty busy at football season. That's pretty demanding. I have meetings two days a week, and then I oftentimes have to travel on the weekends to go to a game site, so for those ten weeks right there it's like an extra job.

She doesn't do that, but she has other activities. Gardening and bird-watching and the like, reading. She reads a lot more than I do. [It's] embarrassing; I don't read much outside of *JBC* [*Journal of Biochemistry]* or *Cell,* other than the Sunday paper, but she does do some pleasure reading. So, like I said, it's not difficult just to keep up with day-to-day activities if you don't have anything else besides work, and we fall into that category.

MAESTREJUAN: In order to be productive and successful and creative and everything else, does science now select against people who want to have children and a lot of responsibilities outside the household? But, then again, your life seems to be fairly interesting outside the lab, as well, by officiating, by golfing with the Tufts policemen that I met this afternoon. So it still seems to me that you must have to do a pretty good job of managing your time. You are unorthodox in your approaches and your own career path to getting to be a PI-- How do you see the pressures creating a scientist? How can scientists balance all of this, and does science require a certain type of individual to be successful?

FAUST: I don't think so, because scientists have always had to balance this kind of stuff, families and profession, and done a good job of it. I don't know what percent of Nobel Prize winners were married and had families, but I've got a feeling that it probably hasn't changed much in the past, say, ten years, versus the preceding twenty, or even time before that. So I don't think there's a selection process going on to say that better scientists are ones that don't have other responsibilities away from the lab, and I think that good scientists are coming about and can handle these other responsibilities. Some people, probably myself included, could not do a very good job of that.

I've never had children. If I had children at the time I was working at Joe [Joseph L. Goldstein] and Mike [Michael S. Brown] 's lab, then I probably wouldn't have been as successful and productive as I would have-- I wouldn't have worked as much, [it's] clear, or somebody would have suffered, either them, me, or my kids. But I don't think that's true for everybody. I think Laura could probably be just as successful with a family. She would just adapt. This was one of the first things we talked about, but what is a concern to me is that perhaps in the process of adapting, say this couple, this scientific couple that's got kids, that they don't have the time or the opportunity to have meaningful interactions with the rest of the public. So that kind of alienates them and alienates our profession from the rest of the public.

Now, if you're working fifty or sixty hours a week in a lab and raising a family, you're not going to officiate football games, or you're not going to spend a whole lot of time being an active member in the Massachusetts Audubon Society: I mean, active, not just going out and looking at birds for yourself, but organizing field trips and leading field trips and meeting with members and the like, which is meeting the public. Or in my case, it's being a football official and going to meetings twice a week with a hundred other professional men--all walks of life--and working towards a common goal.

They're seeing me there, and I 'm seeing them there, and we're sharing a little bit about what each one of us does, and knowing what their lives are like, I think, helps bring us together, like the Kiwanis Club, or the Lions [Club].

If you've got a family, and then you at the same time have to be competitive in our profession, or in grant funding or in biotechnology or whatever, then I just don't think there's enough time to do this other part, to be accessible and interact with the public, and so that's going to suffer. Obviously, it's the low thing on the totem pole compared to family and job, so that's a concern of mine, so I guess we're able to keep that up. I'd like to do more of it.

MAESTREJUAN: How committed are you to maintaining this public life?

FAUST: I'm very much committed to it, because it does give us an opportunity, or gives me an

opportunity, to see the diversity in life, in people. Scientists themselves are a selected subset of society. You've got to have certain traits, just like athletes are a selected subset. They're usually stronger, faster, taller. You don't see any fat athletes. So scientists are probably, by and large, I'd say, smarter than truck drivers. They probably prefer to work inside than outside. They may be a little bit more imaginative than other-- It's a selected subset of the population, so you don't have the exposure to people who like to be outdoors or people who may be more keen on mathematics and [those who] have intuitive sense. And that limits the potential in experiencing diversity in the population.

We don't have that exposure, so it's important to try to keep that exposure level high, just so we have a broad sense of what's going on in society apart from our little selective niche, that being science. You go to meetings, you don't go to political conventions. We go to scientific meetings. Everybody's a scientist. Everybody works full-time at being a scientist. It's not like the Kiwanis Club, a national Kiwanis meeting or Lions meeting or something like that, in which there's people that work full-time at something else.

MAESTREJUAN: Well, I could continue to ask questions all afternoon, but we're now at our time limit. At this point, I want to turn it over to you and ask, is there anything you want to add that we haven't talked about that you might talk about?

FAUST: I should have realized that question was coming up.

MAESTREJUAN: It's on the outline.

FAUST: It is? Oh, my word.

MAESTREJUAN: I'm just teasing you.

FAUST: Well, it could have been.

MAESTREJUAN: No, it is on the outline. It's the last thing that--

FAUST: Well, I didn't get the--

MAESTREJUAN: We don't give tests.

FAUST: I know. But I read it-- You know, I was better prepared the first day. But then when you said the second day that sometimes spontaneity was okay, then I didn't prepare last night. But let me just think a minute on this. I'd like to think that I had something important to say, because this whole process, not only this interview process, but the whole association with the Pew Charitable Trusts has been very important to me. Well, but I guess that doesn't really have any bearing on this, does it? Ah. Wait a minute. Something's germinating.

MAESTREJUAN: Okay. This is your interview.

FAUST: Right.

MAESTREJUAN: You can say what you want.

FAUST: So I-- I'm disappointed that I had forgotten this.

MAESTREJUAN: Well, do you want to pause the tape? Walk around a little bit?

FAUST: No, no. I'm just disappointed that I had forgotten until right now, and I'm certainly glad I remembered. Because it's really the most important feeling that I have about my whole experience in my professional career, and that is that I am really and truly amazed how very, very fortunate I've been. I don't think that I know of anybody that could have the luck that I've benefited from, to be able to more or less come from humble beginnings and with not much preparatory experience and to have the opportunities to be a part of and to witness the development of and the growth that was with Brown and Goldstein. And to learn from that, that environment that, once again, gave me more opportunities to go to graduate school, more opportunities to work at Du Pont, and then continued good luck and good fortune to come to Tufts. And to have smooth transitions as a graduate student and to be able to get a wonderful position with another smooth transition to a faculty appointment. When you stop and think about it, just this continually going on now for almost thirty years is amazing, and it's hard to think that I've done anything to deserve this. I know I haven't done anything to deserve this, except I've always worked hard, because I knew that's the only thing that-- That's what I felt like I could do the best at, was as long as I continued to put the time in and to think about what I was doing and just apply the things that I'd learned, that I could still continue to improve. But just the opportunities that I've had that so few other people have had is really awesome. And it's difficult to ever see repaying some of that, and not that I'm even going to try. So I'm just thankful that I remembered how grateful I am and hope that I'm not losing that feeling to the point of even coming anywhere close to being ungrateful. It's important to me to always remember what people have done for me and what opportunities have come about as a result of

just fortunate luck.

MAESTREJUAN: Well, I have been very fortunate to interview you, and I thank you very much.

FAUST: It's been a pleasure, Andrea.

[END OF TAPE 6, SIDE 2]

[END OF INTERVIEW]

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