

*John C. Liebeskind History of Pain Collection*

**Oral History Interview  
with  
John C. Liebeskind**

Ms. Coll. no. 127.21

Conducted: 17-19 August 1995  
Interviewer: Marcia L. Meldrum  
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History & Special Collections Division  
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## Biographical Sketch

John C. Liebeskind was born in Connecticut in 1935. He graduated from Harvard University in 1957 and earned his Ph.D. in physiological psychology from the University of Michigan in 1962. Following two years as an Instructor at Michigan, he received an NIMH Postdoctoral Fellowship to work with Denise Albe-Fessard at the Institute Marey in Paris. In 1966, Dr. Liebeskind accepted a faculty appointment in the Department of Psychology at UCLA, where he remained throughout his career, running an extraordinarily creative and productive laboratory. His laboratory is perhaps best-known for the description of the phenomenon of stimulation-produced analgesia and for the demonstration that persistent pain promotes tumor growth in animals. Dr. Liebeskind became a Full Professor at UCLA in 1978 and was elected to membership in the National Academy of Sciences in 1995. In 1992, he turned his interest to the history of pain and founded the History of Pain Collection at the Louise M. Darling Biomedical Library at UCLA; the collection was renamed for him in 1997. A founding member of the International Association of the Study of Pain (IASP), he organized the first meeting of the United States' Western Pain Society in 1975; and he was a member of the Steering Committee which implemented the 1977 merger of the Eastern Pain Association and the Western Pain Society to form the American Pain Society (APS). Liebeskind served as President of APS in 1990-91, and was President-Elect of the IASP when ill health forced him to resign in July, 1997. He died of cancer at his home in Los Angeles on September 8, 1997.

## Interview History

Dr. Liebeskind was interviewed at the offices of the UCLA History of Pain Project in Los Angeles, California, by Marcia L. Meldrum on July 17 and 19, 1995. The interview lasted approximately 5.0 hours. The transcript was audit-edited by Marcia Meldrum and reviewed by Dr. Liebeskind prior to its accession by the History of Pain Collection. The tape and transcript are in the public domain, by agreement with the oral author. The original recording, consisting of four (4) 90-minute audiotapes, is in the Library holdings and is available under the regulations governing the use of permanent noncurrent records. Records relating to the interview are located in the offices of the History & Special Collections Division.

## Topical Outline (Scope and Content Note)

The interview is organized chronologically and then topically, beginning with Liebeskind's childhood, early schooling, education at Harvard, decision to enter physiological psychology, and postgraduate training with Stephen Fox at Michigan; and continuing on to discuss his years in Paris with Madame Denise Albe-Fessard; his recruitment to UCLA, developing interest in pain, and the pioneering work on stimulation-produced analgesia in the periaqueductal gray matter; the Issaquah meeting of 1973, Liebeskind's work in pain organizations, and his relationship with John J. Bonica; the growth of his laboratory and his relationships with students; the decision to close the lab and to research the history of pain; his family and personal life; and election to the U.S. National Academy of Sciences. Major topics of interest include

Liebeskind's thoughts about scientific "style" and mentorship; the impact of the 1971 *Science* paper discussing stimulation-produced analgesia; the "multidisciplinary spirit" and the effect of an organization like IASP on its members; the character and achievements of John Bonica; Liebeskind's assessment of the gate control theory; his reflections on science and the pain field today.

## Access to the Interview

This oral history interview, in its audio and transcript forms, is held by the History & Special Collections Division. Those wishing to use the printed transcript (which is available through Interlibrary Loan) or the audiocassette version (which is available by appointment only) should contact: History & Special Collections Division, Louise M. Darling Biomedical Library, UCLA, Los Angeles, California 90095-1798. Phone: (310) 825-6940.

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## Citation Information

The preferred citation for excerpts from this interview is: *Oral History Interview with John C. Liebeskind*, 17-19 July 1995 (Ms. Coll. no. 127.21), John C. Liebeskind History of Pain Collection, History & Special Collections Division, Louise M. Darling Biomedical Library, University of California, Los Angeles.

## Related Materials in the *John C. Liebeskind History of Pain Collection*

The researcher is referred to the following related materials: oral history interviews with Denise Albe-Fessard, Jean-Marie Besson and John J. Bonica; American Pain Society Records (Manuscript Collection no. 123); and the John C. Liebeskind Papers (in process).

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John C. Liebeskind, PhD

*Physiological Psychologist*

## JOHN C. LIEBESKIND INTERVIEW

### TAPE ONE, SIDE ONE

MARCIA MELDRUM: Yeah. Well, both needles [on the tape recorder] are moving just fine.

JOHN LIEBESKIND: Good. Good.

MELDRUM: Okay! So today is July 17th, 1995, right?

LIEBESKIND: So it is.

MELDRUM: And we're here at the History of Pain Project office on the UCLA campus for an interview with Dr. John C. Liebeskind. The interviewer today is Marcia Meldrum. Dr. Liebeskind, thank you for doing this interview.

LIEBESKIND: And it's 10:20 in the morning. [he laughs]

MELDRUM: Ten twenty in the morning. Thank you for reminding me. I'd like to begin by just asking you to tell me a little bit about your family background and your early life, and if there's anything during those years that you think might have led you into your career as a psychologist, as an experimental psychologist, and into the field of pain.

LIEBESKIND: Yeah. Well, let's see. Well, to start, I was born February 2nd, 1935, at 1:20 in the morning. Now, you laugh at 1:20 in the morning --

MELDRUM: [she laughs]

LIEBESKIND: -- but, actually, that's terribly significant in my life, because what it meant was that there was a deadline of being born on February 1st for getting into kindergarten in Connecticut at that time -- this was Waterbury, Connecticut -- and I did not get into kindergarten that year, even though my mother took me to the school and had me read for the teachers to show me how smart I was. And the teacher, however, wisely, took the book, turned it upside down, and I continued reading. [both laugh]

I had memorized it; I wasn't reading at all! So my very first academic test I flunked, and I did not get accepted to kindergarten that year, which I make a joke out of, but in fact it might have had some bearing on my life in the sense that through the formative years I was the oldest student --

MELDRUM: In the class.

LIEBESKIND: -- and one of the biggest in my class, even though I'm not a person of large stature now, but I matured early. And as a young boy, especially in my fifth, sixth, seventh, eighth grade, I was one of the larger kids around, perhaps more mature than some of them or

whatever. That may have had a bearing on my life [he laughs], all for an hour and twenty minutes. Anyway, yeah, I was born in Waterbury, Connecticut, and my family consisted of my mother and father and an older sister and two bachelor uncles and a grandfather -- my mother's brothers and her father -- all under one roof.

MELDRUM: Wow, so it was a big household.

LIEBESKIND: It was a big household, and I went to the local public school through fifth grade, and then in the sixth grade, I believe it was -- yeah, sixth, seventh, and eighth grade, I moved over to a little private school but also walking distance from my house, a little bit farther, but not that much. I could still walk there; it may have been a mile or whatever, through which I trudged in --

MELDRUM: Snow and ice. I know, I know!

LIEBESKIND: -- snow up to my eyeballs! [he laughs]

MELDRUM: And rain and sleet.

LIEBESKIND: Yeah. And this little private school, called the McTernan School, was very good, and I was exposed to all kinds of new things educationally. It was a tiny little school; there were five in my age group.

MELDRUM: Wow! Coed? Boys' school?

LIEBESKIND: No, it was boys. Yeah. And I think there were only like four teachers, and, you know, there were larger classes below me, but for some reason there were only five in my class. And I took English composition and English grammar; they were taught by different teachers. I started taking French and Latin.

MELDRUM: Wow! Pretty impressive.

LIEBESKIND: My earliest recollections of switching over to that school were several of my friends were there, so we were like buddies, so I wasn't lonely or anything like that, from that standpoint. But my earliest recollection was that I was terrified that I wasn't going to be able to do the work properly.

MELDRUM: Right. To keep up.

LIEBESKIND: And it was very, a lot of work, and I wasn't on top of things; I was like behind, and I cried. And, you know, I came home crying for the first few days. And I was a terrible speller, and my mother had one of the teachers come over and give me some remedial spelling lessons. [he laughs] And that went on for a little while. And then things sort of gradually improved.

MELDRUM: And you began to catch up?

LIEBESKIND: Yeah. It ended up a very positive experience. I was there for three years, and went from there to the Choate School, in Wallingford, Connecticut, which is about fifteen miles, twenty miles away, closer to New Haven, Connecticut.

MELDRUM: Right. And that's quite a famous school.

LIEBESKIND: That's a very famous school. That's a boarding -- well, you know, it had boarding and day students, but twenty miles, you wouldn't think to be a day student; that's a horrible distance.

MELDRUM: [she laughs] Too far to trudge.

LIEBESKIND: Ah, much too great a distance. Funny about how distance is different in time, plus Connecticut versus California. Anyway, yeah, so I was a boarding student there for four years, and that, too, didn't start out so great but ended up -- I think I did a little crying [he laughs] when I first went there, too.

MELDRUM: Sounds very hard.

LIEBESKIND: Yeah, well, yeah, there was the thing at that time to send your children away to summer camps, and, you know, I was seven years old or something when I went away for eight weeks to summer camp, and cried most of that. [he laughs]

MELDRUM: Yeah. Yeah, I remember that.

LIEBESKIND: And then came to love that. So it was sort of all this -- but at Choate, I really got a very good education. I had a favorite teacher there, Mr. John Joseph. We called him "the Arab"; he was actually Lebanese, very swarthy, marvelous character.

MELDRUM: What was his subject?

LIEBESKIND: He taught Latin, Greek, and English, and various classes. I actually had a little bit of Russian with him, some Greek, I don't know if I took Latin.

MELDRUM: My goodness. So are you still fairly fluent in Greek?

LIEBESKIND: No, no, no, no, no! Oh, no, I never learned much Greek. That was just -- in the end, I was his only student of Greek, and I'd just sit in the little apartment in the dormitory and we'd talk mostly about etymology, the origin of words, and so forth. I go into mentioning him in some detail because he really was, I think, a very major influence in my life. He kind of pointed me towards a more academic world, a world of knowledge, of learning, and so forth in a way that I had never been oriented before.

I mean, my parents were educated but not vastly educated; they'd each had some college. My mother had gone to a two-year, what they called at the time finishing school, which we would



now call junior college, I suppose. And my father had gone to the University of Michigan, but he didn't finish; he joined World War I before finishing, and he never went back to college. So, you know, and one of my uncles, these bachelor uncles, one of them had had one year of college and then quit, and the other one did go through and went in fact to Harvard Law School and practiced law, but he was the first real college graduate, youngest of that generation, my mother's youngest brother.

MELDRUM: He's the one that's still alive?

LIEBESKIND: He's the one that's still alive, yeah. Yes, he is. He's eighty-whatever, one or two, something like that.

MELDRUM: Your father and uncles were in business together, or was it just --

LIEBESKIND: No, that was the two different sides of the family. My father had, his father had started a clothing store, I remember, and my father was the eldest of five children and became the main person there, running that store, and ended up running it with his youngest brother, but my father was really very much the senior partner, the boss of his youngest brother. And my mother's family, my mother's father, had a furniture store, and her next-youngest brother ran that. The one who got trained as a lawyer [the youngest one], he never practiced law; he went into the furniture business for a little while; that didn't take. He sort of had a number of jobs and ended up working in an unemployment office, on the correct side of the desk. [he laughs] But he was, and remains, a cultured fellow and interesting and bright and so forth.

So, anyway, you know, I had, we had all the right academic values at home, but it was this Mr. Joseph who kind of excited me about learning. And I think he sort of on purpose tried to turn me away from going into my father's business. Something my mother resented -- although my father didn't, but my mother resented it -- he [Mr. Joseph] was twisting my mind, you know, which I think he was. I don't remember exactly how much we talked about that directly, but I think we did some, and he wanted me to go to Harvard, which had been his alma mater, and in fact I was admitted and I went to Harvard. That was, unmitigated, a lovely experience. I enjoyed being in college.

MELDRUM: Yeah. I would think that would be a terrific place to be. Well, let's go back on this a little bit. When you were at Choate, Mr. Joseph was a languages teacher primarily. Did you have science courses as well, and did those interest you?

LIEBESKIND: Well, as you'll see, I didn't really get interested in science until graduate school. And -- well, we'll come to this again. But, no, I would say my orientation was all the other direction. And it almost was --

MELDRUM: Gosh, you could have been a historian! [she laughs]

LIEBESKIND: That's right! [he laughs] It almost was the sort of two cultures thing, and I think children, young people, were very aware that either you were kind of toward the science end of things or you were towards the humanities, the arts, the whatever. And I definitely --

MELDRUM: Exactly. Either one or the other.

LIEBESKIND: Yeah, and I was definitely not in the science camp. And in high school, at Choate, I had the minimum amount of science that one could have. I had a course in chemistry and a course in biology, and I might have opted for a course in physics, but I didn't take it; I didn't have to, and I didn't. I had three years of my four years there of math, which, the way they did things, took me through geometry, but not trigonometry or calculus. That would have been in the fourth year. I was allowed to drop out of that; in fact, I think that's what the Greek -- that's what [he laughs] messing around with Mr. Joseph, screwing around with Greek, I think, permitted me to get out of that fourth-year math, which I opted to do. So when I went to college, I really had not very much background in science.

MELDRUM: No. You had a humanities background.

LIEBESKIND: Definitely oriented in that direction, which is a little bit amazing, as I think about it, that a school as excellent as Choate would have permitted that much specialization at that time. You would have thought, and I strongly suspect now, you can't even do that in school.

MELDRUM: No, I don't think you could. And any good school would want to prepare you as an all-around person.

LIEBESKIND: I would think so.

MELDRUM: That's what colleges are looking for.

LIEBESKIND: I think they gave me choices, and I took choices which, in the long run, I regret. Well, I *should* regret -- [he laughs]

MELDRUM: Should regret. [she laughs] Anyway!

LIEBESKIND: So I think another very formative experience for me as a young person was this summer camp that I went to for -- the first year I went away to summer camp, it was at another one, and then I switched over to this camp the next year. I think I was nine years old when I started it, I think I would be, at a camp called Kennebec, named after the river, and it was up in Maine, near Waterville, Maine, and I went there forever.

MELDRUM: Every summer.

LIEBESKIND: I went there every summer from the age of nine -- did I say nine?

MELDRUM: Well, you said seven, I think.

LIEBESKIND: Well, I think that was the other camp [Camp Wigwam, also in Maine]. Maybe I was eight. And then I was nine, ten, and eleven, at the other camp [Kennebec]. And from then on -- they had a junior camp; I was there three years, and that's how I remember it -- nine, ten,

eleven. And in the senior camp, you were started at age twelve, and you crossed the lake from the other one. And that was twelve through sixteen. And as if that wasn't enough, I stayed on at seventeen and worked as a waiter again! And at eighteen and nineteen, and I don't remember -- I may have been there -- I've forgotten now exactly -- at least two more, maybe three more years, as a counselor.

MELDRUM: Wow. So that was like your summer home.

LIEBESKIND: Yeah.

MELDRUM: Was it like an all-summer camp?

LIEBESKIND: Yes, it was an eight-week camp. And a wonderful camp and very -- a lot of spirit, you know, camp spirit and tradition and so forth and so on. And one of the traditions there, embodied by the director in the years when I was a counselor there, a fellow named Tom Wiener -- I was just remembering him the other day -- it was spelled Wiener, but it was pronounced "weener", W-I-E-N-E-R; and he was a very, another influential person because, I don't know, he was very into -- we got that the older, the counselors, have to be helpful to the boys and kind of psychologically aware. Not just, "Get out there, and here's the way you hold your baseball mitt," but kind of psychologically aware, helping them that were troubled, and so forth -- not that he was a therapist or anything; I don't think he was -- I mean, I know he wasn't. But I think through his influence, I kind of got oriented toward helping younger children, which, I think, crystallized some thought that had been building in me for a long time.

Actually, when I was *very* young -- gosh, I don't know; maybe even like twelve years old or something -- I had a little group of neighborhood kids that I was far and away the oldest, and they were like -- I had some regular friends, and then there were these others, and I would pal around with them a little bit; they were like three or four years younger than I was, or two or three. And I was kind of helping them. So there was this concept of being a mentor or being someone helping younger kids, and I think that crystallized during this camping experience as a counselor when I was in my last years at Choate and my first years at Harvard, when I was doing this counseling thing.

And I think I made a decision at that time of sorts; if anyone would have asked me then, "What do you want to be when you grow up?" "A camp counselor and a teacher," you know. I mean, "I'll be a camp counselor during the summer because that's what I really want to do, but meanwhile I'll be a teacher the rest of the year, because that's how I'll earn my living." And if I had enough money, you know, maybe I would buy a camp or buy into a camp and become a camp director and counselor.

MELDRUM: Oh! So you really enjoyed that. You enjoyed being in the out-of-doors?

LIEBESKIND: It was very meaningful. The outdoors was okay, but the real meaningful part was the working with kids and helping children grow up.

MELDRUM: And the kids looked up to you, and you saw that.

LIEBESKIND: Right. Right. I was a good counselor, yeah. And so I think that was all very formative. And when I went off to college, therefore, I wanted to learn about psychology. Now, at Harvard at that time, there were sort of two psychology departments, one called Psychology, but it had split the usual grouping of different kinds of psychologists; after World War II, it historically had split at Harvard, and a second, another much larger, department got formed called Social Relations, the Department of Social Relations, which included not only -- do you know about that?

MELDRUM: No, I'm just preparing to ask a question. But I do know something about it, but go ahead.

LIEBESKIND: Okay. Did you want to interrupt for the question at this point?

MELDRUM: No.

LIEBESKIND: The Social Relations department was social psychology, personality psychology, clinical psychology, some parts of sociology, and cultural anthropology, all run in together. So most but not all, of the famous names that you would think of in relation to psychology were at Harvard in Social Relations.

MELDRUM: But [B. F.] Skinner [1904-1990, pioneer of operant conditioning], though --

LIEBESKIND: But Skinner was in psychology.

MELDRUM: And I think wasn't that part of what caused the split, was there were Skinnerians and then there was another group --

LIEBESKIND: Well, it was more than just Skinnerians, but it was that what we sometimes call hard-nosed psychology versus soft psychology or whatever; but it was split down those lines. It wasn't just a random division of a bunch of people; it was split between learning psychology, experimental, physiological psychology, such as it was, and that was all standard psychology; and then all the kind of the softer, more humanistic, end of things became Social Relations. Well, I took a psychology course, an introductory course, when I got there, and I took one with Skinner also later. But I wanted to be a Social Relations major, not a Psych major, and I was. And so I went all through in Social Relations, and, again, a pattern recurred in that at Harvard, just as at Choate, you were allowed to make options with respect to how much science you got, and I took again the very, very minimum.

MELDRUM: [she laughs] This is great! I love it.

LIEBESKIND: It's a sort of a joke, because -- it's a story I've told many times -- but I satisfied the under, the lower-division science requirement by taking a course with I. B. Cohen, on the history of science.

MELDRUM: Oh! You took a course with Bernie Cohen!

LIEBESKIND: As a freshman in college.

MELDRUM: That's wonderful.

LIEBESKIND: I don't remember much about it, but I did. And I satisfied the upper-division science requirement by taking a course in "The Science of Human Behavior" by B. F. Skinner. And that was all the science I had in college.

MELDRUM: [she laughs] So you never went and sat inside a laboratory.

LIEBESKIND: Never.

MELDRUM: Wow.

LIEBESKIND: So there was another kind of watershed experience that I had in college that, again, is a story I've told many times, and I make a joke out of it but it's actually very serious. It was a time in my, in the fall of my junior year of college, and I'd been kind of cruising along doing barely minimal performance --

MELDRUM: Let me just get the years straight here. You went to college in --

LIEBESKIND: 1953.

MELDRUM: Okay. So the fall of your junior year, that would have been 1955, about.

LIEBESKIND: That's right.

MELDRUM: Right, two years later. And you said you went to Harvard. Now, Harvard is generally considered to be a highly competitive school; I take it you didn't have much difficulty getting in.

LIEBESKIND: Well, that's a story in itself, too. I used to characterize myself as having spent my whole life at the lowest rung of the highest ladder. [he laughs] And there was some truth in that. I'd gotten through Choate cum laude, but I was the last one admitted in, at the twentieth percentile or whatever. And so, I mean, I did well at Choate, but certainly not brilliantly. And remarkable as it seems, but it really is true, the man who was in charge of college admissions at Choate was retiring the year that I was applying to college, as was the man who was in charge of admissions at Harvard --

MELDRUM: That's amazing.

LIEBESKIND: -- and they were classmates.

MELDRUM: My goodness, John! [she laughs]

LIEBESKIND: And that was a bang-up year, and I've forgotten what it was; I think we got twelve people or something that year [into Harvard], from a graduating class of 125 or whatever it was at Choate. And when I went to have my interview at Choate with the man who was in charge of admissions --

MELDRUM: With the Choate man from Harvard.

LIEBESKIND: -- he said, "Where do you want to go to college?" And I said, "Well, I'd love to go to Harvard," and he said, "Fine. That's it. You're in."

MELDRUM: Wow! [she laughs]

LIEBESKIND: "Where else should I apply as a backup?" He said, "You don't need to. You're in. You will be admitted to Harvard. Just apply to Harvard." So I never heard of a story like that before.

MELDRUM: That's sort of a classic old-boy-network story.

LIEBESKIND: That's it. Because, again, my grades, I don't know what tests, but I suppose I'd taken the SATs or something, but I never heard what they were; I don't know what they were. But I know from much later from applying to graduate school that my GREs were not spectacular, so I assume my SATs weren't either. So, you know --

MELDRUM: But even with all this pressure, you'd have to have achieved a certain level, I would think, or they wouldn't --

LIEBESKIND: Well, I did at least okay, but it was amazing. So, yeah, I went to Harvard. [he laughs]

MELDRUM: Wow. So that's amazing. Okay. So I interrupted. You were talking about the fall of your junior year.

LIEBESKIND: Yeah. Something, there was a weekend that I spent. I went down to Sarah Lawrence College on a date with my old girlfriend, Diane Cassel, who I'd grown up with in Waterbury, and she and I had had this sort of never very satisfactory relationship; I always sort of worshipped her from afar, not afar, but we were a small group, but I always wanted her to be my girlfriend, but she was always playing the field.

MELDRUM: [she laughs]

LIEBESKIND: And she didn't want to be my girlfriend. So -- I don't know; I think maybe something maybe happened in college and it started, went a little bit the other way at that point or something; I don't know. And so, you know, the thought that she got more interested in me of course made me less interested in her or something. I don't know what happened; I can't remember. But anyway, I went down for the weekend -- I don't even remember what it was, a formal thing at Sarah Lawrence or something; I just went down, whatever. And on the way back

after the weekend, I made three momentous decisions, rank ordered in the following way. [he laughs] Well, I don't know how you would rank order them; that's an interesting question. The first one was I wasn't going to really date Diane, not that we weren't --

MELDRUM: You weren't breaking up --

LIEBESKIND: It wasn't breaking up.

MELDRUM: You were just cooling it off.

LIEBESKIND: Yeah, just like -- but that, I sort of felt like something, a separation, a sort of an end of Diane; I don't know whether she figured that'd happen. A second thing, which is a little embarrassing and has been the butt of many jokes against me, but I'll tell the story anyway, is that I decided to give my car back to my parents. I had a little Volkswagen, beautiful little black Volkswagen with beautiful leather -- not leather, red leatherette upholstery, and the little flippers on the side --

MELDRUM: Oh, yeah! My gosh!

LIEBESKIND: -- for the signals [that VW had] in the '50s, and I'd gotten that Volkswagen as a gift from my parents because in my sophomore year, starting with my sophomore year, I had agreed to stop smoking. It was a very big thing in my family. There was always a bottle of scotch; people would come over, "Would you like a drink, John?" "Would you like a sip?" you know. Alcohol was never a thing in my family. Smoking was a thing, although my father had been a smoker and ultimately died of lung cancer -- this was later -- and he wasn't even sick at the time that all this happened to me, but for some reason he had given up smoking, and my mother just really hated the whole idea that I would smoke. So, if I would give up smoking, which I started as a college freshman -- in Choate, if you smoked, if you were seen a hundred yards in proximity to a cigarette, you were thrown out.

MELDRUM: Really?

LIEBESKIND: *Thrown out.*

MELDRUM: My golly!

LIEBESKIND: Yeah. People would go, you know -- I mean, I once smoked a cigarette while I was a student at Choate in the men's room of a hotel in Hartford, Connecticut, thirty miles away, when I was up there for a dentist appointment [Meldrum laughs], shivering, you know, that somebody was going to come busting in.

MELDRUM: Somebody from Choate!

LIEBESKIND: Somebody was going to come busting through the door [he laughs] of the men's room and catch me. You know, there were stories. So, yeah, so I never really smoked at Choate, but as soon as I got to Harvard, I started smoking. And my mother said she would give me a car

if I would stop smoking, and I agreed to that. And I stopped smoking for a year. But at the end of that year, sophomore year, she said, "Well, look, we were going to give you a used car, but I think that really, you know, used cars are other people's troubles, and who needs that? There's this wonderful car," -- this was a telephone conversation up to Harvard -- and she said, "a wonderful car called a Volkswagen; it's so cute, and they have this black one with red upholstery and little direction signals, and we've seen it, and it's new. But we will buy you that car, even though it's a new car, if you'll give up [smoking] for a second year." And I said [makes panting noise], "Okay!"

MELDRUM: "I can hardly wait!"

LIEBESKIND: So I got the car, and I started smoking. And my mother heard about it somehow, that I was smoking -- I don't know, some mutual friend said something -- I don't remember how. She accosted me at once over the telephone. She said, "I hear you're smoking." I said, "Yes." She said, "Well, you promised." I said, "All right, all right, all right. I'll give up smoking. Okay!" So I stopped smoking. On that trip back from Sarah Lawrence, I said, "Screw this. I've had enough of that nonsense. I'm going to start smoking again, and I'm going to give the car back." And I did, and I did.

MELDRUM: Well, it was very honorable of you to give the car back.

LIEBESKIND: Well, I felt I had to do that at that point. I'd been pretty honorable.

MELDRUM: Did you feel like this was sort of an assertion of independence?

LIEBESKIND: I think that's exactly what it was. And the third decision, the third thing that happened on that watershed trip -- [he laughs]

MELDRUM: This was some trip! [she laughs]

LIEBESKIND: -- was I decided I wanted to go to graduate school in psychology. And so all this sort of came together. Well, you know, in my view, I'm sure yours, that these things don't just pop out of nowhere. I hadn't been aware maybe that I had been working on these things, but somehow all of a sudden an environment of decisiveness struck me, which has sort of been characteristic of my life, I must say. I mean, I've kind of gone along with most things; I'm sort of easy-going.

MELDRUM: And then there's a period of time --

LIEBESKIND: You know. And every once in a while, some major thing happens, I make a major decision, a life-transforming kind of major decision; there have been a few of those. And this was certainly the first, and, you know, the first of these. So, coming back to Harvard, then, the idea was that I had to qualify myself for getting into graduate school. I mean, I'd been kind of going on pulling Cs and B minuses and whatever, and I definitely put on a spurt by taking courses that I thought I could do well in, working a little harder than I had.



I got into the honors program. I didn't -- well, let's see; how did that work? You could either graduate from Harvard with honors either because your grades qualified you for that, or, because, with some minimal grade average, you could opt to do an honors thesis. And I did an honors thesis. My grades were not so great that I was going to graduate with honors with them, so I opted. But they were good enough that I could opt to do an honors thesis, which I did. And there were some other parts to that; I had my honors advisor and I had to meet with her, something, something, I don't know.

So, anyway, I did graduate -- probably at the very bottom of *cum laude*! [he laughs] Not *magna cum laude*, not *summa*, but at the bottom of the *cum laude* list. And a year later, in my senior year when I was applying to graduate school, there was actually one professor I had there who thought very well of me, and he put me in for a Woodrow Wilson [Fellowship]; he wanted me to go to Stanford, which is where he had come from and so forth. I didn't get the Wilson, I didn't get into Stanford; in fact, I didn't get in anywhere. And I was papering my walls with these rejection slips. And then I finally got admitted at the master's level at Boston University.

MELDRUM: Oh! I know that school.

LIEBESKIND: And I said, "Ugh! That's the best I can do?" Well, there was one school I had actually been advised *not* to apply to, the University of Michigan. And miraculously, even though my wall had been papered with these rejections, I had a call from Wilbert J. McKeachie. Dr. McKeachie was in charge of admissions of the graduate program.

MELDRUM: Spell it.

LIEBESKIND: M-C--I don't know. Keachie? How do you spell Keachie? [he laughs]

MELDRUM: All right. We'll look it up. Go on.

LIEBESKIND: Wilbert J. McKeachie. And, yeah, he's well known. He subsequently became president of the American Psychological Association [1975-76], very well-known author of a textbook, and so forth. All right. He was a fairly young professor at Michigan but was in charge of admissions. And he called me up and said, "There's good news for you, you're on our wait list."

MELDRUM: So you had applied even though you were told not to.

LIEBESKIND: I did apply even though I was told not to, yeah.

MELDRUM: You just applied everywhere.

LIEBESKIND: Well, not so many places; you know, maybe eight or ten places. I did throw in one at Michigan. And so I got put on the wait list. They said, "We're going to put you on the wait list; are you still interested?" And I said, "Yes, I am." So, he said, "Fine." And he said, "Send us your test scores." Because I think they didn't require test scores at that time or something, only grades, at that particular school or something. It was something unusual. And I

sent in the test scores thinking, “Oh, well; now I’m really fried,” because my GREs weren’t that good, although my Miller Analogies Test, you had to do that too, that was a bit better.

So, despite the fact that those scores were not so great, and maybe even before they got them, I don’t recall, just a few days later, he called me again to tell me that I’d been admitted; someone had dropped out ahead of me and I was being accepted off the wait list. And I remember this conversation; it began a story that I’ve told many times. Maybe it didn’t really happen, but I’ve told it so many times it’s taken on a reality of its own, that he said, “We’re of course very eager to learn of your decision as soon as possible, and I know you must have been admitted to many other schools” [he laughs], and so forth. And I said, “No, I’m coming!” He said, “Oh! How do you know you’ve already made up your mind?” I said, “Oh, I wasn’t admitted anywhere else!” [both laugh]

So he took that with good grace, and he said, “Well, all right,” and he said, “What kind of support would you like?” you know, teaching assistantship or research assistantship, fellowship, or something. I said, “Oh, no,” I said, “you don’t understand. I’m not a scholarship case.” I said, “My father can afford to pay for this.” He said, “*You* don’t understand!” [he laughs] “*Everybody* gets something. This is not scholarships we’re talking about, financial aid.” He said, “All our students are supported here.” I said, “Oh. Well, whatever you’ve got.”

So [he laughs] I was assigned to a Veterans Administration fellowship of some sort. I didn’t have to do anything; it wasn’t a teaching assistantship; I wasn’t working with anyone in particular. When I got to Michigan, I was working at the VA, and that, again, led very shortly to a major kind of decision; because I went to graduate school in clinical psychology, and that was my avowed interest -- not that I wanted to be a clinician; I knew I didn’t want to be a clinician as such. I had worked for three years at Harvard at a mental hospital as a volunteer.

MELDRUM: Oh, good for you.

LIEBESKIND: Yeah. And I did organizational work, very similar to my later M.O. as a scientist. I didn’t so much work with the patients as I did help organize the volunteers. I mean, I did go out there and I would walk around the wards and stuff, but I didn’t do as much on the wards as the ordinary volunteer. I rapidly got into kind of the administration of organizing the volunteers -- speaking with the college students about what we were doing, and trying to recruit more volunteers, and arranging rides, and giving tours of the mental hospitals to prospective volunteers, and this kind of thing. And I knew, you know, “Oh, I don’t want to work in this kind of field.” I wanted to do research.

MELDRUM: In clinical psychology.

LIEBESKIND: Yeah. I think if I had known what it was all about, I probably would have applied in personality psychology, as opposed to clinical psychology. But I don’t think I knew the difference.

MELDRUM: Well, were you still thinking about perhaps being some kind of a counselor?

LIEBESKIND: No, I think at this point I had, I think at that decision moment, you know, coming back from Sarah Lawrence, I think I just had ratcheted up my ambition from a high-school teacher to college teacher. I think that's what going to graduate school signified to me; I would still teach, but at a higher level. I think I was off summer-camp counseling by that time. [both laugh]

MELDRUM: You grew out of that.

LIEBESKIND: In fact, I'd grown out of that, I think, by then. So, in the -- just barely into my first year of graduate school -- my father was dying at that point. He died in November -- no, early December of my first year in graduate school. I don't think that had anything to do with anything; maybe it did, I don't know. But I did decide before that, maybe around Thanksgiving time, that I wanted to switch from clinical into physiological psychology. And I walked into the chairman's office, and I said, "How does one go about switching from one area to another?" I told him I wanted to switch; he said, "You just did it." So now I was in physiological psychology.

MELDRUM: Life was so much easier back then.

LIEBESKIND: Yeah! Well, the borderlines -- even today, the borderlines from one area to another in psychology are not so firmly fixed, except going into clinical. You can go out easily enough, but you can't get in. They want to make sure you're not crazy or whatever or you could screw up in clinical psychology. So, anyway, there are a number of influences behind that, but I did opt to go into physiological [psychology] at that point and started taking some appropriate courses. I took a medical school course in physiology, both because it was sort of the thing to do if you were a physiological psychology student, and because it got you out of the second language requirement. I only had one language; I did speak French pretty well, from Choate.

MELDRUM: Well, now, but wait a minute. I mean, you know, this is a big, it's a big move for you, and you must have known that.

LIEBESKIND: Yeah.

MELDRUM: I mean, to move from clinical into physiological meant that you would have to move into the laboratory --

LIEBESKIND: Right.

MELDRUM: -- which had not previously [she laughs], had not been a focus of yours at all.

LIEBESKIND: This is very true.

MELDRUM: So, what was driving you? Or the influence there?

LIEBESKIND: Yeah. I think there were three things that I recall that influenced me, one in the negative sense. Or maybe four things. One in the negative sense was a course that all clinical

students had to take, testing and how you administered things like intelligence tests. And there was a schoolmarmy teacher, a woman who taught that course, who made us learn the protocol for giving, for administering the Stanford-Binet by heart. You couldn't just write it; you had to have a subject in front of you; "Now, put these beads on there; put the red bead on first;" you had to memorize that. Well, I hadn't memorized anything since I'd been at Choate. I never thought I had a good memory, and I was scared that I wouldn't be able to remember it, memorize it.

MELDRUM: And you would flunk! [she laughs]

LIEBESKIND: And I was offended. I was deeply offended that anyone would ask me to memorize anything. So I wanted to get out of that course. I also was sort of tired of that kind of research, this kind of personality, clinical research. I was doing something at the Veterans Hospital, with some supervision there, that was kind of following up on my undergraduate honors thesis.

MELDRUM: Which had been on what subject?

LIEBESKIND: It had been on the perception of mentally ill children by hospital attendants.

MELDRUM: Him! Interesting.

LIEBESKIND: And, yeah, it's kind of an interesting thing. My thesis, because of who I worked with on it, was oriented around what was called the authoritarian personality. There was a famous book, and there was a kind of tradition within -- a mini-tradition, and it all had to do with fascism and studies that followed on World War II. And so the people I worked with were [working] on that scale, fascism scale, authoritarian personality domain, and I thought that was neat stuff. And so the work I was going to do in graduate school was pursuing that. And I decided, you know, "This is so soft. You can't really grasp this, and, I don't know, it's just too vague." So there was a negative thing there, too. I was kind of getting, finding myself becoming intolerant of that level of abstraction in research.

And then on the positive-pole side was my advisor, the guy I was assigned to as a first-year graduate student, a fellow named James V. McConnell. J. V. McConnell was quite a personality in psychology, very well-known. He'd been a science fiction writer before he became a psychologist. [he laughs]

MELDRUM: Really! My kind of person.

LIEBESKIND: And he became a very famous person because he did the work on so-called transfer of training; that you could train flatworms, and condition the flatworms, and then you could take the RNA of the conditioned flatworms --

MELDRUM: And transfer it to another flatworm.

LIEBESKIND: -- inject it into another worm, and there would be savings in teaching that other worm that task. That other worm wouldn't have to learn the task; it wouldn't take as long, as it were, and he also did cannibalism. He would grind up the trained flatworms and feed them to the untrained flatworms. That was pretty dramatic and sci-fi.

MELDRUM: It does sound sort of more sci-fi than anything else.

LIEBESKIND: Yeah. I think it turned out to be just that, but it was pretty hot stuff for a while. Well, even before, he wasn't really -- I don't know; maybe he'd done some of that, but I wasn't going to do that kind of research. But he kind of excited me. And as my advisor in my first year, the way it was organized then was each incoming student was assigned to one advisor, and the advisor would therefore have four, five, six students that would meet as a group. And they came from different areas; they weren't all learning psychologists with a learning professor; they would be different kinds, and it was meant to be multidisciplinary. And I found it very exciting. It was my first exposure to that psychology, chemistry of the brain, the nervous system, learning -- I mean, I didn't know anything about any of that.

At the same time, all the first-year students took "Basic Concepts in Psychology," one course, all of psychology, starting in the fall, ending in the spring; it was a whole year-long thing. And it started with physiological psychology, and I'd never had any of that. And, again, I thought that was fascinating. I think it was probably different lectures from the faculty. There was a fellow named Robert McCleary there, Bob McCleary was one of the physiological guys, and he was a wonderful lecturer. And I was fascinated by that.

And at the VA, where I wasn't so thrilled with this research I was doing, there was another graduate student from our department, somewhat older fellow, a guy named Joe Rubenstein, very lovely man, and he kind of took me under his wing. And he had me over to his home; he was married, had a couple of children, and I went to his home a few times. And he liked me, and he really -- he said, "Let me show you what I'm doing," and so forth. He was running rats! He was doing rat research. And I was fascinated. And so I think all of these things kind of --

## JOHN C. LIEBESKIND INTERVIEW

### TAPE ONE, SIDE TWO

MARCIA MELDRUM: Okay. Second side.

JOHN LIEBESKIND: Nope. [shows his microphone not on]

MELDRUM: Okay. Now we're on. Are we?

LIEBESKIND: Are these things moving?

MELDRUM: Yes, they are.

LIEBESKIND: Yeah. Okay. So let's see. Where are we?

MELDRUM: So it was a whole new field.

LIEBESKIND: Yeah. So, you know, I took a few courses. I took a whole course in physiological psychology from Robert McCleary somewhere along the line, and I took this medical physiology course -- I started to tell you about that -- in order to get out of the second language, German; you had to get a B in it. I didn't; I got a C. [he laughs] I didn't really have enough background to do decently in that medical physiology course. Had some wonderful teachers in that course -- Robert Doty, who taught the neuro section, neurophysiology section, Horace W. Davenport, who taught -- you know his name?

MELDRUM: Very well.

LIEBESKIND: [Davenport taught] "The ABC of acid-based chemistry." [he laughs] And so forth. So there were some famous people there. I remember Ralph Waldo Gerard, who gave a lecture in there, in the neuro section. So, anyway, I was gradually moving over into physiological. I worked with three different professors in my next, in my last four years. I was in graduate school for five years altogether, and the last four I worked under three different professors. The first one was Ed Walker, who was a very nice man, very gentle man, but extremely unhappy that year. He had been denied the chairmanship of the department and was crushed; he was crushed. He was in a major depressive funk that year, and I didn't see much of him by consequence, but I worked closely with one of his graduate students, a senior graduate student, a woman named Ina Samuels.

MELDRUM: I-N-A?

LIEBESKIND: I-N-A, yeah. We became good friends. She was a little bossy and I came to resent her at one point, but we got over that afterwards, and we were good friends. And she was, I can't even remember what we were doing, but it was something with implanting electrodes in the rat brain; I didn't know how to do that at the time, maybe stimulating the reticular formation,

I don't remember the details on that. But anyway, it was my first hands-on exposure, really, to this kind of brain research.

And in that second year, I took, my roommate and I -- my roommate's name was Sal Cianci, Salvator Nunzio Cianci, C-I-A-N-C-I. We were great buddies, and we took a seminar with the famous James Olds [1922-76], who had, I think, come to Michigan the same year we had started graduate school, as a very young full professor. I mean, he was like, you know, a big hotshot; he'd done the reward areas of the brain, and he was, after only a few years post-doctorate, he was brought in, I think, at the full professor level at Michigan. Michigan was very freewheeling in those days. James Olds was a brilliant man and was giving a seminar, and Sal and I were in his seminar. And we liked that a lot. He was a very exciting teacher, very exciting man. And amazingly, toward the end of that seminar, he called us in, I don't remember separately or together or whatever, and asked us each if we would like to work with him next year --

MELDRUM: Wow! That's great.

LIEBESKIND: In his laboratory. Oh, I was all thrilled about that; I thought that was pretty -- pretty good. So we both said yes, and that led to the single worst experience of my whole life. [he laughs]

MELDRUM: Okay. I remember you talking about that in the earlier interview you did.

LIEBESKIND: Yeah.

MELDRUM: Let me just go back to a couple of things here. One is, I just want to go back to your family a bit. Your father had died by this time.

LIEBESKIND: Yes, he died my first year in graduate school, yeah.

MELDRUM: And so your family, then, your mother was alone?

LIEBESKIND: Not hardly. She had her brothers.

MELDRUM: To support her. Your family was generally supportive, though, of your -- at one point you said your mother wasn't that crazy about your --

LIEBESKIND: Oh. Well, yeah, I think my mother kind of resented Mr. Joseph kind of pulling me away or something, and she was not awfully keen on my going to Harvard. She thought that Harvard was not a good place to go; there were crazy people or Communists or something, I don't know.

MELDRUM: Oh! [she laughs] Not a liberal.

LIEBESKIND: I think she was -- yeah, I came from a sort of Jewish but Republican kind of family; those two things can exist together, although not very often. And they were a bit conservative, and they were concerned about Harvard. My uncle, who'd gone to law school

there years earlier, had seen it as kind of a wild hotbed of something or other [he laughs]; I don't know. And your microphone is on the ground, I see.

MELDRUM: Oh, I'm okay. Go ahead. So it is.

LIEBESKIND: But they didn't deny me; I said I wanted to go there, and they said okay. So they didn't really try to talk me out of it much. And my father, I'd had sort of one blowup with my father about the store, about becoming a merchant, and that was maybe also in my junior year. I don't remember exactly; somewhere in the middle of college. And he asked me if I would -- when I came home for Christmas vacation, he asked me if I would work in the store, as I had done before, which always sort of embarrassed me, to be around all these women, you know. He once had me in the lingerie department, of all things, selling brassieres.

MELDRUM: [she laughs] That wasn't your style.

LIEBESKIND: I mean, that wasn't any -- you know, he could have put me in something a little less embarrassing for a young person to be in; I don't know. Running the elevators [he laughs], or picking up bobby pins in the beauty salon or something, with a magnet. Anyway --

[PAUSE to check tape]

LIEBESKIND: I said "No" to my father, and he said, "Well, then, you can go to the devil!" He raised his voice, and everybody burst out crying, because no one had ever expressed an emotion before in my home, ever. We had this very flat affect around the house, and, I mean, there would be kisses, you know, but there was never a negative emotion, it wouldn't be expressed.

MELDRUM: Right. Not a yelling family.

LIEBESKIND: Right. No yelling, no nothing. So, after that, everybody was crying and crying and apologizing and, you know, talking. And it never came up again. And my father was a very mild-mannered, very nice chap; I never felt awfully close to him. He worked hard and he had his own sort of pastimes.

MELDRUM: Sort of distant. Yeah.

LIEBESKIND: A little distant from the family, slightly. And I was always very emotionally involved with my mother, but not so much my father. But, I mean, I never had another mumbling word about going into the store.

In fact, somewhat later, my father approached me and said he was making a business arrangement with his younger brother about the store, and he wanted to consult me on it. He said, "I gather you don't want to go into the store," he said, "but you now need to make a real decision about that because I'm making a survivorship arrangement such that the first brother to die," he said, "I'm much older than my baby brother, the first of us to die, the family of the deceased are to be bought out by the other brother. We don't want silent partners; that leads to



family trouble,” and so forth. And so they made this, they were advised to make this arrangement, and he said, “I will only do this -- since I am apt to be the first one to die -- I will only make this arrangement under the assumption that you’re not going into the business.” He said, “If you *are* going into the business, I’ll hold a place for you.” I said, “I’m not.”

MELDRUM: So you made this decision -- you were what then? Eighteen or so?

LIEBESKIND: Well, I don’t know. Yeah; maybe a little older. Maybe my freshman or sophomore year. He got sick, he got diagnosed with lung cancer somewhere in that, during college; I can’t remember exactly how long he took to die. It was two or three years or something. I think this may have been before then, but maybe not. I can’t put all that together exactly. So there was really no argument. And once I was starting down that line, my mother always took pride and joy in what I was doing. I’m sorry my father didn’t live long enough to see any of that, but I was just beginning graduate school.

MELDRUM: So, then, the clothing store went to his brothers.

LIEBESKIND: Yeah. They bought us out, his brother, his youngest brother, who had three sons, two of whom ended up running it together, the oldest and the youngest, kind of like the other generation. And then there were some fights between them, and the oldest one bought out the younger one and ran it and finally ran it into the ground, what with small stores like that going under a few years ago.

MELDRUM: Yeah. All the competition.

LIEBESKIND: And it went, last year, finally out of business, in its third generation of ownership. It had gotten smaller and less profitable. Anyway, so, where am I?

MELDRUM: Okay. So I wanted to ask you -- you said to me once that you thought part of your move from clinical into physiological psychology was because you found you had a lower tolerance for ambiguity?

LIEBESKIND: [he laughs] Yeah.

MELDRUM: Did you want to expand on that at all?

LIEBESKIND: Well, that’s the way I sometimes express this. Yeah, I think scientists kind of -- I view that there is a ladder, an abstraction ladder, that people are comfortable with different levels on that ladder, and that social science is too soft for me, and chemistry is too hard-nosed for me or something, but I found my correct level in being interested in brain and behavior.

MELDRUM: But all this work you had done studying psychology, social relations, clinical psychology, you had enjoyed that.

LIEBESKIND: Yeah, kind of, but I kind of didn’t, it got to me afterwards; it’s not precise enough. It’s getting at big important issues without the precision that I require.

MELDRUM: Right. Some of it is pretty vague.

LIEBESKIND: I think that's the tradeoff, you see; the bigger the issue, the more important it is, the more exciting it is, but the less precision you can bring to it. At the other end, you get into issues that you can be just precise as all hell, but who gives a damn about, you know.

MELDRUM: [she laughs] Sometimes seem insignificant. Yeah.

LIEBESKIND: Yeah.

MELDRUM: Little bits, little pieces to the puzzle.

LIEBESKIND: Yeah. I don't know how true that is, but that's sort of the way I sort of generally view it. And I found the right spot for me, no question about it. At the age of sixty, as I look back on my career, there's no question that was the right spot for me on the abstraction ladder, the kind of, the brand of physiological psychology that I practiced. And as I look now at some of the kinds of work that some people in the area are doing, much more molecular, that's not my brand. That's maybe one of the reasons I got out of science when I did, which we may get to talk about at some point. But I think the way I perceived it, the middle, the heart of science [changed], knowing that was not where I wanted to go. And so it didn't seem comfortable. I didn't want to change any more than I wanted to become a social scientist again, nor did I want to become a molecular biologist. The brand of science that I was practicing was going out of vogue after having been very much in vogue for a long time. It was going out of vogue.

MELDRUM: Okay. So you went to work for this guy Olds.

LIEBESKIND: Yeah. Well, that was a very, very bad experience for me. It -- he kind of, he ignored me. I mean, he was off doing his own work. He had a wife who is still alive, Marianne Olds -- she's Belgian, I think; she has a bit of an accent, M. E. Olds, Marianne E. Olds; everyone called her Nicky, which was her nickname. And she kind of ran the lab in a day-to-day fashion, and I don't know, for some reason, maybe it was my idea, maybe it was Jim Olds' idea, somehow I came in with an idea of a plan, of a research thing I wanted to work on, that involved building an apparatus that I could test rats in. It was a little complicated as to how it would be built so that I could test them. It was sort of a reasonable idea, and he approved that or suggested that -- I don't remember, and then said, "Great. Go build the apparatus."

Well, I didn't know how to do that, and I wasn't good working with my hands. I made some attempts and something, and I don't remember the details very much. But I wasn't getting very far with it; I was getting frustrated with it. And I wasn't getting any help even though they had technicians in the lab; they weren't *my* technicians. I had to do, you know, my work; I had to do my work myself. Technicians were there to help Dr. Olds, not to help the students. So it was a very stratified kind of lab.

MELDRUM: So he invited you to work in his lab --

LIEBESKIND: Yeah.

MELDRUM: -- but he sort of left you on your own.

LIEBESKIND: That's the way it was in that lab; that's the way things worked. In fact, you weren't supposed to talk to anybody else in the lab. Mrs. Olds would leave notes criticizing people on the bulletin board, and you would sometimes get notes: "You were seen talking to the technician. He's too busy doing his work. You're not to spend your time talking." My roommate, Sal, who also came over with me and had a good project, he stuck it out with Olds even though he had a real emotional breakdown. He was hospitalized, he couldn't do anything, he was vomiting, he became anorexic, he had to have IV feeding; he had a major nervous breakdown.

MELDRUM: I can't even imagine this!

LIEBESKIND: He stuck it out in that lab and did do his thesis and became James Olds' first-ever doctoral student. But it was very costly for him, bodily and emotionally. I quit at the end of that first year, which was a good thing. I had to stick it out; I had a fellowship that Olds had gotten me for a year, so I had to stay in his lab that year, but toward the end of the year I made provision to move to a different lab.

But the other thing that happened that year was in the fall of that year, right after beginning with Olds, I took the comprehensive exams for the first time, and I flunked them. You had to take two at a time, two different subject areas, and I flunked one, and that meant you had to take them both again. I flunked one of them and had to take them both again, and Olds called me in himself and said, you know, "That's very bad. Now you're going to -- I brought you in here to do research; now you're going to be spending all your damned time studying for these things." And I was told by one of the graduate students that Nicky Olds had said that Jim Olds was going to get me thrown out of graduate school, at which point I had a minor, at least, nervous breakdown.

My roommate was at home vomiting, and I got very nervous, and I had anxiety attacks, is really what I would say. I had sort of unrelenting anxiety all of a sudden; I was shaking and couldn't sleep and so forth and so on. I sought psychiatric help, saw a psychiatrist for a while, put me on some Miltown [meprobamate] or whatever. [he laughs]

MELDRUM: Oh, golly, Miltown?

LIEBESKIND: I had a few sessions with him, and not very long; I stayed with him a month or two. And then something very important happened, very important. This was all in the fall, and I had to study for those exams again. And I said to myself, "Fuck the research and Jim Olds. I'm not going to worry about him. I've got to pass these exams or I *will* be thrown out of graduate school." Olds, this was just a nasty rumor, you know, I mean, as far as I know, he never did try and get me thrown out; he was just angry at me because I was going to have to take

time away from research. And so something very, very important happened. I went to the library and I started to study. And I don't know that I'd ever really done that in my life.

MELDRUM: [she laughs]

LIEBESKIND: And something happened as a function of that experience that I think transformed my life. I would go into the library and open up my book, and I would feel calm. I would feel like, "Ah! Now I'm doing what I need to do. I'm learning this stuff, and I'll be okay. I'll take this exam and it's going to be okay the next time I take it." And so work, studying and work, for the first time, it became a kind of conditioned relaxation stimulus for me. And it is to this day. I come in to work, even if I'm feeling anxious, the minute I walk into Franz Hall [the Psychology building at UCLA], I feel calmer.

MELDRUM: And you start calming down. Yeah.

LIEBESKIND: "Here's where I can get something done. Here's where I can alleviate any anxieties I may have in my life." [he laughs]

MELDRUM: It's something you have control over.

LIEBESKIND: Work anxieties or home anxieties, I can put them aside, I can do it here, and things can move forward. I think that was a very important moment in my life. I never gave another -- well, I mean, I never, basically, didn't even try to do much in the Olds lab after that. I switched to some other silly little project, collected some data, pattered.

MELDRUM: But he didn't really give you much help; I mean, he sort of was mad at you.

LIEBESKIND: He didn't. I mean, he obviously didn't give a damn, he was doing his thing; he saw me as a wash at that point. So on through the spring -- I passed the exams -- on through the spring, I kind of diddled around, and meanwhile I was looking for another place to work. And I went to Bob McCleary. And he said yeah, he would take me, but it would have to be through Chicago, because he was leaving Michigan [he laughs] and he was going to Chicago. And he asked me if I'd like to join him, and I said no, I didn't think that would be too good an idea to leave graduate school, you know, to switch graduate schools at this point, and he said he could understand that. He said, "Why don't I fix you up with my former student, Steve Fox?" He says, "I've got a really nifty idea for you for a thesis project." It wasn't a terribly good project, but he thought it was okay, and he said, "I'll talk to Steve to accept you."

Steve Fox was a year older than me, a year and a half older; he had just gotten his Ph.D. And to make a long story short, I started working with Steve, and I spent those last two years, then, working with Steve Fox on this silly, stupid kind of project, but got some data and it turned into a thesis, and it was not a piece of junk; it was okay. And Steve became a good friend, although he was a very crazy guy, very crazy. He was, well, I don't know how much to say. We were very fond of each other, he was very good to me and good for me at that time.

MELDRUM: That's pretty important.

LIEBESKIND: Yeah. And I got married along in there; married in '61, got my degree in '62, and so my last year in graduate school I was married; Nancy and I were married. And Steve and his wife and Nancy and I used to do some things together, and Steve became sort of a friend. And then, after I finished there, I had my final orals in the end of the summer, just before classes were to resume in the fall of '62, so it was sort of too late to make any post-doc plans. The new year was upon us, and knowing that that was going to be -- I'd arranged that I was going to stay on at Michigan for another year, so I stayed on as an instructor in the department half-time.

I got very into teaching; I was a very good teacher. And so I got put in charge of the introductory psychology course, taught as a natural science then, not as a social science -- this was a big first year, freshman-sophomore, class. I was sort of in charge of it, and that was like half-time, and then Steve paid me half-time on his research grant. I started learning with him, post-doctorally now, some neurophysiology, some recording techniques, electrophysiology recording from the brain, which I'd never done, and he was just learning on his own. And during that year he said, "I can only teach you so much," he said; "you need a real post-doc," and he had a friend who was working with Madame [Denise Albe]-Fessard, a friend named Dick Wendt, who had gotten his Ph.D. --

MELDRUM: W-E-N-D-T?

LIEBESKIND: Yes, Richard Wendt, who had gotten his Ph.D. here at UCLA in the anatomy department, where Steve actually, Steve Fox actually did his dissertation research here, even though he was a Michigan Ph.D. student; he was outplaced doing his research here, and that's how he knew Dick Wendt, and Dick Wendt went to Paris to do a post-doc, and as he was getting ready to leave, he visited Michigan, among other places. I met Dick Wendt on this one occasion; he said, "I'll be glad to go back and tell Madame Fessard about you and help you get into the lab; you can become her next American student post-doc. Apply for an NIH post-doctoral fellowship," which I did; I wrote to Madame Fessard. She accepted me in principle; I got the fellowship; I went. Dick Wendt returned to the United States and committed suicide within the first year back.

MELDRUM: Oh, dear.

LIEBESKIND: Which was a very sad business; he was very loved by the people there, but apparently he was emotionally unstable, and I don't know too much about that. And that's how I got to Paris.

MELDRUM: Okay. You actually finished your thesis project, then, working with Steve Fox.

LIEBESKIND: Right.

MELDRUM: And you worked in his lab?

LIEBESKIND: Right.

MELDRUM: It was a much more -- you could talk to other people at this lab? [both laugh]

LIEBESKIND: It was only him. I was his first student.

MELDRUM: It was just the two of you.

LIEBESKIND: That's right.

MELDRUM: And you sort of worked together; you each had your own project?

LIEBESKIND: He was doing his stuff, and I was doing mine. But the symbolism, I think, of the following thing was profound. When I first went over to meet him and he said, "Yes, you can come and work in my lab." He said, "Come here. Come into this room. This will be your lab." And he said, "Wait a minute here." And he grabbed a broom, and he was sweeping it up. And I said, "Wait a minute, I'll sweep it up!" And he said, "Oh, well, all right, whatever." So, I mean, like he was just -- he had this concept that he was going to mold me into something, and he was going to help me, you know, be fostered in my career, which is exactly what he did do. And though only a year older than me, only a year and a half older than me, he got me this post-doc position, and he kept his eyes and ears open for me and helped me get this job at UCLA. And I mean, he was a major --

MELDRUM: He was a big help to you.

LIEBESKIND: He was a real young hotshot at that time. He had the world in the palm of his hand. And what is very sad, is he left Michigan about the same time I came to UCLA; he went to Iowa a little bit before -- I think my second year in Paris; I stayed there for two years, and I think the beginning of that second year he moved to Iowa, hotshot job there as an associate professor, and immediately started fighting with people. He was a very aggressive, nasty guy basically, but he was very good to his own students. And not very good to some other people he should have been.

MELDRUM: Loyal to my people, not to other people. I know. Yeah.

LIEBESKIND: He was involved in spousal abuse, he was involved in drugs, he harbored criminals, he went off the deep end and got into a lot of trouble in Iowa eventually and dropped out of science.

MELDRUM: That's too bad.

LIEBESKIND: He was doing very hot stuff.

MELDRUM: Were there any particular skills that he taught you or qualities that you think he fostered in you? Any way in which he made you a better --

LIEBESKIND: I would say the latter very specifically, yeah. He made research a very exciting thing. He was on to hot ideas, and he called me creative once.

MELDRUM: Wow! [she laughs]

LIEBESKIND: He said, “John, you’re creative.” Your microphone is just falling. You’re fiddling with your microphone.

MELDRUM: [she laughs] Yes. I need to stop fiddling with my microphone. Yes, go ahead.

LIEBESKIND: He called me creative once, and I never forgot it. It was the first time anyone had ever really complimented me in relation to anything having to do with science or something in some way. So I think I learned from him how to, some important lessons about how to foster the careers of younger people. You didn’t have to be a big shot; you didn’t have to be a Nobel laureate to be influential on people, in regard to your own students. And I learned a lot of things like how to be more proactive, how to kind of take care of myself better in science, talking to people.

MELDRUM: Yeah. That’s really important.

LIEBESKIND: The lessons like that, he was a mover and a shaker, though a year and a half older. Here I am, a humble graduate student, and he’s a cocky professor, a young professor who’s making his name in the world. Of course, he was brilliant, and I knew that was very important, and I wasn’t. But still in all --

MELDRUM: You were creative. [both laugh]

LIEBESKIND: I saw ways of doing things that he was doing, to kind of move and shake a little bit, be excited about, you know, or this concept of being excited about your work.

MELDRUM: And then being able to excite other people.

LIEBESKIND: Excite other people, yeah. So --

MELDRUM: And you met Nancy in Michigan.

LIEBESKIND: Yeah. Nancy was an undergraduate at Michigan, and she and a childhood friend of mine, his aunt and her uncle, were husband and wife. So he was a guy I’d gone to summer camp with, he was in New York City, and his aunt, who was also Nancy’s aunt, knowing that I was in Michigan, said, “Oh, I want to fix you up with this niece of mine named Barbara,” who was Nancy’s younger sister. Somehow I got fixed up with Nancy; I don’t know quite how that happened [he laughs]; they were both in Michigan. But I did get fixed up with Nancy. And I don’t think I ever dated anyone else again. Just sort of hit it off.

MELDRUM: Immediately?

LIEBESKIND: Yeah. And we recognized each other immediately; we were very, very much alike in terms of our backgrounds -- not so much in terms maybe of the kind of people we were,

but in terms of our backgrounds. But we both came from kind of upper-middle-class Jewish homes, similar educational kinds of things, and it just seemed right. And I was maybe a bit young to marry, but I felt like it was about time. So I don't know. Nancy was a couple of years younger than me, but she was now just graduating from college and it was a good time for her to get married.

She wanted to be a doctor. She was her father's only son, the eldest of four daughters, and we got married in the spring and went off to Europe. We were in Europe for the summer, and we came back in the fall for her to start medical school at Michigan. And the first year we were married -- let's see, hold on a second. No, the year before we were married, she came back to Michigan. She had graduated the year before that; she came back to Michigan to be with me so we could be together. We didn't live together, but we spent all our time together. She had her own little apartment, and I had my apartment. And we spent all our time together and during that year became officially engaged and decided to get married. She was at that time finishing one course she lacked for medical school, and at her father's insistence getting a teaching credential, in case she should ever not decide to go to medical school or whatever. [Meldrum laughs]

So we got married, we went to Europe, came back, and she started medical school and quit before Christmas.

MELDRUM: Wow!

LIEBESKIND: Yeah. She got far behind. She was extremely bright, but she was just a very undisciplined student, and that medical school at that time, it was a lot of work, and she got panicked. It was very much too bad I didn't kick her butt and make her stay in. She probably would not have flunked out; she would have found another solution. But she did not get through, and that led to a lot of problems for her personally, and ultimately between us. It took nineteen years before we got divorced, but she was an ungratified woman, a very bright woman who was being a teacher and this and that.

MELDRUM: Yeah. Was never able to really -- yeah.

LIEBESKIND: She didn't really get the satisfaction out of life that she probably would have if she had stayed in medicine.

MELDRUM: Or some sort of career. Yeah.

LIEBESKIND: Yeah.

MELDRUM: It's difficult to know. You always think you know those things when you're only twenty-two.

LIEBESKIND: Yeah. Right. [he laughs]



MELDRUM: So. Okay. So anything else you want to say about Michigan? When you think about Michigan, what do you remember most? The laboratory?

LIEBESKIND: I don't know. I had some good friends; we had some fun there. And that horrible year when I had anxiety attacks, my nervous breakdown scared me. It scared me afterward; will it ever happen again? I sort of decided, "Well, I came out of it, so I'll come out of it again. If I have it again, I'll come out of it again." Since having had that kind of anxiety, having experienced it, my heart was pounding, you know.

MELDRUM: And you can't really -- yeah, it's hard to get things done.

LIEBESKIND: That was certainly a very memorable experience. You know, there were a few women I knew there who were pretty memorable somehow or another, including Nancy, I guess; we did some fun things. And teaching was certainly very memorable.

MELDRUM: You started teaching fairly early.

LIEBESKIND: I did. Yeah. I became a teaching assistant early on, and was making gallons of money by the end of graduate school.

MELDRUM: [she laughs]

LIEBESKIND: I was so well paid because I was getting paid by Steve in research and I was teaching; I was working full time. It was huge amounts of money; I was filthy rich in graduate school. I loved teaching. I was very into it, first just as a humble T.A., and then I sort of got more responsibility, a teaching fellow, a super T.A., running this course, and something and something. It was a very important experience for me. One of my regrets in the last ten, fifteen, twenty years was the gradual erosion of my interest and love for teaching. You know, it's become more of a burden as I've gotten narrower in my career; it's hard for me to sustain my love of teaching first-year students.

MELDRUM: Well, I think it's just hard if you're not able to develop new courses; then it becomes less intellectually challenging.

LIEBESKIND: Well, there's that, and whether you want to or not, the age gap kind of pushes you away.

MELDRUM: I think we all feel that. I look at the students sometimes; I think they're on another planet. [she laughs]

LIEBESKIND: Yeah. Right.

MELDRUM: Jeepers! Okay. So, we're ready to go to Paris, I guess.

LIEBESKIND: Yeah! So that was a very exciting experience and just unmitigated joy for me. It wasn't so great for Nancy; she was pretty unhappy there and didn't know what to do with

herself. She tried various jobs, but nothing worked. So that was -- in a sense, the seeds of the destruction of our relationship were sown in Paris. But I worked hard; I did good work. I worked with Madame Fessard herself my first year there, which was a very gratifying experience. She's a very difficult woman.

MELDRUM: I gather.

LIEBESKIND: But she wasn't difficult with her American, foreign students. She was so kind and considerate and calm, working and, you know, the monkey would die or something would go wrong, she would remain calm. And I just had many wonderful memories. One horrible memory early in my first year there, working in the lab and getting a call at night from Nancy that Jack Kennedy had just been assassinated. So I left the lab -- I was working with Monique Denavit. [he laughs] She was in neuroscience; she's still around. Monique Denavit-Saubié; she's now married. But anyway, you'll hear about her more. She and I were doing a cat together, and she said she would take care of things, and I left to go be with Nancy and some other Americans.

Anyway, you know, just lots of wonderful memories. I had this feeling about living in Paris, that sort of a *Catcher in the Rye* kind of feeling; this is my city. So many people came through we knew, or even we didn't know, but came to stay with us, and I would show them around. And I could do it. I spoke some French, and I knew my way around, and I had sort of conquered Paris, you know, and I wasn't afraid of Paris. I had mastered Paris.

MELDRUM: Where did you live? Do you remember?

LIEBESKIND: Where did I live? We had, our first apartment was fairly near the laboratory; that's why we chose it. I could actually walk to the lab, but it was in, the lab, unfortunately, was in the sixteenth *arrondissement*, which was a high-priced area, and we didn't get much for our apartment and were paying a fair amount. So we found a cheaper one, much nicer, although much farther away, but we had a car, so it wasn't really such a bad thing. But that was over in the thirteenth *arrondissement*, the *boulevard de l'Hôpital*, right up the street from the Salpetriere Hôpital on the boulevard.

MELDRUM: Yeah, there's nothing like Paris, I think, in those early days -- in the '60s, it was a great place to be.

LIEBESKIND: I felt so fortunate. It never went away; the thrill of being in Paris I did not adapt to. You know, I would come home late at night from an experiment or something and drive through Paris, the streets being fairly empty, maybe there was some truck going around watering the streets and lights would be on; maybe a fountain would still be on. And you just see the magnificence of it, and I would be thrilled. Just always thrilled by the city. Just always thrilled from the first time I saw it.

MELDRUM: Well, it's a great city. So in the lab you were working on this electrophysiological recording?

LIEBESKIND: Yes, in the lab, and it was work that didn't really interest me an awful lot, but socially it was fun because I was working with people that I enjoyed being with. And it seemed like it was a good project; we were making progress getting some papers out and so forth; got a good bit of work done. But the thought that I would ever continue to do that work after coming, getting my own job, never would have occurred to me. It was work on whether muscle spindle afferents went to the motor cortex or not. How does the brain know about what's happening in the muscles? Which, you know, is not an unimportant question; it's an interesting question. But it didn't interest me.

MELDRUM: So this wasn't related to pain in any particular way.

LIEBESKIND: Not in any way. It didn't interest me, that question, at all; it wasn't exciting to me.

MELDRUM: It was a job.

LIEBESKIND: Yeah. It was something to do; it was a project. I was making progress with it; it was okay. But just before I left, the gate theory article came out. [he laughs]

MELDRUM: Okay. Let's just talk a little bit about Madame Fessard's lab because I am very interested in the way these labs work. Were there any particular ground rules there, any rules, things you can think of that you could not do or Madame Fessard would be angry, or was it pretty free-wheeling?

LIEBESKIND: Well, I don't know about strictures on things you couldn't do. There was one thing you *had* to do, virtually *had* to do. You had to stop whatever you were doing at 5:00 and go down and have tea with everybody else.

MELDRUM: [she laughs] I like this -- this is great!

LIEBESKIND: True! True story! Monsieur [Albert] Fessard would be there and Madame Fessard; everybody would come down to the library of the building, gather together, and have tea.

MELDRUM: That's very nice.

LIEBESKIND: You could get out of it, but it was pretty important that you went. I don't know; people were very polite there. She was a very powerful figure, and Monsieur Fessard, although quite a bit older and nominally the head of the whole lab, really was sort of in semiretirement by that time, and had very few people working with him even nominally, and she really was the big shot at that time.

But he was a very auspicious character -- I started to say "austere". He wasn't austere; he was a rather friendly man. She was more austere than he. But certainly the students were very deferential to all of them, to both of them, and it was a very large group. There were fifty, sixty people in this whole lab.

MELDRUM: Wow! All students? Some technicians?

LIEBESKIND: No; there were various levels. Yeah, I mean, there were some more senior people who had long since had their Ph.D.s, but who were sort of nominally in Monsieur Fessard's group; there were a couple like that. And there were a few kind of older, now former, students of Madame Fessard who stayed on. Because you didn't like get your degree and then leave. When you were there -- and that's still true in France; their whole system is organized differently, as you know. So people would tend to stay on. So there were different ranks of people. And some still were working on their degree; some already having their degrees. But Madame Fessard was astride that very firmly; I mean, there was no, she didn't have like a close rival or close associate, someone at her level.

MELDRUM: She was the boss.

LIEBESKIND: She was very much the boss. The others were considerably younger than she, who worked with her. And some of them had their own sort of groups, but they still reported to her.

MELDRUM: So did everybody do pretty much all the work? Was there a division of labor?

LIEBESKIND: Oh, yeah, there were designated technicians, and the technicians were -- there were, I don't know, there were half a dozen or more technicians running around, or more than that, probably ten or twelve. Some were just in the shop, and some were working with you and the animals, helping set things up.

MELDRUM: Okay. So, if you had wanted to build an apparatus, there would have been people there to help you do it.

LIEBESKIND: Oh, no question about that. Yeah. Yeah, of historical interest, there was a man there named Monsieur Bull.

MELDRUM: Bull?

LIEBESKIND: Monsieur Bull. I didn't know how his name was spelled, and I never really knew who he was. It turned out it was Bull, B-U-L-L; he was English, but I didn't know. [both laugh]

MELDRUM: I love it! And you spoke to each other in French!

LIEBESKIND: I thought it was [with French accent] "Monsieur Boul", that he was a very French man, you know! He was ancient; he was bent over, and he had been M. [Etienne-Jules] Marey's technician -- M. Marey [1830-1904], who was one of the founders of modern cinematography and was a physiologist and sort of invented, or was one of the early contributors to time-lapse photography, where you could photograph horses in motion and people in motion; he was a physiologist who studied movement, motion, and he did it with cameras, a series of

cameras like this. And somebody would go running by, or a horse would go running by, and it would trip off these things, and then he would put them in sequence, and they had these drums at the museum that you could rotate and you would see, you know, step after step, frame after frame.

MELDRUM: You could analyze the components.

LIEBESKIND: And that's how cinematography got done. And he was a very famous scientist, and this institute, where this building was, was the Institut Marey [founded 1898].

MELDRUM: Oh!

LIEBESKIND: So that was the name of the lab.

MELDRUM: Well, actually, yes, I did know that.

LIEBESKIND: And here was [with French accent] Monsieur Bull {Lucien Bull, 1876-1972}, who was a very old Frenchman, who was not a Frenchman at all, but an old Englishman [he laughs], it turned out, who had been his technician. He was still alive when I was there. So, anyway, you were asking about how the lab was organized. It was sort of grouped --

MELDRUM: Well, do you choose your own project, or was this assigned to you?

LIEBESKIND: When I first came there, Madame Fessard called me in and said, "What would you like to work on?" She said, "You could work with Monsieur Massion; he's doing da-da-da; you could work with Monsieur Argent; he's doing this and this; you could work with Mademoiselle So-and-so; she's doing this," and I sort of stopped her in the middle, and I said, "What are you working on?" And she said, "Oh, well, I'm just going to be starting something in a few months, da-da-da-da." And I said, "Well, I came here to work with you. I'd like to work with you. I don't care what it is." So, she said, "Oh! All right." So she put me with someone else first, this Monique; I first started doing a little project with Monique until Madame Fessard got ready to get going --

## JOHN C. LIEBESKIND INTERVIEW

### TAPE TWO, SIDE ONE

JOHN LIEBESKIND: There you go. Now we're on.

MARCIA MELDRUM: Okay. We're starting the second tape, and it's noon exactly.

LIEBESKIND: Okay. Well, I just said in the second year this fellow from Canada came and joined us, a French Canadian named Yves Lamarre, L-A-M-A-R-R-E, and he and I became great friends, and we worked together sort of, both on this same project with Madame Fessard. But she really at this point didn't work with us on a day-to-day basis; we worked together. And Yves was very well trained in neurophysiology, and I learned a lot from him, although he was a post-doc, as I was. But he had been well trained in neurophysiology and did good work. And so that was a good second year.

I became very close with Madame Fessard; I'm very fond of her. I went over to her home a number of times; whenever a visiting fireman, an important American physiologist or whatever would come through, she would generally invite one of the Americans. There were several of the Americans; she was close to all of us, and I would, in my turn, go over there, you know, for dinner, and I would help her with translating some of her manuscripts into English. I would correct her English. She used to call me her English professor.

MELDRUM: That's neat.

LIEBESKIND: And at the end, something very touching happened that I'll never forget. I was looking for jobs back in the States, and when I got this job at UCLA, and, you know, she was following that and so forth, and as it was getting close to the time when I was going to be leaving, she called me into her office. And she said, "John," with tears in her eyes, she said, "John, I want you to know you don't have to leave if you don't want to. I have been investigating, and I have found a possibility for you to stay in France. I found a job in this hospital right nearby," and so forth, and I understood immediately what that was all about. She was concerned that I, like Dick Wendt, would go and kill myself because I had to live in the United States and couldn't live in France any more.

MELDRUM: [she laughs] Anybody would!

LIEBESKIND: And at that point -- yeah, who wouldn't? And I think that was kind of in her mind, that life in the United States is very tough, nasty people, and I might get involved with one, you know, who might hurt me or whatever, as Dick Wendt had come back and that was her theory, anyway, of what had happened. But I found that very touching.

MELDRUM: Yeah. That's so sweet.

LIEBESKIND: Yeah.

MELDRUM: So it's interesting; she had a very good relationship with her American students, not so much --

LIEBESKIND: Yes. With her own students she was quite authoritarian, more distant, and ultimately, later, just sort of bit them each successively, and as a black widow spider bites its mates, and she just sort of threw them all out -- "Traitors! You're all --" They were traitors.

Well, there were a lot of things that were going on politically, and there was a sort of student uprising in 1968, several years after I'd left as a post-doc and several years before I returned on a sabbatical leave in '72; I was on sabbatical again. And between those two times there'd been great upheaval in the lab, and the lab was on the way out. She had thrown out a lot of her students and continued doing that. "Oh, he's a traitor," and she would confide in me and tell me, you know, "You thought he was a nice guy; he's not a nice guy; you thought she was a nice person; she's not a nice person. Behind my back, they're telling stories," and there was a lot of paranoid imagery, which I take as paranoid because I knew these people well, and I'd talked to a number of them, including people like [Jean-Marie] Besson, who I continue to talk to, as you know, and so forth. And he will tell me, "I've never said a bad word about her; I always loved her and cared for her and was respectful of her, and she just finally threw me out, too." Which he was, the last one that she threw out.

Besson and Giselle Guilbaud were the last ones to be expelled from the lab. So it was really a growing thing. As these people tried to grow and have some independence, she would --

MELDRUM: She couldn't take that.

LIEBESKIND: Yeah, she couldn't take that.

MELDRUM: So maybe it's just as well you didn't stay in Paris.

LIEBESKIND: Well, I don't know; yeah, maybe, probably so. Yeah. George Krauthamer, who was an American who stayed with her for many years, he ultimately came back and they remain very close. So the fact that he ultimately left and so forth, I don't know; again, they did not have any problems between them.

MELDRUM: That's very interesting.

LIEBESKIND: Yeah.

MELDRUM: Okay. But you did come back.

LIEBESKIND: Yeah. I had several job offers; that was a very exciting time. I could have gone to Wisconsin, and I had an earlier job offer at the University of Cincinnati. They had wanted me to come back early from Paris; I'd had that job offer before I went to Paris, and I said, "No, I'm not going to come back early." "We'll hold it for you, even for two years, after Paris, if you promise you won't look anywhere else." I said, "I don't think I can do that."

So I dropped Cincinnati; so I had a job offer at UC Davis, and Wisconsin, and the University of Chicago, where McCleary was. Here he had been writing these letters of recommendation for me, and he called me one day in Paris, and he said, "Have you taken a job yet?" I said no. He said, "I'm writing all these letters for you, and I sort of talked myself into it; we should have you here! You should come to the University of Chicago." I said, "Well, I already have a job offer from Wisconsin, and I just heard yesterday I got one from UCLA." It was all long distance, you see, from Paris, these job offers.

He said, "But have you told either of them that you've accepted?" I said no. I said, "But Steve Fox will kill me if I don't go to UCLA." He said, "But have you accepted UCLA?" I said no. He said, "Well, come." He said, "Stop. Stop what you're doing, hop a plane, come to Chicago; I want to show you what we've got here." So on very short notice -- this was very exciting for me -- I flew off to Chicago.

MELDRUM: Yeah. Boy, you were recruited right and left.

LIEBESKIND: Yeah, and made arrangements, you know, with quick phone calls to Wisconsin and UCLA to visit them on the occasion of this trip, and they each paid for my trip, from Chicago to Madison and from Chicago to Los Angeles. So, in the spring of '65, 1965, I did this, and while I was visiting UCLA on this trip, McCleary called me from Chicago. I remember I was in Gengerelli's office [he laughs], the famous Joseph Gengerelli [1905-2000], who was one of the professors here, and he picked up the phone and he said, "Oh, it's for you!" You know, I'm interviewing here at UCLA. And McCleary said, "You didn't tell UCLA no or anything, did you?" And I said, "No, I didn't." And he said, "Don't, because we're not going to be able to offer you the job in Chicago." [both laugh]

I said, "That's okay, Mac; I really love it here at UCLA." It was something, he claimed, something political had come up and they weren't going to get an appointment in physiological; it was going to go to somebody in clinical or whatever; I don't know. It had nothing to do with the way my showing there -- I don't know whether it did or didn't. But in any case, I took the job at UCLA.

MELDRUM: That's a long way from Connecticut.

LIEBESKIND: Yeah! When I visited UCLA that day, that was my first time west of the Mississippi. I'd visited Chicago once or twice while I was in Ann Arbor, and that was the farthest west I'd ever been. But I thought UCLA was paradise. It was a beautiful May day; it was cool -- I mean, it was warm and clear and there were all these frangipanis or whatever, you know, these great, giant flowers blooming all over the place. I thought, "Oh, my God; it's a tropical paradise."

MELDRUM: Yeah. It is very beautiful.

LIEBESKIND: Westwood was, of course, very different in those days -- smaller and quaint. I stayed at the Claremont Hotel when I visited and a little French restaurant down at the corner



where Monty's [a Westwood restaurant] is now, or where that giant building is; anyway, right around next to the Hammer Center [Armand Hammer Museum and Cultural Center], a little French restaurant there, and I could get a croissant and iced coffee for breakfast before going up to visit the department. I thought, "Wow, this is major civilization here!" you know.

MELDRUM: [she laughs]

LIEBESKIND: Steve Fox was very influential. He thought UCLA was the plum job in the country. He said, "You have just been offered the plum job in the country. If you *don't* take it, I will skin you alive."

MELDRUM: "If you don't take it." [she laughs] Well, strong motivation there.

LIEBESKIND: So I never had any doubt that I would take it.

MELDRUM: Now, when you came here, were you -- I mean, it sounds as though you were recruited fairly heavily. Was this a reflection of the fact that the field was expanding?

LIEBESKIND: Definitely. Oh, yeah. All the departments were growing, and I was in the very fortunate position of being one of very few psychologists who had some credentials in neurophysiology, who could do, supposedly -- I don't know if I *could*, but I was trained for three years with Steve Fox, one year post-doctoral, and two in the Fessard laboratory, recording the electrical activity of the nervous system. Single cells, evoked potentials and so forth, sensory-evoked potentials, and at the time that was the latest, hottest stuff.

MELDRUM: So that was your selling point.

LIEBESKIND: That was a very strong, very strong selling point. That's what made me desirable when I was getting job offers. Of course, everybody was getting job offers. I mean, I remember Steve had one student in my later years there or whatever, who was not very bright and so forth, and he said, "Oh, well, you know, this guy really isn't very good; we won't be able to get him a job at a proper university, so we'll send him off to the University of Kentucky," [he laughs] you know, like that was exile. That was --

MELDRUM: [she laughs] Exile. Siberia.

LIEBESKIND: If you were a card-carrying moron [he laughs], you'd have to go there or something. And now, I mean, any one of my students today would kill to get a job at the University of Kentucky or anyplace, you know. Times were very different, and I very much benefited from having come through when I did.

MELDRUM: Okay. So you took the job.

LIEBESKIND: Yeah! Well, I came here actually a bit late. I came in January of '66. They let me miss the fall semester to finish my work in Paris. Late in '65, just before coming here, the gate theory article came out.

MELDRUM: Ah!

LIEBESKIND: And I read that. I'd had some thoughts about pain before, not much. Madame Fessard and I once found a cell in the brain that seemed to respond only to noxious stimulation; if you pinched the paw, only then it responded. I remember she'd sort of, "Ah! Jean, *voilà!*"

MELDRUM: [she laughs] *Alors!*

LIEBESKIND: Eureka, you know, *mon Dieu!* I said, "Well, what's so interesting?" She said, "Well, you know, we don't really know anything about pain." And I think it was very exciting to me that this was sort of a new, open, virgin territory, and that held a lot of appeal. So between that and the gate theory, when I got here, I don't remember whether in coming here I knew I was going to work on pain, but it was certainly in my mind. And as I started thinking about what I would do here, it, that thought, you know, developed, and I came up with an idea for a grant, you know, something I could apply for, an experiment that I could work on and put in a grant for, based on Pavlovian conditioning ideas and that maybe you could condition the response to pain, and the --

Oh, yeah, Pavlov [Ivan Pavlov, 1849-1936] had done this experiment, or one of his students had, actually, years ago, it was published in French. Madame Araviyeva had done this experiment with Pavlov in which she took a dog and gave him a severe shock on the leg, and the dog howled and screamed and struggled.

MELDRUM: Oh, poor thing.

LIEBESKIND: Yeah. And then she did it again, and the dog was hungry, and immediately put food in front of the dog. And the dog ate the food. And then she shocked him again and gave him more food and shocked him again and gave him more food.

MELDRUM: Oh, yes, I think I've heard this.

LIEBESKIND: And so she turned the shock into a conditioned stimulus for food. And that changed the unconditioned response to shock into a conditioned response to shock. And the dog afterwards would salivate when he got the shock instead of that lunging and crying out. And so I thought this would be a fun thing to try in rats, and there in fact was a study on that, it turns out, by a former Choatie [he laughs], a guy I knew who had become a professor in Pennsylvania [David Williams]. But there was one study on this in rats, which was awfully good. But it was just a behavioral study, and I wanted to use this as a marker for recording the evoked potentials in the brain that had to do with the perception of pain and how they would change when the animal's perception of the pain changed.

MELDRUM: Oh, John, that really *is* creative! [she laughs]

LIEBESKIND: I thought that was pretty creative! [he laughs] So I wrote up a whole grant proposal on this, and during my first year here, and I got, as soon as I got here, I applied for

some kind of a small grant and got that very quickly. So I already, my first summer here I already had summer support --

MELDRUM: Money. Right.

LIEBESKIND: -- and just was ready to roll. And while I was working on the big grant proposal, then I got that. And in fact I got called by another, by a Michigan colleague, former Michigan graduate student who was now a big shot at NIH, and he said, "John," he said, "I've got good news for you." He said, "We've got two study sections, or two divisions here that looked at your grant, and the National Institute" -- two *Institutes*; what am I talking about? -- the National Institute of Mental Health and the National Institute of Neurological and Communicative Disorders and Stroke or whatever it was called at that time, Blindness, I think it was at that time -- "and they both are willing to fund your grant. You're jointly listed between the two, and they both are willing to fund it."

I said, "Well, what should I do?" He said, "Well, one of them would have to cut you a little bit. NIMH would have to reduce your budget; they want to cut this little thing out, but the Neurological Institute will give you the entire thing." I said, "Then, that's it. I'll take the Neuro grant. Why would I take a cut?" So I, you know, I went with the Neurological Institute [NINCDB, now NINDS], and I kept that grant until a few years ago. That was the one grant I had; I kept it for whatever it was, twenty --

MELDRUM: Twenty-five years?

LIEBESKIND: It was more than that. Twenty-eight years or something.

MELDRUM: Yeah. Right.

LIEBESKIND: I just kept renewing that grant. It's the only grant I ever had, really, that I ever applied for.

MELDRUM: Golly.

LIEBESKIND: So, you know, I kind of got going on that, and then Dave Mayer came to work with me; he was my first graduate student to come to work with me. And I have a lot to say about that; I don't know if you want me to start in on that now or say a little bit, or you want to break for lunch or what?

MELDRUM: Yeah, we probably should break for lunch. Let me just ask, go back to the gate control theory, since we've now mentioned that. Now, you said -- I've heard you say it -- that this brought you into the field of pain.

LIEBESKIND: Yeah.

MELDRUM: That this was *the* most influential article in the field of pain, probably because a lot of other -- well, tell me why you think that. I won't put words in your mouth here.

LIEBESKIND: Well, I have alleged, and I think probably everybody would agree, that it is the most influential, single most influential piece of writing that's ever been done in the field of pain. Maybe somebody would disagree; that would be an interesting conversation. I assume it has vastly more citations, if you were to do a citation analysis, than any other piece, whether it be a book or a research article or anything. So I think there would be indices of that sort that would indicate its incredible influence on the field.

Perhaps I speak too strongly when I say that it so much influenced me; I certainly was very attracted to the article, and interested. I found it intriguing. I'm not sure I understood it all that well, but the general position that it took and so forth I found very intriguing. I honestly don't know. I mean, I can't tell you how much I'd been thinking about pain before then versus how much afterwards, but I suspect -- you know, that was after all a long time ago; that was thirty years ago -- but I suspect that it had kind of at least solidified my interest in this field. And I came to view it that this was a small field that was just getting started; here was an interesting article that had come out; there was very little else going on in the field, a few scattered people, and that, by God, if I came into this field and did anything at all valuable, it would make a, it would have an impact, because there was so little going on in the field, and I was very concerned that the work that I did would have an impact, that it would be important, that it would advance knowledge.

And I looked at some of the other fields, and I said, "Well, those are really very crowded," and you could do a decent piece of work and it wouldn't matter to anybody, because there was so much else going on that you would have to do an exceptional piece of work for it to be noticed, for it to be influential and for it to make a real contribution, whereas here we're in a void, and if I just show anything, it could help somebody else show something better. So the idea of being, you know, a large frog in a small pond versus a small frog in a large pond, was kind of, was in my mind. It was up front; I was consciously aware of this idea. I think I chose the field partly with that in mind.

MELDRUM: Well, good career strategy, for sure.

LIEBESKIND: Well, it was very good for me, as it turned out, because the field was a stock that rose; I bought it cheap.

MELDRUM: [she laughs]

LIEBESKIND: So it could have been a stock that went nowhere --

MELDRUM: Declined, yeah.

LIEBESKIND: -- that declined.

MELDRUM: But also you contributed to that, too.

LIEBESKIND: I think I did.

MELDRUM: Okay. So did you draw -- how do I ask this? So certainly the gate control theory had an influence on you in sort of encouraging you to think about this field --

LIEBESKIND: Exactly.

MELDRUM: Realize the possibilities inherent in this field --

LIEBESKIND: That's right.

MELDRUM: -- but you didn't necessary draw your research ideas directly from that?

LIEBESKIND: No, I don't think it did, no, this counterconditioning thing or this Pavlovian thing I don't think had anything much to do with the gate theory -- I mean, in a general sense it probably related in terms of the idea that you could modulate pain, that pain was a -- modulation of pain was certainly the gate theory, was an important element in the gate theory. So that may have contributed in some general way.

But no, I mean, it wasn't like I took some idea directly from the gate theory and said, "Mm, maybe I should run with that or test this idea," or something. Many people did, and got results that ended up being critical of some of the propositions of the gate theory because they said, "Here's a testable idea," and they took it and it didn't work, you know. So the gate theory had a lot of enemies within a few years, and as well as some friends who did find confirming kinds of things; there were both kinds.

MELDRUM: Okay. We're going to pause now because we're hungry.

LIEBESKIND: Twelve fifteen.

[BREAK FOR LUNCH; TAPE OFF]

LIEBESKIND: Okay!

MELDRUM: And our needles are moving. Okay, so we're getting back to work. It's five minutes past one.

LIEBESKIND: All right.

MELDRUM: Okay. Let me -- I have one more, actually a Paris question to ask you, or Madame Fessard question, which is the same one I asked you about Steve Fox. Are there any particular things that you learned in terms of ways of working for Madame Fessard, any particular habits or qualities that you picked up from her in terms of your scientific work that you consider valuable, or invaluable? [both laugh]

LIEBESKIND: I can't think of anything off the top of my head, no. I think it's a good question. I certainly was a lot more directly associated with Steve Fox; we were pals, we hung out together

and so forth, and with Madame Fessard, for all kinds of reasons -- age and whatever, social status -- it was a much more formal relationship, you know, and I didn't spend as much time with her. So I don't know what I would say.

Somehow, when I think of her, I think of only wonderful things; I'm very fond of her, and we had a very pleasant experience at that time and again several years after I came to UCLA; we got together one summer at Holliman Air Force Base in Alamogordo, New Mexico, studying chimpanzees while [with German accent] German rocket scientists were loading rockets down the hall or something [he laughs]; it was a fairly interesting kind of thing. Again, we, that was sort of a fun, very personal experience that we had together. But as far as lessons about science, I don't really know, I can't think of anything off hand..

MELDRUM: Okay. So when we left off, then, you were at UCLA. You had just received your grant, and you were ready to start work on your Pavlovian rat project?

LIEBESKIND: Yeah. Well, it probably says a lot about me and I think Dave Mayer, who fully shares the credit or blame at this juncture because the fact is that we messed around for a while together for a while, trying to prepare for this study; we, somebody had given me three monkeys when I got here, beautiful monkeys, beautiful *Nemestrina* macaque monkeys with long, delicate fingers like you'd expect on a pianist [*Macaca nemestrina*, pig-tailed macaques]. I mean, they weren't friendly; they didn't lick your hand; you had to be careful with them. But the thought was that we would, originally, we would do the experiment on them, implant them and so forth, and I never really wanted to. And we never did, and I ultimately just gave them away.

So we realized, and I sort of credit Dave with this as I credit him with so many things, I think he said at one point, "You know, planning to do research on monkeys is very intimidating. You have to be so careful. Let's just grab a bunch of rats; they're just meat." So there was a concept of "don't overplan; get going on this thing." There was also the concept that this experiment was much too complicated and really wasn't worth doing, and we never did it. We never did that experiment.

MELDRUM: Oh!

LIEBESKIND: It was too fancy. And we started doing some other stuff, and one thing led to another, and before very long we hooked into, somewhat serendipitously, an area of the brain about which relatively little was known -- there were a few studies on it but not much -- called the periaqueductal gray matter. And that became the structure of [he laughs] my laboratory. I mean, we worked on that --

MELDRUM: Liebeskind territory. [she laughs]

LIEBESKIND: That became kind of Liebeskind territory, as it were. We had been going along, the first study that we ever did was kind of a rinky-dink little study; didn't come up with very much and was something that was kind of small effect. And then we did the same thing, but we did it in the periaqueductal gray matter instead of this other brain area, and wham, we got this big results, sore-thumb data. No statistics need apply [he laughs]; no statistics--no statisticians

need apply. And that was the kind of a thing; I mean, that's what we wanted -- big data, you know.

MELDRUM: Right. Make an impact.

LIEBESKIND: Yeah. Well, make an impact; just the results would be really clear. And so we kind of picked up on this periaqueductal gray matter and stuck with it for a while, and we ended up doing work on [he laughs] a number of other brain areas, et cetera; we didn't necessarily stick with it for very long. But, you know, for a number of years we did, and everything we did with that area of the brain has worked out very well and very clearly. And we decided that we would use it as kind of a model area of the brain after we do the pain in some manner, and kind of address it with a number of different techniques from different areas looking at this part of the brain and its relation to pain.

And so we did lesion studies; we recorded the electrical activity from this area of the brain; we electrically stimulated this area of the brain; and these were sort of the three major techniques that people used in behavioral neuroscience and physiological psychology. You ripped out one of the gears out of the watch and made a lesion and [you'd] see which part of the watch would stop working [he laughs]; what stopped working? You would record the electrical activity; you would listen to the workings of it, or you would electrically stimulate it, for which you would have to be in that area. So --

MELDRUM: But it was fairly clear that some kind of pain reaction occurred in this particular area of the brain?

LIEBESKIND: Well, that was known before we got into it. We didn't -- we were not the first to say that this area of the brain had something to do with pain. But we pursued that story in an integrated way. There were isolated studies here and there on one aspect of it or another. And I don't know; things were going very slowly in my assistant professor years; I got off to a good start in teaching, I got this grant right in the beginning, so that looked good, but I was not publishing very much, just sort of eking out a few things here and there.

And around tenure time, I was actually in trouble without really being aware of that. I don't think I ever really knew that I was. People sweat tenure, as we were talking about at lunch the other day, but I didn't want to interrupt the flow of Sharon's conversation [Professor Sharon Traweek at UCLA], but if I had, it would have been to say, "I didn't." Now, either I'm just totally repressed [Meldrum laughs] or crazy or something, I don't know, but I think I can honestly say I never really worried about that, and yet I almost didn't get it.

MELDRUM: Now, I've got my chronology mixed up here, I guess. You came to UCLA --

LIEBESKIND: '66.

MELDRUM: -- in '66.

LIEBESKIND: January, '66.

MELDRUM: So wouldn't you have been up for tenure in 1973?

LIEBESKIND: It was earlier than that. There's no magic time, as you know; you come up whenever you're ready or something. But I came in --

MELDRUM: But there's sort of a deadline, isn't there? I mean, after a certain --

LIEBESKIND: Well, it had been eight years up or out -- seven years up or out the eighth year; there has to be a last year. But I'd have to double check my CV.

MELDRUM: But before that.

LIEBESKIND: I think it was '72 that I got tenure, '71 maybe I was applying for it or something, between '70 and '71 or something like that. In any case, Don Lindsley [Donald B. Lindsley, 1907-2003], who was a very important, now long since retired from this department, member of the National Academy and all that, more than anyone else responsible for my election to the Academy this year, was always kind of a fan of mine. And he probably had something, though he wasn't the chairman of the department when I came in; he had just recently stepped down from being the chairman, but probably had a lot to do with my getting a job here. He has told me that the then-academic vice chancellor, David Saxon, who went on to become the president of the University of California, called him into his office and said, "What's with this Liebeskind character?"

MELDRUM: [she laughs]

LIEBESKIND: "The department is recommending him for tenure. You know, it doesn't look too great. What do you think?" And Lindsley said, "Keep him, good man," [he laughs] or something. Stood up for me, and I did get promoted. But I didn't have very many publications. I had some from the Paris days and one with Steve Fox from way back, and a couple of kind of smallish things that we had gotten out here, but there was one paper that we had written that actually came out in 1971 in *Science* that was my first truly major paper, and that -- I can't remember exactly; I'd have to look into it -- but that was either under review or in press at the time, somewhere in the time that the department was considering me and the ad hoc committee was considering me and the dean was considering me and CAP was considering me and the vice chancellor -- somewhere in there, that manuscript got accepted for publication. And whether it actually came out or not -- and that was an important piece of work. That stands now.

That work became a quote, "citation classic" [he laughs], according to *Current Contents*. And it was the, you know, if anyone said, "Well, what has Liebeskind done, his lab done?" they would point back to that paper as probably the most important thing we ever got started, most important thing we ever did, and so forth. And so that was a paper in which we electrically stimulated the periaqueductal gray matter, which was one of three papers we published -- first three real papers that I published here at UCLA was a lesion study of periaqueductal gray; that came out in 1970, a recording study of periaqueductal gray which came out in '71, and this paper, which came out in '71.



MELDRUM: One, two, three.

LIEBESKIND: One, two, three. We did the three things, and this stimulating paper was the one in which we showed you can produce analgesia; you can produce pain in addition by stimulating this area. And so that really was the real beginning of my career because that was a phenomenon, this phenomenon of stimulation-produced analgesia, or SPA, with which my lab became identified -- we were not the first to do it; we were not the first to stimulate the periaqueductal gray matter and find analgesia. We were second [he laughs], which is in itself an interesting comment on science.

MELDRUM: But you were the first that sort of got broad notice.

LIEBESKIND: That's -- well, the other guy did not exactly bury his article; it was also published in *Science* in 1969, two years before us. But he didn't really understand, I think, what was so interesting about this. His article didn't push it; he just said, "Well, we found it, and maybe this will be of use some day," or something. And he kind of dropped it at that. Plus, he kind of dropped out of the field. The "he" in question is David Reynolds, and he hadn't done much before and he didn't do much afterwards. So it was sort of an instant lesson that I would take from that is that a single paper, unless it's truly amazing -- I mean, finds a cure for cancer or something -- tends not to have that much impact. I mean, you have to kind of pursue things and sell things, almost.

MELDRUM: Sell your work. Right.

LIEBESKIND: Kind of sell your work for it to have impact. You have to kind of keep pushing on it, telling, building on it, and he didn't build on it, he didn't push on it, and even the original article that he wrote didn't make much of it, and we made a lot of it in that article, which leads to a second point I want to make. And let me build up to it by saying that when we first submitted the article, it was rejected by *Science*. And I called the acting editor, the acting, the deputy editor or associate editor, whatever it was called, John Ringle, at *Science*, and I said, "Look, Mr. Ringle, there's something wrong here. You've sent back the reviews; they're not bad; they didn't point out anything wrong, and yet the article hasn't been accepted, and I think it's really a very important piece of work." And he said, "Well," he said, "this happens all the time. We're very selective, and it's not that there's anything wrong; I realize that."

He said, "I'm sure it's a fine thing, but it didn't get the very top ratings that are required -- you know, we rate it one to five, and you need two fives from both referees," or whatever it was, "and this one didn't quite get that, so we can't accept it." So I said, "Well, again, this is an important piece of work. Somebody's not understanding that." I said, "That must be our fault; we didn't explain it well enough." And I said, "So I'd like to rewrite it and submit it again." And he said, "Well, we have provision for that; you can do that."

And I said, and this required incredible *chutzpah*, which for some reason I managed to find at that time and I have found at critical times in my life, where ordinarily I'm a mild-mannered, modest person, sometimes I come up with incredibly *chutzpahdich* things -- and don't ask me to

spell that; I don't know how [Meldrum laughs] -- and I said, "Mr. Ringle, I don't know if you're a Mister or a Doctor, and if you are a doctor, I don't know what field that you're trained in. But I really believe that this is an important piece of work, and rather than just rewrite this and submit it and have you send it out again," I said, "I'm eager to get this thing published." And I said, "I would like *you* to look it over yourself." And I said, "If *you* don't think it's really interesting and important, then you tell me that and you won't hear from me again about it."

MELDRUM: Boy!

LIEBESKIND: "But if you do think it's important, then publish it." I said, "I'm going to write it for you, but I don't know who you are." It was something to that effect; I'm probably making it sound more dramatic than it was, but it was something to that effect. And he bought into that. And he said, "I'll give you a tip." He said, "It's a little too long, and make the introduction a little sexier." That was his advice to us: "Cut it by about twenty-five percent." So we made the introduction a little sexier, we *lengthened* it by about twenty-five to thirty percent, and he accepted it by return mail.

Now, we lengthened it -- why, what did we do? And I think there's, again, a critical story there. It was that we basically rewrote it, and the whole thing, but the main thing we did was we added a true discussion section, which really painted a whole picture. If you look at the article today, the reprint, you'll see it's a four-page article [Mayer, Wolfle, Akil, Carder, and Liebeskind. *Science* 1971 Dec 24; 174(16): 1351-4.]. And the whole fourth page -- not the whole page, but it's a half page or whatever -- but that whole last page is all what was added and all new; it wasn't in the original manuscript. And what was there? Well, what was there was, I think, a very nicely laid out argument about what this all means, that you can electrically stimulate this part of the brain and inhibit pain, and not only what it means, but kind of a blueprint, it turns out, for what subsequently was done. I mean, it like got at really the key issues that became the key issues in this subfield of pain research, this pain modulation field. It kind of defined what came later.

And there was one key idea in there -- I don't know how much you wanted to get down --

MELDRUM: Keep going.

LIEBESKIND: -- on this specific line, but there was one key idea in there which is hard to explain briefly, but fundamentally what it had to do with was the assertion that in stimulating this area of the brain, we were activating a normal function --

MELDRUM: Oh!

LIEBESKIND: -- normally taking place in this area, that pain inhibition was a function of the brain, a normal function of the brain.

MELDRUM: Wow.

LIEBESKIND: And that was a very bold thing to say, but Dave Mayer had an idea as to why we could say that. He had been trying to explain this to me for a long time without success, and I would hear him and I would say, “Yeah, blah, blah, blah, you know, I kind of know what you’re talking about, but maybe we’re wrong; maybe this can be proven wrong in some way. It sounds dangerous. How do we really know?” And so when we wrote the article up originally, we didn’t have any of that in there, and after we got it back to rewrite it, I said, “Well, Dave, tell me this one more time,” or whatever, or a hundred more times [he laughs], or whatever it was. We sat down to it again, and this time, finally, I kind of really got to understand what he was talking about. And I found a way to explain it.

So I think a kind of theme of my laboratory after that was that where I helped out was making these very bright, wonderful students that I had, making them, forcing them, to really think carefully about what things meant and finding good ways to express that, clear ways, backing it up and making it clear so that anyone could understand it. If John Liebeskind can understand, then anyone can understand. And I think I contributed to that not only by kind of forcing them to do this and insisting on this, but in also finding good ways of expressing things. I’m capable of writing well and speaking and writing with clarity, and I think any of my students would acknowledge that is one of the things that I have done.

We can talk about this more later, but it’s a fact that I basically never worked in my laboratory; I almost never touched a rat; I almost never went down there. I would stay upstairs on the 8th floor, and my lab was on the C floor. So many jokes have been made about does Liebeskind know how to find his lab, would he know where it is? [both laugh]

MELDRUM: Give him a map and he’ll find it.

LIEBESKIND: Like in this recent little reception the department gave me, some of my colleagues gave me, after I got into the National Academy, one of them roasted me by -- the dean was there, the department chairman was there, and a few other of my colleagues, and he roasted me after dinner by saying, “I just want everyone to know that,” he said -- this is Gaylord Ellison; he said, “I just want everyone to know that I had something to do with Liebeskind’s getting into the Academy here. He deserves a lot of credit and we all acknowledge that, but I deserve some credit, too.” He says, “Liebeskind got a lot of publicity for his work; the TV cameras would be rolling down there on the C floor, and when they would need him to come down to be interviewed on the C floor in front of the camera, I was to go up and show him how to find the lab!” [both laugh]

MELDRUM: That’s funny!

LIEBESKIND: But there’s a lot of truth to that. So, you know, what role I did play and what I brought to the situation is something that I think needs some discussion. But here already is the beginning of my career. Dave Mayer had a very good idea, and I think that, at least in part because of me, we found a good way to express that. And that’s what made that article sing, in my opinion. And I think it has, that article ended up being very influential. And, again, not just because people read it; you had to hit them over the head with it, tell them about it, but then they would go back and read it, and talk about it and it was all there, so we got a lot of credit for that.

MELDRUM: And people used it, then, too.

LIEBESKIND: People used it, yeah. So I think Reynolds deserves a lot of credit for finding the phenomenon that he found; he wasn't looking for it, and that gives him, in my mind, all the more credit because his eyes were --

MELDRUM: You knew his work before.

LIEBESKIND: -- we knew his work, absolutely, and we sought, in fact, to show that he was wrong; we couldn't believe it, and found that he was right.

MELDRUM: Very interesting.

LIEBESKIND: Oh, yeah. In fact, we were working on the periaqueductal gray and knew very well its relation to pain; we'd stimulate the periaqueductal gray and saw, as others had seen before us, that it *caused* pain, and here was this jerk saying you could stimulate the periaqueductal gray and *inhibit* pain! And we figured there were, you know, probably some very good reasons to believe he was making some mistakes -- holding the animals down and restraining them, and that was why they were giving up and not responding and it had nothing to do with the brain stimulation and so forth, so we had even some theories as to why he was wrong. And we knew about his work not from the *Science* article, which came out only later, but he gave a talk on it. It was some crazy meeting; he --

## JOHN C. LIEBESKIND INTERVIEW

### TAPE TWO, SIDE TWO

JOHN LIEBESKIND: David Reynolds was trying to stimulate the brain to find where you could produce sleep. Everything going all right there?

MARCIA MELDRUM: Mm-hm.

LIEBESKIND: And he didn't; he couldn't find it. But he said, "Oh, well, I couldn't find what I was looking for, but blocking pain -- that may be interesting and helpful." So yeah, we saw -- and we got a circular in the mail; here's this funny little meeting on electrosleep going to be held next month in San Francisco, and here's this paper going to be delivered by somebody named David Reynolds, who we'd never heard of, who stimulates the periaqueductal gray and inhibits pain. And we looked at that and we said, "What? This guy's crazy," you know.

So I sent Dave Mayer and another student, Tom Wolfe, who was working with us. I said, "I'll pay for you guys. Go on up to the meeting in San Francisco and hear this guy and see what the hell he's up to and get a little trip out of it; and blow him out of the water. This guy's some kind of turkey. See what he's got going on." They came back; "John, he's got something; it looks real. He even showed a movie," or something, "and it looks like he knows what he's doing." So we started to work, to look at that in a different way, and here we'd been reliably stimulating this area of the brain and causing pain, and now that we knew you could stimulate it and inhibit pain, we found that was true, too. Not that it was either-or; it could be both.

MELDRUM: Was it a matter of the difference in the stimulation or a different area?

LIEBESKIND: It matters how you look at it. You can stimulate the brain and the animal clearly doesn't like it; I mean, he learns to escape from it and so forth. He will cry out, motor behaviors and so forth, to indicate he doesn't like it. And then you turn it off, and you start pinching him, and he doesn't respond. So after the stimulation, his pain is inhibited. You can see it that way, or you can sometimes play with the current and lower it until you just get the current below where it seems he doesn't like it; and now he doesn't cry, he doesn't move, he's just sitting there getting his brain stimulated. But during that time you can pinch him and he doesn't feel pain. So there are different ways you can look at it, but you can show that the same place that you stimulate both can produce pain at a higher current, or during stimulation, and can inhibit pain at a slightly lower current or after stimulation.

MELDRUM: Can inhibit pain at a slightly lower current and inhibit pain produced from another source.

LIEBESKIND: Right.

MELDRUM: Ah. Very interesting. Well, you realize I don't know anything about this. [she laughs] I'm just learning.

LIEBESKIND: Well, I do realize, and I'm very capable, in the fullness of time, of talking for hours about my work in a way that you would understand, which we're not going to do now, but not that I can't do that. I can make you understand it very easily.

MELDRUM: So okay. Let me get back and ask, what was, I mean, you really pushed this article. You called this acting editor up and you --

LIEBESKIND: Right. We had a concept.

MELDRUM: You called his hand -- [she laughs]

LIEBESKIND: As it were. We invented a concept, which a lot of people heard about and made a lot of funny jokes about later; we called it "wrangling Ringle." [Meldrum laughs] Mr. Ringle. This story got told about how Liebeskind wrangled Ringle and got his article in.

MELDRUM: So what made you sort of fix on this article? Did you have a sense that it was going to be a critically important article?

LIEBESKIND: We knew very much that, yes. We were very aware that this was a fascinating phenomenon, very powerful phenomenon, one that --

MELDRUM: And that thereby by dramatizing it, which Reynolds had not done, okay.

LIEBESKIND: Well, it's more than just dramatizing it; we felt that we understood something about it and understood it in a way that itself was as important as the phenomenon.

MELDRUM: It's putting it in an intelligible context.

LIEBESKIND: That, well, by the way we understood it would lead to other things, would lead to other kinds of experiments and applications and so forth and so on. So that, I mean, for example, one of the things that we said in the article, of course -- it wasn't in Reynolds' article at all -- was, we drew some fascinating parallels between how brain stimulation produced analgesia and where in the nervous system it produced analgesia, and how and where opiate drugs worked.

So we were the first to make that link to opiate drugs, and of course we followed that up by showing a year later, 1972, we published in a little article in French, while I was back in Paris on a sabbatical leave, we published just a little, teeny note here saying that naloxone blocked stimulation-produced analgesia, and that tore the roof off in a way that we did not anticipate. We didn't know what that meant at the time; we just said, "Oh, yeah, morphine and brain stimulation work alike," but there were people who knew that the endorphin revolution was about to happen, and this little humble finding of ours stuck away in French, in a French journal, some people heard about. [Akil, Mayer, and Liebeskind. *C R Acad Sci Hebd Seances Acad Sci D*. 1972 Jun 26; 74(26):3603-5]

And one of my students -- again, someone who's gone on to great fame as Dave Mayer [professor of anesthesiology at Medical College of Virginia] has done, named Huda Akil, a Syrian woman, H-U-D-A, A-K-I-L, Huda [now Gardner Quarten Professor of Neuroscience at the University of Michigan] gave a presentation on that naloxone work at an international pharmacology meeting we were at; you know, Avram Goldstein was there and Sol Snyder was there, and, most importantly, Hans Kosterlitz [1903-1996], who picked up the ball and ran with it all the way to the discovery of the first of the endogenous opioids, enkephalins, in which he credited us as having helped him see the world more clearly and go off in that direction. So books have now been written on the great discoveries of the endogenous opioids and the opiate receptors, and I have one of them, that has an early chapter, or subchapter, it's a heading of a chapter, "Early Work of the Liebeskind Lab" or something. [both laugh]

So it got into the lore. And so, you know, we saw things clearly and our work proved heuristic and correct. And so there are a number of things to be said about that first *Science* article experience, but it galvanized me in a lot of ways. It was like an electric bolt through my career because, well, for one thing it made me want to publish all our exciting articles in *Science*, and I have nine *Science* articles to my credit, about which I could not be more proud. That is the single thing in my professional career of which I am most proud, apart from my students -- who they are and what they've done on their own and what I've been able to do to help them. But from the standpoint of kind of facts, it's -- we've gotten nine *Science* articles out.

And even when our articles were not published in *Science*, as of course the vast majority were published elsewhere, specialty journals, still, I would always push the discussion section and try and be very open about what I thought this meant and try to be very clear and have the discussion not only relate to what has been done before, but kind of make suggestions about what might be done next, so that I think our work has been influential, in part because of the way that we wrote and the emphasis we gave to the discussion section to thinking and talking about our work. And this got translated over to the talks, and I got invited to do a lot of talks, and Dave Mayer gave a lot of talks, and Huda Akil gave a lot of talks, so, you know, I mean, on and on. So kind of the word spread, as it were, and people caught on to these concepts and then pursued them; they were interested by them. In the way that I was interested in the gate theory, other people became interested in our work and so forth in ways and pursued different aspects of it, because they were, I think, because they were attracted to it. So it's a kind of a selling. Maybe I never should have gone into science; I should have sold coats and brassieres in my father's store. [both laugh]

One of my graduate students [Gideon Urca], an Israeli, said, "John, as a scientist, you would have made a wonderful used car salesman." [both laugh] And I think there's a certain truth in that.

MELDRUM: I think in addition to selling, what you're doing is you present your findings, which most scientists do, in a clear, scientific manner, but then you draw the connections.

LIEBESKIND: We try very hard to do that.

MELDRUM: Or suggest the connections to other areas and other lines of inquiry.

LIEBESKIND: Yeah. I like to tell the story again, because I think it's funny, but it also sort of typifies the approach, of an article we once wrote for *Brain Research*, and we published a lot of our articles in *Brain Research*, and at least through the mail got to know the editor fairly well [he laughs] in correspondence, Dominic Purpura, and he still is the editor of *Brain Research* and has been almost since the beginning. And for many years, twenty-five years or more, he's been the editor of the journal. And we wrote an article once, and it had a kind of short introduction and very short methods and a *very* short results section, and then this gigantic discussion section. And he had penciled in a remark on the discussion section, which was, "Get serious, John," [he laughs] you know, which, I mean, that just typifies this thing to me that we push so hard that we sort of angered him almost about this discussion section. And in the end we cut it a little bit, but we tried to preserve as much as possible, and we did. And he let it go through, and it too was influential.

I mean, so, you know, we always pushed on that; we wanted to present important findings and what we think about them, what we think they mean, what we think they can lead to, and I've tried, my lab has tried very hard, and I think our -- not always succeeded --but I think our best work is characterized by that kind of emphasis. And some people no doubt resent it. You know, some people think that's not the way to do science, and for me, I think I'm sort of different from most scientists. For me, the heart of an article is the discussion section, and many people would, I mean the vast majority of people, would disagree completely and say, "That's just puffery. The real issue is the results. What methods did you follow? Or the historical background?" or something. "But God forbid, not the discussion section." So I just turn it all upside down.

MELDRUM: That's so unusual, you think?

LIEBESKIND: I think it is. Most scientists wouldn't agree with that. I think a lot of scientists play their cards very close to their vest, and theory and what you think about things really is quite immaterial; it's got to be impersonal. Here are the cold, hard facts; here's exactly what we did; here's exactly what we found; goodbye. And, you know, I'm not saying everybody, but I think there's a lot of that.

MELDRUM: Well, in addition, you might have made, in addition to making claims that might be considered puffery or making statements that might be considered puffery, you might make claims which later prove to be unfounded?

LIEBESKIND: Yeah.

MELDRUM: Have you been criticized on that basis? As speculating beyond your data?

LIEBESKIND: Yeah -- not so much. I think -- I don't know. I think we found a way of doing that responsibly. I think there's something between making overexaggerated claims and not saying anything, and that something in the middle is, "An interesting idea which you boys and girls may wish to consider -- "

MELDRUM: [she laughs]



LIEBESKIND: “Of course, we haven’t proven it, but it might prove heuristic to consider the possibility that da-da-da-da-da.” And, you know, you do something in that way, you’re not pushing it too hard, but you’re letting it, you’re running it up the flagpole a little bit of the way. You see what I’m saying?

MELDRUM: Right. Exactly.

LIEBESKIND: And I think we found ways of doing that and being kind of modest and -- modestly bold or something.

MELDRUM: Yeah. Suggestive but not overconfident. Very interesting.

LIEBESKIND: Well, I think there are different styles of science, and I think that my laboratory exemplifies a style that not everyone espouses, and some people perhaps are, or would be, very critical of it, maybe have been very critical. It’s hard to argue with success; we’ve been successful, and maybe some people deep down in their bowels do not much like what we do, but they have to acknowledge we’ve been successful. So they wouldn’t say too much about it, but they wouldn’t do it that way. Or if we had been spectacularly wrong at some point and we’d said something, you know, somebody had blown us out of the water totally, and we’d had to make like a major recantation or something, there might have been a bunch of people out there who would have been applauding like crazy. “Finally those sons of bitches got their comeuppance!” you know. [Meldrum laughs]

And that never happened. I mean, we -- not to say our work hasn’t been wrong at times, but never in a major blowing-us-out-of-the-water way. There was an article that, in fact, “We gave naloxone; we couldn’t inhibit brain-stimulation analgesia,” and ultimately we showed, yeah, it depends where in the brain you stimulate. Here you can stimulate and then inhibit with naloxone; stimulate there, you can’t. [*Author’s clarification: You can block SPA with naloxone if you stimulate here, but if you stimulate there, you still get SPA, but naloxone doesn’t block it.*] And so both sides are correct.

MELDRUM: Yeah, but that’s a sort of a “can you replicate the method” question.

LIEBESKIND: Yeah. That’s right.

MELDRUM: I mean, everybody gets into those sorts of things.

LIEBESKIND: Exactly.

MELDRUM: Okay. Well, that’s very interesting. So this was the paper that made your big --

LIEBESKIND: Well, it was the start.

MELDRUM: -- sort of moved the Liebeskind lab into the picture.

LIEBESKIND: Into the limelight. No question about it. And it was at a time when the pond was still very small; this was 1971 when our article came out, and two years later John Bonica invited me to Issaquah to the pain meeting. [he laughs] I met Melzack, I met Ed Perl between those, between 1971 and 1973. Ron Melzack and Ed Perl both came to visit UCLA to give colloquia. And that proved an occasion for me to meet both of them for the first time.

And I brought Ron Melzack down to the lab and I said, "Look what we're doing. We stimulate the brain and da-da-da-da." This was before our article came out, so this was in 1971 or '70. But we were working on it at the time. And he said, "Wow, that's incredible! You can stimulate the brain and inhibit pain! That's exciting! Wonderful!" And I said, "Well, you know, it's great, but, you know, it's just basically replicating what David Reynolds showed in *Science* in 1969." He said, "Who?" I said, "David Reynolds! In *Science*!" Not in the *Albanian Journal of Irrelevant Results*! He didn't remember this guy! Must have missed that article. So this issue of impact keeps hitting me in the face.

And Ed Perl was out here, and he was not somebody I would have known about, but some people in the medical school invited him, and I'd heard that he was coming, and I called the people who had organized the lecture, and I said, "I'd love to meet Dr. Perl." And they said, "Well, you know, we're having a dinner for him and a few people are coming," and so on and so on. And I said, "Well, can I -- is there any other time I could sort of spend alone with him? Can I like pick him up at the airport?" "Oh, well, I'm picking him up at the airport; I've already arranged that; I'm a personal friend. On the other hand," he says, "we haven't got anyone to take him *back* to the airport. We were going to put him in a cab because I would normally have taken him back, but I have a conflict." "I'll take him back." So after his talk, I got to take Ed Perl -- or after dinner, whatever it was -- back to the airport, during which time I told him about our work. [both laugh]

There I am, pitching our stuff! He thought that was exciting. And kind of a sequence of things came out of that. He invited me to come and give a talk at Chapel Hill [University of North Carolina], and I think I did that kind of en route to Paris, where I went back in '72 on sabbatical. He invited me to be part of a symposium on pain that he had organized at the first meeting of this new society called the Society for Neuroscience. And somebody was in the audience of that who came up to me afterwards, somebody named James McGaugh, from the University of California at Irvine [Director of the Center for the Neurobiology of Learning and Memory there], a member of the National Academy as a matter of fact, this year head of the psychology section of the National Academy of Science. He came up to me after my talk and clapped me on the back, and I don't remember exactly, he was *very* complimentary and invited me down to Irvine to give a colloquium.

So I mean, it was like getting notoriety or acclaim for this work. I later wrote to James McGaugh, and he started a new journal at that time or shortly around that time. I wrote to him, I said, "Geez," you know, "if you need somebody to do some reviewing, I'll be on your editorial board," he wrote back, "I would love it! Be on our editorial board!" So I had an editorial board membership to put on my CV. So you can kind of see how one thing leads to another, how responsible self-assertion, all right, not, "Hey, I'm terrific; hey, everybody, notice me!" but

doing it in a kind of delicate way, putting yourself forward in a nice way, I hope, can have terrific effects on your career.

So I'm very much a believer that people make their own breaks, to a large extent, and that you can do it badly, in which case you'll get slapped down; you can not do it at all, in which case you'll be ignored, or somewhere in the middle, you can do it well, you can do it in a way that, you know -- there are a lot of examples in my career where I can point to something good that's happened to me, and I can point to what I did to make it happen even though it technically came from outside, from elsewhere. I helped it along in a kind of not-too-self-promoting way. I mean, I think there's a delicate borderline there, and I skirt that borderline, maybe sometimes stepping over it in some people's minds. I'm sure of that; I'm sure some people resent the way I operate, choosing that word carefully -- "operate" -- they view me as an operator. But personally, I'd have to say I think I've played it pretty well for the most part. I wouldn't have gotten to where I got if I hadn't played it.

MELDRUM: Well, you seem to have the respect of most people in your field. That's not a small accomplishment.

LIEBESKIND: Well, yeah, I think some people are really nasty and aggressive, and I've never been that way.

MELDRUM: Yeah. I think so, too.

LIEBESKIND: Yeah.

MELDRUM: Okay. Now, let's see; I've got two directions I could go from here. What time have we got? I guess, let's talk a little more about your laboratory for a few minutes. So your style in your laboratory -- [both laugh]

LIEBESKIND: Right.

MELDRUM: Was to observe it from the eighth floor.

LIEBESKIND: Eighth floor. Well, again, something figuratively, literally, happened that has -- we've made figurative jokes out of, but early on I got my grant, and it was a three-year grant, and in the second year I had to reapply, to make it continue after the third year. And I went upstairs to write the renewal, and I never came back down again. I mean, that's the way I've characterized that, and it's literally true. Before that, I used to at least pretend to work in my laboratory, and after that I never did. Never.

MELDRUM: Okay. So when your students came along, then, they were pretty much responsible for setting up their own projects, doing their own work?

LIEBESKIND: Yeah, sure. You know, it worked differently for different ones. For most of them, they came into an existing sociological entity. I mean, there was the group already there, so a new student would come in and join an existing group and would learn, they would learn

from each other. One would mentor the other. There were a couple of times, a sort of interregnum or whatever, periods during which the lab went to almost zero, where a very young student, not necessarily a totally beginning one, but a very young one, would find himself alone or herself alone.

And there were two students that I think of who particularly found themselves in that position. A young woman named Dell Rhodes, who was there when Huda and Dave -- Huda Akil and Dave Mayer, and Tom Wolfle were there, and then they all left, and she was alone at a time when she wasn't altogether ready to be alone. And some years later a student named Mike Morgan was basically alone after a rather large group had just left, preceding him. And Dell did a good piece of work, and some other people came along and she didn't necessarily train them, but she got through, and they kind of formed a new group, this other group coming through.

And in the case of Mike Morgan, he did train two new people, although he wasn't quite ready to do that; he did it, which was terrific. And a pretty girl came through; she thought Mike Morgan was awful cute and decided to come work with us because Mike Morgan was in the lab. [he laughs] He trained her, you know, and actually she didn't stay in our lab, but that's okay. She helped get us through that period. But those were just, I think, the only two times I could mention, but otherwise people always came in when there was a group here.

MELDRUM: Okay. So there would be like a group project that everybody would work on?

LIEBESKIND: Well, it wasn't always one -- no, I wouldn't say that. Sometimes it would be --

MELDRUM: Several group projects.

LIEBESKIND: Several projects; everybody would kind of have their own thing, and then a new person would come along and kind of usually latch on to somebody, and then, you know, sometimes I would direct them a little bit or not necessarily, and it varied. I mean, one thing is clear and certain; though out of the lab, I was not a puppeteer. I didn't just bring in *tabula rasas* and write on their slates; I mean, these were bright, independent people for the most part who, in some cases, certainly this was the obvious case for David Mayer, he would *not* have come to work with me if he thought I was going to interfere with what he wanted to do, and he told me that. He said, "The only reason I came to work with you, John, is I realized you wouldn't get in my way." [he laughs]

And, again, I sort of was that way, and that's the kind of student I got; I got a lot of tough-minded, independent-minded people who didn't want to be interfered with, who wanted to go their own way. And I let them. And so, you know, it wasn't entirely a passive thing; we talked, I was very close personally with almost all of my students. Again I would draw lines; I would try to keep out of, as much as possible, their personal, personal lives, but sometimes that would come in. But I tried not to be mother hen and too interfering in terms of their personal lives. Sometimes that would come up.

But certainly we worked closely together professionally, you know, and we'd talk a lot -- again, more with some than with others, and sort of just gradually, as the lab got bigger and I got more

famous and whatever, even this started eroding, and I think in the last few years of the laboratory, over the last, say, decade of my laboratory I became progressively more distant from what was going on. But yeah, I mean, we would talk about what they were doing from time to time, which direction to go, and we would certainly talk intimately at the time for writing up the work.

And the general pattern was they would write up a draft, and I would go over it word by painful word. Now, again, that didn't always happen, but for those articles that we really cared about, including *very* especially the nine *Science* articles, plus a lot of other work, we would literally pore over them for days. Some people can write an article in a day or two, you know, just spit it right out; we would pore over it and argue about words and argue it out, sometimes violently. I mean, I remember one article where I was coauthoring it with two Israeli students [Urca and Frenk], and they were the best of friends; they had come from Israel together, were the dearest of friends here, and they had a falling out over a phrase.

MELDRUM: Oh! [she laughs]

LIEBESKIND: Yeah. I mean, I and one of them wanted to include this phrase, which was kind of expansive and pushing the frontier a little bit, and the other was kind of conservative, and he didn't want to include it, and he was the first author [Urca]. And in the end, we acceded to him, but we got some kind of compromise, but it took -- I mean, they almost came to blows. They were stopping -- they wouldn't talk to each other for like a week, and I felt so broken up by that because they were dear, dear friends, and they came together again afterwards and resumed [he laughs] being the best of friends. But I mean, just to say, and I would have the same thing with the students. Sometimes it was very challenging working with them on the write-ups. It took a long time.

MELDRUM: Okay. So that was, you looked for -- maybe you didn't look for, you attracted students who were self-starters, who had initiative, who were independent, who liked working for themselves.

LIEBESKIND: I think it was a self-selection on their part. I didn't turn away very many students; there may have been a few over the years who approached me and I discouraged them in some fashion, but usually I didn't. And usually it was only, I took basically everybody who came along. And I'm not going to say they were all, you know, equally fantastic; some were more fantastic than others, but I like to think, with very few exceptions, I permitted them to do the best work they could have done when they were in my laboratory. And I think that, again, is something for which I want to take some credit. I think I did provide a terrific atmosphere for these students in which they could work and express themselves and learn and grow. And it sounds so easy.

MELDRUM: [she laughs] No, it's not.

LIEBESKIND: Well, it *was* easy for me; it didn't take effort on my part. It all came naturally, as it were, for me, but what's amazing is how few other people seem to be able to do that. I mean, you look --

MELDRUM: Why do you think that is?

LIEBESKIND: I don't know. I think that academia attracts a lot of narcissistic people, and I am narcissistic in some ways, but somehow I learned early on that selflessness is the best way to success.

MELDRUM: Well, your ego is invested in other ways, maybe.

LIEBESKIND: Maybe that's it. I don't know how to phrase it exactly. I found a formula -- I attribute it in some respects to Steve Fox more than any other individual, for working with Steve --oh, I'm so different from Steve; he's a very aggressive, you know, kind of nasty guy, and I was never that way, but I always took on, as he did, an attitude of what's in the best interest of my student is in *my* best interest. I think Steve expressed that outwardly; I think he made that an overt statement. "Whatever is good for you, John, and your career, is going to reflect back on me and is going to end up being good for me. So that's why I'm working so hard for you, to see you be successful, because that's going to have a payoff for me." I think he voiced that. And certainly, whether he did or not actually, it certainly was implicit in the way he went about things.

And it was something that was a guiding principle for me and that never failed me. Never. I always feel like I've never lost in giving up something. We would always sit down, I would sit down with the students, and say, "For the most part, what we want to do, we're going to be heading in the same direction. What's going to be good for you is going to be good for me and vice versa. There may be times when what's good for you is bad for me or what's good for me is bad for you, but when and if we come to those, we'll face that and we will just deal with it in some sort of fair manner as best we can and maybe we'll trade off, you know, and this time it'll be for you and next time it'll be for me or something," I don't know. But in the long run, we had very few instances of that. Almost always whatever was good for them was good for me. I could afford it, I wasn't like right at the edge where if I gave them a first authorship, then, ahh! or something, you know. [he laughs]

MELDRUM: Your whole career would fall apart! [she laughs]

LIEBESKIND: So it was easy to do in that sense. I don't ascribe to myself any great virtues or anything in being this way; it came naturally. But I saw a lot of other labs around me which for one reason or another couldn't make that work.

MELDRUM: Well, I think all of us -- my experiences knowing both professors and graduate students and fellows, you hear many interesting stories of labs and professors who abuse -- there's no other word for it -- who abuse their students or who, you know, just --

LIEBESKIND: Ignore them.

MELDRUM: -- ignore them or whatever, don't really give them much nurturing.

LIEBESKIND: I guess it's because I didn't do science myself that it was clear to me that any science that was going to get my name stuck on it [Meldrum laughs], had to be done by the cleverer students, and I don't know, somehow it just worked out very well for those years.

MELDRUM: Certainly seems to. You don't seem to regret very much bowing out of the laboratory yourself.

LIEBESKIND: Well, that's of course a very long story, and I'd be happy to go into it; you know --

MELDRUM: If not now, when?

LIEBESKIND: Yeah. Well, okay. I don't regret having bowed out of the laboratory. The facts of the matter are the following: I was going along, it was the biggest, in some ways the best, lab I ever had, more people doing more exciting stuff and a lot of support, grant support and everything; we were doing just, really cruising in whatever that was, 1992 or something like that. And I was up for renewal; I'd just had a [Jacob] Javits [Neuroscience Investigator] Award [from NIH] the previous year, so that was 1985 to 1992, the Javits Award was, as you know, a seven-year, special seven-year award which they don't give very often. It was a great honor, the greatest honor that I had had up to that point in my life was to get that Javits Award.

And in those seven years we did terrific stuff -- again, I think very thematic, you know. There was this interesting theme and we had these articles that related to that, and that interesting theme and these articles that related to that one and so forth, and all of those themes had things that needed to be continued. It was the perfect place to be in trying to renew the grant. I mean, like, we had these hot themes, all of which had medical-related, health-related applications. It was so clear in my mind what was the significance of what we had been working on.

And there had been periods before that when I despaired. You know, I said, "The lab is kind of just going off in various directions; there's no central theme. I don't know what we're really doing; we're just kind of putting out stuff." We were always productive, but some of it just seemed like it was stuff. And here now, finally, again, after some period of time, we were really thematically organized, and the themes were interesting and cogent and coherent, and I could explain them to other people easily. They weren't arcane, and I could say, "Look, we're working on something that could lead to this new category of analgesic drugs," and I could tell you what it was all about, and rickety-rickety-rack. "We're doing this work on this that shows that pain can be actually a mortal problem, it can actually kill," we had that idea.

And there were four or five ideas like that, all of which were, you know, resounding, kind of bell-ringing ideas. Lots of progress in each; none was just brand-new or was just an idea *in vacuo*; they were all like solidly based with half a dozen articles or more that we've already done, and each of which had lots of promise, things it could go to, places where it could grow to. And the perfect timing to be up for renewal on the grant. And I wrote what I thought was just that, a terrific grant application, best ever, and I didn't get it. I got a priority, a relatively good priority number, but not quite good enough, whatever that means.

I view it as a jiggle, you know. When you've got a five-point scoring scheme, one to five for approved grants, you know how that works, it can be disapproved, or if approved, you can rank it on a one-to-five point scale, and when you need a 1.1 or a 1.2 to get it, a 1.3 or 1.4 is out of the money, you know you're dealing, you're up against the potential danger of jiggle. So there was some jiggle. Some guy got out of the bed on the wrong side, I heard, in the study section, and made some criticisms, I don't know who the hell he was -- I was told who it was, but I still don't know who he is or what he would know about my work or why he would find any reason to criticize it -- and somebody else said, "That's ridiculous; this is da-da-da, and this is great," and they defended it, but his vote or whoever else he might have influenced a little bit, boom, I was out of the money. Okay. That happens. That happens all the time.

So it happened in July, and I heard the news back from study section, and I got the pink sheet or whatever it was in August or something, and the next application deadline was, I think it was October 1. And I was supposed to go off with Julia and Ben [wife and son] to England for several weeks in August, as we do every year, to visit Julia's family, and I said, "I can't go. I've got to stay and write, I've got to resubmit this grant application, make a revised application."

What the fellow at the head of the study section told me was, "Don't take this badly, now, John, you're out of the money but don't take it badly -- half or more of the grants that we fund in a given period are resubmits. So it's just a new thing that's happening now, since you applied for a grant last. A lot of grants are having to be resubmitted, so don't take it personally. Your grant's fine; everybody appreciates you're doing wonderful work," rickety-rickety-rack. "Just take into account the critiques that have been made and resubmit and, you know, I can't guarantee you're going to get it, but I think personally, I have to tell you, I think it's highly likely you'll get it the next time." Not only that, then, he said, "You can apply for some continuation money to kind of tide you over," which I did, and which I got, which was about half of my normal budget they gave me to kind of tide me over during that period.

So I saw Julia and Ben off to England, and I sat down, I was going to get ready to revise and resubmit, and every time I looked at that pink sheet and saw these idiotic critiques -- because that's what they were -- I never did it. I never wrote it. I never resubmitted it. I just said, "Screw it. I don't want to do it any more. I'm tired of fighting, and I'm tired. I'm tired." That's really what I felt. I felt like I was tired. I didn't want to fight this. I could have written something; I thought, "I could write on that; we should explain this," something, something, "I think we weren't wrong here, but we could still do it your way," something, something. I could have, you know, eaten a little humble crow there and put it in, and I didn't want to.

And somehow, as I thought about it, this idea came into my mind that I will stop doing research, which is a momentous decision. It's totally unheard of in my field.

MELDRUM: Nobody does that.

LIEBESKIND: Nobody does that and turns out -- nobody. And if they really do do it, they don't tell anyone that they're doing it, and not even themselves. "Well, I'm going to be getting back; I'm working on these ideas, and I'm going to be putting in a grant maybe next year," and, you know, something, and meanwhile they're off goofing around, playing tennis or whatever it



is. But nobody just says to the world, "I'm quitting," unless they're seventy, eighty years old; they say, "Well, I'm not too tough; I'm not in great health, and I have to look after Susie," and whatever, you know, I don't know. But younger people --

MELDRUM: Don't do it. People sixty don't do it.

LIEBESKIND: People my age just don't do that, and that's very, it wasn't evident to me at the time. I didn't realize that I was doing something so unusual, but I've been made to feel that ever since, accused almost weekly of having retired, by even close friends who said, "Oh, what's life like now that you've retired?" "Uh, George, I didn't retire!"

MELDRUM: I'll vouch for the fact that you haven't retired.

LIEBESKIND: I haven't retired. Or that, it's almost like they think that, either I'm dead, I'm retired, or I am, I've got some terminal disease, something unusual to have accounted for that, is what people think. So I like it. I feel like it's under control. I did it my way kind of thing; it feels good. I mean, you know, somebody told me no, and then I said no back, as it were. So I said, I'm doing that, I kind of brought that about -- made it *my* decision, not somebody else's, which, I confess, feels good. I left at a time, science, at a time when we were doing this, again, I think, spectacular work, and a lot of it. I mean, we still have, still, just the other day I was working on a manuscript with Wendy [Sternberg].

MELDRUM: Yeah, you're finishing with the papers.

LIEBESKIND: It will be, you know, probably the last paper to come out, and that's two years down the line.

MELDRUM: So a lot of papers -- you finished up the work that was in progress, more or less.

LIEBESKIND: Well, that's it, you see; it took about two years for the lab to cycle down. I had a lot of support, including private money from Bristol-Myers and several other private foundations, which I can keep in perpetuity. I don't have to spend that on any annual basis. So I was able to tell everyone, "Look, I'm hanging it up, I'm quitting, but you don't have to flee. Just leave in an organized manner. Finish your work, start looking elsewhere, finish your work, and move on as you would be fairly soon anyway; see if you can speed it up a little bit. But first make sure you do your work properly. I have enough money to support it."

And that's exactly what happened. It took almost two years for the last student to kind of cycle through, and that was Wendy Sternberg, who, at the time I made the decision, was just a middle-level graduate student; she hadn't started her dissertation yet. So I was able to see her all the way through. The others were even more advanced graduate students, more post-docs, who were able to finish what they were doing and over the course of a year were able to cycle out.

MELDRUM: All these exciting new ideas that you were working on at this particular point, then they've come out in this published work?

LIEBESKIND: Some of them have. Some of them came out as work that we did after, or concluded or whatever, you know, after that decision got made. Some of it's coming out because other people are working on it. Some of those other people are people who used to be here but are now elsewhere, so some of them are continuing that work, taking their projects elsewhere, which is good. On the pain-can-kill thing, for example, the two people who were doing most of that work in the lab, one went back to Israel [Shamgar Ben-Eliyahu], one went to Ohio State [Gayle Page, now Independence Foundation Chair in Nursing at Johns Hopkins], back to jobs; they're working together this summer in Columbus, still working together, reunited, working on related matters, showing the important adverse health consequences of pain.

MELDRUM: That's very interesting. I would like to know more about that. Okay. We've talked a little bit about your style with your students, and obviously this is something you felt fairly comfortable with, and I also see that we're coming to the end of this tape. So let's let this tape run out.

LIEBESKIND: Well, we'll just take a new one.

MELDRUM: And we'll start a new one.

## JOHN C. LIEBESKIND INTERVIEW

### TAPE THREE, SIDE ONE

LIEBESKIND: Are we going now?

MELDRUM: Needles go up.

LIEBESKIND: The needles go up.

MELDRUM: Okay. We're starting tape number three; it's about 2:15 on the seventeenth of July. I have a question in here. Why do scientists [she laughs], and I think you've answered this question in part, but maybe you can answer it a little more specifically, what particular rewards - - I don't mean just honors and so forth -- but what particular rewards do you think you've gotten from your career in science, and could you have gotten it perhaps in another field? I mean, it sounds like a lot of your work, a lot of your rewards, your psychic rewards, are coming from your work with students.

LIEBESKIND: Absolutely, and that was just what I was going to say. I think more than with other scientists, typical of me, is that I *haven't* been in the lab, I haven't had the daily commerce with the scientific problems that many scientists have. I've been more distant from the day-to-day; the main reward has been the interpersonal one with students. I think that is undeniably the case with me. As such, one could ask, as you have, could you not have gotten the same thing dealing with history graduate students or employees in the lingerie department of your father's store? [Meldrum laughs]

And, you know, I think the answer is yes in some sense. I think I could have been happy doing anything interpersonal where I was involved in something and there were other people and I was maybe helping them learn their job better -- or something didactic, I think, was definitely a theme for me, where I was in a position of professorial teaching or mentoring kind of position. I think one can have that in business or in a law firm, as an engineer [he laughs], as a whatever, you know, a cowboy or an Indian or a tinkerer or whatever. So, yeah, I think I could have found that in any other walk of life. Yeah, I did find it in science.

And I found a lot of excitement about what we did, too, and the ideas and the discoveries and so forth, and there's no question that that's a definite high. I mean, when some data come through and a particular experiment works out, and somebody, one of the students comes in and says, "I've found that so and so, such and such, and that could mean something, something," you know, you kind of work something and you figure it -- I mean that's, God, you've got a tiger by the tail here; this is big! This is exciting. This could really have implications. So there have been highs of that sort, a lot of them.

I think, just thinking back here over what I've just said, there's something good and bad embedded in it. When I was, when I worked with Jim McConnell my first year in graduate school -- he was my advisor -- he said something which I've always remembered, and perceived

then and still think of with great scorn. He said --he was not a nice person, really; I didn't really approve of him. He excited me and interested me, but I didn't really approve of him. One of the reasons was because he said the following thing. He said, "Remember, folks, always shoot for the big marbles in science." And I found that anathema; I found that disgusting, that you should always look for the big findings, the flashy this and that. And yet I've kind of done that, so there's this sort of schizy reaction I have in respect to this. I don't quite know how to rationalize it.

MELDRUM: Well, is that the same? It sounds to me that what you've done is sort of pick out the marbles and show other people how big they are. [both laugh] Sort of made your marbles look bigger. I don't know.

LIEBESKIND: I don't know. I just, well, I mean, maybe we're far afield here even going down that line, but suffice it to say that we played for some big marbles, I think, and that's something that's been in my mind, is that what we're doing is important. I remember once saying to Steve Fox years afterwards, when I got out here and I was doing this work, and it was at the very -- I'm not sure that I completely understood, but I think you will understand better than I did. I said to him, "Steve, I'm calling you to let you know that we got this article accepted to *Science*," or "I've been promoted to associate professor," or one of those things; I was laying a brag on my old mentor. And I said, "You know, we're doing this really important work. We can stimulate the brain and inhibit pain." And he said, "That's nice." I said, "No, not 'nice'; Steve, this is really important." And he said, "It's nice that *you* think it's important, John. *That's* what's important." [Meldrum laughs]

And he just took the piss out of me on that, and, you know, every once in a while I stop and think about it, and think that maybe nothing we do is important, nothing in science, nothing really leads to anything else, and we paint these rosy pictures that what we do matters, and it's going to lead to cures and help people and something and something. That kind of thing has been a guidepost for me throughout my career at a time well before it was okay to feel that way. Even today it's not altogether okay to feel that way, but it's gotten more so with the granting organizations emphasizing health relevance and so forth. But in the early days, if you even if your work even smacked, smelled faintly of a potential application [gasp], you were doing applied research!

MELDRUM: Not pure research.

LIEBESKIND: Yeah. And there was something anathema about that.

[BRIEF INTERRUPTION TO CHECK RECORDER]

LIEBESKIND: Okay. So I've always been concerned at a time when it wasn't fashionable, and it still isn't altogether fashionable, with health relevance, with the, quote, "importance of what we're doing with respect to an ultimate human application," and this kind of thing. And I've been that way more than most of my colleagues. And so when what we have done has seemed to have a bearing on that, it's been a high for me. It's been a reward that I wouldn't have had if I'd just sold a whole boxload of brassieres. [he laughs]

So I think people in business can be honest with the customers, not only by not cheating them but in telling them, “You really should wear purple; it looks very good on you. You shouldn’t wear green,” or something, or whatever, and then they can, in other aspects of their life, they can be active civically and give money to charity or be active in their community in some charitable manner and do good, because that’s what life’s all about for me. I mean, it’s, if you’re just sort of eating and surviving, just kind of nibbling at the world, then I don’t, that’s not, I mean, I don’t know what I am. I have this kind of credo that we’re supposed to put into the world something, you know. And I always feel self-congratulatory in terms of what I do, both in terms of the science that comes out of my lab and the people, which is really the number one product that I take the responsibility for, that that’s the good that I’ve done when we tally this whole thing up at the end, that’ll be the positive --

MELDRUM: Right. That your work does have some direct relevance to human welfare.

LIEBESKIND: Yeah. Maybe I’ve eaten too much [he laughs] and wasted too much gasoline or left my water running out in the yard and wasted natural resources, but I’ve at least plowed something back of good into the world. I’ve known too many people who are narcissistic or selfish.

MELDRUM: Okay. Well, let’s get back -- I have a question which I want to include in my interviews; I’m going to try it out on you. I say the word pain; what do you think of? I mean, you know, you and I talk about pain a lot. What do you think of, what image comes into your mind?

LIEBESKIND: Well, you know, I have so many different associations I don’t know how to answer you. I mean, I think of pain, I think of a field, and I think of a science, of a human problem, medical, psychological problem, I mean, all those things, and they all have different contexts. I’m a member of the pain field, the pain community, and I have a sense of what I mean by that. I think of pain as a sensory, perceptual event as we think of vision, audition, and so forth; I could talk about it from that standpoint. I think about it as a human affliction, something that comes about through disasters natural or unnatural and therefore something to be conquered or prevented. And then I can think about it more, at a greater distance, in more abstract terms, as synonymous with anguish, and other things that I don’t think of as part of my pain field, but they are using the same term, as in a pain in the neck, somebody is colloquially a pain in the neck.

MELDRUM: Yeah, right. [she laughs]

LIEBESKIND: The angst that somebody feels, something like that. So, I mean, I have all those different ways of thinking about it, and, you know, if this were like a free association, “What’s the first word that comes into your mind? Quick!” kind of thing, I mean, it would just depend on the context of where I was and what I was thinking about, that I would pop up with X or whatever, because I would recently have been doing something else, and on some other occasion, I would think of it in terms of Y. And if something had been plaguing my mind and I was reading something, it would be Z on still a third occasion.

MELDRUM: Well, let's talk about the pain field, then, if we're going to talk about pain. In 1971, you published this paper in *Science*, and you were beginning to sort of make yourself known in the field. And so shortly after that, you heard from John Bonica.

LIEBESKIND: Yes, I had a call from John Bonica one day in my office. I don't remember it, but I've thought about it and tried to remember it [he laughs], since I know it was a surprise, and I didn't know who he was. And he said that he had heard about my work, and I don't remember whether he said through Ron Melzack or not; he might have, probably did, hear about my work through Ron Melzack. And that he was putting together a meeting the following year; I think this was probably in the fall or the summer, and he was putting together the meeting for the next May or June, whatever it was, 1973. And would I come give a presentation on this work that we were doing? He said he knew something about the work that we were doing. I said, "Yes, sure, I'd like to hear more about it," or something encouraging, and I then subsequently got a form letter -- I'm sure that maybe had some particularization to me but maybe not; it had my name on it, and it had maybe some topic that I would talk about, but that otherwise was kind of a speechy thing about his general objectives that I'm sure went to everybody who got one of these.

And I accepted formally to give the talk, and I was pretty excited about it because this was one of the first talks I had given -- I'd spoken at the neuroscience meeting, as I told you earlier, I'd given a colloquium at UC Irvine, through Jim McGaugh, I think, before this, maybe a few other things here or there. But this sounded, you know, bigger and better, and at this time, unlike previously, I felt, and Dave and Huda working with me, that we all felt this was a time that we could talk about *all* of the work we had done in relation to this, and it was getting time to summarize it a little bit, because already this was a year or two after the *Science* article was out. So it was time to kind of go over some of that, larded over with some of our theoretical ideas to give a little more emphasis to them. So I saw this as a good opportunity to do that, because it was going to be a book and a chapter.

Then, in the course of things, and I wouldn't have remembered this, but looking through some of that correspondence -- I have a folder on this -- I have folders for every trip I've ever taken, and I have a folder on the Issaquah meeting, and in there is a letter, subsequent letter, I don't remember when it's dated, but a few months later or whatever, saying, and I can't even remember exactly, but it was something like, "We're going to be bringing a small group together to talk about" -- I think it was -- "creating a society. Would you be willing to join this small group, at the head of the meeting or one evening after the meeting or something, in order to plan this? And here's some of my ideas; I'd like you to be thinking about this." And John asked, "Will you come to the meeting with some of these ideas?"

So I was kind of in a smaller group of discussion on one of the plans that John Bonica had in mind, and really what I'm trying to say is that Bonica, this incredible genius, had everything very clearly worked out in his mind for this meeting, starting with where he wanted things to go, and it was an expression of his organizational genius and interpersonal genius that it happened just that way [Meldrum laughs], the way he organized it. He left nothing to chance. He organized it in this manner. And so small groups met during the five-day Issaquah meeting at one time or another when talks weren't being given, in the evening or what have you, at lunch or separate lunch meetings or whatever.

And then toward the end of the five-day meeting, Bonica brought it all together. “We’ve been all doing some thinking here, and groups have met and talked about various things, and here’s, let me give a summary of what everybody seems to think, and let’s all together now take some votes. There seems to be a lot of sentiment for pulling together a society, that we should do this more often, not just this one meeting, but pull together a society that would hold world congresses every three years. And everybody seems to agree it should be an international organization, and it should be a multidisciplinary organization.” You know, he kind of laid all this out. I mean, he knew all that in advance! He programmed this whole thing in advance. He put out these ideas through these letters, and people thought about it and they agreed with it, and they came, and then he reported back, “Well, here’s what the group wants. Let’s take a vote; does everybody agree?” [Meldrum laughs]

Every hand would go up. “We should have a journal. Let’s call it the journal *Pain*. We’ve had a subcommittee that’s met, and they are happy to announce that they have recommended that Pat Wall be the editor of this journal.” [he laughs] “Everybody who agrees for Pat Wall, put your hand up!” You know, I don’t know, people may have objected to a vote like that, but he just laid everything out. And I felt after that meeting, because, you know, I was just promoted to associate professor at that time, a year or so, whatever -- still very young in my career -- I felt like I was part of the “in group.”

MELDRUM: Yes.

LIEBESKIND: John Bonica came up to me after my talk and he congratulated me, and I’ve teased him many times about what he said. I don’t remember exactly what he said, but I’ve said to him that he said to me, “Liebeskind, that was a hell of a talk you gave. I didn’t believe anything you said, but you have a lot of pizzazz!” [Meldrum laughs]

I don’t think he said quite that, but I’ve teased him about it. He said something like that; he liked my style or something. And so one of the things that he said at that meeting was that we’ll have this international association, and there should be regional and national chapters. So I came home from that meeting with the idea that I would start a chapter. And I think I have never done anything more creative of an entrepreneurial, organizational sort, than I did in creating what became the first-ever chapter of the International Association for the Study of Pain [IASP], the Western Pain Society. It wasn’t even that grand; I think that’s when we gave it that name. I called together a meeting, and in a very clever way -- I don’t want to go through the details with you of that -- because I was a nobody, and who was I?

MELDRUM: No, no! I want to know.

LIEBESKIND: All right. What I did was --

MELDRUM: I might want to call together some meeting of my own some day. I need information.

LIEBESKIND: Okay. What I did was I decided I would use my UCLA colleagues in some way as I could, to rely, lean on them because I was a nobody; nobody would have heard of me if I started putting out notices. So I drafted a letter, and put “draft” right across it in big, bold letters: “Dear colleague: We, the undersigned, are thinking of organizing -- the IASP has recently been formed,” and on and on and on, “and it has been said there should be local chapters, and we think there should be a chapter and hold a meeting here and talk about pain,” and something and something, and on and on for a page or two like this. And then a page and a half of signatures. And I put down everyone’s name at UCLA, the most important people at UCLA --

MELDRUM: But none of these people --

LIEBESKIND: -- in any department, who had anything even remotely that could be thought to maybe relate to pain. Okay?

MELDRUM: Goodness. Okay.

LIEBESKIND: Obviously, there wasn’t anyone here who was really working on pain -- well, there were a few people that --

MELDRUM: It was mostly you.

LIEBESKIND: There were a few others, but not very many. And I then wrote a cover letter to each of the people whose names I had put on this list. Some of them I didn’t know very well. “Dear Dr. So-and-so” --

MELDRUM: There’s that *chutzpah* again.

LIEBESKIND: No, well, you see, it was very cleverly done because I said, “Enclosed is a draft of a memo I would like to circulate in order to -- and I thought we here at UCLA could take leadership in this, and I would like to use your name in this manner if you agree to it. If you don’t, of course I’ll be happy to withdraw your name. Please let me know what you think and who I should send it to; do you have any suggestions around the state of California or up the coast, you know, Oregon or Washington, who might we invite to this?” And there must have been twenty, twenty-five names on this I put down. And every one of them replied. A few didn’t; I called them up and said, “Did you get that thing from me? I didn’t hear from you.” “Oh, yeah, I’ve been meaning to.” “Well, do you think I could use your name on this?” “Oh, sure, fine, yeah. I’m all for it. That would be nice to have a meeting like that here. You’re going to do all the work; there’s no money involved -- you can use my name; I’m happy!” [both laugh]

So it was a no money, no nothing, you know, just some effort, and I was doing the effort. And so they all agreed. I don’t think anyone pulled off. Maybe one person did; I don’t remember. And I got a few others to help me, there was a young neurosurgeon here at the time [Stanley Goodman] who was keen on this, and somebody else [Ronald Katz], and we kind of were the organizing committee. And we decided who we wanted as speakers; people from San Francisco, there was somebody at Stanford, there was somebody here, there was somebody there. And



there was a no-host bar; people would come on their own hook. I think it may have been before the [Society for] Neuroscience meeting; I think there may have been a neuroscience meeting in the vicinity, and we planned around that.

MELDRUM: Yeah. Piggy-backed.

LIEBESKIND: Yeah, piggy-backed. Right. [he laughs] Anyway, it was a big success. Seventy-five, a hundred people came. And at that time you needed twenty-five signatures from a geographic region -- that was what the rule was -- you could send that to Louisa Jones in Seattle, and she would sign you up as a chapter. We had two, three times that number of people who signed. We sent around -- while we were sitting there, we had a little organizational -- after all the speeches, I got up, I said, "I think we ought to really have this chapter; we've talked about that, and I'll circulate this. Put your name on there if you agree, and we'll do this again next year."

And somebody had come up to me, a neurosurgeon from Pasadena way, City of Hope Hospital, a guy named Ben Crue, and he said, "You know, Liebeskind, you've preempted me on this. I've had this idea. I was going to do this. I congratulate you. You beat me to the punch." He said, "What I'd like to do is host this meeting next year." I said, "That would be wonderful." So I announced to the group that Dr. Crue and his crew had decided to host this next year, and I think somebody might have even piped up, someone named John Loeser, and said, "We'll do it the year after that up in Seattle." So, boom, boom, boom, we had the first three years all laid out, and --

MELDRUM: So how did you pick the people to attend this meeting?

LIEBESKIND: It was, you know, names I knew. People, pain people. We had Ernest Hilgard, very, one of the most important psychologists in the world who was doing at the time experimental work on hypnosis and pain; we had him come down and give a talk.

MELDRUM: But you became aware of these people through Issaquah, through your reading?

LIEBESKIND: In part; I don't remember now. I'd have to look at the list. These were people that, you know, either I knew them directly, or one of the others knew them; I mean, there were several others I brought in, a surgeon suggested somebody or whatever. We made sure we got -- it wasn't a lot of speeches; it was a one-day meeting. So we had six people speak or something, I don't know. It wasn't that big a deal. No money. There wasn't one penny that was spent; I mean, everybody just spent their own money to come here. They brought their own lunches; we gave nothing, it cost nothing. And then it got going after that and it got going more formally. And we set dues and so forth. Well, what that led to was that already Bonica was planning the first World Congress on pain.

MELDRUM: Right. In Florence.

LIEBESKIND: So he kind of knew who I was from the Issaquah meeting -- "you've got a lot of pizzazz," you know, and stuff and stuff, and he had now seen that I had started this thing. So he

put me on the nominating committee for the officers of the first World Congress on pain for who's going to be the first President of the IASP? And there was an election to be held, and board members -- what do they call it -- members of Council. So I think he asked for nominations or something, and guess who got an awful lot of nominations? An unknown named John Liebeskind, who had put out this memo all up and down the West Coast, and everybody knew my name because, although it had all these signatures on it, mine was on top. It said, "I am joined by the following -- I, John, you know, I've been joined by the following colleagues," and it was my name, and "Please respond to me." But "I am joined by all these other folks." So a lot of people knew who I was at that point.

And, you know, the world of pain was unformed; it wasn't as if the Germans were all together and could back, you know, Dr. Somebody-or-other, or the French somebody else, or what have you. So I got a lot of nominations, and I became a nominee, and, in fact, won the election for member of Council. So I was on the first Council as a member; there were eighteen members of Council in the world or whatever it was; I was one of them. It was a six-year appointment, I think, and I got re-elected. So I was a member of Council for the first twelve years of IASP. I attribute that to this rather clever idea of putting on a little meeting of my own here, creating this chapter.

MELDRUM: Right. Recognition.

LIEBESKIND: Huh?

MELDRUM: Name recognition.

LIEBESKIND: Name recognition. More self promotion; done well.

MELDRUM: Now, you've asked everyone this question. You ask them to tell you about the Issaquah meeting.

LIEBESKIND: Yeah.

MELDRUM: So do you remember anything, any anecdotes that we haven't heard already?

LIEBESKIND: Probably not. I think I led a number of the oral authors to -- I said, "Do you recall -- ?"

MELDRUM: Yeah, I remember that.

LIEBESKIND: So, I mean, probably my recollections are their recollections embedded in all this. I mean, those things that were salient for me were -- I think the most salient was Pat Wall attacking this group of acupuncture researchers. I'd never heard anything like that. It just sent chills down my spine. He said, "These papers are ridiculous and they're awful, and da-da, da-da, da-da," A, B, and C, that was wrong with them, and "I think we all ought to agree on this that these papers should not be part of the book."

MELDRUM: Oh!

LIEBESKIND: Yeah. I even know who one of the authors was; a UCLA alumni. So that was, whoa, Jesus, that scared the hell out of me. So there was that, and there was this incredible character named Bill Mehler, M-E-H-L-E-R –

MELDRUM: Who off and went salmon fishing. [she laughs]

LIEBESKIND: -- who off and went salmon fishing [he laughs], yeah, you remember the stories, too. And other than that, I don't know. I remember physically sitting next to Madame Fessard, as my session was just getting under way, whoever was up there first gave his talk or whatever, and then it was getting to be my turn. It was just before I was going to be called up, she saw that I was the next on the program, and she said [whispering], "Have you turned in your slides?" I said [whispering], "I don't have any slides." And she went, "*Zut alors!*" or whatever she said, you know, "What? You're not showing any slides?" [both laugh]

So that was, I think, the first talk I ever gave that I didn't show any slides, and that became kind of a hallmark for me.

MELDRUM: Hm. That's interesting.

LIEBESKIND: I almost never show slides since then. I remember Dave Mayer was there, and I remember Jean Marie Besson came and visited me here in L.A. just before the Issaquah meeting; we worked on a manuscript together.

MELDRUM: There must have been a lot of people whom you didn't know there, though.

LIEBESKIND: There were a lot. I mean, most of the people there, I don't think I knew them afterwards, either, and it was a big meeting; there were three hundred people, and a lot of these people knew each other. And the average age was older than me, so you know, I was a little on the periphery. Dave Mayer and I hung out a little bit together. But I don't remember it as being a terrifically pleasant kind of thing.

MELDRUM: Hm. Kind of nerve-racking?

LIEBESKIND: A little nerve-racking, yeah; I mean, I was sort of a young guy; I was kind of nervous. You know, I think there was that issue. I remember distinctly the one evening that I was invited to the Bonica home -- I don't know if everybody was, or just the speakers they brought over to the home. Half one night and half another night -- maybe everybody, or maybe just the speakers. I think it was just the speakers -- half one night and half another night. They put on this gigantic dinner in this incredible home, and with cold poached salmon or something. Again, the thing I generally took away was this feeling that I'm kind of part of this group now; something happened here. I felt very excited about it, good about it. The talk had gone well, and this idea to start this Western Pain Society.

MELDRUM: Did you, now, before this period, I mean, when you published this article in *Science*, you were thinking about all the implications of your work. So you must have had some idea that there was a field there that would follow up on those implications? Or not?

LIEBESKIND: Not a field; just people.

MELDRUM: Just people; individuals. So what's the difference between a field and individuals? What constitutes a field?

LIEBESKIND: Well, it's obviously a group of people who identify with each other in some formal manner, who have a name, have a flag under which they march. They do things together; they march together.

MELDRUM: On account of the cause.

LIEBESKIND: On account of the cause. They have meetings and tacitly agreed-upon activities that scholarly groups do. You act like other scholarly groups, and they hold meetings, so we're going to hold meetings. They have a board of directors or a council, so we're going to. They hold elections, they put out a journal, they put out books, they have committees on -- I mean, I became active as a council member of the IASP, I was one of the more active participants in those council meetings. I've had effects in small ways. Why? Because I was good at those things. I spoke English, which some of the others didn't do, or spoke only haltingly, because it had to be international, so I was already among the subgroup that was comfortable speaking up.

MELDRUM: An international group that speaks English. [both laugh]

LIEBESKIND: Spoke English! Plus, you know, I just was sort of good at that. I can just kind of talk in a group; I'm not too shy about that. And I would have little ideas, and they'd say, "All right; moved by John Liebeskind; anyone second it? Yes, we've got a second," and some little organizational thing would happen. I'm sure if you went back through the records of the IASP as it was in its early formative years, you'll see my name appearing. I did this, and I suggested that. I suggested that there be an ethics committee, just as a for-instance; I don't know if it was specifically that. "Oh, yeah, that's a good idea."

Years later I suggested there be a foundation, and they said, "That's a good idea; why don't you start it?" I said okay. And the International Pain Foundation started. I don't know; there are a lot of things like that. Mme. Fessard, who became the first President of the IASP -- John Bonica was the second President -- I got asked to run, head the scientific program committee for the second World Congress. That's a big job.

MELDRUM: My gosh. Yeah.

LIEBESKIND: I was still fairly young. So that's where the book came from, Albe-Fessard, Bonica, and Liebeskind. My name is on there as one of the editors.

MELDRUM: Yeah. I do remember that.

LIEBESKIND: Basically I did --

MELDRUM: Ninety percent of the work. [she laughs]

LIEBESKIND: Well, you know, I had a committee, but they were the two officers and I didn't want to burden them. Anyway, so I held positions, a number of positions of importance in the new organization. Interestingly, I never became President, and there's probably a strategy thing there. I don't know that I was specifically shooting for it, but I may have peaked too soon or something; I don't know. But had my heart set on becoming the President of the IASP, it may have been -- *if* my heart was set in that direction, it may have been a better way to have played my cards, given what I did, there might have been some things I did that kind of precluded that or let somebody else in. Because there's a huge time issue; you see, it's every three years, and they don't want two Americans in a row, so if you don't get it on that occasion, it's already --

MELDRUM: You have to wait for six years. Yeah.

LIEBESKIND: -- six years, you know, and something and something. I don't know; some deals got cut at various times; brought John Loeser in and something and something. So, well, I used to think maybe it would be nice some day if I were IASP President; I think probably it would have been better if I'd been on Council later instead of -- after twelve years on Council, then I was off. I wasn't in anything, I wasn't in anyone's mind. If I didn't make it in those years, I was still pretty young by then.

MELDRUM: Right. At that point they were still going through Bonica, and then who was the third President?

LIEBESKIND: Iggo and Albe-Fessard, Bonica, Melzack -- or Iggo, then Melzack, then Michael Cousins, Ulf Lindblom --

MELDRUM: And now Besson.

LIEBESKIND: -- Loeser, and now going to be Besson. That's all.

MELDRUM: It's not a very big chance for very many people to be President in any case.

LIEBESKIND: So it's not anything I feel badly about. I think when I became head of the Foundation, that sort of waved its giant flag, I think that blew it right there if I ever thought I would become IASP President. At that point --

MELDRUM: Put that one away.

LIEBESKIND: Stow that one. [he laughs]

MELDRUM: Well, what -- okay. So you were in IASP on the Council during these extremely formative years.

LIEBESKIND: And on the APS [American Pain Society].

MELDRUM: And on the APS.

LIEBESKIND: And the way the APS got started, I was in on the building up of that. I did become President of APS, and I did a good job there for one year. That's not so tough, and there's a big organizational group that runs it -- well, so does IASP. That's Louisa Jones and her staff, so that's not such a tough job, either. But yeah, no, I was very active in both institutions and contributed a lot to the organizations as such. And I think that's an important thing to do. I think it has, it's not just, you know, people put that kind of activity down and refer to it as politics and old-boys' networks and so forth, which it is -- both of the above, all of the above -- but good management, well-run organizations have very salutary effects on the members and on the field as a whole -- I mean, even on patients. I mean, you can see where things come out of and X leads to Y leads to Z, where the organization itself --

MELDRUM: Well, that's my next question. [she laughs]

LIEBESKIND: What?

MELDRUM: We talk about pain as a disciplinary field. What does that mean to you? What exactly does the IASP do that people couldn't do individually just by, you know, reading each others' papers and writing letters and so forth and so on?

LIEBESKIND: Well, it, I mean, any organization promotes activity in the field, and so by bringing people together they aspire to do things collectively that they wouldn't do in some instances. Science, itself, is something that cannot be done by, very well by an organization; it's done by individuals or small groups of individuals. But a lot of what surrounds science is done by organizations, which in turn promote the science. And so if you apply for a grant in a given year doing pain research and you get it, partly that's attributable to the public image of pain in the NIH, at the level of the NIH people, at the level of the study section, at the level of council, and at the level of the people in Congress and so forth who appropriate monies, whatever and whatever, and so, you know, I think it can have effects like that. I think there are lots of things like that that we sometimes just take for granted -- just the meetings, getting together, the journals, that have important effects on the conduct of science and the treatment of patients -- which is not something I'm involved in, but I believe that these organizations have a very powerful impact on how doctors treat patients.

I mean, I have seen in action at the mini-level, but I'm sure it happens a lot, when individual people join an organization such as the IASP or the APS and get changed by the organization, become familiar with and committed to an interdisciplinary approach to pain management that they didn't have to begin with. I've seen it time and again. I can think of some dramatic examples.

MELDRUM: Can you talk about one of those examples?

LIEBESKIND: There's a man named Marty Grabois, G-R-A-B-O-I-S, "gra-BOYS," he pronounces it, he's a physiatrist from physical and rehabilitation medicine, who is very well trained in his field and was a leader in his field -- active in the American Society of Physiatrists or whatever they call themselves, and he was President of the American organization of that and so forth and so on. So not a kid -- a senior person who, when I first knew him, was already a fairly senior person in the field. And he was a member of the American Pain Society, probably also the IASP, but I got to know him through the American Pain Society. And I don't remember, I can't remember the details. He was either a member of the board of directors or he was the head of a committee or something.

And my first encounters with him in this group setting were unpleasant. I found him abrasive and unpleasant and always sort of in left field, off in left field. I mean, if all of us wanted this, and he was, "Well, I don't understand; why do you have to do like that?" And he was always off there somewhere in the blue. And I thought he was stupid, as a matter of fact. Subsequently, I've changed my mind. I think he's a bright guy, bright as the next guy, very decent sort of fellow, honorable and nice and so forth and so on. So, you know, there are three possibilities: either he changed, or I changed, or we both did. [both laugh]

I think he changed! [he laughs] In part -- I think what happened -- and I shouldn't really pick on him; it just, at the time, it seemed, when I saw this happening, I saw the significance of it. And I said, "Here's this; this is what the American Pain Society does." I can't remember exactly what it was, but there was some -- oh, I know what the issue was, more or less. It had to do with whether we should schedule our annual meetings of the American Pain Society, whether we should continue to schedule them in concert with the neuroscience society. And he was saying, "You know, it's not fair!" He said, "We do this every year. For years we've been in concert with the neuroscience meeting. Do you realize there are only about ten percent, twenty percent of our membership are neuroscientists? Why are we doing this?"

Well, you know, the reason we're doing it is because that's where all the basic science is coming from. It's not like there were ten other sciences out there that's contributing to the field of pain. So, I mean, he, over the course of time he changed his attitude toward that; he was inculcated into the understanding and the belief that basic scientists were an important part of what makes up the APS, even if it's only ten or twenty percent of the membership; it's a full fifty percent of the *importance* of what we do in bringing together science and clinical, and that whole thing.

He got the idea. Took him a while, but he got the idea. And I see that now, I mean, maybe I make too big a deal out of it; maybe I'm wrong that I criticized him at the beginning, or maybe I'm wrong believing that he changed [he laughs], or something; I don't know that much about it. But I just saw it as kind of an exemplar of how the multidisciplinary spirit can catch on, can be contagious and affect individuals who then bring that new way of thinking about things back to the --

## JOHN C. LIEBESKIND INTERVIEW

### TAPE THREE, SIDE TWO

MARCIA MELDRUM: We're recording again. Yes, we are recording again --

JOHN LIEBESKIND: On both channels?

MELDRUM: -- on both channels, looking good. Okay. So we're now starting side B of tape three, and today is the nineteenth of July, and it's 2:20 in the afternoon.

LIEBESKIND: Very good.

MELDRUM: Okay. Talking with Dr. John Liebeskind regarding his contributions to the pain field, I wanted to ask you about another contribution, a more unofficial one, and that's your personal friendship with John Bonica.

LIEBESKIND: Oh, gosh. Yeah.

MELDRUM: Yeah. Apparently, you had not known Dr. Bonica until the Issaquah meeting.

LIEBESKIND: That's right.

MELDRUM: He sort of introduced you to the pain field. And over the years you developed a very close relationship with him.

LIEBESKIND: Yes, I did. Yeah, I mean, people told me in more recent years that he sort of viewed me as one of his sons, one of his academic children. And I have reason to believe he did. That pleased me a lot. I think I knew that, but I just assumed that we were an extremely large family; I was told we weren't as large as all that. We were maybe three or four people that fit that description of being one of his academic children. So, yeah, well, our lives crossed at many points from 1973 until his death last summer. And there's lots to say, you know.

Right from the beginning, which was at Issaquah, I think I related that experience, and he seemed to come to know me a bit then and, I felt, kind of brought me a little bit into the inner sanctum -- I don't know how inner [he laughs] -- but I felt like I was part of a smaller subgroup that was helping to organize things a little bit, which would have felt just fine under any conditions; but, given my kind of relative youth in the field -- I mean, I was still a pretty junior faculty person, and not awfully well known; the work we had done still by that point was just getting to be known -- [it] felt especially good. And then, as I think I mentioned, I was elected to the first Council of the IASP, and in fact, as I also think I mentioned, but I'll give you a little more detail on that, as they were planning the first World Congress that was held in 1974 in Florence, Bonica asked me if I would be a member of the nominating committee. You see, everything was kind of ad hoc at that point; there were no officers, everything was sort of *pro tem*, and John Bonica was --



MELDRUM: Yeah. Was calling the shots. [she laughs]

LIEBESKIND: -- President *pro tem*, kind of running things, organizing things with Louisa Jones at their Seattle office, where they worked together. And so John wanted proper elections to be held at the time of the Florence meeting and appointed, I think it was Dr. [Carlo] Pagni, P-A-G-N-I, as the head of that nominating committee. I have some documents about that somewhere, and I'm quite sure that's correct. And he told me -- that is to say, Bonica did, in advance -- that he wanted to talk to me about this. He was not himself on the nominating committee because he wanted to distance himself from that process, and he told me that he wanted to know my advice about who should be the first President, and he told me that I would need to be the one in Florence who spoke at the general assembly meeting to announce the slate of candidates of the committee, because Pagni's English wasn't so good. So that got me fairly excited, too, that I would be addressing the whole group in that way, and that was kind of a fun thing.

So he asked my advice, and I said -- I mean, I don't remember the exact words of the conversation, needless to say; it was more than twenty years ago, but I do remember being astonished that there would be any question in his mind as to who the first President would be. I said, "John, everyone naturally assumes it will be you." And he said, "No." He said no, he felt that wasn't good for the society, that he had gotten things organized and so forth, and he was very concerned that the Europeans -- that is, Europeans or non-American colleagues -- would see this as an American, what's the word --

MELDRUM: Not hegemony; monopoly?

LIEBESKIND: Well, yes, that kind of thing. I wouldn't use the word hegemony, but yes, that's right. And so he felt very certain that the first President should be not American. And he thought, and he said this, he thinks it would be nice if it were a woman! And therefore, what did I think of Mme. Fessard? Well, you know, he was obviously reasoning backwards; if he really thought there should be a woman, now let's see, where can we find a woman? I think he thought Mme. Fessard should be the one, and, you know, what did I think of that?

So he knew I had worked with her and I was friends with her, and I said, "Well, gee," you know, "given that you're not going to be the first President, I think that would be an excellent choice. She's a wonderful person," and I may have told him that she could be a bit difficult and paranoid, but I didn't think that would be a major problem and she was certainly liked and respected worldwide, her work was excellent and so forth. So he said, "Well, I hope your committee will work towards that with the proper slate. I'm not going to tell you what to do, but meanwhile, let it be Mme. Fessard." [both laugh]

MELDRUM: "Do anything you want --"

LIEBESKIND: "Do anything you want, but do it my way, or there will be a horse's head in your bed." I mean, I have to say, there is something of the Mafioso, there was always something of the Mafioso chieftain about John Bonica; must just be in the blood or something; I don't know.

But of course he was a force for good, not a force for evil. But there was this kind of, you know, “I’ll do for you, and when the time comes --”

MELDRUM: “You’re going to do for me.”

LIEBESKIND: “-- you’re going to do for me.” And the people who worked with him very closely geographically felt that a lot; it was a strain, I know that from some of them. And I think I had all the advantages of some sort of emotional proximity without geographic proximity.

MELDRUM: You were a little bit distant, yeah.

LIEBESKIND: Yeah. And I know Loeser talked about this in his oral history that we did with him and where he said, “You know, I never worked for John Bonica; I was close with him and I was his doctor, but I always had my independence. I didn’t owe him my paycheck,” and he felt that was important. At least he could --

MELDRUM: Right. To keep his space, yeah.

LIEBESKIND: -- keep his autonomy as well as he did, but John Bonica could be smothering and demanding, and no question that that’s so. So anyway, I said, “Well, gee, everyone is going to be so puzzled by this, John, that you would not be the first President.” I said, “I don’t know if we can get away with this. They’re going to wonder. You’re going to have to say something.” He said, “Well, look,” he said, “I would consider, if your committee sought it, accepting the position of first President-Elect.”

MELDRUM: President-Elect! [she laughs]

LIEBESKIND: So then the little pieces fell into place in my brain, click, and I suddenly realized what he was doing, which is, since it was a three-year term as President, he was basically saying, “I’ll take nine years, three as President-Elect, three as President, three as Past President, to see this organization on its feet, keep the office in Seattle,” with Louisa there, and so forth and so on. And this is exactly what unfolded. So there was Mme. Fessard before Bonica, and Ainsley Iggo, I think, succeeded Bonica, but neither of them -- you know, as still today, the society is run by Louisa.

MELDRUM: In Seattle.

LIEBESKIND: Out of Seattle. The Secretariat has been there since before the society was actually formed, so that has remained stable, and the Presidents have been more or less vigorous in what they’ve done, but they’ve always had that. So anyway, I wrote -- well, this isn’t really in answer to your question, I guess, but thinking back to Florence, I would say that Mme. Fessard came to me when she heard about this, and she wanted my advice and my reassurance, and she was a little concerned that Dr. Bonica might just really do everything, you know, not allow her to be the President.

MELDRUM: Have his own way.

LIEBESKIND: I had to reassure her on that. She didn't know Bonica particularly well, but saw him as a powerful figure. And she didn't want to be made a fool, and I was able to reassure her on that. And she wanted help in writing her little speech, so I wrote my own speech in which I presented the slate to the whole thing. I actually have a copy of that; I kept that somewhere. That would be a fun thing to look at some time, as I gave the report on behalf of Pagni of the nominating committee and who our nominees were for all the different slates. And then I don't remember whether there was a voice or hand vote or whether ballots were passed out at that time, but basically we kind of told everybody, "Here's who we think the slate should be," and I think the slate was just accepted by acclamation, and that was the first, quote, "election."

MELDRUM: So, essentially, was this *your* slate?

LIEBESKIND: Not mine personally. It was the slate of the nominating committee, and each of them had input from various sources, including, most especially, from John Bonica.

MELDRUM: I see. So he probably talked to the entire committee.

LIEBESKIND: That's right. I mean, you see, the slate involved, it was a large slate because there was not only a President and President Elect, Secretary, Treasurer, Vice Presidents, there were Regional Vice Presidents -- we don't have those any more; that got changed in the bylaws -- plus, you know, a slew of Council members. So there was quite a large number of people -- whatever it was, twenty or more people -- who would be all the officers and Council members.

MELDRUM: And you tried to get geographic representation?

LIEBESKIND: And we tried to get geographic and disciplinary distribution, representation. That's correct. And so, you know, no one of us would have known all these people, so there was good, broad input. John Bonica didn't just pull this out of his hat, you know; I mean, he's a smart guy. He wouldn't be that autocratic. He had a big influence, but he wasn't that autocratic. So I think even though a member of that committee, I was on that first Council, so I guess I was, I presented myself [he laughs] as on the slate. I don't know how we got away with that, but we did. And the society was off and running.

The next thing I knew, probably right, pretty soon thereafter if not right there, I was asked to run the program committee for the next World Congress, the second World Congress, which was in Montreal, and that was a big job. So I was very involved in the IASP in that kind of major way from the beginning, and of course this meant rubbing shoulders with John Bonica. And the IASP met, as I recall, in those early days annually somewhere in the world, and we tried to move that around. So these would be business meetings, I'm talking about the Council.

MELDRUM: Right. Not the formal Congresses --

LIEBESKIND: Right.

MELDRUM: -- not the scientific Congresses, but business meetings.

LIEBESKIND: Right. Exactly. So they would meet at the time of the World Congress, but in the other two years they would meet somewhere else. So every year there would be some meeting, usually in some nice place, and I remember we met up in the Austrian Alps one time; we met, very significantly -- I'll never forget this -- in Lisbon, Portugal, because it was on the way back from that that I stopped in to visit Julia, and that was sort of the beginning of Julia's and my relationship; that was 1980. Anyway, so there were these posh meetings like that, and Emma [Bonica] would always come along, occasionally Nancy, my first wife, would come along. So it was kind of the family. And we all kind of got to know each other and we were kind of friendly and there would be jokes and drinks and walks and stuff and stuff, and, you know, a lot of spouses came along. And so we really did -- and I was involved for twelve years, I was on the Council for two terms, and, you know, that's a long time. So you really do get to know people.

So I don't know; I mean, there's a lot I could mention. There were other kinds of meetings of other societies that Bonica was involved in and that I was involved in, some of which he organized and invited me; I was on the original editorial board of the journal *Pain* and the only real negative interaction I ever had with Bonica relates to that, because, well, not *my* being on the editorial board; but *he* was on the editorial board in charge of soliciting review chapters. And fairly early on, he asked me to contribute a review chapter on pain modulation or brain stimulation and analgesia or whatever, and I said I would do that, and he gave me a deadline. And I didn't make it.

And a day before the deadline [he laughs], or the day of the deadline, I was very freaked out because I did not want to disappoint this man. And I don't know what it was; it really was one of maybe the only, or one of certainly very few, times in my life where I just, I basically failed, I didn't do what I had to do. I didn't get it together. And I called him up, and he, it was a very poignant moment because he said, "I have to tell you, John, I'm very disappointed in you."

MELDRUM: Ooh.

LIEBESKIND: And it was like, you know, it was a very father-son thing, and I just, I felt terrible already, and that didn't help [he laughs], you know. And I said, "Well," I said -- he said, "If you need another week or two -- I was counting on you for this issue, but if you need," whatever. And I said, "Look, I'm just, I really feel up against it, I'm just under a lot of pressure at this time in my career, and I would just like to get out of this obligation altogether, but what I will try and do for you, John, is to line up somebody else to do this review. And if you'll permit me, I'll make a few attempts at that and call you back. I'll try and get Dave Mayer, who's my former student." And he said, "Well, okay," reluctantly or whatever, and I did call Dave, and Dave agreed to it. Dave was already on his own by that time, and a very famous article came out of it -- Mayer and [Donald] Price. I say famous because it was really the first review article to be on this topic --

MELDRUM: Of pain modulation.

LIEBESKIND: -- that my lab had made these important contributions. We weren't the only ones; we weren't the first, as we saw last time [he laughs], David Reynolds, there were certainly many people who came afterwards. So that was a much-cited article, and after Dave and Don Price wrote it, they were working together at the Medical College of Virginia, where they are still working together, by the way. This was back in 1975 or something like that. Dave called me and said, "We've got it," you know, "and we've written it now, and if you want to be an author, you may." And I said, "Dave, I didn't have anything to do with this." There was no way I would have put my name to that. So anyway, that came out as Mayer and Price and kind of got me off the hook. But boy, did I feel lower than a snake's belly [he laughs] in that episode.

But, again, there were many times when Bonica and I came together. We had a meeting down here in L.A.; we got this local foundation, Larry Kruger and I were asked to put on a pain meeting, and one of the requisites was that we name a winner of the prize. This foundation supported having this meeting; they were called the Intrascience Foundation. And there had to be, one of the conditions was that this year, okay, we agree, we'll do it on pain; you guys will do it; here's the money to do it, but you have to name a winner of the prize. And so Kruger and I, with surprising ease, had no disputes in relation to who that should be. We both agreed that should be John Bonica.

I have this wonderful picture --and you've seen it, I know, in my office -- of John Bonica blowing out the candles on his birthday cake. And that meeting was held on his birthday, and, yeah, and he was the prize winner and I had to make a little speech about John Bonica, which was, I kind of made some sort of a semi-roast. I called him the father of the field of pain, and, as a matter of fact, the godfather [he laughs] of the field of pain or something. You know, but I also extolled his praises, and there were other things like that. Somehow I got to introduce John Bonica on several occasions. I don't know.

And then there's a long story that could be told about the whole episode of the International Pain Foundation, and my relation to John Bonica in connection to that, with, I mean, I don't think it's appropriate to take time to go into that in this interview, but let it be stated, it's a long and involved, somewhat complicated, and emotionally charged episode in my life that involved me, John Bonica, but mostly me and Bob Wald, W-A-L-D, who was a friend of mine -- he died last summer also, by coincidence, a young man in his early fifties, of cancer. But Bob and I had been friends ever since I came to UCLA; he was a law student, and we met through a mutual friend at the same time I was just coming to UCLA, and he and his wife and me and my wife became fast friends and the children kind of grew up together and were at each others' homes and so forth.

And early on, I mean, I'd have to get out my books to see when this was, but really early on, Bob and I had talked about this concept of creating a pain foundation. And Bob was a brilliant man. Of course, he's not into the field of pain at all; he's not a scientist or a doctor; he was an attorney, entrepreneur, businessman, and but he loved hearing me talk about my work, and he was so proud of me as his friend, you know, that he used to brag about me around other people. He'd say, "Here, meet John Liebeskind. Let me tell you about his work." And it always amazed me how well he understood it and remembered it. I mean, he was just so bright.

MELDRUM: Well, you explained it very well.

LIEBESKIND: No, well, I've explained my work to many people, but with most of them, it would go in one ear and out the other. Bob retained that kind of stuff, could talk about it intelligently and see implications of things and so forth. And he became kind of a pain maven, became kind of ultimately a maven of our field. I mean, he kind of got involved, first with this sort of general idea that we could start a foundation, and that he would contribute to it ultimately at the outset. And John Bonica was coming through Los Angeles on his way somewhere, and he had to hang out at the airport, he had to change planes and be at the airport for a while. Bob Wald and I went out and met with him and talked to him --

MELDRUM: At the airport.

LIEBESKIND: -- at the airport about this idea, and John became very excited about it. Well, it languished for a long time, and then some years later at a Council meeting, in fact, in Buenos Aires -- I don't know, I'm getting confused here on times here now; I'd have to get that out. Would it be the late '70s or something, early '80s?

MELDRUM: I have the impression it was the late -- no, early '80s, do you think?

LIEBESKIND: Well, maybe. I don't know. Anyway, I'd have to get this out. I'm losing track of time. But anyway, well, let's see here; hold on a second. It would be late '80s, yes, so it would be the late '80s. So maybe even like '87 or something like that [Ed: it was probably a year earlier]. There was a Council meeting in Buenos Aires, and I had talked about it before that or something; I said we were going to have this Council meeting, and he said, "Why don't we get this God-damned foundation off the ground? Why don't you get those guys to really get going on this?" or something. So at one point when Melzack was president at the time, he said, "Well, is there any other business?" I said, "Yeah, I'd like to propose that we get a foundation going. I've talked to this fellow," and something and something, I don't remember exactly what happened. Maybe Bonica was there and said, "Yeah, I met him; yeah, he's an interesting man," or something.

So Melzack said, "Well, gee, John, why don't I just appoint you to head a committee to investigate this business of starting a foundation, and you look into it, and here, it's done. The president has the right to make an appointment; you're it. Go investigate this and report back." So I said fine, and I went back and told Bob about this, and, then, you know, right away he said, "All right, just invite everybody down. Who do we want? Let's get all the big shots -- John Bonica, Ron Melzack, invite them to L.A., and all the officers of IASP, invite them to L.A.; I'll pay for everything," you know. "Come here, and we'll have a little meeting, and we'll do it. I'll get my lawyer to draw up the things, and we'll have a foundation." Well, again, I'm going through this now in great detail. Let me just now say that that led to the creation of the International Pain Foundation, and ultimately, well, so it failed. Bob Wald --

MELDRUM: So the purpose of the Foundation was to raise money --

LIEBESKIND: That's right.

MELDRUM: -- so that pain centers and pain researchers could apply and be funded in various projects.

LIEBESKIND: Yes.

MELDRUM: And the field would not -- possibly be less dependent on the government, among other things.

LIEBESKIND: Absolutely. Yes.

MELDRUM: And also there would be this sort of lay group advocating further work on pain, is that -- ?

LIEBESKIND: All correct. I mean, we drew up articles of incorporation, including our mission statement and so forth and so on, and the idea, we had the idea early on that our goal would be public education; that was really the purpose. If we were raising money, it was to address, you know, to try and let the world know about pain and about what was being done and so forth and so on. And, well, again, there's so much that can be said. I think what, really, we're talking about me and Bonica at this point and my relationship to John Bonica, and I do think I need to say something about the IPF in this regard because there was a, we had a meeting of our board here in L.A., it may have been one year into --

MELDRUM: Into the Foundation.

LIEBESKIND: -- into the Foundation, may have been our first annual meeting or something like that. I was the President of the Foundation, Bob the Treasurer, Bob having chipped in the only money we had, \$100,000 at that time, you know, so that was agreed, but I think agreed to a little reluctantly on the part of some of the others. And without naming names at this point, I think there was some resentment at my being President. And --

MELDRUM: They thought you were too close to the IASP maybe?

LIEBESKIND: No, this was the IASP; the whole thing was the IASP.

MELDRUM: They thought you already had too many important responsibilities?

LIEBESKIND: No. No, no, not that at all. It's really hard to say. There were so many currents and cross-currents. And Bob got involved in this himself in peculiar ways; I don't know what was going on, and I mean, one of the things that makes it difficult to talk about is, I really don't know exactly what went on. But there was some movement at this meeting in Los Angeles to unseat me as President that surprised me. I don't know where that had come from. And I certainly never intended -- it was not my idea to hold on to power or something; I was working very hard. I was spending an incredible amount of my time, while still, you know, very much trying to run a laboratory, be a professor, advance my career, this was occupying the vast majority of my time. It was very self-sacrificing, if I may say so, for better or for ill, whatever I did or didn't do for the Foundation, it was certainly costing me a lot of time.

And there was a very emotional moment at this board meeting where John Bonica just, I think he slammed his hand on the table, and with tears in his eyes and choked up, as he could get -- when he got emotional he could hardly talk [demonstrating strained voice], his voice would get all strained, you know, like that [he laughs], and he said, "I won't stand for this." He said, "I won't hear this." And he shut this guy up. He said, "John is our President, and that's going to be the way it is from now, and I don't want to hear any more talk along this line," he said. "Otherwise, you're going to have to answer to me," or some statement like that. And everybody just shut up.

MELDRUM: [she laughs] I guess so!

LIEBESKIND: And that was the end of that. But it came back a year later, and at that point, again, there had been a lot more politicking going around behind the scenes, and I was asked to step down as President, and I said, "Fine, that's fine by me; I just would like to think the Foundation is going to go on; who's going to deal with it?" And Mike Cousins said that -- he was currently the President; he was at that time the President of the IASP -- and he said he would take over the Foundation, and he basically, in fairly short order, made it disappear of its own, in the sense that he kind of brought it into the IASP. The only problem the IPF had ever had was being too close to IASP; he brought it in even closer until it disappeared and became again the IASP.

So the problem with the IPF all along was that we didn't have anyone to really run it who knew what they were doing -- certainly I didn't know what I was doing. I was working hard at it and trying my best, but, you know, I didn't know how to raise money. Effectively the only real money we ever raised, the only big money we ever raised, was from Bob Wald, who kind of wanted it that way, which was one of the big problems.

MELDRUM: That *is* a big problem.

LIEBESKIND: It was a very big problem. He, in many ways, he was the most to blame for the dissolution of the IPF. It was very complicated.

MELDRUM: So then the monies were just absorbed by the IASP?

LIEBESKIND: We gave away a lot along the way, but in the end, whatever was left over, there wasn't that much, became absorbed.

MELDRUM: Well, a hundred thousand really isn't that much.

LIEBESKIND: And the tax status of the IASP and the IPF were identical. They were both, as we call them, 501(c)(3)s, meaning they were tax-exempt, charitable foundations to raise money and so forth, and from that standpoint there wasn't any problem. Well, there's a lot that could be said about what we should have done, you know, or shouldn't have done and so forth and so on, lots of mistakes were made, and in the end, really, the bottom line is we didn't know how to raise money. If we'd known how to raise money and could have broadened our base, I mean, we did



have one fund-raising drive within the membership of the IASP, and basically raised something near \$40,000.

MELDRUM: Yeah, it is very difficult to do. I know; I've done it. I've tried to do it myself.

LIEBESKIND: Well, it's still sitting out there; we all know that. We in the American Pain Society have talked about this, and I've been asked many times, would I be involved in trying to get the American Pain Society to do something like this and do it properly? And I have told three or four Presidents in a row that the answer is, "I will do anything I can to see this happen in the American Pain Society, short of doing it myself. I've done it; I've tried to do it; it was a painful experience for me. I'm not in a place where I want to try that again, but I will be happy to give whatever advice I can, be part of any planning group," and, you know, there's still some talk about it.

I don't know whether it's going to happen or not, but, you know, the bottom line, which in a sense is what Bob Wald saw in a way that no one else did, he was definitely the first to really see this, and it's still just as clear today, is that there are hundreds, literally hundreds of diseases out there with names you can't pronounce that affect a fraction of one percent of the population, that are out there raising millions of dollars in the name of that disease -- to promote research, public education, professional education, you know, la-de-da, la-de-da; why the hell is there no organization that's raising money in the name of pain, which affects millions of people worldwide? Everybody recognizes pain problems; all we have to do is show them this and this, and why wouldn't people be dropping their nickels and dimes and millions of dollars, whatever, you know, into the hopper, to do this? It's crying to be done.

So that has yet to be figured out. No one has yet picked up the gauntlet on that. Bob Wald gave a God-damned good try. I like to think I gave it a God-damned good try. We worked very hard on that for a while. We each made mistakes that contributed to, you know, a lot of us did -- the whole board made mistakes in terms of some very crucial decisions, including one perilous one that we all, the board, made despite my advice, so, I mean, I was on the side [he laughs], the right side on some; I was on the wrong side on some. We all made a lot of mistakes. And in the end, the issue was, there was no one really to pick it up. There was no one really to say, "I will do it. I know what I'm doing; I'll get this thing off the ground; I'll get us the help we need, and I'll do it." No one was willing to do that. And I tried, couldn't, no one else really, effectively tried.

MELDRUM: That is, unfortunately, something which frequently happens to really good organizations.

LIEBESKIND: Well, you've got the wrong people trying to do this job. I mean, we were people, who were professors and doctors and scientists and whatever, what did we know about running a major group? At one time, we had coffers of about half a million dollars, and, you know, the potential for being vast, and what did we know about dealing with that? And Bob Wald, unfortunately, I mean, he actually said at one time, "I don't want any other big shots, any other businesspeople. I'm your businessperson," you know; he wanted to be the one guy who we all had to bow down to.

MELDRUM: Yeah, that's kind of a mistake.

LIEBESKIND: Yeah, that we allowed that to happen, we allowed him to kind of take over in that way, was a disaster. Anyway.

MELDRUM: Anyway, you learned.

LIEBESKIND: Anyway, that was just another instance where John Bonica and I --

MELDRUM: Were together, and where he came to your defense.

LIEBESKIND: Yeah, he did. He actually wasn't there a year later when I did go out; I don't know what would have happened if he had been there [he laughs]. But it's just as well. By that time, it was a good decision to have me step down, a good decision for me and a good decision for the Foundation.

MELDRUM: Well, now, Bonica seems to have been -- I mean, he's obviously an excellent leader.

LIEBESKIND: Yeah.

MELDRUM: He was a strong person. You and several other people have commented on his emotionality, which is something that many scientists don't hold in very high regard.

LIEBESKIND: That's right. Well, we're not, most of us are not very accustomed to that. I gather it increased as he aged. Emma, his wife, made some comments about that. "Oh, he cries all the time." [he laughs] But he -- this happened a number of times. He would choke up as he would -- well, it was just an involuntary reaction to emotions, thinking about past wonderful things or awful things, you know. Either way. He couldn't talk about his mother without tears coming to his eyes, or his old days wrestling.

In the oral history, that was, again, an extremely intense, emotional experience for us, for him and me, coming together, when I went out to Hawaii, spent four or five days there and interviewed him over four different days, and we basically spent all that time together, all the days and the evenings. John and I would have dinner every evening together; she [Emma] made lunch for us. We were just together for all that time, and at the end he hugged me and he said, "I love you," and I said, "Yes, I love you, too, John," and that's the way we felt about each other. And so, yeah.

MELDRUM: So he was a very open guy emotionally.

LIEBESKIND: He was vulnerable emotionally.

MELDRUM: Was that something you admired in him?

LIEBESKIND: Huh?

MELDRUM: Was that something you admired in him? What was it that attracted you about him so much?

LIEBESKIND: Oh, well, everybody felt this way.

MELDRUM: Everybody? Apparently?

LIEBESKIND: Well, all right, not everybody. Some people resented him; some people didn't like him, I suppose, because he could be overbearing. But I think everybody recognized his strength, his brilliance, in so many ways. I mean, he was not a polished, sophisticated, intellectual individual. He didn't have that Eastern prep-school kind of education or polish; he grew up in the streets of New York and hung out with wrestlers and carnival folks and so forth, so he was a rough stone in some ways. He talked with a thick accent, which people who didn't know what they were talking about assumed was Italian; it was, as Louisa points out, Brooklyn. [he laughs]

And -- but he was a brilliant man in terms of what his brain could store and the synthesis that he could provide to thoughts and ideas, and the man, he was a visionary. He was a man of great vision who saw, who had a passion for this field and saw something that could be done, and knew how to do it and did it. And pursued it, you know. I mean, he's -- you have to be a jerk not to admire somebody who could live a life like that.

Not to say that everything was perfect; you know, in living a life like that, you have to pay the piper. And he did, and others did for him, like his children; they've got some resentments about their absent father, that he had to work so hard, he wasn't available to them as he was to the rest of the world, when they see, especially at the funeral of their mother, at the funeral of their father a few months later, the love that so many people had for them, for their father, let's say, they could say, "Well, shit, how do these -- where did he get off giving all this love to these other people who loved him in return, why didn't he have more time for us?"

So I mean, there's that whole issue. And, you know, he was tough on people. He rode people unmercifully who worked with him or something. He himself worked extremely hard and expected everyone near him to work that hard. But he was a remarkable man. He was remarkable in his fundamental humility. An aspect of his accomplishment depended on his humility, I think, because he was someone who was ready to listen and learn from other people. He listened to other people, and you don't bump into a lot of big shots --

MELDRUM: No, frequently you do not.

LIEBESKIND: -- who are overburdened with that trait. Most of them, you know, most big shots you meet in the world think they know it all, and they have their very strong ideas, especially people with strong personalities. So here was a man with a strong personality, with strong passions --

MELDRUM: And a great deal of intelligence.

LIEBESKIND: -- and a great deal of intelligence and a great deal of accomplishments, who was nonetheless ready to listen at any time and sought that kind of information. And I think that's a very rare combination. No one else jumps to mind when I think of that combination of traits. I don't know who else personified that combination of traits, and you say, "Well, all right, he was ready to listen; what does that mean? How do you know?"

Well, that's what the multidisciplinary approach meant, that he stood for, that he, as it were, invented; that's what it means -- that "I, the anesthesiologist," in his case, "don't know everything; I need to listen to neurologists, I need to listen, for Christ's sake, to psychologists! I need to know something about basic science. I don't know anything about basic science. I need to listen to these basic scientists. We need to get together. The IASP should be like this. I don't want to surround myself with fellow anesthesiologists in running the IASP, or fellow Americans 'because Lord knows those Europeans and Japanese don't know anything' -- no, I mean, we've got to hear this other point of view."

This is the very essence of what the field of pain has been about since he created it! He imbued it with that spirit, which has, as its basis, the fundamental humility of saying, "No one person has all the answers, so we have to band together." And a man as bright, as accomplished, and as forceful -- he wasn't just some meek, mild-mannered person who was running around being insecure and "No, no, I don't really know; gosh, humble me; what do I know?" He wasn't that kind of person! [he laughs] So he had this great strength and self-assurance at the same time as he was willing and able and very ready to listen and benefit by what other people had to say.

MELDRUM: That's really an excellent description.

LIEBESKIND: When you stop and think about it, when you put it that way, it's quite remarkable. And the other thing for me was this closeness. I mean, he was a hugger, a kisser, and someone who expressed in words, the closeness of his feelings. And he did that for me progressively more as I knew him, got to know him better over the years. He came to my home and he loved Julia, he loved Ben, and so did Emma, and you couldn't talk to them without them asking, "How's Ben? How's Julia?" They knew the family story, you know, and of course Julia spoke Italian, so they could speak Italian together, and, oh! you know, she could cook nice Italian meals for him, which she did at least on one occasion [he laughs], two occasions, that I can think of, at my home, and of course he would never, never forget that.

MELDRUM: Something you always remember.

LIEBESKIND: So that's a long answer to your question.

MELDRUM: Helps to bring some of this stuff out, I think. And, okay, I'd like to go from there just briefly [she laughs], because we have not mentioned it previously, you told how you got the Western Pain Society started, and then some years after that it was merged with the Eastern Pain Society; have I got this right?

LIEBESKIND: Yeah.

MELDRUM: And so could you tell a little bit about how that came about?

LIEBESKIND: That came about at the, officially came about -- there was groundwork laid, but officially came about -- at the third meeting of the Western Pain Society -- well, hold on a second. I'm misspeaking here. The third meeting of the Western Pain Society, the western group agreed to merge, agreed in principle to merge with the eastern group to form an American Pain Society. My recollection -- I like to brag about this -- is that the vote was whatever it was --

## JOHN C. LIEBESKIND INTERVIEW

### TAPE FOUR, SIDE ONE

JOHN LIEBESKIND: The needles are moving forward?

MARCIA MELDRUM: Yeah, the needles are moving now, and it's ten past three. We're beginning tape four, and we're talking about the Western Pain Society, the vote of the Western Pain Society to merge with the Eastern Pain Society.

LIEBESKIND: Yeah. Now, this had come to the Western as a proposal from the Eastern, and I mean there was a lot going on, some of which I was not aware of, but I should hasten to say that the prime mover for creating an American Pain Society was Bert [Berthold] Wolff. And he was running the Eastern Pain Association at that time, and there had been a group of people in the East who'd been getting together for quite a while, and I've heard Bert claim, even before IASP was formed, certainly before the Western Pain Society was formed. I don't know how big a group or where they came from or how often they met or how formal, but there were, they did have some meetings. And at some point they became a real organization, and at some point further down the line they became a chapter of the IASP, and at some point further along the line they sought to create an American Pain Foundation. And Bonica was interested in forming an American Pain Society, American chapter.

MELDRUM: Right. Seems logical.

LIEBESKIND: And he was interested in that, but he was very concerned that it not be allowed to be the tail that wagged the dog. I mean, his baby was the International. It was fine that there should be an American [chapter], you know, so he had some concerns about that. Bert Wolff, on the other hand, had major concerns that an American Pain Society should not prevent the Eastern Pain Association from continuing as a chapter of the IASP. He did not want his Eastern group to lose its chapter status, which, by the laws of the IASP, which I think he didn't understand, it would. Because you cannot have two chapters of the IASP with overlapping geography. So they would be at war with each other. So there needs to be an American, and the Eastern and Western, which were already chapters, would lose their chapter status in the IASP; they could have chapter status in the APS, which is ultimately what happened. APS itself has regional chapters.

MELDRUM: But essentially they would, you know, lose status --

LIEBESKIND: Lose status as organizations for the greater benefit. I voted against the Western forming this American [society] because I was concerned -- it was sort of a silly thing; I think I was being a little silly at the time, but I knew that it would win. I just put in a sort of a little protest vote -- I would have voted the other way if I'd thought anyone would join me -- just to say that we had a kind of a special thing in the Western; it was very informal, you know, sort of no-cost, and the way that we had started it here in L.A., I was concerned that if it got too big it

would lose that status, and basically, by forming an American Pain Society, it might obviate the need for a Western. Maybe the Western would continue, and I liked it small.

Anyway, so, very much because of the energy of Bert Wolff, and he really, you know, I think of Bert as a foolish person in many ways; I have lots of complicated thoughts about Bert, but I have to, we all have to acknowledge that he did do a good job in getting this organization off the ground, and he wanted to do it, sought to do it, took the responsibility, he was the prime mover in the American Pain Society in a way, I think it's fair to say, not dissimilar to the way John Bonica had done it setting up the IASP in the first place. And so it was a kind of a, we in the East and our henchmen and you in the West and your henchmen will get together, and we'll get together in Chicago. And so there was actually a meeting in Chicago, which is terribly funny when you think about it.

John Bonica did not attend that meeting; he meant to, but he was having one of his operations. He had many operations on his shoulders and hips for osteoarthritis, and I think that was what that was for; he had a lot of operations, and I think that was one of those. And anyway, everyone who was anybody in the leadership of the IASP or the Western group or the Eastern group got together in Chicago in whatever that was, [March] 1977, something like that to, it was already agreed upon by the two separate organizations to merge; now we had to figure out exactly how to do that. And I was one of the delegates; I think there were twenty-something delegates there from all sides, and I was one representing the West, as it were.

And, you know, there's a record, and some of this we have, and I have some correspondence and whatever and whatever, and there was then a series of meetings and bylaws were created, and a slate of candidates, and the thing got off the ground. Yeah. And so that's how the American got started, and it's proven to be a very successful organization. Bert was the first president; in fact, he was the first two presidents. He was re-elected for a second year, which we after that disallowed. Enough said on that point. [he laughs] No one was allowed to succeed himself or herself thereafter. And so here we are, whatever it is, sixteen years later or whatever since it actually got really started, and cooking along. It's a good organization, well-run organization.

MELDRUM: Well, it certainly seems to be, from the meeting they had. Okay. I'd like to get that into the record. Okay, changing gears, I was wondering, okay, since I have now read a great deal, I have heard Melzack and Wall talk about the gate theory, and I've heard Ed Perl talk about, talk sort of against the gate theory, I was wondering if you would care to take a position.

LIEBESKIND: Oh! On the gate theory?

MELDRUM: Yeah.

LIEBESKIND: Oh, sure. Well, I, you know, I think without a question it's the single most important article --

MELDRUM: Yeah, you've said that. [she laughs]

LIEBESKIND: -- and set of ideas to have happened in the history of pain scholarship. Certainly from the standpoint of science as opposed to, let's say, straight clinical innovations or clinical concepts or something. From the standpoint of our basic understanding of what pain is as a biological, psychobiological phenomenon, the gate theory, and far, far below that would be some second-place concept [he laughs] or whatever, and then on from there. Now, you know, like any good and complicated article or piece of work, you can view it in various ways, and if you take it at the level of the specific scientific constructs and predictions and explanations and details, it was wrong in a number of those details, and that was shown pretty quickly. Some people already knew it was wrong or thought it was, they hastened to prove it.

MELDRUM: Yeah. Instant critique.

LIEBESKIND: So the gate theory got bashed around quite a bit the first few years, deservedly so, and so what do you take that to mean? What I take that to mean is bravo for the gate theory – it's proven heuristic; that's what a good theory is supposed to do. I mean, I'm kind of a Karl Popper-ian or whatever; a good theory is meant to self destruct. That's how science gets advanced. So it was incorrect in some of its details; it was also correct in some of its details, and it was certainly extremely useful and has continued to prove very useful in terms of the general way in which it viewed pain. And I don't know, that's something that's a little tough to discuss briefly; it's almost the difference -- when I think of what Melzack and Wall meant by "pain," and when I think of what Perl and other people like that meant by "pain," I think they're talking about two different things.

MELDRUM: I see that too.

LIEBESKIND: They stood toe to toe and insulted each other -- [Meldrum laughs] everything but came to blows, disagreeing where in a sense they're not talking about the same thing, so they've got no right to disagree! I mean, I think Melzack and Wall are talking about a process that we can clearly call perceptual, and I think Perl was talking about something that we would call not perceptual but strictly sensory. And that one is a higher-order construct and the other is a more primitive construct, lower-level construct, that doesn't involve the same higher-order --

MELDRUM: Right. A more mechanical concept?

LIEBESKIND: Yeah.

MELDRUM: A more stimulus-response kind of thing maybe.

LIEBESKIND: Yes, that Perl was really talking about the peripheral nervous system and seeing the central nervous system as more of the same, and Melzack and Wall were saying, you know, "Look, the central nervous system is really what pain is all about; it's a conscious perception, and that involves a lot of complicated circuitry, and sure it's important what comes in, but how it gets modulated and worked on and modified," and whatever and whatever, "is really what's important, after that, as to whether we even perceive it or not."



So again, I think we could go into a lot of details on that; I think the point of view of pain as a kind of broader construct was very refreshing, was a smack in the face to the neurophysiologists, sensory neurophysiologists, who weren't really prepared to deal with a construct of that order. They just, "Consciousness? Wow!" you know. "Psychology? Come on! Get serious!" [he laughs] They just weren't ready for that. They just didn't -- this abstraction ladder thing we were talking about last time, it's just, they weren't comfortable at that level of abstraction. They didn't know how to deal with a construct like that. And I think that's really what the difference was; it was a person on one rung of the ladder looking at a person on the other rung of the ladder above or below, and spitting, saying, "You're off in the clouds; you don't need that crap," or, "You're down in the mud and you don't see where the light is." And it was that kind of a thing.

So I think the gate theory had been, and continues to be, very useful. It was very useful for clinicians, very attractive to clinicians, who found answers embedded in one or another aspect of the gate theory, with the issues that they --

MELDRUM: Right. That they had to treat.

LIEBESKIND: -- yes, that they were trying to treat in the clinic, and so they saw, you know, the gate theory as clarifying for them, being a set of scientific constructs that related to what they were dealing with in a way that peripheral nerve action potentials never could have been. And so it was a tremendous growing experience for a lot of people, the gate theory, and whatever they felt about it, then or now, people have grown with the gate theory -- grown fighting it, grown hating it, but grown. Grown loving it, using it, and so forth.

MELDRUM: They were forced to sort of let their concept sort of expand to the potential framework.

LIEBESKIND: Yes. And let their view of things, which of course can be fit, if you were real smart in a way that I don't know if I can do it, certainly not right here and now, can be fit with the whole multidisciplinary concept of pain in the first place. I mean, it was a lot closer to being a multidisciplinary theory than specificity was. It said, "We have to take account of psychology, we have to take account of the brain." And of course for me personally, it was the right way to think about things, both because I am a psychologist, I think in psychological constructs, and more particularly because of the work we did, which was brain down to the spinal gate, that that was the concept that was in the gate theory.

MELDRUM: Which was really sort of supportive of it.

LIEBESKIND: It was very supportive of the gate theory in that aspect of it. I mean, most people think of the gate theory as being a theory of afferent interactions between large fibers and small fibers coming in, and that was sort of the centerpiece of the gate theory, but there was also the central control, central trigger, which was to say -- I'm sure Melzack had a lot to do with that; well, Wall would have, too; he knew about that, was very interested in that, of how the brain could play down upon spinal reflexes, spinal sensory integration, to allow psychological factors to influence perception.

MELDRUM: To modulate perception of the data.

LIEBESKIND: And they didn't know quite how that happened at that time in 1965.

MELDRUM: Do we still not know? I mean, we know a lot more than we did thirty years ago.

LIEBESKIND: That's really what our work has been all about, our work and the work of all the others. I mean, we stated right from the beginning that this was a top-down pain inhibition system. We are stimulating the brain and we are causing pain inhibition in the spinal cord, and we said, "This is kind of like the gate theory." That's the way we ended the article. "It's kind of like what the gate theory is saying about it." So we invoked that concept. They fit together beautifully. And the whole field of pain modulation came out as kind of a subfield of pain, a very important part of the field, fully derives from that. So, you know, I think it's --

MELDRUM: Ed Perl's not wrong, but he's kind of --

LIEBESKIND: Well, it's the kind of thing where there's not exactly a right and wrong.

MELDRUM: He had this tunnel vision or something. Not looking at precisely --

LIEBESKIND: Well, his views have changed, too. He's talked about modulation, and he understands that it's not a thin copper wire that goes from the nociceptors to the top of the head, you know, I don't know that he understood that quite at the beginning, but he understands that a lot better now. But, you know, he's focusing and has always focused his attention on that aspect of the system which can be seen in specificity terms. There are aspects of it; so it's not like one person could ever be wholly wrong or wholly right on this. It's kind of what's useful for you and what aspect of it do you want to look at, focus your attention on?

MELDRUM: What interests you and what kind of problems you can address.

LIEBESKIND: I mean, Ed Perl has done, you know, exceedingly important work, whatever Pat Wall may say about that.

MELDRUM: Yeah. I wasn't attempting to be critical, but it's obviously extremely interesting and important work.

LIEBESKIND: And it's not just Ed; I mean, there are other people. Ainsley Iggo is another one who stands shoulder to shoulder with Ed Perl, and there are others, many others as well who worked on peripheral nerve and have done very important work, who have worked on the spinal cord and even the brain. I mean, Ed Perl started working on the brain. So, you know, it's not like he doesn't know where the brain is. Sometimes I think he forgets that it's there [Meldrum laughs], but in his early work, I sometimes think he did.

So it's a style thing. It's a "How do you like to think?" thing. It's a rung on the abstraction ladder.

MELDRUM: I think you're perfectly right about the gate control lending itself to the multidisciplinary approach. It sort of welcomes input from different fields, from clinicians as well as from basic scientists.

LIEBESKIND: It talked about pain in clinical terms, partly, in a way that no other neuroscientist people ever had before, so it certainly said, "Hey, clinicians, we know you're out there and have something to contribute to this, and there's something that you need from this," and no one had ever done that before for basic science, I don't think, certainly not to that degree.

MELDRUM: Okay. That's good. Now, you have asked, I know, in most of your interviews, and now I'm going to ask you, what the impact has been of your career on your personal life.

LIEBESKIND: [he laughs] Yeah. Well, I think it's probably very telling, and has been one of my constant questions of others because naturally I'm very interested in that in relation to myself. And, well, like almost everything else we're talking about, it's difficult to talk about that briefly. But we must do that, so I'll try and give you an outline, but needless to say, there will be, by the time I'm through, a lot more that can be said on the subject.

I think I've paid a certain price in my career in working as hard as I did, and it's something that I don't feel altogether good about. I was kind of driven by my work; I didn't do my work for the love of it; I did my work in a sense for the fear that if I didn't, some devil would bite my ass, you know what I mean? And that's not a good way to work, and it is the way that I think I work. I mean, that's not to say there wasn't a love for the work, a love of aspects of it, but I was bedeviled by it from the beginning. I felt nervous about it and inadequate to the job and stuff and stuff, so there was always this kind of tension about it. And in the early years of my career, I wanted more and more of whatever anyone could give me. "Give me your work," you know. "Let me be on your editorial board."

MELDRUM: Yeah. "Let me be on your committee."

LIEBESKIND: "Let me read your article. Let me be on your grant review. Give me this, give me that. I'll do anything," because I wanted to be able to say I'd done it; that's how you built a career. Well, after a while, once my career got kind of built, the tables kind of turned on that, and I became suddenly aware that work was the way you got paid off in academics, and if you were a good boy --

MELDRUM: They gave you more work. [she laughs]

LIEBESKIND: -- they gave you work to show you, you know, "Oh, we think well of you; here. Polish it and clean it up," you know. And I got to resent that. Now, all this was happening while I was going through kind of a change of life. Now, whether it was, you know, male menopause [he laughs], or whether it was hormonal or not, I don't know, but it was coming at a time when my nearly twenty-year marriage was breaking apart, Nancy and I ended up divorcing in our nineteenth year of our marriage. And after nineteen years of marriage, and a few years before that, things were starting to come apart, a lot of tensions were building and so forth. And then

the whole Julia story, that's a long story; I'd met her before, I knew who she was, but then we sort of fell in love in 1980, as my marriage was just about out the window, and we were both pretty wounded people at that moment. She had just lost her lover of five years, this man that she had been living with for five years.

MELDRUM: He died, right?

LIEBESKIND: He died, my friend, and that's how I got to know Julia; that's Peter Powell, so she was, as it were, a widow in mourning, and I was, you know, a guy who was just about ready to divorce and was in bad emotional shape with respect to my marriage. And so, anyway, over the course of those next two years, 1980 to, into early 1982, Julia and I sort of built this relationship, first at long distance, L.A. to London, and then at slightly shorter distance, L.A. to Miami. [he laughs] She got her job transferred, the same company she was working with in London had an office in Miami, and she got to work there and do a project for almost a year, and so we were commuting back and forth to Miami.

And at the end of that, she left that job in order to move to Los Angeles in order to live with me so we could get married. My divorce came through, and we did get married. And at that time, I was romantically in love -- and I still am, I think -- but at that time it was new love, it was young love. I felt, you know, like something I had never experienced before. The only thing I could relate it to is these sort of intense feelings I'd had as an adolescent toward Diane Cassel.

MELDRUM: Diane Cassel! [both laugh]

LIEBESKIND: Or whoever. I mean, it was like puppy love; it was a very exciting emotional experience, something that, although Nancy and I had our times of closeness, I must say I never quite experienced it with her and I'm sure she never did with me, either. And so that was a very transforming kind of period, and I was, you know, things were happening and I just felt like this was a major life-change thing, and somehow I think Peter's death also was part of that. It was a kind of sense of my own mortality and here I am, whatever, forty-five years old or whatever I was at the time, 1980, you know, forty-five years old, forty-six, forty-seven years old. And I kind of came to feel that my career had, that I was very happy about it, I didn't want to give it up, but that I didn't want to, and *I didn't have to* live it the way I had lived it, that I could pull back some.

Well, not only didn't that hurt, if anything, it helped my career. I mean, I don't know that there was -- I hope there wasn't a causal relation; there may have been. But that the further I got away from the day to day of my career, the better things went. But at least there was enough momentum that things really were just perking along, and things just kept getting better. And the first few years that Julia and I were together, there was just a major dogleg in the way I worked. I just stopped working so very hard and stopped caring about things as much and started turning work down, started taking a little more control of my life in that sense.

Now, through all of this, there was a strong sense that the major victims of my living the way I had lived were my children, my two big boys [Gabriel and Nicholas]. And I did and do feel guilty about that. The oldest boy, Gabe, had some quite serious emotional problems in high

school, and that was at a time when Nancy and I were splitting, divorcing, and Julia was coming out here, and a time when my career was starting to change, but, you know, so I don't feel real good about that. And I think that there are probably a lot -- I guess I'm looking to see others, Bonica, Melzack, whomever, you know, say, "Yeah, I worked too hard, and my kids paid for that and my wife paid for that," you know, "my married life," or whatever. And sometimes I see that, some evidence of that, sometimes I don't. And I think that's very interesting.

And I guess that what I believe is that there are people, I'm not quite sure I can name many, but I guess I believe that there are people who work very hard in a very positive way in their life who are not troubled by their work in the same way, who work hard, but for whom it's a joy, and who don't bring home the weight of the world when they finish their work at the end of the day and bring home some psychological energy and ability to set work aside and spend, as they say, quality time, even if it's a reduced amount in quantity, in a way that I could not. I was, you know, harried and victimized, self victimized, in the work situation, where I feel it's still kind of part of me.

I mean, part of my decision, ultimately, the final decision, the final solution [he laughs], was to close my lab, which I did. Even then, it's taken a while, and it's not over yet. I still am a lot more relaxed; I have a lot less I have to do; I'm a lot less bedeviled by my career, and a lot more -- I like to think and I hope it's going to continue this way -- positively attracted by what I am doing, this historical thing that you and I are doing together, Marcia, I don't feel bedevils me as I felt by the other stuff. I feel like I have an area of confidence within this, and something to contribute to it, and that so far it's just been pure joy doing it, you know. There's no huge deadlines that I'm facing and, you know, like, "Oh, God, if I don't do this, something bad will happen." Plus of course, now I have succeeded in some formal sense, you know, beyond my wildest hopes when I was young as to what might happen in my career, so that, I mean, I would have to be a complete maniac if I didn't feel a sense of satisfaction.

MELDRUM: Accomplishment, for sure.

LIEBESKIND: And a sense of accomplishment and a sense that I can now step back, step aside, step off the treadmill, and I've paid my dues and I can quit if I want to, you know, without being, you know, without that being some horrible thing. So --

MELDRUM: Without seeming like a failure.

LIEBESKIND: Yeah. So I guess that's at least the short answer to your question.

MELDRUM: Okay. Seems reasonable. Oh, yes [she laughs], so let me ask you this. What -- well, let's see, you don't have any students these days. Well, let's assume one of your former students comes back for a visit.

LIEBESKIND: Raz [Yirmiya] is upstairs right now, or was this afternoon.

MELDRUM: Okay. This is a person who's sort of just basically starting out with their career --

LIEBESKIND: A hypothetical student.

MELDRUM: A hypothetical student who's interested, who did some work in pain with you, did some work in your type of, in your particular area, stimulation-analgesia, modified -- pain modulation, that's what I want to say. And now has a job somewhere, is about ready to open their lab or something. What kind of advice might you give them? There's a couple things you said that suggest that maybe the field's changing to a great degree, that it's going in different directions.

LIEBESKIND: Well, it is.

MELDRUM: Certainly we see funding is a lot harder to get these days.

LIEBESKIND: Yeah. I don't know that I have anything terribly profound to say about that; I think --

MELDRUM: Well, how do you perceive the field?

LIEBESKIND: I think, like, as a lot of other people do, like a lot of other people see it, I mean, there are forces at work that make things very different today from what they were when I was starting out. So I mean, funding is one of the issues, what kind of work is esteemed today is another issue within my field, I think those are both major concerns. You can still get funding; you have to be more patient for it. It's, if you can, you try to get a job where they can back you up for a while, where some of that, some security gets built in so that you don't just depend on getting an RO1 grant in your first five years or three, four, years, because if you don't, you'll get shot down, you'll never get tenure, you'll be out on the streets. You want certainly to look for a job, if you can find one, that will say, "Yeah, we recognize it's a tough world out there, and we're going to give you at least some small resources to kind of tide you over until you get your grant continued so you can get started and moving along."

And we have a faculty member in our department now who's already gotten tenure, I think he's been tenured now for one or two years. He still hasn't gotten a grant, and he's still running a laboratory and producing stuff. And I don't know whether he's selling drugs on the side [he laughs] or what, because he does this physiological psychology kind of work, not just paper and pencils and volunteers coming in to fill out questionnaires; this is research with rats and, you know, buying equipment and buying supplies and so forth, and somehow he's doing it on a shoestring. So, you know, I think you can get around that grant problem in certain ways, but it's a lot harder.

Now, as far as the other thing, the changes in our field, I mean, any young person has to be current. You can't live in the past in science; you have to live in the present and, hopefully, in the future. And I like to think that the students who came through my lab, doing whatever they were doing here, still were wise enough and bright enough and educated enough to know what else was out there, and to know what of what was out there that represents now and tomorrow, what piece of that they were going to grab hold of so they could go into the future in a modern, up-to-date manner, and I think most of my students have done that; certainly for the most part

they have. I know some that have gone on into this immunological work that we've been doing and that we started here and continued that; that's certainly sophisticated, technically sophisticated and current. Several have gone more molecular, the more molecular neuroscience approaches, made use of those techniques and so forth.

So I think the students who've come out in the last ten years where these changes have been so marked, for the most part have been attuned to that, have adapted to that in the way that they needed to do, even though I couldn't teach them those things myself, they here were open to that, had an opportunity to pursue, whether it would be class work or informal education about some of these things, begin to learn some of those things. And someone like Jeff Mogil, who got out of here two, three years ago, whatever, he's off learning molecular genetics and molecular biology techniques, and when he left here, before he left here, he started learning some of those and knew that that was the direction that he wanted to go. And that was the way that he was going to keep up with what was happening today and what's going to be happening tomorrow. And he said, "I'm going to become a gene jock. I'm going to get out there and clone and get into that thing, but try and keep it related to pain. So I'm not going to lose sight of my foundation in psychology and behavioral neuroscience; I want to keep addressing those questions, but I want to make use of, I want to learn, make use of these techniques, so that I will have as powerful an armamentarium to use to address this as anyone else." And so I think he's certainly a good example of one of my recent students who's very much on top of things out there in the world and doing very well. [Ed: Jeffrey Mogil became E.P. Taylor Professor of Psychology at McGill in 2002, succeeding Ronald Melzack.]

MELDRUM: So would you say --I don't want to put words in your mouth --

LIEBESKIND: Go ahead. Put a word in my mouth. [he laughs]

MELDRUM: Well, it sounds as if you're saying, you know, to be a scientist -- I think this is true; see if you agree -- that to be a successful scientist, on the one hand you have to know something about how to do science, you have to know certainly the skills and the way you approach experiments and approach scientific problems, but you also have to have a very acute idea of how to manage the skills that you have, of how to manage and market whatever knowledge and skills that you have to make them applicable to whatever is going to be funded in the next twenty years.

LIEBESKIND: Well, yeah, I certainly agree; I'm not sure I would phrase it quite that way, but I think that's right.

MELDRUM: You can phrase it some other way.

LIEBESKIND: Well, I just think yes, you have to be very adaptable to what's going on. In order to be successful, you cannot just have a paradigm here in 1971 and, you know, follow this particular technique and go ahead and continue using that specific technique and just keep asking this question and that question and this question, whatever; you have to grow with the field.

Which is strange; I mean, I'd like to really talk to somebody who knows more about these things than I, because it's like, it's a little similar to the question of, "Why can't a composer today compose wonderful motets and music, Baroque-style music? Why does he have to have it sounds like a bunch of tin pans falling on the floor?" Well, you cannot, and you'd just be laughed out of the --you could, I mean, somebody could write music today that would sound a bit like -- you would say, "Well, gee, I'm listening to that; that sounds like -- it's not Mozart because I know Mozart from one end of the Koechel listings to the other; it's not Mozart, but it must be someone very -- somebody who was influenced by him, somebody more, someone who he was influenced by. It has those tones; it has those phrases," and something, and you find out it was Sammy Birnbaum down the street who wrote it [laughs], you know, last year. And you say, "Oh, garbage," you see. Well, it sounded awfully damned good, but we don't accept that.

Well, in the same way, we don't accept science today that makes use of very old techniques and asking kind of old questions, and so the questions go out of style, and approaches to the questions go out of style before they really have been used up, certainly before the questions get answered. We set aside questions; we don't answer them --

MELDRUM: No.

LIEBESKIND: -- altogether, and it would be very hard today to do a, you know, make an experimental lesion in a rat's brain, look at some pain-related behavior before and after, and try to publish that. They'd say, "What? That's old stuff." You say, "Well, no one has put the electrode in quite this place or made quite that lesion, and our finding is original." And they'd say, "Yeah; right." Just like, you know, Sammy Birnbaum wrote that Mozart sonata, you know; it wasn't exactly Mozart, it was new and original, but retro.

So I don't quite understand that, I confess. I mean, I know it's so, but I don't quite understand why it's so. I mean, I don't quite altogether approve that it is so. I somehow believe one ought to be able to write a prelude and fugue, you know, a good prelude, whatever, motet or what have you in the old style, and if it's good, it's good. And you could do a lesion study and you found something new with it and interesting that led you, and so I think you ought to be able to do that. I don't know. So I haven't come to grips with that issue.

MELDRUM: No. It's an interesting issue, though.

LIEBESKIND: It is.

MELDRUM: We should talk about it with Sharon some time.

LIEBESKIND: It's true in all walks of scholarship. It's the same in history, isn't it?

MELDRUM: Pretty much.

LIEBESKIND: There's the modern approach to history; I mean, we're in that transition now, the old is gone, the new isn't completely something, something, but, you know, there are sexy topics in history.



MELDRUM: Right.

LIEBESKIND: Postcolonial this and social that and something and something, and you know, throw in a little feminism or something. [he laughs]

MELDRUM: When I was an undergraduate, I actually did my undergraduate thesis on voting behavior. Voting behavior was very hot then. Nobody does voting behavior now. [she laughs] I couldn't even get my thesis published now.

LIEBESKIND: Apart from the approach, there's the topic, no matter how you approach it, that they're just in today and out tomorrow. Yeah. And it's, one could be resentful about that, and I could be resentful about that, in the sense that in some sense the changes in the field propelled me out. But I'm not resentful of that because I'm fully aware of how much I have benefited earlier in my career by being in the right place at the right time -- you know, latched on, wittingly or unwittingly, to what turned out to be a very sexy topic and some approaches to that topic which were sexy and so forth. And so, I mean, we were part of the endorphin revolution and the peptides revolution and whatever and whatever, and I got lots of mileage out of that, just because we had bought a hot stock early. So who am I to -- you live by the gun, you die by the gun. So at some point along the line, because I stayed with that, in a sense, to some degree --

MELDRUM: Right. You kept your money in the same place.

LIEBESKIND: Yeah.

MELDRUM: Now you get the dividends, but you don't get the growth -- it's not a growth stock any more.

LIEBESKIND: That's it.

MELDRUM: Well, do you have anything else you wanted to add, because for some reason I think this is the right place to stop. [she laughs]

LIEBESKIND: I think that's fine. I guess I just want to say something about having been elected to the National Academy.

MELDRUM: I think we should.

LIEBESKIND: That's a good place to end, since it happened recently, and it is such a capstone or whatever, if you will, of my career. I have a lot of funny thoughts about it that I feel are worth making part of this record. One is, and I think very much most important of all, is a sense of -- I guess what I want to say is it hasn't changed me. [both laugh] I'm not any smarter than I used to be. I don't feel that -- I mean, some people have kindly written, congratulating me and saying, you know, "much deserved, and I don't know why this didn't happen to you twenty years ago," and all that stuff. Well, that's crap.

I mean, it's a wonderful thing that it happened, it denotes the -- it was given to me because of the work of all these people who worked with me. And I know that and they know that and my colleagues know it, and at the same time that I'm fully aware of that and that it's -- you know, I'm not, it's not that I feel I don't deserve it, and I guess I want to say that, too. I think a few years ago if somebody told me, "You're going to get elected to the National Academy in 1995," I would have maybe thought, "I don't know; well, I don't deserve it, and I'm not going to feel good about it because I know I don't deserve it," and something and something. And I don't really feel that. I feel I do deserve it, but I deserve it as the lab head, chief cheerleader, whatever, knowing full well, I think, being very much in touch with what I have done, with what my role has been in my lab and what my role has *not* been. I mean, who did what, and having a full appreciation of the extremely major contributions that these different students --

## JOHN C. LIEBESKIND INTERVIEW

### TAPE FOUR, SIDE TWO

JOHN LIEBESKIND: It just happens that, you know, this particular team had me as a quarterback. It wasn't like we all tried out [Meldrum laughs], and I'm the one who was best qualified to be the quarterback. I came here, I was alone, I started the team, Dave Mayer joined me, and I was a professor and he was a student, so I was the quarterback. And then others came along and we had a team, and I was the quarterback, and the quarterback's the one who gets elected --

MARCIA MELDRUM: To the National Academy.

LIEBESKIND: -- to the National Academy. So, and you know, I have some understanding, not complete, of the politics of that and how a lot of people who might have done what I have done and gotten to where I have gotten, might not have gotten elected to the National Academy, because Don Lindsley wasn't there to push them or the stars weren't aligned quite the right way and something and something; there are a lot of different factors that go into it.

MELDRUM: Right. Always.

LIEBESKIND: So I guess I'm trying to say I feel humble about it and I think very appropriately. And yet not, you know, not badly about it, in the sense that I think somehow this is some freak thing and that I don't deserve it. I think that the work that has come out of my lab merits this kind of award, and who else are you going to give it to? You've got to give it to me. You can't give it to one, just one of the people who was here, so --

MELDRUM: Well, it is your lab. It's true, you were here first, you started the lab, and you were the professor, but you also ran it in such a way over the years --

LIEBESKIND: I ran it in such a way -- this is the important thing -- that so many of these students have gone on to have fine careers of their own, some of whom they will become members of the National Academy or whatever in their turn, who are doing that kind of work today, that very much merits that. I think Dave Mayer and Huda Akil already have done National Academy kind of work on their own since leaving my laboratory. And, you know, there are probably others who are on course for that kind of a thing also. So it's one of those lovely things. It's very meaningful to me. I always, I knew about it, and of course the closer you get to something -- early on in my career, somebody told me, Arnold Scheibel said in a class -- he's here at UCLA; he's a professor of anatomy -- that he in talking, in giving a lecture, I don't know whether he was lecturing on pain or something, mentioned our work, and he referred to it as "virtually of Nobel quality."

MELDRUM: Ooh!

LIEBESKIND: I don't know; maybe that was one of the first times that I said, "Whoa! There are people out there who not only, who kind of think actually the way I think, that it's really God-damned exciting, this story of ours," that there's some outside confirmation of that; and then somewhere along the line Don Lindsley had spoken to me or something, and then once he did, said something about the Academy, boy, boom, the bug was in there and it wouldn't go away. And I would think about it, you know, every so often and Don would come to me and say, "Well, they had the election, you didn't make it this year," or, "They had the preliminary election and you didn't make it to the final list," or something; he would give me a little feedback like that. And he would say, "But we can try again in a year or two," and something. So I would taste it every once in a while; it would just jump into my mind. I would think, "Some day, maybe I can get it." But if I'd had it to bet on, I would have bet against.

MELDRUM: Would you?

LIEBESKIND: I never really believed that it would happen.

MELDRUM: So it was kind of a surprise?

LIEBESKIND: It was definitely a surprise.

MELDRUM: I mean, you seemed surprised at the time.

LIEBESKIND: This year, even this year, was a surprise, and though this year, because I did have more feedback than I should have gotten, I knew that I had made it through a first cut and so forth in a way that I hadn't before, so it became even more, it became more possible this year. It sounded realer, and so it was more on my mind. I would say the last, I don't know, four or five months maybe, until I was notified that I was elected, there probably was not a day that went by that it didn't at some point during the day or other, if not oftener --

MELDRUM: Occur to you.

LIEBESKIND: -- during the day bump into my mind and then come out and become a conscious thought, where, "Gee, I wonder what's happening," and they probably had already held the election a month ago, and of course Don Lindsley, having been retired many years ago, was not a daily presence around the building; I didn't see him, I couldn't exactly, I didn't want to call him up.

MELDRUM: [she laughs] Call him up and say, "Hey!"

LIEBESKIND: "Had any votes lately? When is the vote?" I mean, I didn't even know when the voting was. And so I mean I didn't really know exactly how close things were. And it was a very exciting moment; I don't know if I ever told you, or put on the tape anyway, the way [he laughs] I was notified.

MELDRUM: Well, you called me up. No, tell me again.

LIEBESKIND: Well, they hold their voting early in the morning, 9:00 in the morning or whatever, back in Washington.

MELDRUM: It would be six in California.

LIEBESKIND: And I think around 6:20 in the morning, I was off taking, at that time, anyway, my customary walk around the park, a two-mile circuit, and I was off for my morning walk. And Julia got the call from --

MELDRUM: No, you didn't tell me this.

LIEBESKIND: Yeah, she got a telephone call actually from someone here at UCLA who was not in Washington, but he's a member, and was someone named -- I forget his name now; someone I hadn't, I didn't know and hadn't heard of before. He's an emeritus professor here in the biology department or something. And he was very eager to know who got elected, and I guess he was in phone contact with the people back there, and so he, here in L.A., heard, and immediately called the house. He knew my number; I guess he knew I was, that it was possible, that I was on the list and I had good numbers and was apt to be. So apparently he was poised to call me. And he called me but I wasn't there. So at six he woke up Julia, she was still asleep. [Meldrum laughs] And she and Ben jumped into their bathrobes or whatever and into the car.

MELDRUM: And set out to look for you.

LIEBESKIND: And went out and looked for me, followed my circuit, knowing which way I was going, and came across me, just walking along Pico Boulevard, pulled over, threw the door open, and said something like, "This car is only for members of the National Academy!" [both laugh] or something like that.

MELDRUM: I love that! That's great!

LIEBESKIND: Yeah! And I kind of got emotional, and she said, "You want me to drive you home?" And I said, "No, I'll finish my walk." So she and Ben drove home, and by the time I got there, Jim McGaugh had called and Julia had spoken to him as well. And it just went on from there. It was a Tuesday, I remember, because I had CAP that day, so I went over to my CAP meeting, Council on Academic Personnel, so, anyway, so that was it. But, you know, I mean, it's a great thing, but I don't know how much pride can you take in being a member of an organization where the average age is deceased? That's a joke. [both laugh]

MELDRUM: Yes, I knew it was a joke!

LIEBESKIND: I tell that sometimes. Anyway --

MELDRUM: I'm afraid in that case you're still below average! [she laughs]

LIEBESKIND: That's right -- barely! [he laughs] Well, thank you.

MELDRUM: Well, you have a new career as a scientist -- as a historian. Thank you very much, Dr. Liebeskind.

LIEBESKIND: The interview is over at --

MELDRUM: Five after four.

LIEBESKIND: Five after four, just the right time.

MELDRUM: On the nineteenth of July. Perfect.

**END OF INTERVIEW**