

*John C. Liebeskind History of Pain Collection*

**Oral History Interview  
with  
Ronald Dubner**

Ms. Coll. no. 127.12

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History & Special Collections Division  
Louise M. Darling Biomedical Library, UCLA  
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## Biographical Sketch

Ronald Dubner was born in New York in 1934. He graduated from Columbia College in 1955 and entered Columbia University School of Dentistry, earning the D.D.S. degree in 1958. Following an internship at the Baltimore Public Health Service Hospital, he worked at the National Institutes of Health (NIH) for two years before pursuing graduate research training at the University of Michigan. After receiving his Ph.D in Physiology in 1964, Dubner began a distinguished research career at the National Institute of Dental Research (NIDR). He is perhaps best-known for his studies of pain in awake behaving animals. Dubner was appointed Chief of the Neural Mechanisms Section in 1968 and Chief of the Neurobiology and Anesthesiology Branch in 1973. Since 1996, he has been Chair of Oral and Craniofacial Biological Sciences at the University of Maryland at Baltimore Dental School, where he has developed a new pain research center. A founding member of the International Association for the Study of Pain (IASP) and of the American Pain Society (APS), he was one of the first Section Editors of the journal *Pain* in 1974 and is currently [2001] Editor-in-Chief. He was elected President of the APS in 1987-88, and has twice been Vice-President of the IASP (1981-84 and 1984-87).

## Interview History

Dr. Dubner was interviewed at his home in Castleton, Virginia, by John Liebeskind on August 25, 1994. The interview lasted approximately 4.0 hours. The transcript was audit-edited by Fiona McPherson and Marcia Meldrum and reviewed by Dr. Dubner 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 three (3) 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 with topical digressions, beginning with Dubner's family background, education at Columbia, and postgraduate training under Harold Stanley at NIH and Lester Rutledge at Michigan; his introduction to pain through dentistry; his research career at NIH and sabbatical in 1970-71 with Patrick Wall in London; the growth of Dubner's laboratory and diversity of its work; the Issaquah meeting and his relationship with John Bonica; Dubner's philosophy of research and perspective on pain research in the 1990s; his co-editorship of *Pain*; his marriage and family; and his thoughts about the future. Major topics of interest include Dubner's research work on the association cortex, with the awake behaving monkey, and other projects; his consistent interest in the plasticity of the nervous system; the evolution of NIDR and of his laboratory; his relationship with his associates, in particular Steve Gobel, Gary Bennett, M.A. Ruda, Sammy Zeki, Richard Gracely, Mitchell Max, and Donald Price, and their work; his views on the major achievements in pain research in the 20th century and on the tension between molecular biology and systems neuroscience.

## 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

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

The researcher is referred to the following related materials: oral history interview with Patrick Wall; oral history interviews with Gary Bennett, Raymond Dionne, Richard Gracely, Michael Iadarola, Mitchell Max, M.A. Ruda, and Barry Sessle; American Pain Society Records (Manuscript Collection no. 123); and the Ronald Dubner Papers (in process).

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Ronald Dubner, DDS, PhD

*Neurobiologist*

## RONALD DUBNER INTERVIEW

### TAPE ONE, SIDE ONE

RONALD DUBNER: Oh, my name is Ron Dubner.

JOHN LIEBESKIND: Oh, so you're -- See, I've got this, it's stereo.

DUBNER: It's stereo.

LIEBESKIND: It's stereo, so that's why it just gets the sound here. It minimizes it from elsewhere. Okay, so let's note that it is August 25th, it's 9:30 in the morning, and we're in Virginia.

DUBNER: Castleton, Virginia.

LIEBESKIND: Castleton, C-A-S-T-L-E-T-O-N.

DUBNER: Exactly, T-O-N.

LIEBESKIND: Castleton, Virginia. What? About a hundred miles?

DUBNER: It's seventy miles from Washington, D.C.

LIEBESKIND: Seventy miles.

DUBNER: We're in the foothills of the Shenandoah Mountains, near the Blue Ridge Mountains. We're sitting on the --

LIEBESKIND: Shenandoah is the river. No.

DUBNER: There's a river Shenandoah. The river that goes -- There are a number of rivers here, but this county that we're in is called Rappahannock County, and there is a Rappahannock River.

LIEBESKIND: That's the one that George Washington threw a dollar across so that --

DUBNER: No, it's not the one that he threw the -- Yes, this is the one he threw the dollar across. It's not the one he --

LIEBESKIND: Not the one he crossed in winter. That was the Delaware.

DUBNER: That was the Delaware. This is the one he threw the dollar across, supposedly.

LIEBESKIND: And you threw a few dollars across, it would seem, in order to buy this place.

DUBNER: Right. And there is a -- There is a town that's very close to here, within ten miles, called Washington, Virginia, and that is supposedly the first Washington in this country.

LIEBESKIND: Oh, really?

DUBNER: Because George Washington, as you know, was a surveyor, and this is one of the first places that he surveyed because he lived not too far from here. And so the whole area, he did a lot of surveying. And the streets in Washington, Virginia, are set up and still designed according to the plan he put forward.

LIEBESKIND: Is that really true? Isn't that amazing.

DUBNER: So this is a very historical area. We had a lot of Civil War battles that were fought in this area.

LIEBESKIND: Where is Mount Vernon? That's around here?

DUBNER: Mount Vernon is very close to Washington, D.C. It's just across the Potomac. But this is where the battle of Manassas -- Driving here we went through Manassas, the town of Manassas. Bull Run was on part of our trip. And some of the really famous battles of the Civil War were fought not too far from here. So it's a historical site.

LIEBESKIND: This is an exciting and beautiful place you have. So that's lovely. All right. Let me begin. Let's see. I've already said what the date was and the time.

DUBNER: Yes, what would --

LIEBESKIND: So let's start in. My first question, Ron -- I've got a series of questions here. I've got more questions than you would ever want to try and answer.

DUBNER: You probably have more questions than I have answers, John.

LIEBESKIND: That may be so. But they're just meant as stimuli in case the conversation should lag. But I do want to get some -- just to kind of get started, some biographical things that -- about your background, and family, and I don't know, and early life, and then sort of leading that up into how you -- you know, education and how you got into the field of pain. So when were you born and tell me about your family a little bit and then, you know, go on from there.

DUBNER: Well, I was born in 1934 in Brooklyn, New York.

LIEBESKIND: What was your birth date?

DUBNER: October 12th, 1934. Columbus Day. Which is -- It was always nice to have a holiday on my birthday. In New York City, where I grew up, Columbus Day was always a holiday, so I always had a holiday on my birthday, which was very nice. And I spent all of my childhood growing up in Brooklyn, New York. I went to public schools in Brooklyn, New York.

And then I went to college in New York City. I went to Columbia College, and then from Columbia College, I went to dental school at Columbia.

LIEBESKIND: At Columbia also.

DUBNER: Yeah. And then -- It was at that time that I left New York. I joined the Public Health Service. I graduated dental school in 1958. I had been deferred from the military in order to complete dental school. And after completing dental school, I was under obligation to spend two years in the military. An alternative to that was joining the Public Health Service. The Public Health Service is, as you probably are aware, is a commissioned -- or has commissioned officers and they -- The Public Health Service at that time ran all of the hospitals that provided service to merchant seamen. In addition, the Public Health Service always handled the medical and dental needs of the Indian Health Service and also the Coast Guard.

So they had, at that time, a number of hospitals around the country, and they had internships for physicians as well as dentists. And one alternative for me was not to go into the army, but to serve an internship and then subsequently two years after the internship in the Public Health Service at one of their facilities: hospital, out at the Indian reservation, wherever. And so I applied to both the army and to the Public Health Service, and I actually was awarded both an army internship and a Public Health Service internship.

LIEBESKIND: So you had your choice.

DUBNER: So I had my choice. And I visited -- I came down to Washington, D.C., and I visited the Public Health Service people and I talked about what my interests were. And they assigned me -- And I chose to go into the Public Health Service, and they assigned me to Baltimore. So my first year --

LIEBESKIND: So what year is this, then?

DUBNER: This is 1959. This was the year that the Baltimore Colts won the championship in the National Football League.

LIEBESKIND: With Johnny Unitas.

DUBNER: With Johnny Unitas. And I remember that. That's a vivid memory because Mary Ann and I, we were married when we left New York and we went to live in Baltimore, and we rented an apartment that was not too far from the football stadium. In fact, a number of the football players had apartments in the same development that we had an apartment in. And we could hear the roar of the crowds at the football stadium all the time. Of course, at that time I rooted for the New York Giants football team, and they lost to the Colts so it was a -- it was not the happiest feeling to hear the stadium rocking. But it was a nice year. It was a nice year in Baltimore.

And then during that year I became -- Those of us who were affiliated with the hospital -- I was a dental intern -- we were visited by people from the NIH [National Institutes of Health] who did



research, clinical research, in dentistry. And they came and gave us a couple of talks and, in fact, we made a visit to the NIH. And that's when I realized that there was this tremendous research enterprise, that was part of the Public Health Service, that was present within forty miles of Baltimore. And I initiated a research project right at the hospital there.

LIEBESKIND: You didn't have any graduate training at this point, just the professional dental training.

DUBNER: No. It's interesting. While I was in dental school I had no graduate training; I had just gone through dental school. And in dental school I did not participate in any research activities. There were some people at Columbia who did, but I just didn't -- at that point hadn't been -- didn't have the interest or the time or whatever it was.

LIEBESKIND: You hadn't been exposed to it.

DUBNER: I hadn't been exposed to it, possibly. And so, with this minimal exposure while I was an intern, I initiated a research project, and I got some help from the people at NIH, and I did this project while I was an intern. I think that helped because when it came time to apply for where I wanted to go for the two years, my first choice was to go to NIH. And fortunately, they gave me that opportunity and so --

LIEBESKIND: This was a clinical research project?

DUBNER: This was a clinical research project. It was a project in which I was testing --

LIEBESKIND: Still remember it, huh?

DUBNER: Oh, yes, an antibiotic that was placed into the -- into the socket after you extracted a tooth. The idea was to try to reduce the incidence of dry sockets. Dry sockets are where the blood clot breaks down in the socket, and you get exposure of bone, and it's quite painful. And so we were -- I designed the study to see whether putting an antibiotic would be helpful. It turned out that the study worked completely the other way. Those people who had the antibiotic pellet got more dry sockets than those who didn't.

LIEBESKIND: Well, you learn from your mistakes. [he laughs]

DUBNER: So that was my first exposure to research.

LIEBESKIND: Right. Well, before you go on, because I mean, this is -- We're now at the threshold of your career here. But let me go back a little minute and just get a few facts about your family and so forth.

DUBNER: Okay.

LIEBESKIND: Were you the --

DUBNER: Okay, I had a brother who was six --

LIEBESKIND: Yeah, he died, did he not?

DUBNER: He was six years older than I.

LIEBESKIND: He died a few years ago, I remember.

DUBNER: He died -- It's about eleven years now.

LIEBESKIND: Oh, is it eleven?

DUBNER: And we lived in a small row house. Not -- By "row", I don't mean it was a tenement house. It was a house that was attached on both sides.

LIEBESKIND: Which was sort of the common way of houses --

DUBNER: It was the common houses in that part of the -- New York City. And we lived in a -- It was a small house and I know I shared a bedroom with my brother.

LIEBESKIND: Six years older?

DUBNER: He was six years older, yes.

LIEBESKIND: Just the two of you.

DUBNER: Just the two of us. In fact, there was a time when I was very, very young, that we shared that bedroom with a grandmother who was very aged, and who subsequently, you know, died while I was still very young. But I do remember her.

LIEBESKIND: Bubba.

DUBNER: Bubba, yes. Bubba, that's what we called her, Bubba.

LIEBESKIND: What did your father do? What was his profession?

DUBNER: My father was a salesman, a dress salesman. In fact, my --

LIEBESKIND: He might have sold to my father. My father was --

DUBNER: What was your father?

LIEBESKIND: He had a dress store, you know.

DUBNER: He probably did. My father's territory ran up to Connecticut.

LIEBESKIND: That would be funny if they knew each other. [he laughs]

DUBNER: And my first exposure to the work ethic was helping my father when I was a very young teenager, thirteen, going out with him on the road and helping him, helping him haul his cases, suitcases that carried all the dresses.

LIEBESKIND: He was a real Willy Loman, huh?

DUBNER: Yes, he would haul these into the store, like a store like your father owned, and we'd go into the back of the store and we'd -- I'd open up the --

LIEBESKIND: Display them.

DUBNER: -- and display them in the back -- usually the back area of the -- sometimes, even, we'd display them in the front area of the store where there were customers, and some of the customers would come up and say, "Ooh, that's nice," you know, and that would --

LIEBESKIND: They'd buy it right off the rack.

DUBNER: They couldn't do that, but that did influence the retailer to buy some of those. So that was my first exposure to work. And then subsequent to that I started --

LIEBESKIND: Your mother didn't work? She was a homemaker?

DUBNER: My mother was a homemaker, yeah. She stayed at home and did the cooking, and the cleaning, and took care of us all, and gave us a great deal of affection and love. It was a nice family. It was a very narrowly defined family. None of my family had gone on beyond -- My father -- Both my mother and father were born in what you would now call Russia -- Poland. Or it would be-- Now it would be Poland. And they both came here when they were very young. Actually, my mother was twelve years old and she came across --

LIEBESKIND: Oh, they both came from the Old Country?

DUBNER: Yes. My father was, I guess -- He was a teenager, too, when he came here. My mother was a little younger. She was, I think, twelve, and she came by herself. She came across the Atlantic by herself because they didn't have enough money for them all to travel at once, and so she was the oldest of three children.

LIEBESKIND: So she came first and kind of set things up?

DUBNER: She came first, and then her two brothers and her mother came later.

LIEBESKIND: Yeah.

DUBNER: But she was only twelve and she lived with some relatives. And I think it was a traumatic experience for her. I think it affected her for the rest of her life.

LIEBESKIND: Really?

DUBNER: Yeah. She was always afraid of the dark. She had a certain fear of things and I think it was part of her --

LIEBESKIND: I can imagine. That's such a sensitive age.

DUBNER: Yeah, so -- But she was a very good mother. And --

LIEBESKIND: A good cook?

DUBNER: And she was a very good cook. She made the best gefilte fish that you could ever eat. [he laughs]

LIEBESKIND: And she was proud of her boys.

DUBNER: She was very proud of her sons.

LIEBESKIND: Now, she lived until not so long ago.

DUBNER: She lived, actually, until less than a year ago.

LIEBESKIND: I remember you talking about her.

DUBNER: She died of some type of senile dementia. We don't know whether it was a --

LIEBESKIND: Yeah, well, she was pretty old by then, right?

DUBNER: Yeah, she was 87 when she died.

LIEBESKIND: Your father died many years before?

DUBNER: My father died in '67, when he was 72 years old.

LIEBESKIND: What did your brother do? Was he in business also?

DUBNER: My brother was a lawyer and an accountant, and he had his own business. And so -- And he stayed in New York. He lived in New York and my mother lived only about twenty minutes from where he lived, and so he really was very important to her after my father died. In fact, he was very important to her -- He was the first born child, and there was a -- There clearly was a closeness that my mother had with my brother that -- not that I didn't feel that I was loved. I did feel I was loved, but there was a certain interdependency that she had with my brother that she didn't have with me. And I think that made it a lot easier to leave New York and go somewhere else, whereas my brother stayed in the city, and stayed very close to his parents.

LIEBESKIND: They had him kind of roped in a little more or something.

DUBNER: Yes, I believe so. But then when he died -- A few years after that she came -- When she began to lose some of her memory and some of her thought processes, it became difficult for her to live alone in Brooklyn, so that's when I had -- I moved her down to Washington, and she moved into a residence home about ten minutes from where we live, lived there for about a year, and then things got even worse and she had to sort of go into a nursing home. And that's where she was until she died, a nursing home. But she was comfortable, and she seemed happy and content.

LIEBESKIND: Yes. When you think back on those early days and growing up and so forth, I mean, what can you point to that led you in any way towards dentistry, at least? I mean, you must have obviously had an interest in science. You did well in science in school or something?

DUBNER: Yeah, I was a, you know, a very good student, and I was interested in science, and my parents always said to me, "Don't work for anybody."

LIEBESKIND: Right, be your own boss.

DUBNER: "Choose a profession where you can be your own boss." And that --

LIEBESKIND: It's a very Jewish thing; I heard the same. [he laughs]

DUBNER: It's a very Jewish thing. And that had a strong influence on me, and so I chose a profession, which was dentistry. And one might ask, why did I choose dentistry instead of medicine? Well, I think there were a number of reasons. I think I chose dentistry because I like to work with my hands. I like the artistic aspect of dentistry. So I thought that dentistry was a good --

LIEBESKIND: You were good at carving chalk.

DUBNER: That's right. You know about that. [he laughs] Actually, I was very good at that. In dental school I was very, very proficient at the mechanical aspects of dentistry, the so-called surgical-mechanical aspects. And I enjoyed it. In fact, I continued to do that even after I got my Ph.D. But that's going ahead in the story. So I went to dental school.

LIEBESKIND: Were there any --? Before even dental school -- I'm just trying to slow you down a little bit. I'm interested in how people make career decisions and so on. I mean, like, was there a teacher that you had that you could single out who inspired you?

DUBNER: No, there wasn't any teacher. There was the dentist, the private dentist that I went to.

LIEBESKIND: That you went to.

DUBNER: Right, who -- And I saw what his practice was like and it seemed like a nice -- No, when you're that young, you really don't know very much about life. And so my decision was made based on very flimsy evidence and flimsy knowledge. But it seemed like a nice career and

it seemed like something that combined, as I said, artistic tendencies with science that I had a great deal of interest in. And so I chose to do that. I guess I could have chosen to go into medicine and be a surgeon, but I guess my family background was such that we were sort of preached to that -- you know, don't try to do more than you might be able to accomplish. You know, you might fail if you try to do too much. And at that time it was very difficult to get into medical school, especially for Jewish boys.

LIEBESKIND: Exactly. And Columbia wasn't a very Jewish university to go to, was it?

DUBNER: No, no. And so I opted -- I think that might have been part of the decision making process. I don't regret it. I have no regrets about going to dental school. I think it was a very important part of my life. I think probably the more important part of my life, actually, was going to Columbia College.

LIEBESKIND: Really?

DUBNER: Yeah, because --

LIEBESKIND: It's a really fine college.

DUBNER: Columbia College is a very fine institution. And it exposed me to things that I never knew anything about. I lived in --

LIEBESKIND: Was Eisenhower president then?

DUBNER: Yes, yes, yes.

LIEBESKIND: What did it expose you to?

DUBNER: It exposed me to a world out there that I didn't know anything about it, as I said.

LIEBESKIND: Did you live at Columbia or did you commute?

DUBNER: I lived -- No, I commuted, actually. But you know, my parents -- As I said, they were not -- didn't have, you know, educational backgrounds and so I was not -- you know, I wasn't exposed to art, or to music, or to literature. My mother read a great deal, but she read things from the Book of the Month Club. I went to a very good high school, but certainly high school is somewhat limiting. So Columbia College opened my eyes to a western civilization, an artistic background -- background in the arts, and music, too, that I knew very little about.

LIEBESKIND: Did you take classes in some of these subjects?

DUBNER: Oh, yes, yes. You see, at that time -- At that time there were major requirements that you had to fulfill --

LIEBESKIND: General education.

DUBNER: -- in a general education. I didn't major in a science. My major was liberal arts.

LIEBESKIND: Really? You could have a major called liberal arts and it didn't focus you on one--

DUBNER: Yes, it didn't focus in one area.

LIEBESKIND: You could just sample.

DUBNER: In fact, if I remember correctly, you couldn't even take a major other than in liberal arts. You didn't get a degree, you know, a B.A. in let's say -- or a B.S. You got a B.A. in liberal arts, not in chemistry, or physics, or biology. I believe that was the case. But you could take much more science than I did. I didn't take -- I took the minimal amount of science.

LIEBESKIND: The prerequisite courses.

DUBNER: The prerequisite courses. And I took lots of other things.

LIEBESKIND: What was your favorite? Did you have a favorite subject in college particularly, or a favorite class?

DUBNER: Well, my favorite subject was probably chemistry. I really loved chemistry.

LIEBESKIND: Is that right?

DUBNER: I loved chemistry. But outside of the sciences, my favorite subjects were studying western civilization and western history. I enjoyed that a great deal. So that was a --

LIEBESKIND: To what extent was Columbia a commuter school? I know a lot of --

DUBNER: Columbia was a major commuter school. I would say about 40 percent of the -- at that time, about 40 percent of the students commuted. I don't know if it's that many today, but at that time it was that many. Well, maybe there were 40 percent from the New York area and maybe two thirds of those people commuted. I'm not sure of these numbers anymore.

LIEBESKIND: Sure, sure. But you didn't feel different, or -- I mean, you weren't singled out by being a commuter?

DUBNER: I wasn't singled out because I was part of a major group of people who did that. And I spent a lot of time. I used to -- I wasn't a commuter who got there at 8:30 and left at 1:00, you know, and came home. I was in the band, I --

LIEBESKIND: Really? What did you play?

DUBNER: In the marching band I played saxophone and clarinet. So I played in the marching band during the football season.

LIEBESKIND: I didn't know that. Do you still play?

DUBNER: No, I don't, unfortunately. I stopped playing when I got into dental school. I just didn't have the time.

LIEBESKIND: The marching band -- That's -- [he laughs]

DUBNER: I regret that, yeah. And also the -- there was a concert band and I also played in a concert band throughout the year. So we would play at the football games, occasionally at basketball games, and we would go on tour during the concert season and do things. So it was --

LIEBESKIND: Did you go to these -- to other colleges for the games?

DUBNER: Oh, yes. I remember going to Annapolis. Of course, at that time Columbia played Army and played Navy. One year we went to -- What's that?

LIEBESKIND: Poor you. You probably got whipped all the time.

DUBNER: Yeah, all the time. One year we tied Army, I believe, when I was there. So I went to West Point, I went to the University of Pennsylvania, I went to the Navy Academy, went to all the other Ivy League schools: Princeton, Yale. So I had -- It was very nice and I enjoyed that. So I used to be there all day. I wouldn't get home until -- I would work in the library, I wouldn't come home. Often I wouldn't even come home to dinner.

LIEBESKIND: Yeah, you were part of college life.

DUBNER: Yeah, I did really focus on that and participate in that.

LIEBESKIND: And at that time you knew that you were going into dentistry? Was your decision already well made by then?

DUBNER: Oh, yes. The decision to go into dental school was made in high school. And one of the reasons I went to Columbia is because I convinced my parents that my best chance of getting into dental school was to go to Columbia rather than to go to Brooklyn College or -- See, in fact I convinced them that I needed to go to Columbia or to NYU. NYU, New York University, at that time had a sort of -- it was like an honors program. And so that was the other school that I applied to, and I applied to Brooklyn College. And I got into all the schools, and I decided I wanted to go to Columbia.

LIEBESKIND: Was Columbia a financial stretch for your family because that was the one real private school?

DUBNER: Yes, that was the problem. Well, New York University was also a private school.



LIEBESKIND: Oh, that's right.

DUBNER: But they --

LIEBESKIND: They went for it.

DUBNER: I sold them a bill of goods.

LIEBESKIND: Your brother was, by that time, probably out earning a living, right? So he was off the dole by then, being six years older.

DUBNER: That's right, that's right. And he went to City College. So he -- It wasn't expensive for him to go to school. And so they agreed to send me to Columbia. That was the best decision they ever made, I think. My father probably never agreed with that. He never really understood why it was important to go to Columbia, but he went ahead with it.

LIEBESKIND: Well, he lived long enough --

DUBNER: He also never understood why I never practiced dentistry, why I didn't open an office somewhere. "When are you going to open an office?" And he would also --

LIEBESKIND: Well, he lived long enough to see you succeed already in science, right?

DUBNER: Yeah, yeah, you know, but I don't think he really -- he ever really appreciated why I was doing this, why I was working for the government, had a boss when I could be out running my own business.

LIEBESKIND: That's right.

DUBNER: I guess it was a dream of his own which he never was able to fulfill, was to have his own business. So even he would corner Mary Ann -- my wife, Mary Ann. He would corner her sometimes after we were married, and he'd say, after we were living in Bethesda in the Washington, D.C., Bethesda area, and he would corner her and he would say, "Why doesn't Ron want to open up a private practice?" [he laughs] And she would give him some answer. She had lots of answers that she would give him, but I don't think they ever really placated him because he kept asking her again and again.

LIEBESKIND: When did you and Mary Ann get married? That's early then, huh?

DUBNER: Yeah, we got married when I graduated dental school.

LIEBESKIND: Oh, from dental school.

DUBNER: Yeah, see, I met her -- She was at college at Barnard and we met while I was in dental school and she was at Barnard. And so --

LIEBESKIND: So you went right from college to dental school?

DUBNER: Right out of -- In fact, I didn't -- I didn't take my senior year in college. I went to dental school. At that time they had a program where you could combine college and dental school or medical school in seven years, and so --

LIEBESKIND: And you entered knowing that?

DUBNER: I entered knowing.

LIEBESKIND: You entered college knowing that you would do a seven year -- ?

DUBNER: I entered college knowing that I would do that because I felt that I was costing my parents a good deal of change anyway, and I wanted to do it as quickly as possible. If I had to do it again I would have stayed the extra year. You make decisions then, that you think about later on, you might have changed. So I went to dental school in 1954, graduated in '58.

LIEBESKIND: Sounds like you enjoyed it, the dental school.

DUBNER: Yeah, I-- Dental school is not an easy time for most people. It was difficult, but I was talented and I didn't have any trouble at school. I was talented in the -- A lot of people go to dental school and do not realize how much is required of them in terms of working with their hands and doing mechanical things. And they have a lot of trouble developing those skills. And then also at Columbia, the dental school, a lot of our courses were taken with the medical students, and so we had a very, very strong basic science component to our training.

LIEBESKIND: It's probably stood you in good stead.

DUBNER: And that -- I was good at that, too, and so I didn't have any troubles there.

LIEBESKIND: What was the difficulty? You say you didn't have any troubles in school, and yet it was a difficult time. What -- ? Were students uptight or -- ?

DUBNER: The students were very uptight because either they had trouble in the sciences or they had trouble in the mechanical aspects of dentistry. And it was stressful. And also they ran it in a very rigid way. Dental school was very rigid. At that time the program was very rigid. You'd sit next to the same people all the time, you know, in your classes, and you could sense their stress and you'd sense the competitiveness. And then there were people who by the end of the sophomore year just didn't hack it and had to leave or had to repeat the second year.

The second year was exceptionally difficult, because in the second year you were doing a lot of the difficult sciences. I can remember one of the courses that threw people was a course in pharmacology that was taken with the medical students. It was a very difficult course. By that time the medical students had taken things like physical medicine. They had taken other courses that sort of rounded out their education a little more than ours. While they were taking physical

medicine and -- or physical diagnosis, we were taking carving a tooth, you see. Operative dentistry at the non-clinical level. And then we were all --

LIEBESKIND: Which didn't help you in pharmacology.

DUBNER: Didn't help in pharmacology and we were thrown into this environment, and it was not easy for people. And then -- And so I remember the second year as being a very stressful time, because I was in that environment and I was part of it. I lived up at school that year. That was the one year that I lived away from home because we had --

LIEBESKIND: Did they have dormitories?

DUBNER: No, there were no dormitories available to the dental students.

LIEBESKIND: Did you have an apartment?

DUBNER: You had an apartment, which I shared with a classmate. And so it was a -- And so I did that because, again, I felt that this was going to be a very difficult year. We knew from previous, older -- from upperclassmen, that the second year was really the year that really made you or broke you. And so I wanted to have as much time up there as I could, so I decided to live up there. So that's the dental school experience, and as I said, it was purely academic in terms of course work, and clinical in terms of the practice of dentistry.

LIEBESKIND: But you had enough time to meet, and court and marry Mary Ann.

DUBNER: Yes, yes. Yes, we met --

LIEBESKIND: So you weren't that much of a bookworm.

DUBNER: No, I think I was -- Well, I think I was blessed. I was able to accomplish these things with some ease. It wasn't something -- I had to study, but I could study and do well, so I wasn't -- It wasn't a concern that I had that I was going to pass or fail.

LIEBESKIND: So at this time, certainly, you were thinking that you would end up in clinical practice and that was your --

DUBNER: Well, as I -- Yeah, but as I approached graduation, I did feel that I wanted to remain in academic dentistry, that I would not do private practice full time, that I wanted to teach.

LIEBESKIND: Really?

DUBNER: I hadn't thought much about research at that time, but that I wanted to become a specialist, get -- have a specialty in dentistry, and the specialty that I was interested actually in was prosthodontics, which is very mechanical, making crowns, and making bridges, and making dentures. And that's what I thought that I would ultimately do. That I would do graduate training--

LIEBESKIND: Have an appointment at the --

DUBNER: -- have an appointment at a dental school, and have a private practice part-time. That was my early vision.

LIEBESKIND: Now, where did that come from? Now, I understand the prosthodontics because that sort of comes out of your interests in the mechanical aspects of dentistry. But, wherefore the academic appointment? Where did you get that idea? What was there about that --

DUBNER: Well, because I felt that -- By that time I realized that just having a private practice by myself would be very isolating, and it could become difficult for me to be happy in that kind of a situation.

LIEBESKIND: Well, plus, you must have had some sense that you --

DUBNER: See, I was never somebody who was an entrepreneur, see, that I looked upon a dental practice as a way of developing a business, you see, and so my interests were more in the science aspects and the artistic tendencies and components of the profession.

LIEBESKIND: Some people go into dentistry because they want to make a lot of money and that's their main objective, but that was clearly not yours.

DUBNER: No, no. And actually they were wrong. It's very difficult to make a lot of money in dentistry, even then it was difficult. But so all of that meant that I wanted to be associated with an academic environment.

LIEBESKIND: Yeah, but you must have had a sense that you were doing well and this was coming easily for you.

DUBNER: Yes, exactly, yeah.

LIEBESKIND: That you were talented, and this would be a way --

DUBNER: Yeah, I was. I was talented in it. Actually, I graduated first in my class.

LIEBESKIND: Is that right? That's quite an honor.

DUBNER: It was a small class [40 students], but still, it was a nice accomplishment. And so -- Especially since there were -- There were a number of people who were very talented in the mechanical aspects of dentistry. There were a number of individuals who had been dental technicians, and so they suffered through the science courses, but once we got into the third year, they just zoomed. But I developed that talent very well. And in fact, as I said, that's what I wanted to do. And so then, as I mentioned before, I went off to Baltimore as an intern.

LIEBESKIND: All right, so you were being -- You were being assisted materially, financially, by the Service?

DUBNER: No, no. No, it was all paid for by my parents and a little bit of money I earned in the summers.

LIEBESKIND: But at the end of that, you had --

DUBNER: I had an obligation to go into the military because I had been deferred.

LIEBESKIND: Oh, that's right. It was the deferment. I see, right.

DUBNER: At that time there was a draft, so I had a draft deferment to finish dental school. And then once I finished dental school --

LIEBESKIND: -- that obligated you.

DUBNER: I was obligated to go into the military or into the Public Health Service.

LIEBESKIND: So that's -- You went into the Public Health Service and --

DUBNER: Well, I wasn't -- I could have interned anywhere for a year. They allowed you to intern for a year. Like, I applied to the V.A. Hospital in Brooklyn as a possible internship. But once you completed the internship, whether you took it in the military or out of the military, you had to serve two years. The advantage, of course, of going -- interning in the military is you made a lot more money. At that time you made \$6,000.

LIEBESKIND: Big bucks.

DUBNER: You made a lot of money as an intern. Of course, if you interned at the V.A. Hospital, you made \$2,400, something like that. So there was a great advantage to going into the Public Health Service or going into the army. You got a commission and you made your \$6,000. So that's what I did. As I said, I went to Baltimore.

LIEBESKIND: So Lieutenant Dubner reports for duty in Baltimore. And that was a good year? You did your internship and you got involved with -- in this research project.

DUBNER: That was a good year. It was our first year of marriage.

LIEBESKIND: First year of marriage, the Colts won.

DUBNER: That's right. We set up our household. It was fun. Mary Ann did some teaching. She got a teaching job in the steel mill area of Baltimore.

LIEBESKIND: Is that right?

DUBNER: Yeah. And we had a very nice year. And then we went to the Washington, D.C. area and I started at NIH.

LIEBESKIND: And so that was for two years there, and to do your obligation.

DUBNER: Well, that was supposed to be for two years to fulfill my military obligation. And I got more and more involved in research.

LIEBESKIND: So let's hear about this, now. What was your assignment at the NIH?

DUBNER: My assignment at the NIH, I was a clinical dentist. I was assigned to the Clinical Center. The Clinical Center is the clinical arm which, then as now, provides the services for patients. And also is the component that does clinical research. And at that time there was a dental department that was part of the Clinical Center. It was not part of NIDR [National Institute of Dental Research]. It was part of the Clinical Center. And it did services for the other institutes, the other clinics, in the rest of the hospital. And so that's what my assignment was. So I took care of patients who had dental problems.

LIEBESKIND: So you practiced dentistry.

DUBNER: I practiced dentistry for part of the time because I got interested in research and I was located exactly where the Dental Research Institute was, at NIDR. And so I immediately got involved in doing research. I worked with a neuropathologist, Harold Stanley. And the first paper I ever published was with Harold Stanley.

LIEBESKIND: Really? From that time, huh?

DUBNER: Yes. It was on -- It was published in the Triple-O journal. I guess that's the *Oral Medicine, Oral Surgery, Oral Pathology* journal. It still exists. It's rather a low-brow journal. Oh, I shouldn't say that. It's not one of the most reputable dental journals today. But that -- But then it was a decent journal. And I did a study on the effects of gutta percha.

LIEBESKIND: What is that? I've never heard of that. [he laughs]

DUBNER: Gutta percha is a temporary filling material that you put in the tooth.

LIEBESKIND: How do you spell that, for the record?

DUBNER: G-U-T-T-A P-E-R-C-H-A.

LIEBESKIND: Just like it sounds.

DUBNER: When we were in dental school we used to do little artistic drawings of gutter percha. It would be somebody sitting on the curb with his feet in the street, perched in the gutter. [both laugh] But gutta percha was a temporary material that was hard, and you heated it up to soften it. Then you stuffed it into the tooth after you removed the caries, if you weren't able to put in the final, the permanent filling material at that visit. And I felt, and so did Harold Stanley, that this stuff was awful. That one, that you were putting something hot up against the -- near

the tooth pulp and you're probably damaging the tooth pulp, and also it had very poor margins, and so it would leak terribly, and so you know, after a couple of days the patients would have a lot of leakage under this and that would have effects on the tooth pulp. So we decided to show the world that gutta percha shouldn't be used, and see what it does to the pathology of the tooth pulp. And sure enough, it caused havoc to the tooth pulp and we published the paper. That was my first paper.

And so-- As I said, so during that time I got interested in research. That was a project that I did. But what I really got involved in was developing my own research program there, which was in temporomandibular joint pain and dysfunction.

LIEBESKIND: Oh, really?

DUBNER: That was an area that I got interested in dental school.

LIEBESKIND: So the p-word comes into this interview.

DUBNER: Pain comes in very early because -- because that's what I decided to do a research project on. That came about because I was interested in temporomandibular joint dysfunction through a small -- a very short course we took in dental school, given by Laszlo Schwartz. Laszlo Schwartz is probably -- Laszlo Schwartz is the dental -- I should say the dental parallel to Janet Travell. He, at that time, in the late fifties -- he was taken by Janet Travell and her approaches in treating myofascial pain. And he adapted them to the oral-facial region, and he wrote a number of papers. And he gave this course at Columbia on treating TMD [temporomandibular disorder] with exercise, with ethyl chloride spray, training people in physical medicine, exercise techniques.

LIEBESKIND: Spread and stretch, or whatever that was.

DUBNER: That's right. And so when I had the opportunity to develop a research project, this is what I thought of doing. And also, I was -- It also involved an understanding of the masticatory apparatus. I mean, one of my favorite subjects in dental school was physiology. And neurophysiology. And so I started this research project. Nothing -- I did complete the project. It went nicely. It was actually a nice design.

LIEBESKIND: Did you have your own laboratory? Were you working with someone else?

DUBNER: No, no. I did this with free time that I had between the service work that I did as part of the clinic and --

LIEBESKIND: This wasn't in any sense a requirement of your job? It wasn't in your job description?

DUBNER: Absolutely not, no.

LIEBESKIND: This was your interest.

DUBNER: This was something extra that I did, my interest. And today it would have been -- I guess it would have been a poorly designed study, but at that time I knew very little. But what I did was, I decided that I would treat people first with a muscle relaxant or a placebo. So it was a double blind study. Okay, I did a double blind study, and I had the pharmacy at NIH make up these placebo pills that looked exactly like the regular pills. So it was a true double blind study. And then after the patients went through this double blind study for about four weeks, if they still were in pain, I would either assign them to either an occlusal adjustment group or into a muscle exercise group. And I would -- So that was the study. It turned out that everybody got better, no matter what I did to them. [he laughs] I probably should have stayed in clinical dentistry.

LIEBESKIND: You were good at it.

DUBNER: I was pretty good at it. [he laughs]

LIEBESKIND: Just hanging around, you got better.

DUBNER: Everybody got better. So I put all the data in a closet somewhere and never published it. At that time it was not a big field. It was an almost unknown field of research in dentistry. Subsequently -- This was before people like Chuck Green and --

LIEBESKIND: Danny Laskin.

DUBNER: That's right. And Laskin got involved.

LIEBESKIND: So you were a pioneer in TMD.

DUBNER: I was an unknown pioneer, because none of -- nothing -- none of the work I did ever saw the light of day at that time.

LIEBESKIND: But it introduced you --

DUBNER: But it introduced -- And what it really did, was it introduced me to pain, and it also got me very interested in getting further training and getting a Ph.D. in physiology, in neurophysiology. And so as my two years were drawing to a close, I decided to apply to go for a Ph.D. And one way of doing that was to do it -- to stay in the Public Health Service. And at that time they were sending individuals off for training. You keep your commission, you go off somewhere for training, you continue to make your nice salary as a commissioned officer, and you get your Ph.D.

LIEBESKIND: So what's wrong? That sounds pretty good.

DUBNER: So it sounded pretty good to me. But they didn't want me -- They didn't want to do it right away. They said, "Stay around another couple of years and then we'll send you. We want you to develop your research, or stay around here and get more involved with what we're doing here." And I said, "Well, I can't do that because I don't have the background to really do



this well. And this is what I want to do, and I'm going to do it. I'm going to apply and get a postdoctoral fellowship. And I'm gonna go as a postdoctoral fellow." So I applied to Michigan and got accepted to Michigan. And I applied for a grant. And while I was waiting to hear about the grant, they said, "Okay, we'll send you." So they decided to send me.

LIEBESKIND: Ah, I see.

DUBNER: So I went.

LIEBESKIND: So you did maintain your commission.

DUBNER: I maintained my commission, so I was a commissioned officer for the four years of -  
-

LIEBESKIND: So instead of going as a post-doctoral fellow, you went as a doctoral student.

DUBNER: As a -- I went as a doctoral student, but I went as a commissioned officer. So it was very nice. It worked out very, very well.

LIEBESKIND: Did they give you a set amount of time? Were you limited in how --

DUBNER: No, no. Actually, I did my Ph.D. in a little over three years.

LIEBESKIND: Is that right? Whoa!

DUBNER: I went in August or September, and I finished up actually in -- sometime in October, I defended. But I didn't tell anybody because I wanted to stay until the end of the football season. [he laughs] See, I went to college, John. I went to Columbia College, and I played in the band, and I went to all the football games. You know, Ivy League football was just that, Ivy League football. It was fun.

LIEBESKIND: And Columbia was the doormat of the league.

DUBNER: Columbia was the doormat. So when I went to Michigan, the -- as part of your tuition, you got free tickets to go to the football games, so why not? And then I think for a very modest sum I could buy an extra ticket for Mary Ann. And we lived in an area called Pittsfield Village, which had been built after the Second World War to house all of the GIs coming back on the GI Bill, who were married or were getting married.

LIEBESKIND: Is that west of Ann Arbor?

DUBNER: That's right. West of Ann Arbor. So we lived there where -- At the time we were there it had been taken over by graduate students and medical students, or graduate medical -- or physicians who were in graduate training, because they had families and were married and so on.

LIEBESKIND: What year is this again, now?

DUBNER: This is 1961.

LIEBESKIND: Yeah, that's when you arrived.

DUBNER: Yeah. So we lived there. And there was a group of us that we would go to the football games. They said, "Well, come on!" Those who had been there before said, "Get your football tickets. We have -- We go to the games. We get babysitters that -- We get a babysitter or two to take care of all our children."

## RONALD DUBNER INTERVIEW

### TAPE ONE, SIDE TWO

LIEBESKIND: Are we on? There we go. Okay, so you got babysitters for the -- for Susan.

DUBNER: So we got babysitters and we'd all leave early. We'd leave before lunch, by 11:30, and everyone was assigned to bring a part of the lunch. Some one was assigned to make the drinks. And we'd have a tailgate party. And each week someone else would --

LIEBESKIND: You and your neighbors.

DUBNER: Each week someone else brought their station wagon and we'd have a tailgate party on the station wagon. And then we'd go in and see the football game. So the first time I walked into that stadium --

LIEBESKIND: A hundred and one thousand and one.

DUBNER: A hundred and one thousand people screaming and shouting, and the colors, the beautiful colors! And then this magnificent band goes out on the field!

LIEBESKIND: Right, all male.

DUBNER: All male band that marches across this field, you know, in magnificent unison. I was just so impressed! [both laugh]

LIEBESKIND: Right!

DUBNER: And then the football game started and, you know, it was like seeing pro football. So --

LIEBESKIND: And Michigan won more than its share of games.

DUBNER: And Michigan won. They won quite a few in those days. And so we went to all the football games. And so when it was time to leave, we decided that we couldn't leave until the football season ended.

Of course, the other thing was, that was the -- an election year. This was 1964. And if you recall the election year of 1964, it was Lyndon [B.] Johnson versus Barry Goldwater. And so being part of a liberal college community, there was very strong sentiment, of course, for Lyndon Johnson at this time. And we were all involved in the idea of the election, what had been proposed by Johnson, and what he had already accomplished. And so I didn't want to leave until after the election, either, because I wanted to be able to vote. And so that's what we did. We stayed till the end of the football season, we voted in the election, and then we came back to Maryland.

LIEBESKIND: That's a very short time for a doctorate. I mean, even though you have a DDS at that point, I mean, it's not like -- I mean, you know, you were in neurophysiology. Let's, you know, let's talk a little bit about what you did do in graduate school.

DUBNER: Well, let's back off a little bit. You see, I went to Michigan -- I chose Michigan because at that time it had a very strong, what I thought was a strong group of people doing work in neuromuscular mechanisms related to the head and neck. And so I wanted to be --

LIEBESKIND: Who was that?

DUBNER: There was a fellow -- They were clinical people. There was a fellow by the name of [Sig] Ramfjord, and then Jim Avery was involved in doing work, I think, at that time on the anatomy of tooth pulp. There were some other people who -- There seemed to be a strong science component to the dental school. And so -- And in addition, Bob Doty -- I don't know if you know Bob Doty.

LIEBESKIND: Right, I know him very well.

DUBNER: Bob Doty -- That's right, you were there --

LIEBESKIND: I was in Michigan.

DUBNER: I forget for the moment. You were at Michigan, so you know all these people.

LIEBESKIND: We were there at the same time.

DUBNER: We were there at the same time and didn't know each other. So I was going mainly because I would be able to do my Ph.D. with Bob Doty, okay, who had an interest in swallowing and in neuromuscular mechanisms in the -- brainstem mechanisms, and who also did a lot of work on vision. So that's what -- And at the same time I felt that I could interact with the dental school people because they had this strong group of dental school people there.

LIEBESKIND: You were in the physiology department.

DUBNER: And at that time Art Storey was already there. Art Storey had gone through the clinical programs there and he was in the Ph.D. program there, so I had met Art Storey. So there was a strong reason for me to go to Michigan. Well, about two months or so, three months before I'm ready to go to Michigan, Bob Doty calls me and tells me, "I've taken a position at Rochester."

LIEBESKIND: He was going to Rochester.

DUBNER: "And I have a spot for you, Dubner. You can come with me. Okay? I have a spot for you and it'll be no problem." So I had to think about whether I'd go to Rochester or to go to Michigan. And I probably should have gone to Rochester, because the person I would really

learn from would be Bob Doty. And this part of my training wouldn't be available. And this was the major thing I was going to be doing.

LIEBESKIND: But the football team at Rochester wasn't very good, so --

DUBNER: No, I didn't know about football then. This was before I went there.

LIEBESKIND: I'm teasing.

DUBNER: But I made a decision based on that I wanted -- There was nobody at Rochester in the graduate dental school who cared anything about the things I was interested in. So I made the decision on that basis, okay? And I figured, well, I'll -- Michigan's a good place. I'll get some good training and I'll still be able to interact with the dental school.

As it turned out, I didn't do -- I did very little interacting with the dental school, and I did my thesis under Les Rutledge's supervision, and Les was interested in association cortex and convergence of inputs in association cortex. And Les did one experiment with me, and that was the last I saw of him in the lab, and I did my thesis. I developed a project related to that and I --

LIEBESKIND: What was your project?

DUBNER: My project -- I ultimately did both extracellular and intracellular recording in the cerebral cortex in this -- what we now know to be, I guess, area five.

LIEBESKIND: S-2 or something?

DUBNER: No, it's area five. It's -- I guess that's what you --

LIEBESKIND: This is in monkey or -- ?

DUBNER: No, it's area seven. What am I talking about? It's area seven.

LIEBESKIND: This is in primate or in cat?

DUBNER: No, this is in cat. This is equivalent to the area seven that was -- that a lot of work that [Vernon] Mountcastle did later on in the awake monkey, I did under chloralose in the anesthetized cat.

And you know, at that time there had been some initial papers that Purpura had published in the cortex and there -- He had done a little bit of intracellular recording at the time.

LIEBESKIND: This is all on sensory convergence?

DUBNER: It's all on sensory convergence.

LIEBESKIND: Somatosensory inputs?

DUBNER: Somatosensory inputs into association areas of the cerebral cortex. So I studied the properties of these neurons and I basically --

LIEBESKIND: You didn't get to meet Steve Fox, did you, at the -- ?

DUBNER: Sure I met Steve Fox there. Sure, I knew Steve Fox.

LIEBESKIND: Is that right?

DUBNER: Of course.

LIEBESKIND: He was my thesis chairman.

DUBNER: He was your -- Yeah, yeah. He and Rutledge didn't get along too well, but --

LIEBESKIND: No, Steve didn't get along with anybody too well. [he laughs]

DUBNER: But we did have some interaction --

LIEBESKIND: I don't think Rutledge got along with anybody too well, either.

DUBNER: Well, he was a --

LIEBESKIND: He was kind of a cold fish.

DUBNER: He was a cold fish, but he was a -- Look at the birds now and see the -- They're now coming to the feeder.

LIEBESKIND: Oh, that's bright yellow!

DUBNER: These are yellow finches; they're lovely birds.

LIEBESKIND: Bright yellow. It's so gorgeous here.

DUBNER: If we keep looking we may see the cardinal -- family of cardinals. And --

LIEBESKIND: But you didn't see much of Rutledge, huh? He just sort of turned you loose?

DUBNER: Well, I saw him, but he didn't really do much in the lab. He let me sort of -- As you said, he turned me loose, let me work on my own.

LIEBESKIND: You didn't have any training in neurophysiology and the techniques. Did he at least show you what buttons to push?

DUBNER: Well, he showed me the techniques, and then the rest I picked up on my own, also from --

LIEBESKIND: Were there other students there?

DUBNER: -- other students there who helped me. You know, we learned from each other.

LIEBESKIND: Did you work closely with any of the other people there?

DUBNER: There was a fellow by the name of Keith Bignall.

LIEBESKIND: Keith Bignall. God, I know him well.

DUBNER: Did you know him?

LIEBESKIND: I knew him in Paris on the post doc. He came to Paris and worked with Busser.

DUBNER: Right, he came to Paris. He worked with Busser, exactly.

LIEBESKIND: Isn't that funny? He was a technician first.

DUBNER: He was first a technician. He was a very good person. Unfortunately, he died at a young age.

LIEBESKIND: Did he?

DUBNER: Yeah, yeah.

LIEBESKIND: I wonder if I knew that. I lost all track of him.

DUBNER: He had problems with alcohol.

LIEBESKIND: Yeah.

DUBNER: And this great guy, bright guy -- He was a -- He was doing his research right next to me, and he was doing all of these lesions and stimulation studies--

LIEBESKIND: Could he help you with your work, then, if you had questions in your early days?

DUBNER: Sure, sure. He was very helpful. Yeah, Keith was really good.

LIEBESKIND: He had been a technician for a while.

DUBNER: Yeah. We had -- We spent a lot of time talking about the area and questions that needed to be addressed.

LIEBESKIND: Did you meet “the wife?” I always remember he never referred to her by name. He always called her “the wife.”

DUBNER: Yeah, I met her. She was actually a lovely person.

LIEBESKIND: Yes, she was.

DUBNER: She was a very lovely woman. And so that -- So that was my introduction to neurophysiology.

LIEBESKIND: Three years. Jeez, that’s quick.

DUBNER: Well, it was a little more than three years, but not much.

LIEBESKIND: Well, a few months more, but I mean, that’s quick.

DUBNER: Yeah, I did it fairly quickly.

LIEBESKIND: Do you know Ralph Gerard there?

DUBNER: Sure, I knew Gerard and then there was a fellow, Chamberlain, who worked -- Did you know Chamberlain?

LIEBESKIND: Yeah.

DUBNER: Yeah, he did his thesis with Gerard and he was in the physiology department. He defended his thesis while I was there. And Art Storey defended his thesis while I was there. I liked that department. There were some really nice -- It was a nice department at the time. Of course -- Oh, gosh. Remind me of the chairman. You know the chairman of the department, the big six foot seven --

LIEBESKIND: Davenport --

DUBNER: Davenport, kidney --

LIEBESKIND: Horace W. Davenport.

DUBNER: Horace W. Fascinating.

LIEBESKIND: The ABC of acid-based chemistry.

DUBNER: That’s right. Horace was just a spectacular person.

LIEBESKIND: I didn’t get to know him at all, but I heard his lectures.



DUBNER: He was a spectacular person. He was such a -- He was like this, you know, big, jaunty, loud person that would walk down -- come down the hall and, you know, you could just hear him coming down the hall.

LIEBESKIND: Well, the anatomy department was pretty famous, too, with Ma Crosby [Elizabeth Caroline Crosby].

DUBNER: Yeah, but she wasn't involved when I was there very much.

LIEBESKIND: Really? She was already pretty old.

DUBNER: The person who was -- what was his name?

LIEBESKIND: Charlie something.

DUBNER: Charlie Lauer. I think that was his first name, Charlie.

LIEBESKIND: Wasn't it?

DUBNER: I believe so. He was very good. He taught me. I really learned anatomy there. I knew my anatomy cold in that place. And of course the physiology department was spectacular. We had people in different areas like --

LIEBESKIND: -- Malvin.

DUBNER: -- Dick Malvin. Dick Malvin, who was a --

LIEBESKIND: -- kidney guy.

DUBNER: -- kidney physiologist. Art Vander, who was also kidney, who was very, very bright. I got to know him very well.

After my first year there they treated me almost like I was a junior faculty person. They were very nice to me. I mean, I had a dental degree, and I was -- you know, I knew the stuff. They could tell, you know, that I was really into it and so they treated me very well. So I wasn't -- I didn't feel like I was a real graduate student. So it was a good experience.

LIEBESKIND: It was a pleasant place to live for a few years, anyway.

DUBNER: In fact, I would have stayed there if they would have offered me something worthwhile. They offered me the job of teaching the dental students, and I said, "I don't want to be a second class citizen here. I mean, I don't want to be teaching the dental students. I want to be just like anybody else in the department. We'll all share in teaching the medical students and the dental students. I don't want there to be someone who's in charge and responsible for teaching the dental students. I don't want to be a second -- "

LIEBESKIND: I think they used to do it -- Yeah, they used to do it that way. They still do it that way, I think, in a lot of places.

DUBNER: "I don't want to be a second class member of the department and I'm assigned to something where everyone else gets a chance to rotate and choose, and do different things." Because Horace wanted me to stay, and so ultimately they hired [Kenneth] Casey, and Casey got the job that I --

LIEBESKIND: Is that right?

DUBNER: Well, sure. It was a couple of years later that they hired Casey. And it was the position that they were thinking of giving me.

And so I left. I went back to NIH.

LIEBESKIND: But you were -- I mean, you still had your commission that NIH would welcome you back.

DUBNER: Oh, yes, they -- Well, and I was- - You know, I went back and I said -- At that time, when I went back, Harold Stanley was the clinical director at the time. This is the fellow I did the study with, my first study. I said, "Harold," I said, "I'm coming back here only if you do the following. And that is, you have to set up -- I have to have money to set up a laboratory to do neurophysiological research, okay?"

"I need to have two people to work with me. I need an electronics technician to work with me, because though I know the rudimentary things about setting up this lab, I really need someone who has an in-depth knowledge of electronic circuitry, who can help me set up the lab and also develop the kinds of equipment that I'll need to do my research." And I said, "And third, I need someone to help me do the experiments." This was the golden era of NIH.

LIEBESKIND: It sure was.

DUBNER: And they said, "Oh, sure, Ron, we'll do it all for you." So they gave me a module or two -- a couple of modules. I set up my research, I purchased all the equipment, and I came back and I hired a technician to help me with the experiments, you know.

LIEBESKIND: Somebody who stayed with you for many years?

DUBNER: No, but I hired an electronics technician, Fred Brown, who you must have met in the lab at some point.

LIEBESKIND: Probably so.

DUBNER: Fred is still in the lab.

LIEBESKIND: Is that right?

DUBNER: Fred had just graduated from Maryland Engineering School and was looking for a position, and he came to the lab. And he's been there ever since.

LIEBESKIND: Isn't that amazing? And how about a student? Did you get a student at that time or somebody to work with you?

DUBNER: No, no. There were no students. There were no -- It was only after a while that I learned how one could develop to have --

[PAUSE; TAPE RECORDER OFF]

DUBNER: Yeah, so I came back, they gave me laboratory space, I purchased the necessary equipment to set up a neurophysiology lab.

LIEBESKIND: What was your position? I mean, you are now a lab chief and you have your own --

DUBNER: No, no, no. All I am was an investigator.

LIEBESKIND: No, I'm saying now. Now you are.

DUBNER: Now I'm clinical lab --

LIEBESKIND: But I mean, at that time--

DUBNER: At that time I was just an investigator.

LIEBESKIND: Were you in someone else's laboratory?

DUBNER: Yes. Did you ever know Jim Bosma?

LIEBESKIND: No.

DUBNER: Probably not. Jim Bosma was an expert in the area of oral pharyngeal function. And he was hired -- Just about the time that I went off to Michigan, he was hired to set up a program in oral pharyngeal function in the NIDR. And I was put in his section. He had a section, and I was put in his section. And when I returned, I was in that section. But I was very independent of him, because it turned out that during that three year period he had fallen out of favor, so they didn't require me to work very much with him, if anything at all. So I was part of his section nominally, but I really ran my own little program, which was to do neurophysiological research. So that's how it started.

LIEBESKIND: You were the only one in the institute who was doing --

DUBNER: I was the only one, you know. But I was very fortunate --

LIEBESKIND: Did you make colleagues with other people at NIH?

DUBNER: Yes. At that time, you know -- The neuroscience community in 1964 at the NIH was a wonderful community. That was Wade Marshall. Wade Marshall had his laboratory, and in that laboratory were people like K Frank. Phil Nelson was part of that laboratory. And Wade Marshall had a Friday meeting at noon every Friday, where they would invite people from the outside to give talks as well as people on the campus. And I became part of that, and I would participate in that every Friday. And that's --

LIEBESKIND: Who else was there? Who were some of the other people?

DUBNER: Bob Burke was there.

LIEBESKIND: Still is, right?

DUBNER: Still is there.

LIEBESKIND: What's his name? Evarts?

DUBNER: Ed Evarts was part of that, sure. And by that time, of course, Kandel had left. He had left a few years before that. Bob Wurtz was there. Still is there, of course. Yeah, so there's a group of us who have stayed on who are now lab chiefs who were all part of that group.

LIEBESKIND: In setting up there -- I mean --

DUBNER: Stanley Rappaport was there. Worked with Wade Marshall.

LIEBESKIND: Was there one person in particular that you were close to at that time who influenced you or helped you get your own thing set up, your own lab set up?

DUBNER: Probably Wade Marshall more than anybody else. Whenever I had some questions or problems, I would go to Wade. He would either help me or direct me to somebody.

LIEBESKIND: What was he like?

DUBNER: He was not an easy person to talk to.

LIEBESKIND: I met him. You know, he came over and visited Denise Albe-Fessard, and Jean-Marie ended up working with him back at NIH. Did you meet Jean-Marie at that time by any chance, when he came to work with Wade?

DUBNER: I think I did meet him.

LIEBESKIND: You may have met before.

DUBNER: I don't remember whether we actually -- I know I did meet Jean-Marie at that time, but it was just at one of these meetings; it wasn't on a regular basis.

LIEBESKIND: Yeah, Wade was a -- very taciturn.

DUBNER: See, actually, what I should have done if -- See, I was in a hurry to get back to NIH. I wanted to set up my lab. I wanted to start, you know, getting my independent research going. If I had to do that over again, I'd take a year and go to Paris and work with Busser.

LIEBESKIND: Do a post doc.

DUBNER: Do a post doc with Busser.

LIEBESKIND: With Busser, yeah.

DUBNER: Because it was Busser or Madame Fessard's work that was close to mine. So it was a wonderful opportunity that I had to do that, you see. But I never even proposed it to the NIH people. I don't know whether they would have sent me. They probably would have balked. But you never know. They might have gone --

LIEBESKIND: Well, you didn't take bloody long to get your degree. They might have figured they owed you another year of training. [he laughs]

DUBNER: So I didn't do that, and that was unfortunate. But I did it a number of years later. I did it in '70, when I went to work with Pat [Wall]. We'll get to that later. But so I started this program and I -- You know, I said to myself, "I know what's doing in the association areas. Let me continue with that research and take that on to its next step." So I continued to do those studies.

LIEBESKIND: Nothing on pain at this point.

DUBNER: Nothing on pain. But I was interested in the trigeminal system and I wanted to get work started on the trigeminal system.

LIEBESKIND: You saw that as related to your dental training, obviously.

DUBNER: Right. And so I started doing work -- The next step in these studies on the association cortex was to look at the visual responses in more detail because the -- because it was clear that the most potent input that I could find in the anesthetized cat under chloralose was the visual input to this site.

LIEBESKIND: Really?

DUBNER: So I initiated studies to begin to look at the visual input in more detail; what are the characteristics of it? I mean, does it respond to directionality, does it respond to bars, slits, things like that? And at that time, a fellow by the name of Bruce Dow applied at NIH to come

and work at NIH, and one of the places he applied to -- He applied and there was this program by which young physicians could come to the NIH and do their draft requirement. And he -- This -- I don't know how it came about, but Harold Stanley let me look at some of the applications to see whether there was someone I might be interested in. And again, this is the golden era of NIH, positions all over the place, okay? And I come upon this Bruce Dow, and I see that he's interested in doing work on neurophysiology, and--

LIEBESKIND: Where did he come from?

DUBNER: He -- I forget now exactly where he came from. It's been such a long time. But he was right out of medical school.

LIEBESKIND: There was somebody by that name, but he would have been much older. I wonder if he was his son or something.

DUBNER: No, he was unrelated to --

LIEBESKIND: He did cerebellar --

DUBNER: He was unrelated to the cerebellar Dow. And so I said, "Yeah, I'd like to bring this person to come and work with me." And so they offered him one of these positions. And Bruce Dow came and worked with me and he--

LIEBESKIND: So he was your first student?

DUBNER: He was my first student. And we pursued the visual studies together.

LIEBESKIND: What year are you in now? What year is this we're talking about?

DUBNER: We're now in 1966. '65/'66. '66, probably. And then all of a sudden I get a letter in the mail from -- or Harold Stanley gets a letter in the mail from some Australian who had worked -- was getting -- he was a dentist and wanted to come to the United States and work at the NIDR. He was getting his Ph.D. from somebody by the name of [Ian] Darian-Smith. Okay, and this is now '67 already and I had started doing a study in the trigeminal system where I did a study in the main sensory nucleus, which is the most rostral brain stem nucleus of the trigeminal system. And I did a study there, and I was interested in further developing the trigeminal system. And Barry [Sessle] had worked on the trigeminal system with --

LIEBESKIND: Darian-Smith.

DUBNER: -- Darian-Smith. And so I said to Harold Stanley, "My gosh, this is a great person. He's a dentist. Let's offer him an opportunity to come here as a visiting fellow." And they did. So that's how --

LIEBESKIND: So that's how Barry Sessle came.

DUBNER: -- Barry Sessle came.

LIEBESKIND: He stayed with you for a while, right?

DUBNER: Yes, he was there for three years, from like '68 to '71. And then about that time, Marie Nylen, who was a lab chief at the time -- she ultimately became a scientific director of NIDR, and then head of the extramural program for a while. Marie was a lab chief and she got a letter from this guy from NYU who was an electromicroscopist. He had done some training at NYU in electromicroscopy after he got his dental degree. He didn't have a Ph.D., he just had a dental degree.

But he wanted to come to the NIH and pursue his research, which was on cerebellum, the electromicroscopy of the cerebellum. You know, the ultrastructure of the cerebellum. And I didn't know anything about this guy, and all of a sudden she comes in one day and tells me she hired this guy. His name is -- What's his name again? [he laughs] No, I'm only joking.

LIEBESKIND: I don't know who you're talking about.

DUBNER: You don't know who I'm talking about?

LIEBESKIND: No.

DUBNER: Steve Gobel.

LIEBESKIND: Oh, Gobel. Oh, sure. Of course. Okay, right. An anatomist.

DUBNER: And all of a sudden one day Steve Gobel shows up.

LIEBESKIND: I see.

DUBNER: So here was this nucleus of four people who were in neuroscience. So within three years the NIDR had a neuroscience program. Myself, Steve Gobel, Barry Sessle and Bruce Dow --

LIEBESKIND: So they were all there at the same time at this point.

DUBNER: Well, Bruce Dow left pretty much soon after Barry got there. Yeah, about a year after. Barry stayed about three years.

LIEBESKIND: But it's the beginning of the empire. [he laughs]

DUBNER: It was the beginning of the program, yeah. So that's what took place. And Steve and I collaborated on a study in the main sensory nucleus.

LIEBESKIND: And he stayed for a long time.

DUBNER: And he stayed for a long time. And then Barry got the job at Toronto through me, because I knew Art Storey. And so I recommended him to Art Storey. They were looking for a neuro person to join their staff at the dental school, so I recommended him and that's how Barry got started in Toronto.

LIEBESKIND: What were the first pain studies you did at the Dental Institute?

DUBNER: All right. So the work that I did on the trigeminal system at first was not pain related. It was -- You know, it was looking at tactile inputs into main sensory. And not only that, I was looking also at visual inputs into main sensory. I was looking at convergence.

LIEBESKIND: That's a very timely thing. I mean, there's a lot of people --

DUBNER: I was always interested in -- See, if you look at my whole career, this whole issue of plasticity really goes way back. It goes way back to that time period, even to the association areas of cerebral cortex, and what's the function of these areas, and how are they involved in learning, and what do they mean in terms of our ability to extract information from the environment, and the plasticity of these areas. They respond to these different inputs. How does the nervous system decipher what these inputs mean in these areas? And so it goes way back. And so--

But I wasn't doing pain research and I wanted to do pain research.

LIEBESKIND: Really?

DUBNER: Yeah.

LIEBESKIND: What was the impetus there? I mean, what were you thinking of?

DUBNER: Well, here I was doing vision research, and I was doing some trigeminal system work, somatosensory work. And I realized at this point, you know, Barry was going to be leaving. And I needed to make a decision. It was too difficult to carry on research in two different areas, the somatosensory system and the visual system. The visual system research was, you know, burgeoning at the time. And so I had to decide which way I was going to go. I decided that since I was part of a Dental Institute, it might be most parsimonious to do research that had some relationship to the Dental Institute, which would be the trigeminal system in pain, rather than the visual system.

So I said to myself, "I'm not going to do any more vision research. I'm going to focus my attention purely on somatosensory research and I'm going to get into pain research." And I read all the literature on pain, and I said, "What's needed here is we've got to know how the input gets into the dorsal horn." I have got to first study what's going on in the primary afferents and how they respond to different inputs.

LIEBESKIND: You knew the gate theory at that time?



DUBNER: Oh, yeah, you know, of course I knew the gate theory. I knew --

LIEBESKIND: Pat Wall's work?

DUBNER: Yeah, I knew all of Wall's work and I knew, you know, all the other work that had been done at that time.

LIEBESKIND: Nociceptors?

DUBNER: Nociceptors that -- Well, [Ed] Perl -- Yeah, the beginning of nociceptors by Perl I knew about, and then of course Ainsley's [Iggo's] work. And so -- And I said to myself, "Well, look, I never had a post doc. This is the time to go and take a post doc and work with some one who's an expert in dorsal horn neurophysiology as well as in the pain area, maybe." So I talked about -- I thought about going to one or two places. One was to go to work with Anders Lundberg in Sweden. The other was to go to work with Pat in London. And I applied to take a sabbatical leave for a year.

LIEBESKIND: You could do that at the NIH?

DUBNER: And again, we were in the golden era, and the sabbaticals were available to go and take a year and spend a year learning in another laboratory. And so I proposed that, and I applied for it, and they said --

LIEBESKIND: In their largess -- [he laughs]

DUBNER: And they -- In their largess, they said, "Of course, Ron, anything you say."

LIEBESKIND: Naturally. Well, you must have been a bit of a golden haired fellow, too, in your institute.

DUBNER: Yeah, I guess so. I guess so.

LIEBESKIND: I mean, you make it all sound like it was the times but --

DUBNER: Yeah, I was -- Here I went to -- Look, I went to Michigan, spent -- finished a Ph.D. in three years, came back, was productive, was carrying out research. So they were pleased.

LIEBESKIND: They didn't have any neurophysiology going.

DUBNER: No. So they were pleased. And so --

LIEBESKIND: Who was the director, by the way, at that time?

DUBNER: Seymour Kreshover.

LIEBESKIND: Oh, yeah. Did you get to know him?

DUBNER: Oh, yes, very well. I still know him.

LIEBESKIND: He was quite a distinguished guy, right?

DUBNER: And he was the guy who really made the NIDR into the real program what it is today.

LIEBESKIND: K-R-E--

DUBNER: K-R-E-S-H-O-V-E-R. He had this vision which I strongly believe in, and that is that dental research needs to be on a par with medical research. We need to be a component of the biomedical research community. Sure, as dental researchers, we have our own unique interests in this upper part of the body, but the research we do needs to be thought of as good as, if not better than, what's going on in research in other parts of the body. And we need to be involved in systemic diseases that have their manifestations in the oral-facial area. We need to do studies related to that. And he said that the best way to accomplish this is to bring in the very best people, bring in research excellence into the intramural program. It doesn't matter whether they have a dental degree or they have a medical degree, whether they have a Ph.D. You bring in the people who have the training, who have these abilities. And that's what he did. And he's--

LIEBESKIND: He must have been quite a guy.

DUBNER: Yes. And he allowed programs that were developed by very, very fine research scientists who had very little dental background but whose interests were related to overall what the Dental Institute should be involved in. And he built a very, very fine intramural research program that was world renowned. And the people who were the lab chiefs of these programs were highly respected on the campus as lab chiefs would be in any other institute.

And you must remember, when I talk about the heyday, you know, of NIH, it was not only a heyday in terms of positions and resources; it was also the heyday in terms of the people. NIH at that time was the premier institution in the country if not in the world.

LIEBESKIND: Do you feel it still is, or less that?

DUBNER: Well, I think it is a premier institution, but there are lots of premier institutions now, because the extramural research funding has given lots of institutions throughout the country the opportunity to develop research enterprises and research programs in many areas that are equivalent if not better than the NIH. So it's not that we're *the* premier, we're *one* of the premier now. You see?

And I think there is a little problem. I think -- Which is one of the things that [NIH Director Harold] Varmus is working on, and that is that we're a middle-aged institution now, and we have to decide where our future's going to be. And we have to sort of make sure that research excellence remains at the NIH intramural program.

So that's what I'm -- So I --

LIEBESKIND: Was Kreshover somebody that you got to know? Is he inspirational to you and helpful, or -- ?

DUBNER: Well, he was inspirational to me because of his doing this. And it was he who gave me the opportunity to go and be trained as a neurophysiologist. It was he who really made a decision for me to be able to come back and set up my own research program. It was he who provided all the support.

LIEBESKIND: You hadn't mentioned his name up to now, but you've always talked in terms of this other fellow that you had trained with and did the first research project.

DUBNER: Yeah, because Harold Stanley was the person on the job. But Harold didn't do anything without consulting with Sy Kreshover, who at that time was the scientific director, then became the director, and then they brought in Dick Gruelich to become the scientific director, who came actually from UCLA [University of California, Los Angeles]. He was in the oral -- He was in the dental school, I believe. Or was it -- Either it was UCLA or USC [University of Southern California]. And he was an anatomist who did work on development of the tooth, a Ph.D. person. And Kreshover hired him to be the scientific director. Here again he hired a scientific director who didn't have a dental degree. He didn't believe that in order to accomplish dental research, you needed to have a dental degree.

LIEBESKIND: He wasn't parochial.

DUBNER: He wasn't as parochial as some people are. And, you know, and Dick Gruelich came and he didn't know who the hell I was, and he saw this guy doing neuroscience research and said, "What the hell do we need this guy for?" And one of his first -- The first time he met me, he said, "Well, I don't know if there's really any place for you here. I mean, you're like -- You stick out like a sore thumb." [he laughs] And so I said -- So when I heard that, I decided I'd better go find another job.

LIEBESKIND: Oh, really?

DUBNER: So I started looking for another job.

LIEBESKIND: Is that right?

DUBNER: And I told Kreshover, "I'm looking for another job." And the next thing I knew, Gruelich said, "There's a place for you here." [both laugh]

LIEBESKIND: Kreshover put out the word.

DUBNER: He put out the word, "This is a guy we've trained. This is a guy we want to see develop."

LIEBESKIND: So you were now thinking about going to Sweden or going to London.

DUBNER: All right, so I was thinking about going to Sweden or going to London. This was in 1969, okay? And both Anders Lundberg and Pat Wall were interested in me. And Lundberg was in Stockholm at the time, but he was moving to Gothenburg. And I looked on the map and I said, "Gothenburg? Oy, oy, oy!" [both laugh] And Mary Ann looked on the map and she said, "Gothenburg? No way!"

LIEBESKIND: I see. Did she really?

DUBNER: She said-- She was finishing her Ph.D. then.

LIEBESKIND: Oh she was, yeah.

DUBNER: And she said, "What am I going to do in Gothenburg? They don't even speak English in Gothenburg," she says, "Whereas in England, I could go and maybe I can do a post doc there." Which is what she did and which I -- So the decision was easy. Actually, it should have been the decision right away, because I was interested in pain and certainly --

But you see, I was interested in Lundberg because he was so much -- he was so much involved in descending modulation. And I saw that as, again, this plasticity issue. And I thought he -- I thought the work that he had done was first rate and I thought I couldn't do wrong in any way going through the training in his --

LIEBESKIND: Well, those were both giants.

DUBNER: So we went to London and Mary Ann worked--

LIEBESKIND: That's 1970?

DUBNER: 1970. And I worked with Pat Wall.

LIEBESKIND: That's for one year.

DUBNER: One year, yeah.

LIEBESKIND: Well, tell me about that. What was that like?

DUBNER: Well, that was a fascinating experience. I mean, Pat Wall is the brightest person in the field of pain, without question. There is no one who is as bright as Pat. Pat has ideas -- And I think you can see that. I mean -- The discoveries that Pat has made through the years are really -- you know, they are seminal discoveries that go way back.

You can start out, you know, in primary afferent depolarization, the work that he did in that area. You can go on to his work on -- with [Lorne] Mendell, studying the spinal cord neurons, WDR [wide dynamic range] neurons, responses of spinal cord neurons. From there he went on to the

work that he did with -- on the neuroma model, and activation of the neuroma model. I mean, and you can, you know, go on from there in terms of plasticity after injury that he started out -- that he was beginning to do while I was there, looking in the thalamus with David Edgar. With David Edgar, very early work.

LIEBESKIND: Egger, Egger.

DUBNER: Egger -- David Egger, right. David Egger was there the year before I was. In fact, I rented the house that David Egger had rented, you see, in London.

LIEBESKIND: I know David Egger because I -- a little bit because I've got friends at UCLA who are from Yale [University] and who knew him from his Yale days. They were buddies with -- Yeah, anyway.

DUBNER: And so--

LIEBESKIND: Who else was in the lab when you were there? Were you the only other -- ?

DUBNER: The other person in the lab when I was there was Vivian Abrams from Queens --

LIEBESKIND: Yeah, in Canada.

DUBNER: -- in Canada, Queens University. And Vivian came over and he -- Vivian actually brought most of his lab there and he set up a lab right there.

LIEBESKIND: He's an interesting guy.

DUBNER: Yeah, yeah, interesting guy. But Pat and I worked together.

LIEBESKIND: Did you? You sat down at the preparation together?

DUBNER: Yeah, we worked -- We did experiments together, and --

LIEBESKIND: What a thrill.

DUBNER: And that was really the exciting thing, because we talked. We spent half the time just talking about things. And I got to -- You know, I learned the way Pat thinks. I never worked in neurophysiology, up to that point, with a leader in the field. I mean, you know, Les [Rutledge] was a fine neuroscientist, but he was not one of your eminent neuroscientists. And I didn't do a post doc with anybody. I came right back to NIH and I worked by myself. And so the opportunity to do -- to interact on a day-to-day basis with someone who was a -- who had made so many seminal accomplishments and who was very bright and who was thinking all the time.

LIEBESKIND: Well, you know, we ordinarily --

DUBNER: It was just a great opportunity. You know what it did, it gave me a lot of confidence in myself. It really, you know, made me realize that, you know, what I could do, you see.

LIEBESKIND: You know, we ordinarily think of postdocs as preparing us for our independent work, but in a sense you could say that your independent work had really -- you were the prepared mind when you went to Pat Wall's lab.

DUBNER: Right. I had done independent work.

LIEBESKIND: You had done independent work, you had done reading, you knew his work and so forth.

DUBNER: Right. What Pat was doing was sort of filling in the gaps, in my training. I think there is something of importance to working with a master. There is something you learn by working with a master. And I had not done that. So that's what it was all about.

LIEBESKIND: So if there were ever any quaverings as to which direction you were going to be heading professionally, they were settled by 1970. You were hooked on pain at that point.

DUBNER: Yeah, I knew -- Well, that's an interesting story in itself because -- because Pat was spending a lot of time in Israel that year and so -- And I was --

LIEBESKIND: He was working with Marshall [Devor]?

DUBNER: He was working with Marshall. He had met Vera by that time.

LIEBESKIND: He had met Vera, yeah.

DUBNER: And so he was going over to Israel, making trips fairly often. I was -- Mary Ann and I wanted to see Europe and so every time the kids were off from school, I took off, okay, and went traveling on the continent. So between those two things we would get something going. We never really accomplished a full research project during the year.

LIEBESKIND: One year is a short time for that kind of work.

DUBNER: It's a short time. But also we chose a project that was -- little did we know what we had chosen. What we chose to do was to antidromically backfire neurons from the brain stem that had their cells of origin in the lumbar spinal cord. And we were going to trace their terminals. And it got to be too tedious.

LIEBESKIND: This is before [William] Willis and other -- those guys?

DUBNER: Before anyone had done any of this work, tracing axons into the brain stem. Okay? We weren't tracing them into the thalamus.

LIEBESKIND: HRP [horseradish peroxidase] was not around then? This was neurophysiology--

DUBNER: This was neurophysiology days, right. And we were tracing these axons into the brain stem. And lo and behold, as we now know, they had beaucoup collaterals going into the brain stem and even going up into the hypothalamus. So the reason we did this project was because I knew about doing the surgery in the brain stem, and where the structures were in the brain stem. He was the expert on the laminectomy in the spinal cord.

So he did the laminectomy. I exposed the brain stem, took out part of the cerebellum, and we traced. And we found axons that -- They kept going, they kept going. We had them going into the cerebellum and crossing over to the other side. Little did we know that Glenn Giesler would show, you know, how the hypothalamic projections were. We never got to the hypothalamus. And this -- It used to take all day almost to do one of these.

LIEBESKIND: Let the record show that Glenn Giesler was my student. [he laughs]

DUBNER: I know, I know. [he laughs] And so it took a long time to do this and we got impatient. Pat in particular got impatient with how long it was taking us to track these things, and we kept looking for shortcuts on how we could track them.

LIEBESKIND: Let me -- I'm not quite understanding the science here. Why is it taking a long time? What does it mean? You put in a stimulating electrode and you're recording from the cell and the spinal cord, and you do it.

DUBNER: Yeah, but you're antidromically activating it and it's branching, and you're trying to --

LIEBESKIND: Oh, so you have to keep moving the electrode. Is that what you're saying?

DUBNER: Yeah, you have to keep following the -- making multiple penetrations at higher and higher levels.

LIEBESKIND: But how do you know it's the same axon?

DUBNER: By the antidromic activation of the cell. We knew it was different -- We know it's different axons because, you see, as it thins out, it has different conduction time.

LIEBESKIND: Right.

DUBNER: So we knew we were going into a smaller branch. So we take a small branch and it would terminate, but then we would see that the main axon was still there because we could go up a little higher.

LIEBESKIND: So you're holding the cell body.

DUBNER: Holding it hours, hours, hours.

LIEBESKIND: I see, and then just moving the stimulating probe to see how far forward --

DUBNER: That's right. And using -- And you really can only do this with a microelectrode, stimulating microelectrode, in order to be able to activate these small terminals, these small axons. So it wasn't something where you could put a -- And that's what we tried to do. We tried to put an array of big electrodes at different higher levels and that got us nowhere. You know, and so --

LIEBESKIND: So did you actually publish this study?

DUBNER: We published a review in the -- I guess it's the -- not *Physiological Reviews* -- but what is it? You know, there's a book that comes out every year.

LIEBESKIND: I know.

DUBNER: Now there's one in neurosciences, but then it was physiology. Yearbook -- It wasn't called the yearbook, but it was called --

LIEBESKIND: *Progress in Brain*?

DUBNER: No, not *Progress in Brain*. I forget. But we published a paper in that, and that's our paper. And we included-- It was a paper on spine -- you know, supraspinal pathways. And Pat wrote part of it and I wrote part of it and we published it there. *Annual Review of Physiology*, that's what it was.

LIEBESKIND: Oh, the *Annual Review*. Oh.

DUBNER: *Annual Review of Physiology*. So we published that paper. Well, one of the people that I spent a lot of time with while I was in London was Sammy Zeki.

LIEBESKIND: Semi?

DUBNER: Sammy Zeki, Z-E-K-I. Sammy is -- has done extremely well through the years because Sammy has been in the vision areas. And he knew of my work in the visual areas of the cerebral cortex and he wanted to collaborate. He had done some of the early Nauta and Funcke and Heimer work looking at projections from Area 17 to an area on the anterior bank of the superior temporal sulcus, okay? And he found that there was a direct projection from Area 17 to this area. And he was terribly anxious to find out what the function of the neurons were in this area. He kept saying, "Ron, come on. Let's do a study. Let's do a study." He knew nothing about neurophysiology.

Also at that time in the lab was Gene Merrill. I don't know if you knew Gene Merrill. Gene Merrill had come from MIT and had gone to London with Pat, and had become independent from Pat.

LIEBESKIND: Gene Merrill?



DUBNER: Gene Merrill, M-E-R-R-I-L-L. He had done some work on respiratory --

LIEBESKIND: G-E-N-E?

DUBNER: G-E-N-E. He worked on respiratory neurons.

LIEBESKIND: I'm just -- I'm going nuts because there's this woman at UCLA who I know very well who's named Jean Merrill, J-E-A-N.

DUBNER: Yeah, a different person. The other people in the lab we could talk about later, who are still there, John -- Oh, gosh, it's terrible. But Lyn Nadel was there. They did the work on the hippocampus, Lyn Nadel and John O'Keefe -- They are the ones who showed that the hippocampal neurons responded to object placement in the field.

LIEBESKIND: And this is Zeki's lab you're talking about.

DUBNER: No, it's not. I've digressed. These are people who worked in the department there.

LIEBESKIND: In the department, not in Pat's lab?

DUBNER: Not -- What they were part of was --

## RONALD DUBNER INTERVIEW

### TAPE TWO, SIDE ONE

LIEBESKIND: Let me just mention that the previous tape went off without my hearing it and we cut off part of this. I think we lost a couple of minutes, so maybe you can pick up again where you're --

DUBNER: I think it's an important -- I think this whole issue of this -- of my working with Sammy. What I was saying was that the department -- Pat was the primary investigator on an MRC [Medical Research Council] group grant that -- and that included Lyn Nadel and John O'Keefe, who were doing studies on hippocampus. And they were using chronic recordings from awake rat and putting them in various environments and looking at the response properties of these neurons. And they published this book. They never published an individual paper on it until they published this book on hippocampal place function.

But to get back to Sammy, Sammy was after me the whole time that I was there to do a visual study. And I kept saying to him, "No, no, no, Sammy. I'm not going to do any more studies in the visual system."

But he said, "Ron, you know, I've got this area that receives direct projections from Area 17 of the visual system and we -- it would -- and it's really a very discrete area, and it would be great to try to record from that area."

I said, "Sammy, no, Sammy. I don't have the time. You know, let's forget it, Sammy."

He said, "Well --" So and he kept pursuing it and then we would take these -- We would go to lunch in London. He would take me down the street where they sold a lot of the medical equipment, and he'd say, "Well, what do I need to do this?"

I said, "Well, you need a small projector, Sammy. You can do it -- You can just have a hand-held projector and you can do this."

And he said, "You mean a projector like that?" And he'd point to a projector in a window.

And I said, "Yes, a little projector like that. We would mount a handle on it and you could just move it across a screen. You'd have to get a screen we'd -- you could put in front of the monkey, and we'd move this projector across the screen and we'd see whether we could activate any of these neurons."

And so the next day, or the next week, sure enough there was this projector in his lab. And then he got Gene Merrill, who I mentioned to you, Gene Merrill, who had come to London from MIT with Pat and did work on respiratory neurons and Gene Merrill was very much into design of the electronic equipment. And he had -- He did a lot of the electronic work in the labs there. And so

he -- Sammy asked him to help him put together an electro-physiological lab. And Gene helped, and they bought-- They had some old oscilloscopes, and Sammy bought some other equipment.

LIEBESKIND: What is that noise?

DUBNER: Those are the crickets.

LIEBESKIND: Good grief!

DUBNER: Those are our friendly crickets sounding off.

LIEBESKIND: In the middle of the day, some background noise.

DUBNER: In the middle of the day, right. And we'll hear more birds. The birds come back periodically. They cycle back to the feeders, and so we'll see them maybe a little later.

LIEBESKIND: Well, luckily this isn't the Amazon. We don't have howler monkeys or something. [he laughs] That's a loud cricket.

DUBNER: Yeah.

LIEBESKIND: Yeah. Go ahead, Ron. So you did this study with Sammy.

DUBNER: So I did this study with Sammy and he finally convinced me, and we got -- we made up some slides that had bars and slits and small circles and bigger circles, and we did this study in the primate. And the first experiment we did, he -- I set everything up and he directed the electrode where it should go in. We went through this area, part of Area 18, or it was Area 19, maybe, and we couldn't even, you know, we couldn't figure out what the cells are doing and all the sudden we fall into this area --

LIEBESKIND: MT.

DUBNER: MT -- Area MT [middle temporal area], as it's now called. And the neurons responded to directionality only. It didn't depend on the form of the stimulus, whether it was a small circle, a big circle, a bar, a slit; it responded to the directionality of the stimulus. And we published a paper quickly in *Brain Research*. And there was a Dubner and Zeki paper published. It gets quoted fairly often, but not as often as it should because Sammy subsequently the next year did a few more studies on this area and with some better illustrations, and published a paper in the *Journal of Physiology*, a full-length paper just with his name. And so many people quote the Zeki paper, not the Dubner and Zeki paper. But the people at NIH who do work in this area, like Bob Wurtz and his colleagues --

LIEBESKIND: Whose arms you can twist. [he laughs]

DUBNER: Well, no, they appreciate that.

LIEBESKIND: No, I'm teasing you.

DUBNER: They appreciate that the Dubner and Zeki paper had everything in it and it was the first paper. And so they quote the Dubner and Zeki paper.

So that was my last fling in the visual system, as I said. It's a major paper, actually, because that's a major area in visual function. And so I'm proud of that accomplishment.

LIEBESKIND: Good. You know, there was a question I had here to ask you, about while you were in London. Did you have any clinical experiences at all? Did you go into Peter Nathan's lab or anything?

DUBNER: No, it was interesting, I didn't. You know -- I didn't actually. Pat occasionally went and did some things with Peter Nathan, but it wasn't very often. And that year he didn't do very much. The subsequent year, when Allan -- when Allan came to the lab --

LIEBESKIND: That's what I was going to say because Allan told me that he --

DUBNER: When Allan came to the lab subsequently, Allan Basbaum, he did. He made it a point to spend time.

LIEBESKIND: That was a few years after you.

DUBNER: Yeah, it was actually a year later. It was the next year.

LIEBESKIND: A year later.

DUBNER: He spent time with Peter Nathan doing clinical work. But I didn't do much of that at all.

LIEBESKIND: Well, we started to say, so now it's -- You know, it's 1971 or whatever, and you're coming back.

DUBNER: Right, I go back and --

LIEBESKIND: And the Issaquah meeting is coming up soon in the calendar.

DUBNER: At that time Rhyuji Sumino was the first Japanese scientist, of a long list of Japanese scientists, who have worked in my lab.

LIEBESKIND: That's right, you had a very close association with --

DUBNER: And Sumino came and we started doing primary afferent studies. And --

LIEBESKIND: Well, I mean, you weren't doing spinal cord?

DUBNER: No, we started doing primary afferents.

LIEBESKIND: Did he know how to do that or did you have to?

DUBNER: No, we learned together. We started doing that together. We started recording from primary afferents from the trigeminal system, from the intraorbital nerve. And so we started doing the studies. We started doing the studies of the primary afferents. We started recording from nociceptive neurons. And that was the beginning.

And then Ralph Beitel came to the lab. And Ralph and I continued those studies. We were the first ones to record from c-nociceptors in the primate. We published before Ed Perl published.

LIEBESKIND: Is that right?

DUBNER: So the first publication of primate nociceptors, c-nociceptors was the Beitel and Dubner paper that was published in the *Journal of Neurophysiology*. About a year later, Kumazawa and Perl published a paper in which they said that they had done this too. They had done this at the time that -- They said -- Actually, the papers were published within the same year, and they said that while they were -- while their paper was in press, the paper was published by Dubner and -- by Beitel and Dubner.

LIEBESKIND: So they at least referred to you.

DUBNER: They did refer to us, yes. And -- But Ralph Beitel and I were very interested in developing a behavioral model. And so that was one of the things at that time, was that we developed our monkey model.

LIEBESKIND: The awake monkey?

DUBNER: Our awake monkey model. And Ralph played a very important role in that. And then he left the lab and he went -- He actually left research for quite a while and went out to San Francisco and did some teaching. More recently he's come back into research. But he was a very good person. And so we developed this model, we did some behavioral studies with it, and we published studies on the response of these afferents from the same part of the face where we had done the behavioral studies. And we did some correlative behavioral and neural studies on primary afferents and on the behavior.

LIEBESKIND: You're always right with the times if not ahead of the time. I mean, this is -- Again, it's very timely kind of stuff, isn't it?

DUBNER: Well, and then -- It was. And then-- And of course, we were helped in developing these behavioral animals by Ed Evarts, because --

LIEBESKIND: Yes, he had all that --

DUBNER: Because Ed Evarts and Bob Wurtz were doing awake monkey studies, too. But we developed the behavioral paradigm. The technique of how to do this we adapted from Wurtz and from Evarts. But we did it differently because we had to go down into the brain stem. They were recording from the cortex. And so we developed a method by which we hid the electrode within a sleeve -- a metal sleeve -- and then protruded the electrode once we got it close to the brain stem site.

LIEBESKIND: You had a lot of stuff to go through.

DUBNER: Yeah, so then we went through the cerebellum, the cerebrum and cerebellum. And then when we got close to the brain stem, we would then protrude the microelectrode out of this sleeve. That's the way we did it.

LIEBESKIND: Broke fewer microelectrodes that way.

DUBNER: Yes, yes. And we started recording and at that time Ralph had left, and I started doing these recordings with Donna Hoffman and -- Oh, it's terrible how at the spur of the moment you can't pick up these names, but he's the fellow who came up from Richmond.

LIEBESKIND: Not Donald Price?

DUBNER: No, no. No, the first studies we did were with Price. They were done in the spinal cord. They were done in anesthetized animals with Jimmy Hu when Jimmy Hu was in the lab. We recorded from spinal cord neurons, and from the trigeminal, from the dorsal horn in the anesthetized animal and we looked at the response --

LIEBESKIND: From the spinal nucleus of the trigeminal, is that what you're saying?

DUBNER: Right, from nucleus caudalis. And we studied those in the anesthetized animal. But the awake monkey work followed that, when Ron Hayes and Donna Hoffman joined the laboratory, we started doing the recording.

LIEBESKIND: Hayes had been trained by Dave Mayer.

DUBNER: Hayes had been trained by Dave Mayer.

LIEBESKIND: So Hayes is my academic grandson. [he laughs]

DUBNER: Right. And of course, Don Price was a very close colleague of David Mayer at that time, and they had done these studies where they did correlative behavior in the human with --

LIEBESKIND: -- with Don Becker.

DUBNER: -- with Becker, with studying the neurons in the monkey.

LIEBESKIND: It's all coming together, isn't it? There's starting to be some critical mass getting going here.

DUBNER: Yeah, so here was Don who came to the lab, and here was Ron Hayes who then came to the lab. And Don left and went out to the west coast, and so there was this, you know, here was this -- these ties to Dave Mayer and to you through Don Price, Ron Hayes, and ultimately, of course, Gary Bennett who has stayed in my lab for many, many years.

LIEBESKIND: Another Dave Mayer product.

DUBNER: Right, who was a student of Dave Mayer. But this was a fascinating time for Ron Hayes and for Donna Hoffman and myself because we saw things that no one else had ever seen in the brain stem, recording from these awake neurons. And we wrote a series of papers that were published in the *Journal of Neurophysiology*. And then Cathy Bushnell joined the laboratory.

LIEBESKIND: Is she also -- Is she a psychologist by training?

DUBNER: Yes, she was a psychologist by training. She trained at American University with Bert Slotnik. And then she came and she did a post doc with Micky Goldberg in Bob Wurtz's lab. And then she joined me in these studies in the trigeminal system. But these were fascinating because we saw all these task-related responses of these neurons --

LIEBESKIND: Yeah, I remember those.

DUBNER: -- where the neurons would respond when the monkeys would be initiating a trial. And here were neurons that responded to noxious heat stimuli, but they showed these task-related responses at the beginning of the trial and at the time when they released -- when they recognized the cue for releasing the panel. And --

LIEBESKIND: Well, Ron, you know, I think the first --

DUBNER: So this again gets into the neuroplasticity again.

LIEBESKIND: Yeah. Let me interrupt here because it just -- all of the sudden it strikes me, I think I've asked you one question so far, so we're having a John Loeser-type interview.

DUBNER: Oh, no.

LIEBESKIND: I said, "Where did it all begin?" And you know, you're telling me that -- But I want to interrupt you --

DUBNER: Well, now, you've asked other questions along the way.

LIEBESKIND: Well, little ones here and there. But I want to ask one now, that I think maybe relates here, because it's one of the ones I had written on the plane. I mean, you know, your lab

is characterized now, here, 1994, you know, your lab, as you look back over it, is characterized by all these different kinds of studies and different techniques you brought to bear.

DUBNER: Well, we can get into that.

LIEBESKIND: Well, we're going to. I'm not trying to jump over it. I just want to say, as I hear you, I hear a sense of excitement about these behavioral studies and I'm just wondering -- Maybe I should wait until the end to ask this, but I was just wanting to ask you, you know, as you look back now over all these different kinds of studies, are there one or two studies that particularly excited you and that, you know, that you look back on with special fondness or something as being especially interesting?

DUBNER: Well, I think the most -- I think these were some of the most exciting experiments that I ever did, and that was recording from neurons in the awake behaving monkey.

LIEBESKIND: In the awake behaving. I mean, that's what I was getting a sense of, that's why I asked you.

DUBNER: This was the most exciting discovery, I think, that I ever was involved in my career. I think the MT studies were very exciting, too, because we came on this area that no one else had -- knew what was going on and the -- You know, what's most exciting in science is when you come upon a discovery, that is like taking candy from a baby. In other words, there are some studies you do that are difficult. You get some exciting data, but it's -- you know, you have to really -- It's like pulling teeth sometimes, but you do get some exciting data. But the awake monkey experiments was like taking candy from a baby, because the neurons just -- they just shouted at us. They just shouted at us and said, "Look what we can do in the awake behaving animal -- organism, the awake organism. Look how our activity can be modulated by the environment, by the environmental cues, and by the relevance of the stimulus to the behavior." And this was very, very exciting. The MT discovery was also like taking candy from a baby.

LIEBESKIND: A similar thing, yeah.

DUBNER: We went into this area and it just started shouting out at us about what the function of this area was. So that was very exciting. I think the other thing that was exciting was -- but other people had shown it before, was the expansion of the receptor fields that we found after injury. You see? And then -- You know, other people had shown that.

And then the -- Well, we're getting ahead of our story, but the work that we've done in the lab with -- the molecular work with dynorphin, gene expression. That's very exciting. And that -- But of course, the most exciting things you do are the ones that you really have your hands into. And my hands were really into the MT studies and into the awake behaving monkey studies.

LIEBESKIND: And now your lab is so big you've got these different people --



DUBNER: In a lot of the subsequent studies, the lab got bigger and other people were at the bench, and I was involved in the network, but writing the papers and talking about it on a day-to-day basis, but it wasn't -- I wasn't -- My hands were not wet or dirty.

LIEBESKIND: Right, right. I understand.

DUBNER: But they were in the monkey experiments. So that was really a fascinating time. But when I was trying to -- What I was alluding to was that this is the plasticity again, plasticity of the nervous system that was so exciting to me that I saw in the awake monkey behaving experiments. But you're right. I mean, there were other things going on in the lab already at that time. I mean, I was involved in these awake behaving monkey experiments and Steve [Gobel] was doing his studies.

LIEBESKIND: He was doing his anatomy.

DUBNER: Susan Hockfield had joined -- was a graduate student who worked with --

LIEBESKIND: I forgot she was with you, that's right.

DUBNER: -- with Steve before and did her degree with Steve. She did an HRP study looking at projections.

LIEBESKIND: She is a bright, bright girl.

DUBNER: A very bright person. And then of course the clinical group started. What happened --

LIEBESKIND: Is that with Mitchell [Max]?

DUBNER: No, no. Mitchell came along in -- not until the eighties. But in the seventies, in 1974, Sy Kreshover was the director of the NIDR at that time, and he -- And there was a person who was a dental anesthesiologist who was part of the institute. His name was Ed Driscoll. And Ed Driscoll saw the possibility that his program in anesthesiology, where he was interested in I.V. sedation, and systemic anesthetics and sedatives that could be used in dentistry -- And he saw the possibility of revitalizing his program by combining it with my basic research program in neurobiology.

LIEBESKIND: Did he later become a program man? Didn't he and --?

DUBNER: Yes, he worked with --

LIEBESKIND: What's that funny guy we always --

DUBNER: He worked with -- Oh, what's his name? The big guy [Aaron Ganz]. He preceded Pat Bryant [in NIDCR Extramural Programs]. I'll remember his name in a minute. But Ed Driscoll at the time wanted to be involved in research again, and so he persuaded Dick Gruelich,

who was scientific director after Sy Kreshover, to set up this lab. At that time I was a section chief and he persuaded them to join our groups and make this one branch in neurobiology and anesthesiology. And they approached me and asked me whether --

LIEBESKIND: Oh, that's where that name comes from. I see.

DUBNER: -- whether I would be the chief of this branch. And I mean, it was a wonderful opportunity.

LIEBESKIND: Wow. So what year is this, now?

DUBNER: This is 1974-75.

LIEBESKIND: Seventy-four. So you become a lab chief.

DUBNER: I became a lab chief at that time.

LIEBESKIND: Fantastic. Anesthesiology and --

DUBNER: Neurobiology and Anesthesiology Branch.

LIEBESKIND: Neurobiology and Anesthesiology Branch.

DUBNER: And that's what led to this mushrooming of this group.

LIEBESKIND: I was going to say, so with that then you had even more resources, more space, and your group got -- started getting really large.

DUBNER: The first person I brought for the clinical program was Rick Gracely because I said, "If I'm going to do clinical pain research, I've got to have some one who knows how to measure pain. We've got to begin to measure pain." At that time no one measured pain. All they did, they used terrible category scales. And so -- And all of the sudden this guy appeared out of nowhere. Well, no, he didn't appear out of nowhere. I wrote to people who did work in measurement.

And I wrote to this guy at Brown, Engen, and he said, "I've got just the person for you. I've got this graduate student who's interested in pain. His name is Rick Gracely. He'd love to come and do his thesis while he's working in your lab." So that's how Rick --

LIEBESKIND: So he was a student at Brown, but he did his thesis in your lab.

DUBNER: Trig Engen. Trig Engen was his thesis advisor.

LIEBESKIND: Who?

DUBNER: Trig Engen, E-N-G-E-N. He's a psychophysicist. And Rick came and did his thesis while he was working in my lab.

LIEBESKIND: That's great.

DUBNER: We collaborated on this. All those original papers that we published in pain were part of his thesis. So that was that. And then we go on.

LIEBESKIND: Then M.A. [Ruda] came.

DUBNER: And then M.A. came to the lab, and Gary Bennett came about the same time, about a year later after M.A. And M.A. worked with Steve and she developed -- She was the one who brought a lot of the neurochemistry into the laboratory, because Steve wasn't interested in the neurochemistry that much. So she brought that work into the lab.

Then Gary came, and Gary -- I said to Gary, "Well, if we're going to --" Steve had done all this beautiful Golgi work in the trigeminal system and in the spinal cord. And I said, "Let's now see whether we can do intracellular recordings and label these neurons." And that's what Gary started to do. And then we went -- And then we combined that with the neurochemistry because we learned to do the immunocytochemistry. M.A. did that. And we combined that, so there were combined studies on immunocytochemistry and intracellular recordings from neurons. We were the first ones, really, to do that. People are still doing those studies. We stopped doing them years ago.

LIEBESKIND: Well, you've passed on. I mean, to the extent to which we're going chronologically here, you've passed 1973 and I want to have you pause on that and talk to me not about the lab now but about Issaquah. You were there. It was a --

DUBNER: Yeah, yeah. John Bonica, may he -- may his soul rest very comfortably because he was, as we all know -- I mean, he was the man, as far as I'm concerned, who provided the impetus for this field. He was the one who had this vision that there be a society devoted to the study of pain. And not just a clinical society, but a society that brought in the finest basic research scientists to come in and get involved in doing research and to intermingle with the clinical scientists and the clinical practitioners. And so --

LIEBESKIND: Now, why was that so important, do you think, to create a society, a pain society?

DUBNER: It was very important because it was an opportunity to generate interest in pain via a society which was multi-disciplinary. Up until that time, societies were all individual disciplines. It was only in the early seventies that the Society for Neuroscience came into existence. Up until that time there was a physiology society, there was an anatomy society, and so on. And John Bonica had the vision that he needed a multidisciplinary society for this pain field to get going, because pain was multidisciplinary. It involved a number of different basic science areas.

LIEBESKIND: He's the one who made that clear, isn't it? I mean, more than anyone else did.

DUBNER: Exactly. And so that's what Issaquah was all about. He brought in all of these scientists who had anything to do with pain. And he -- I don't know how he learned about me. He must have learned about me through Ed Driscoll. Well, maybe through Pat and also through the NIDR. He was very interested in the NIH and getting the NIH to support research on pain. And he found out that there was this guy, Dubner, who was doing work at the NIDR on pain. And he invited me very early on to participate in Issaquah.

LIEBESKIND: Do you remember when he called you?

DUBNER: Oh, yes. He called me. I don't know how it was. He called me and said, "I want you there. I want you to --"

LIEBESKIND: -- "give a paper."

DUBNER: -- "give a paper." And so -- And you see, I was at -- This is at the infancy of my involvement in pain, because I came back from working with Pat and I started doing these primary afferent recordings with Sumino. And the paper that I presented at Issaquah was a paper that was published with Sumino on the primary afferents. And so I was at the very beginning of my pain career at that time.

LIEBESKIND: What are your recollections of that meeting? I mean, just scientific, personal and --

DUBNER: Well, I recollect, you know, Pat getting up and lambasting some guy who had spoke about acupuncture. He was from UCLA, I think.

LIEBESKIND: Yes, he was.

DUBNER: I forget his name. Yeah, and Pat just ripped him.

LIEBESKIND: Actually, from UC Santa Barbara.

DUBNER: Was it UC Santa Barbara? He ripped him apart.

LIEBESKIND: Well, there were two. There was one from UCLA and one from -- Yes, I remember that happened. That was a shocking experience. Wasn't it? What did you think of that? I mean, because you knew Pat. Did you know he was -- ?

DUBNER: I didn't know he was going to do that, but I did know Pat was capable of doing that. I'd seen him do that to Manfred Zimmermann on other occasions. And -- But that's something that -- You know, that's a characteristic of Pat's. But I also remember the excitement of people thinking about a society being formed out of this. And I knew I wanted to be part of that. I knew that this was something that I wanted to be involved in very, very much.

LIEBESKIND: What was your earliest involvement in the IASP?

DUBNER: I was one of the founding members of the IASP. I was on -- You know, and I think I was elected to Council very early on. I don't remember exactly when I was, but I think I was on the first Council.

LIEBESKIND: First Council, yeah.

DUBNER: Not the first. I think I was on the first Council, if not the second Council or something like that, three years later. So I was involved with IASP from the very, very beginning. And I remember that Issaquah meeting. I remember where we stayed.

LIEBESKIND: Yeah, the nunnery.

DUBNER: In the nunnery. I remember how stark that place was. And that what we did was we talked science all the time, we talked pain. We were involved in talking pain the whole time we were there. It was a wonderful meeting.

LIEBESKIND: Do you remember some of the papers particularly or people that you met that, you know, maybe for the first time?

DUBNER: I met everybody, you know, who was involved in the pain field at that meeting.

LIEBESKIND: We probably met for the first time there. I don't specifically remember it, but --

DUBNER: I don't -- Yeah, but we probably did. It was probably the first time we met.

LIEBESKIND: Just got to know who each other were --

DUBNER: Yeah, I mean, because before that there were no meetings of people involved in pain, and so this was our opportunity to get to know each other.

LIEBESKIND: But I want to focus on your comment, because it seems to me it is not a usual one. I share it with you, but it's not -- I think you and I may be not alone, but I mean -- That you sensed the importance of the coming together of the thing at that time. You understood at that time that this was a coming together and that this was --

DUBNER: Remember, this was '73.

LIEBESKIND: Right.

DUBNER: This -- I had -- I believe I had already been approached to head up this lab.

LIEBESKIND: Yeah.

DUBNER: The idea of bringing together a basic science group and a clinical science, that was my vision too. My vision was John Bonica's vision.

LIEBESKIND: So it fit right in, right.

DUBNER: My vision was that we were going to take our basic science findings and apply them to the clinical situation. This was my vision early on. And that's why it was so exciting to me to take on this job of adding, you know -- Because I had a clinical background. I wanted to integrate my clinical experiences. Look, my first real research was on temporomandibular joint disorders.

LIEBESKIND: Right, right.

DUBNER: I told you about that. And so I wanted to have -- to see an integration of the clinical aspects of this pain field with the basic sciences.

LIEBESKIND: And you said you wanted to be part of this organization, that you --

DUBNER: And I wanted to be --

LIEBESKIND: You wanted to be active in it. You knew it was going to be important, and --

DUBNER: Yeah. This was where I wanted to be at. This is where I wanted to give my time and my effort outside of the actual science, in terms of societal participation.

LIEBESKIND: As opposed to, let's say, the Society for Neuroscience, which you could have been --

DUBNER: The Society for Neuroscience or -- I mean, I was active a little bit in the International Association for Dental Research, where I was the first president of the neuroscience group of the International Association for Dental Research. But -- And I participated in that. But my real devotion and love was to the IASP right from the beginning. And the American Pain Society, both of those organizations. This is where I wanted to be, to expend my efforts outside of my science.

LIEBESKIND: What do you recall of the early days of the American Pain Society or its getting together? Were you -- ?

DUBNER: I remember that first meeting, that Bert -- Was it Bert Wolff's meeting?

LIEBESKIND: Yeah.

DUBNER: And it was in San Diego.

LIEBESKIND: That's right, yeah.

DUBNER: And that meeting and that wonderful banquet. [he laughs]

LIEBESKIND: We were talking about that at Loeser's house the other night after dinner.

DUBNER: But that was great. And I always felt that the American Pain Society should try to emulate the idea that had been promulgated for the IASP, and that is that basic science should be an important component.

LIEBESKIND: Which was something we had to keep working on with the APS.

DUBNER: We had to work on that, right.

LIEBESKIND: There would always be new people coming in and saying, "Wow! What's with the science? Who gives a damn about that?" We had to keep instructing them.

DUBNER: In fact, the APS is moving away somewhat from the science today. There's a lot of involvement, you know, in the clinical aspects because of the situation of managed care and what -- how are --

LIEBESKIND: Yeah, well, these things will come and go.

DUBNER: How are our clinical colleagues going to be funded, and the importance of chronic pain. And so there's a lot of emphasis on that today. And Peter Vicente, whom I have strongly supported all these years, is making strong efforts in that area. I think it's important, but I hope that the basic science continues. I mean, Gary Bennett is the scientific program chairman this year, and I'm sure there's going to be a strong basic science program.

LIEBESKIND: The meetings have been very good from that standpoint.

DUBNER: Yes, they have.

LIEBESKIND: With very few exceptions.

DUBNER: Allan Basbaum's was --

LIEBESKIND: Allan's was excellent.

DUBNER: -- was instrumental in getting these Society for Neuroscience abstracts presented.

LIEBESKIND: That's right. That was a very good idea.

DUBNER: I loved that. It's not going to happen this year, unfortunately.

LIEBESKIND: Yeah, there was some goof on that.

DUBNER: I don't know what it was, but they're not going to be there in any number, because they weren't there last year at the meeting, maybe the break was bad. But Gary has had a lot of trouble getting people to present the neuroscience abstracts. I love that because I could go to the APS and see all of the pain abstracts for the neuroscience ahead of time and do them leisurely, and I wouldn't have to go to them at the Society for Neuroscience meeting, and I could do other things at that meeting.

LIEBESKIND: Talk to me now -- As long as we're on the subject of Issaquah, talk to me a little more about John Bonica. You know this is a very significant moment. We've just been together in Seattle at his funeral. I mean, we sat around Loeser's house and talked about his foibles, and sort of the funny stories and you know and so forth, but we loved this man. Why? You know, what is there about him?

DUBNER: Well, I loved this man because I saw right from the very beginning that this man was -- His strength, and his devotion, and his determination to make this happen, and his -- also, his love and his caring for other people. I mean, I saw right away that he was -- he really cared for me and he wanted to -- He wanted to help me further my career. He wanted to support me in any way he could. He always -- He went out of his way to say nice things about me and my program. He always had -- He always did that. Even before you did that, John, he always got up there and came to NIH. There were meetings that he was very much involved in, trying to get things going at NIH in the pain field. And whenever he had a chance he would say -- he would mention Dubner's lab and the important work that's going on in Dubner's lab. So he was --

LIEBESKIND: He commanded a lot of respect, that guy.

DUBNER: And he did. And he was a very powerful person. He looked powerful and he spoke, you know, in this very -- You know, of course, his English was not perfect, but he spoke with such force and such -- you knew it was coming from deep down. And he -- Also, he spent a lot of time preparing the things that he said and so they were said very, very well. I mean, he got help -- I'm sure he was helped by people in Seattle, but it just came across very forcefully. And he made an impression on people. And I think that some of the ad hoc study sections on pain that were developed early on were probably -- he was instrumental in getting that done. I mean, he was instrumental in the acupuncture story and in opening up China --

LIEBESKIND: Is that right?

DUBNER: Yes, he made that very early visit.

LIEBESKIND: Oh, yeah.

DUBNER: He went with a number of people, the *New York Times* reporter who became the editor of the --

LIEBESKIND: Reston?

DUBNER: Reston, Jim Reston. He went on -- He was part of that Reston visit.



LIEBESKIND: With Nixon? Because Reston went with Nixon. No, that's not the same.

DUBNER: No, I thought Reston went -- I think he was on a trip with Reston. I don't know whether that was Nixon or not. But he -- You see, you didn't go.

LIEBESKIND: I've never been.

DUBNER: You weren't part of the acupuncture group. But we went a year later. You see, we went a year after Bonica went. Bonica did not join us on that trip. He had gone already the year before, because he was the one who recommended --

LIEBESKIND: What year did you go?

DUBNER: This was in '73 that we went.

LIEBESKIND: You went in '73?

DUBNER: Or was it '74? Seventy-four, yeah, I went in '74. Yeah, I went in '74 and '83. Yeah, I've been there twice.

LIEBESKIND: He had been there --

DUBNER: He had been there in '73, and that was when it first opened. And so when he went with Reston or -- I think he did. He went with non -- with a multidisciplinary group of people from all fields.

LIEBESKIND: Yeah.

DUBNER: He was part of that, John. And then he had -- he set up this -- He proposed that this acupuncture study group go and study acupuncture. And -- now, wait a second. I think I have this wrong. He -- Bonica was with us. No, was he? Isn't that funny? I've lost this. I don't think Bonica-- Because Manny, the person who used to be the head -- He was the head of the anesthesiology at Columbia and then he went to Miami and became head of anesthesiology --

LIEBESKIND: Yes, I know who you mean. With a P.

DUBNER: Papper, Manny Papper.

LIEBESKIND: Papper. How do you remember? The mind is so curious.

DUBNER: Manny Papper was the head of that group. Bonica was not with us. Bonica had gone before.

LIEBESKIND: Was Katz there?

DUBNER: No, Katz wasn't on it, but the fellow from Montefiore was on it and there was an --

LIEBESKIND: A lot of anesthesiology?

DUBNER: There was a few anesthesiologists, and then there was myself and Ken Casey, and Dick Burgess, and Dick Chapman. And who else? There were a few others. We went on that trip. That was a fascinating trip.

LIEBESKIND: That was '74.

DUBNER: That was '74. That was really a fascinating trip because Chairman Mao was still the figure at that time that ran China. And you know, and everything you read was from Chairman Mao's lips. And the people who were running the show there were those who were politically proper.

LIEBESKIND: The Cultural Revolution was strong.

DUBNER: The Cultural Revolution still -- It had a major effect. When I went back in '83, it was totally changed. Outside influences were now much more prominent. And the political -- Being political right at that time -- Being politically proper at that time wasn't as important, though it's always important in China. So that's a little digression, but --

LIEBESKIND: No, I wanted to have you say something about John on this -- you know, at this time. It's not one of my standard questions, but it's --

DUBNER: Yeah, and John was -- For me he was a very important person, because he always supported me. And I, you know -- He tried his darndest to get the Bristol-Myers people to give my lab an award, but they wouldn't do it because they were a government agency and they'd just had some -- the vice president had this policy that they would not give funds to a government agency. Of course, they had given funds to Ken Casey, who was part of the V.A., but they didn't realize that when they did it.

LIEBESKIND: Well, that's part, but he's also --

DUBNER: But he was connected with the University of Michigan. But you know --

LIEBESKIND: You came out okay with Bristol-Myers. [he laughs]

DUBNER: I was going to say that I got the second award and I was very, very proud.

LIEBESKIND: Pat Wall and Ron Dubner. Not bad.

DUBNER: I was always very, very proud of that. And I thank all of my colleagues who were on that committee for making that possible because that was a wonderful thing. And I think I gave part of that money to the IASP --

LIEBESKIND: I remember.

DUBNER: -- because I felt so strongly about the IASP and the fact that I wanted to see young people be able to-- young people get awarded from the IASP, and set up that, you know, that special training award for them.

LIEBESKIND: That was a lovely thing. I remember that, yeah.

DUBNER: And I think part of my doing that was also, you know, because of John Bonica, because I felt so strongly about this organization that he had founded and that I wanted it to continue to grow, and I wanted it to be an important part of people's careers. So I did that. So I have no regrets about any of that.

LIEBESKIND: How you doing?

DUBNER: Maybe we should take a break. Maybe we should take a break now.

LIEBESKIND: I was just going to say maybe this has been a long session.

DUBNER: I think it's been a long session and we ought to --

LIEBESKIND: Let's stretch, all right?

DUBNER: -- take a stretch.

LIEBESKIND: We're going to take a break.

[TAPE RECORDER OFF]

LIEBESKIND: Okay, we're picking-- Sorry. Okay, we're picking up again where we left off. It's now ten after two. We've had some lunch and a swim and Ron, I don't know, why don't you-- As we were saying, why don't you pick up and kind of continue with your story of your lab and the work that you guys have been doing? It's been so varied, so many different things and always at the cutting edge. Well, truly.

DUBNER: Well, it is varied in some respects, but in other respects there is a thread of research goal that does go through the program. That may not be as clear to people.

LIEBESKIND: Maybe you could address that now. We were talking off tape at lunch or at the swimming pool, about you know, that your career is sort of -- your scientific career is kind of a piece, of a whole, that there's a thread that runs all through it. And maybe you could talk about that a little bit now.

DUBNER: Well, I think if you go back to the very earliest work that I did, even the work that I did for my thesis, my Ph.D. thesis, it's work that was interested in understanding how environmental information is processed in the nervous system, and how that information can be

modified by control systems in the brain. And that thread seems to run through my whole career. Some of my work that I did when I came back to NIH in 1967 -- The first work that I did in the trigeminal system --

LIEBESKIND: Let me interrupt you a second. [ADJUSTS DUBNER'S MICROPHONE]

DUBNER: Okay. Even that very first work on the trigeminal system where I was studying neurons in the main sensory nucleus -- I mean, rather than studying their sensory coding properties to tactile stimuli, I also looked at how these neurons responded to stimuli from other modalities. Of course, that probably came from my work that I did in the association cortex and looking at convergent inputs. But I was surprised to find that I could activate these neurons with visual stimuli, and that there was a population of neurons in the medial part of the nucleus that had this convergent property.

And it sort of was the beginning of my thinking that what one sees in anesthetized animals, when one looks at neuronal activity, is just the tip of the iceberg and that there's a lot going on in the nervous system that has to do with the behavioral set in which the organism is at a particular time, whether it be sleep, whether it be wakefulness, whether it be the relevance of various stimuli, some that are important for survival, as well as important for completing various everyday tasks and so on.

LIEBESKIND: Let me throw you one way out of left field and see what you do with this question here. I've always had the feeling that people's personalities are reflected in the kind of work that they do in science in some manner, and that broad people -- I mean, I don't know how to put this exactly, but here we're getting an image of Ron Dubner and the breadth of his, the scope, kind of, of his interest. Maybe I misspoke before when I said, "Well, you know, you've done a lot of different things," and it made it sound like it was dispersed -- and not --

DUBNER: -- focused.

LIEBESKIND: -- not focused. It is focused, but it's still very broad.

DUBNER: Oh, well, there's no question that it is --

LIEBESKIND: There are scientists and, you know, we know them, who have focused on, you know, way down here at a molecular level, let's say, only, and are only interested in that, or in the peripheral nerve, and would never be able to tolerate the complexity of the brain, or even of the spinal cord or something. I mean, do you feel that there -- that this is some reflection of who you are as a person in some way? I can't quite frame my question. But do you see what I'm driving at?

DUBNER: Yeah, I understand what you're saying. I think that's probably true.

LIEBESKIND: And as you look around you at other scientists, do you see some of them being broader and some of them being narrower?

DUBNER: I don't know whether it's broad versus narrow that I see a lot. I see more -- To be a scientist one has to have a certain kind of an ego. You have to have a big ego. I mean, to think that you can discover what nobody else has ever found before requires that you have a big ego. But I think some people who have this ego want to reserve all the credit for themselves, whereas others are willing to share the credit with their colleagues and with their collaborators. And so you can look at various laboratories and -- For example, you can look at the laboratory of a Sol Snyder, you know, and Sol --

## RONALD DUBNER INTERVIEW

### TAPE TWO, SIDE TWO

LIEBESKIND: Okay, yeah, Sol Snyder --

DUBNER: Yeah, and he would say that he only wanted to work with post docs. He didn't want to have any more senior people in his laboratory because post docs were new. They had new, fresh ideas and they imparted all that information to you and made you think in new ways that you hadn't thought about before. Well, all of that is very, very true and I agree with him 100 percent, but I somehow have a suspicion that another reason exists for Sol, and that is that he didn't want to share too much the glory that comes from the discoveries. And if your co-authors are post docs, they usually take a secondary position when it comes to sharing in the --

LIEBESKIND: As opposed to visiting scientists or more senior people.

DUBNER: As opposed to visiting scientists, or my own laboratory where I have a lot of senior people. And they have their own independent programs and they function fairly independently and they get the credit for what they do. However, what is also true is that my influence on what goes on in the laboratory goes much further than you can see from that and I don't get any, and I don't expect any credit from that. And I could give a number of instances of--

LIEBESKIND: Talk about that a little bit because I actually have a question on that. So this answers it without my having to ask about the managerial aspects and how, you know, these people, the more senior people with you, have gotten their independence and, you know, how does that work? Give me some examples. You were going to give me some examples.

DUBNER: Well, they get their independence because if someone is developing as an independent researcher, in order to give them a satisfying experience and to be enjoying their work, and to feel that they are getting their own reputation, you have to give them independence; otherwise, they're not going to stay around.

And there are some people who've come through the lab, who were very, very good, and I would have loved to have them stay around, but there was no way they would ever want to stay in this situation where even though they had a lot of independence, the lab would be known as Dubner's lab, you see. Whereas there are other people, obviously the people who have stayed, who can live with that kind of a designation.

But this is what you need to do. These people need to have their own programs, and have their own independence, and to feel that they build their own personal reputation while they're part of a larger laboratory environment.

LIEBESKIND: Do you find you have to talk to some of the students? I mean, has this been sort of an active process that you've initiated, or is it just, you know, the ones who have to leave, leave; the ones who are willing to stay, stay? Do you in any sense -- I mean, can you think of an

example? I don't know whether it would be M.A., or Gary, or Rick, or whoever, you know, who've kind of been around for a long time where you've had to kind of talk to them about their -- about your feelings about this and their careers and wanting them to have their own -- ?

DUBNER: No, I think the people who have stayed around, it's been very clear from the beginning that I was giving them an opportunity to develop an independent program. And they had this opportunity. They have to take advantage of it. And if they did, they would have it -- They would ultimately get tenure. On the other hand, there are other people where I've made it pretty clear from the very beginning that there was no opportunity. And there were others who along the way I've indicated that the opportunity is not going to be available.

LIEBESKIND: And that really reflected a kind of a judgment on your part at the time?

DUBNER: Yeah, yeah, sure.

LIEBESKIND: A prediction, as it were, that you would have to do that?

DUBNER: Sure, there were people whom I brought into the lab early on, when I did have more flexibility. Now we have so many senior people, the flexibility is greatly reduced, and I really can't think of, at the moment, bringing on any new permanent people until some people leave, which may not happen.

LIEBESKIND: So even if the most brilliant, wonderful scientist came into your lab now --

DUBNER: It would be very, very difficult to convert that person to a tenured position. And NIH has instituted changes which also make it very difficult, more difficult than it was in the past.

But I don't regret having done this about converting people because I always took the position that two heads were better than one, and if I had a bright colleague who I was working with and I would prefer to have that person's input and to treat that person as an equal, and that I would want them to be part of the research program. And that certainly started out with Steve Gobel. Steve Gobel was the first one who was -- though he came through a different channel to get to the NIDR, ultimately he became part of my group and we worked as equals in developing the program.

LIEBESKIND: What do you think would have been his thinking when he left? I mean, why would he have left? He went into grant management.

DUBNER: I think those changes occurred because there were some personal things going on between us. See, one of the problems you have when someone becomes senior is that it's a question of how much independence. And as far as I'm concerned, there always has to be some control of the program that I have.

LIEBESKIND: There can only be one boss.

DUBNER: That's right. Exactly. I always have to reserve some control of the program. And when people become senior, it turns out that in some instances, they can't even tolerate that tiny bit that's left. And I think that to some extent that happened with Steve.

LIEBESKIND: Where would be a domain in which that kind of issue would come up. In other words, might it have to do with a manuscript and whether something is said this way or that way?

DUBNER: No, it might have to do with -- As you said, there's only one boss, and so if one is making decisions about other people, ultimately one person makes that decision, you see. And the other person might not agree with it. Okay, it sometimes also has -- in terms of research direction, I think part of the problem with Steve and I was that I wanted to move the program more into chemistry and pharmacology and Steve was still very much inclined to the classic and anatomical approaches. And I saw that as -- not the past, but less important for the kinds of questions I wanted to ask. You see?

LIEBESKIND: It sounds like he wasn't ready to grow with the field, in a sense.

DUBNER: I wouldn't say ready to grow --

LIEBESKIND: Where a lot of anatomists were growing into this -- the anatomy done through neurochemical techniques --

DUBNER: Yeah, yeah. I would say that it was very hard for him to accept the kind of work that M.A. was doing. He had difficulty accepting that and whether that kind of work could be integrated into his own program.

LIEBESKIND: He got out of research at that point.

DUBNER: He got out of research.

LIEBESKIND: Went into grants management. I was going to ask about Don [Price], because wasn't there a thing where he didn't want to do animal research anymore?

DUBNER: Right, that's why Don left. I mean, Don had a tenured position in the laboratory.

LIEBESKIND: Oh, is that right?

DUBNER: And Don left because he didn't want to do animal work anymore. He said, "Ron, I'd like to stay, but I'd like to do what Gracely does. I want to do work in humans; I want to do psychophysics."

And I said, "Don, I can't offer you that opportunity because Gracely's here and I don't think the program can have two Gracelys or two Prices doing -- or Price and Gracely doing the same thing."



LIEBESKIND: What were his thoughts?

DUBNER: I don't think that was a bad decision because I feel that it was good for him and it was good for the program, too.

LIEBESKIND: When he thought he didn't want to work on animals anymore, it was a -- I mean, it had to do with humanitarian concerns, right?

DUBNER: Yeah, and he's changed, you know. He does do animal work. He may not be at the bench as much as he used to be, but he is involved in animal studies.

LIEBESKIND: But was it your understanding that he really sort of developed a concern for animals' welfare?

DUBNER: Absolutely. He did and he went into -- It was part of his reason to want to do human subject research and patient research. And he has devoted a good part of his career to that. And I must say, I don't take any -- I don't want to take any credit for anything other than, you know, what's -- you know, in terms of the work that I've done and so on. But I think there are a number of people who came through the lab who left who were really strongly influenced by the philosophy that they observed within the program. And that philosophy was a philosophy of multidisciplinary approaches. You ask questions with the techniques that you have available to you and you don't let the techniques dictate your research, and that one should, if one is interested in pain, then pain by its very nature is multidisciplinary, and one has to study it from a very basic science standpoint to a clinical science standpoint. And that is the trademark of my laboratory.

I mean, I think, John, you're very nice to me and John Bonica was very nice to me getting up and saying very nice things about the program, but I think the real trademark of my program -- There are lots of good pain programs, and certainly the program that Allan Basbaum has, Howard Fields, Bill Willis, yourself, etc. These programs are first-rate programs. The difference is that the program that I developed was a program that had a tremendous amount of breadth in terms of asking questions about pain. And I think that is very unique. I think it's--

LIEBESKIND: That's the hallmark of the Dubner program.

DUBNER: Yeah, of this program. And I think that is what was very important to me. Now, I think in some ways I've suffered because of that.

LIEBESKIND: How do you mean?

DUBNER: Well, you see, because the people, the individuals who really get known in a field, who ultimately get into the Academy and so on, are individuals who have remained highly focused and who -- where it's very obvious that their research is going from A to B to C. Okay? When you have a broad program and you ask questions in different ways, and there's a very broad philosophical approach, it's not as evident to people what that string of continuity is

within what you're doing. And I feel that there are people who don't recognize the contributions I've made if they don't see this thread of continuity.

LIEBESKIND: In the same way they would had you followed from A to B to C.

DUBNER: Yes, if I had continued, let's say continued working in the awake monkey and if I had stayed involved and if I had put my resources in my program -- I mean, I had a lot of resources at my disposal, okay? I have divided that resources not only between myself and other people, but also -- But I must say I do control the purse strings. I don't want to give any impression that I say, "M.A., you have X amount of money to spend; Gary, you have X amount to spend." I don't do that. I feel that if I'm going to be able to move this program in the future where I think it has to be done, in discussions with these people, I need to have that kind of control of the grant money, so to speak, and to delve it out on a year-to-year basis.

LIEBESKIND: Well, I want to --

DUBNER: But you see, I wanted to go on with this concept because I think it's important. I think that by making this kind of decision -- Not that I'm a selfless person who's willing to give everything up, but I think in making this kind of decision, I did make a decision in which the work has been identified not only with me, but in many instances with my associates.

LIEBESKIND: Well, certainly over the last ten years, I guess you could say, they have grown up and become, you know, independently major figures.

DUBNER: In fact, that's what happened. It happened. They've become independent major figures in the field themselves.

LIEBESKIND: Three or four of these people.

DUBNER: And they're the ones -- They now -- They're the ones who are basically taking advantage of their reputations and going forward with their reputations.

LIEBESKIND: Now, there's one little inconsistency here I wish you'd address. I mean, you know, you talk about the breadth and you're sort of sitting astride all this breadth, and there's the neurochemistry, and the molecular biology, and the behavior, and yet your people are specialized. I mean, those people, those senior people --

DUBNER: Yeah, they're very --

LIEBESKIND: Those very senior people are specialized. And there must be some -- They must feel some constraint to stay within their domain.

DUBNER: That's not true. Look at -- Gary Bennett is a perfect example.

LIEBESKIND: Okay.

DUBNER: Look at the people who stayed. Look at Gary. Gary started out in the laboratory. Forget about what he did before he came to the laboratory. He started out doing intracellular recordings and staining individual neurons. He went on from -- And he recorded and stained neurons -- the first person to do this in substantia gelatinosa. He went on from there to do a similar study and develop research on the dorsal column post-synaptic neurons in the spinal cord. From there he identified a new pathway from the spinal cord to the mesencephalon, the spinal mesencephalic pathway, okay? And then he went on from there to -- in which he identified this neuropathic pain model. Now, how -- When he developed the --

LIEBESKIND: Which got into some behavioral testing and so forth.

DUBNER: That's right. When he developed this model, his work really became much, much broader. It wasn't just electrophysiology. He got into behavior. He got into pharmacology. He stayed with electrophysiology. And he has really -- And also clinical pain. He now runs a part of the program that has to do with clinical neuropathic pain. He is the immediate supervisor of Rick Gracely and Mitchell Max. He basically has a lot to say about the direction of the work that they do. So he has really -- His program has, in a sense, taken on almost the breadth of the whole lab program, you see. Whereas on the other hand, M.A. has stayed pretty much -- she goes into new techniques, but they're related.

LIEBESKIND: Yeah, they're within her domain --

DUBNER: -- in her domain of neurochemical approach.

LIEBESKIND: -- as it were, rather than going off into clinical or behavioral or --

DUBNER: Exactly, exactly.

LIEBESKIND: Okay, fair enough.

DUBNER: And certainly Rick Gracely, who did a lot of measurement work. He got involved in the neuropathic pain studies and did this beautiful study on neuropathic pain. And so the opportunity to be broader and to go into these other areas does exist.

LIEBESKIND: So, for example, if M.A. were to come to you tomorrow and say, you know, I've stayed kind of within these boundaries for all these years, now I'd like -- without dropping my program -- I would like to try and do some collaborative thing with Mitchell or, I don't know, whatever, you would not discourage her? You would encourage her?

DUBNER: Well, actually, I encouraged her when she wanted to go into the area of molecular biology.

LIEBESKIND: Right.

DUBNER: I encouraged and supported that development of the program in that direction. I mean, there are limits to what I'm willing to do, depending upon what the situation is with the resources and also my view of what the field is. And I mean, I still have that -- that's my --

LIEBESKIND: That's the boss, doing the top-level thinking.

DUBNER: That's my role and that is to try to find where we should be going in the future. And that's why a lot of the work we're doing now is molecular biology and it's not pain. And a lot of the pain people wonder what's happened to the laboratory because they don't hear about what we're doing. But it's there, it's published. It's not published in the journals that they read. And so there is this shift in emphasis. We'll come back. We're already coming back. And we do have a lot of interest in the systems neuroscience, and we're moving back in that direction, but integrating the molecular biology into that in a better way.

LIEBESKIND: Well, the same thing happened in my lab for a period. We were doing all this neuroimmunology and then, as you know, we were able to bring that technique back into the pain thing with this postoperative pain model.

DUBNER: -- into the pain field, exactly, exactly, yeah.

LIEBESKIND: That was good, yeah. Well, just --

DUBNER: So I was also saying -- At one point I was saying that this philosophy that you -- that I have, that is the trademark of the laboratory -- You know, there are people who've left the laboratory, who you can see that this trademark has had an imprint on what they do.

LIEBESKIND: It rubbed off on them; they've carried it forward.

DUBNER: That's right.

LIEBESKIND: Who?

DUBNER: People like Gary Bennett and Cathy Bushnell.

LIEBESKIND: Not Gary Bennett, Gary --

DUBNER: Gary Duncan and Cathy Bushnell, their program in Montreal, which is broad, goes from electrophysiology, to psychophysics, now to neuroimaging to -- When Ron Hayes started out in Richmond, you know, he left the pain field and went into the spinal cord injury, he was running a broad type of program when he did that. Ken Hargreaves who's at Minnesota clearly has a broad approach to the program that he has been developing with great success.

LIEBESKIND: How about your Japanese connection? How are those guys doing?

DUBNER: The Japanese -- They're doing very well. All the Japanese that came through the lab, they have been highly successful, and I'm very proud of them, though their approaches are more focused. Because that's a more -- I think it's more Japanese.

LIEBESKIND: That may come from the Japanese system or something, yeah.

DUBNER: Right, they're much more focused in what they do. So I don't --

LIEBESKIND: It's probably the tight department structure, maybe, more than here.

DUBNER: That's right. Yeah, yeah. But I'm very proud of their accomplishments. They've done extremely well, all of them.

LIEBESKIND: Well, just for the record I want to read a few questions that I have here that you have now answered just because I don't want anyone to think that I didn't think to ask these questions.

DUBNER: Don't worry, John. I'm sure that everybody knows that if I wasn't answering questions you wanted me to answer, you would have interrupted me. Because you do.

LIEBESKIND: But you will be amused to hear some of them.

DUBNER: You have been interrupting me. [he laughs]

LIEBESKIND: I've long said that --

DUBNER: Because we still haven't discussed -- I still want to discuss this thread, this stream of consciousness that runs through my whole career.

LIEBESKIND: Go ahead. Do it.

DUBNER: No, go ahead. You go first.

LIEBESKIND: Well, maybe it'll come up -- Maybe this will back fill a little bit. I've long said that Ron Dubner runs the best pain lab in the world. I've teased, trying to embarrass you by saying this publicly in your presence, but I also happen to feel it's true. What do you say?

DUBNER: I've already told you.

LIEBESKIND: I think you've kind of answered that; I think you've addressed that.

DUBNER: That's not to say that I don't think all the work that we do is excellent. I don't want to give the impression that I think we're a jack of all trades and master of none. I think what's -- What I'm really saying is that what's unique, in a sense, is the breadth, but also the excellence within each area, within each special area. And that can only happen with choosing bright people and giving them an opportunity to run their own show to some extent.

LIEBESKIND: If you had sat too hard on any of these people they would have left --

DUBNER: They would have left.

LIEBESKIND: -- or you would have smothered them even if they had stayed.

DUBNER: That's right, exactly. And they're all excellent, really. It's not 100 percent. Nothing is 100 percent.

LIEBESKIND: Who are the people in your lab, by name, who have these permanent posts? There's M.A. --

DUBNER: Okay, so in the basic science area -- You know, one thing I didn't remember -- Did you ever come to our new building, building 49?

LIEBESKIND: No, I've never seen that.

DUBNER: And that's one thing that I -- by coming out here --

LIEBESKIND: -- we missed seeing that.

DUBNER: -- you missed seeing that. That's too bad. I just realized that.

LIEBESKIND: I wouldn't have given up this experience coming out here, however, but the next time we'll do it the other way.

DUBNER: Yes, okay. Well, M.A. -- Well, there are three section chiefs of the program now.

LIEBESKIND: That's an official NIH designation?

DUBNER: An official NIH designation. Under a lab chief, there are sections and I have set up three sections. I am in charge of one, M.A. is in charge of one, and Gary Bennett is in charge of one of those. And so the permanent people who work with -- M.A. has no other permanent scientists working with her. She has technical support staff, who are permanent, who work with her. She has a number of temporary, non-permanent people who work with her.

Gary, on the other hand, Mitchell, and Rick are both permanent staff. And they have their own programs, but those programs are under Gary's section. Mitchell has a -- really a section on -- a unit called the clinical trials unit. And Rick, though he doesn't have a unit, he has a program. Unit is a designation at NIH that's somewhat meaningless. It doesn't really carry any great stature although you can put it on the bottom of a letter. And Rick certainly is our program leader in measurement, in psychophysics.

And then in the section that I run, there is a --

LIEBESKIND: Why do you choose to run a section? Wouldn't your -- As lab chief, wouldn't you just stay there and have the sections run by different individuals?

DUBNER: Well, then I'd be totally out of interaction in the research.

LIEBESKIND: I presume there are lab chiefs at NIH who do it that other way, but you choose to stay a little closer to the bench.

DUBNER: Yes, yes. It's not a good idea, because then you get even further away. If you're going to get that far away from the bench, you might as well go on to another position, a more total administrative position. Because a lab chief is really supposed to be a research position and the administration that you do, most of it should be research administration. And if you don't -- There are other -- There are lab chiefs who have sections and then they're not a section chief, but maybe they have a small lab that they run with a postdoc or two. I could do that too, but I -- In a sense, my section is larger than that, too. See, like I have a program in molecular biology that Mike Iadarola really is in charge of. But he's in -- But I work on a day-to-day basis with Mike and the people in the section, okay? Then there's the program that Dan Kenshalo runs, which is the awake monkey work, and he is the program leader of that. And I work closely with him.

LIEBESKIND: He's been with you for a long time now.

DUBNER: Yes he has. All these people have been with me a while now.

LIEBESKIND: Ten years and more.

DUBNER: Or close to that. I guess Mike is the -- Well, Mike is probably close to ten years already, yeah. And then there is -- Okay, so that's Mike and Dan and then there's a part of a clinical program that I supervise and Ray Dionne is the person in charge of that. And so -- And then there are a couple of postdocs, usually, that I sort of -- They don't have programs, they're just individual people, and I sort of supervise their work more closely.

LIEBESKIND: So that's how that works.

DUBNER: That's how it works.

LIEBESKIND: Well, that's another question, which is to ask you how -- you know, what have been your managerial challenges? Some of the people have stayed with you for a long time, how have you been able to give them a sense of their own careers and so forth? I think you've answered that pretty well. Another one I think you've also answered, I have here. You have a long list, an honor roll of successful trainees. What do you look for in a student? Do you feel you can predict their success? Maybe you could say at least what do you look for? I mean, somebody comes to you, is it just perfectly obvious the answer to that, or do you have some-- a unique Dubner answer.

DUBNER: I don't have a unique -- I look for someone who's bright and who looks like they're interested in what we're all about and is fascinated by the structure of a laboratory and the

opportunity to work with a number of different people who have major reputations of their own, and to be exposed to this breadth of research in the area of pain. But mainly someone who has a really good reputation as being a bright young person.

LIEBESKIND: Well, let me ask you this: have there ever been any that have surprised you, that when you first saw them you weren't convinced they were really all that good -- gee, what have we got here? But then over the course of a year or so you came to realize they were very good? I mean, have there been pleasant surprises like that? Or unpleasant surprises?

DUBNER: Usually it's the reverse. Usually I think they're going to be good and they don't turn out as good as I thought.

LIEBESKIND: Maybe the motivation is lacking?

DUBNER: The motivation is lacking, right. Yeah, usually I tend not to bring people to the lab that I don't think are going to be successful and are going to be good, and so it usually works the other way.

LIEBESKIND: That's right. Well, I can see where that would happen.

DUBNER: But it's been very, very satisfying. I have, you know, I counted up -- Because we've gone through the -- In the last couple of years the Institute, as you know, the Dental Institute, has gone through a great deal of upheaval. Harald Løe, before he retired, I guess he wanted to put his stamp on the future, and what he did was one, he set up this blue-ribbon panel that was to look at the future of where the intramural program should be going, and, two, he fired the present, at that time, scientific director, and searched for a new scientific director throughout the country. And in doing that we all, as lab chiefs or branch chiefs in the institute, were -- participated in some of this -- these things in a number of different ways. We made presentations to this blue-ribbon panel and so forth. And in doing that, I went back and I looked at the training.

LIEBESKIND: It impressed you, huh? It's impressed everybody else. I can tell you it impressed the Bristol-Myers folks.

DUBNER: And I realized that I trained over -- in the last, I guess, what was it -- ten years, we've trained over seventy people. And a third of those individuals had dental degrees. Because one of the issues that came up was, what is this intramural research program doing for dental research? And so I wanted to make the very strong point that in my program we've trained -- a third of the people that we trained had dental degrees when they came to us. And so I'm very proud of that. That's one of the most. I'm proud --

You know what I'm also proud of, John? I really feel good about -- When I think of all these people, where they are today, most of them are still in the pain field. It's wonderful. It's wonderful that I've been able to train people who must have had a good feeling about what they were doing early on because they've stayed in the field. And that is really very satisfying.



LIEBESKIND: Are there any particular students that you-- or trainees, or however you want to refer to them, that you would single out as being -- I don't know, that's, you know, a great special joy to have worked with or something like that?

DUBNER: Well, people who I maintain strong friendships until today. Barry Sessle for one. I mean, Barry who has really --- he's gone on to become the dean of a major dental school in North America. Very proud of his accomplishments, his ability to do that.

LIEBESKIND: It's very --

DUBNER: What?

LIEBESKIND: Go ahead.

DUBNER: I'm very proud of the people in Montreal, Duncan and Bushnell. They've done very, very well.

LIEBESKIND: Very creative stuff.

DUBNER: I'm very proud of Hargreaves, who really -- who was bright all along and I knew he would never stay because he needed to have his own program. But he has -- He's done so well and I'm very pleased to see that happen. And I don't want to leave anybody out, but there are --

LIEBESKIND: Well, not to mention all the senior people in your lab who have stayed on all these years, naturally, yes.

DUBNER: Well, of course, of course. Bill Maixner there at North Carolina, I'm proud of his work.

LIEBESKIND: Oh, yeah. He's a dentist.

DUBNER: Yes. And then there's, you know -- I was thinking of people who have -- I was thinking of somebody in particular and I can't come up with it. Maybe it'll come back later on. Too much lunch, John.

LIEBESKIND: Well, it's a lot of talk, too. We've been at it a long time. But we're going strong, I feel. I hope you feel good still.

DUBNER: I feel fine. No problem.

LIEBESKIND: What's it been like spending your whole career at NIH? There must be advantages to not -- and obviously, some disadvantages too. Have you missed not having a professorial appointment, a teaching appointment, for example?

DUBNER: I did have that for a while. You know, I did teach for a while at Howard University, but then I gave that up.

LIEBESKIND: I don't think -- I actually didn't know that. How long did you do that?

DUBNER: For about ten years. Yeah, I used to teach a course there in the dental school on oral physiology. And yeah, I think that's something I've missed out on, the professorial, the academic environment. NIH is not an academic environment and never will be. I know there are people who think they can make it into more of an academic environment in terms of -- like a university, but that's not -- It never was meant to be that and I don't think it ever will be.

But I guess the most satisfying thing about me for working at NIH is that I got into research after I had a dental background. And I -- When I did my -- After I completed my Ph.D., there was a while there when I was looking around for alternative opportunities. I wasn't sure I wanted to stay at NIH or that the opportunity would remain there for me at NIH. And so I was looking for other opportunities, and what I found out as I looked for other opportunities, was that most universities wanted me to be their dental expert because of my background, and I didn't want that. I wanted, as I said earlier, I wanted to be considered equal to everyone else in the department in being able to choose my graduate students.

LIEBESKIND: That's a strange -- I hear you, but that's a strange business because the other side, you see, of the coin is to say, well, you so sit astride your field. I mean, there is no one in physiology and dentistry, you know, that --

DUBNER: But I'm not a dental physiologist, I'm a physiologist.

LIEBESKIND: You're not, but I mean --

DUBNER: I'm a neuroscientist.

LIEBESKIND: But if you're running a dental school or you need to make an appointment that has something in relation to dentistry, who are you going to think of? You know, you're going to think of Ron Dubner, so even though --

DUBNER: Well, I'm just giving you -- Let me give you this as the past of what has kept me at NIH, and that was that I felt that these other opportunities, that I was being pigeon-holed and I didn't want to be pigeon-holed. Whereas, at NIH I felt that I was treated as an equal, just like anybody else at any other university.

LIEBESKIND: Part of a community of scientists.

DUBNER: I was part of a biomedical research community. That was the environment that had been created by Sy Kreshover, an environment that we were regular equals of everyone else on that campus, and the intramural program in the Dental Institute was not a partisan program that recruited dental scientists. It was a program that recruited the best scientists.

LIEBESKIND: There is this --

DUBNER: So let me-- Let me interrupt you, okay?

LIEBESKIND: No, no, don't worry.

DUBNER: Because my thinking is changing somewhat.

LIEBESKIND: Oh?

DUBNER: And my thinking is changing in that, if I was to accept any new challenges for the future, that that's where I could probably really do something special. And that is to go back to my beginnings, to go back more to, let's say that if I was to choose an academic position it would better be -- I should choose a position in a dental school, where I could do something very special for the future of dentistry.

LIEBESKIND: Because you would carry such a big stick, being who you are.

DUBNER: But that's because I am in my position today.

LIEBESKIND: You are the king of the field.

DUBNER: Well, that --

LIEBESKIND: Well, that's right.

DUBNER: Okay. So there I have an opportunity now in terms of a possible future challenge is to look towards those types of challenges for the future, rather than if I was to leave NIH to take a position as a chairman of a neuroscience department, or chairman of a center for neuroscience research, or something like that. There I would not -- I would be unique in terms of my experience and my reputation, but I would not be unique in terms of what I could do in the special environment. So that's a change of my thinking.

LIEBESKIND: I understand that. That makes a lot of sense. That really addresses that question, which is --

DUBNER: You have any other questions? Do you want -- ? That you haven't -- Do you want me to go on to my stream of consciousness story?

LIEBESKIND: Yeah, do. I've got a lot of other questions.

DUBNER: Okay, well, you can -- You'll have time to ask them.

You know, the stream of consciousness, I think one can -- And this is why I've thought about writing a book. One of the phrases that I've used -- I don't know whether it's my own phrase or I heard it somewhere. I mean, to study pain is to understand the brain. And I think that's really true. I think that pain is such a complex, complex sensory experience that to study pain is to learn how the whole brain works, because it not only involves the sensory qualities of a particular stimulus, but it involves how that stimulus is perceived, or based on your previous

experiences in pain, your previous memories, the situation that you're in, your motivational set, how attentive you are in a particular time. And also, certainly, motor systems are accessed by pain sensory pathways, obviously in order to escape from something that's life threatening.

So really to understand pain is really to study the whole brain. And my major -- What I have focused my attention on is this plasticity that the nervous system exhibits. And I guess the real exciting time, when I really recognized how vivid this was, was the awake monkey experiments that we talked about before and the task-related responses that we discovered in the awake monkey. Before those experiments, I recognized the plasticity. I saw that there were areas of the brain that presumably were related to coding of tactile information of the face and neck, but yet these areas had -- there were neurons there that were responding to stimuli of other modalities. And I didn't quite understand what all that meant, but it meant to me something about the interconnections between our sensory systems and how those connections relate to how we respond within our environment, okay? And the awake monkey experiments made that more vivid. And so we --

LIEBESKIND: You keep coming back to those experiments. They really are your favorite experiments, the awake monkey ones.

DUBNER: They are my favorite experiments.

LIEBESKIND: They sort of define you, in a sense, don't they?

DUBNER: Yeah, they do.

LIEBESKIND: They symbolize, let's say, not define you so much as symbolize.

DUBNER: Well, you see the other -- And then we went -- We went on to, you know, working out circuitry, that was with the awake monkey, and also in anesthetized preparations where we did the intracellular HRP work. I was involved in that work and when we did the correlation of anatomy, intracellular HRP, the electrophysiology and the immunocytochemistry, where we were able to identify serotonin content in substantia gelatinosa neurons. And those were also very exciting experiments. And they also talked about the malleability, the modifiability, of the nervous systems by descending control systems. Something you know, obviously you've been associated with, for most of your career. And so those were very important -- And of course, the relationship to the opioids and the opioids in the spinal cord. We did a lot of work on opioids in the spinal cord.

Then the next thing that happened was the development of these injury models. And when I saw the injury models, the inflammation models, and I realized that this was a tremendous opportunity to really focus on the plasticity in another way, as opposed to the awake monkey preparation which is very, very difficult. And so I wanted to develop those programs in the laboratory.

So that's how Mike Iadarola got to the laboratory, because he had been working with the inflammation model, using complete Freund's adjuvant as the inflammatory agent. And when Ken Hargreaves came, we started working on the carrageenan model in the laboratory. And I

saw as far as the nerve injury models -- In '86 we had just begun to think about these models and I went off to Israel for three months to --

LIEBESKIND: You worked with Marshall.

DUBNER: I spent some time with Marshall, and I spent some time --

LIEBESKIND: Marshall Devor, for the record.

DUBNER: Marshall Devor, and I spent some time with Ze'ev Seltzer, and with Yair Sharav. You know, clinical stuff with Yair Sharav, and the basic work with -- very basic -- with Marshall, and this behavior model stuff with Seltzer.

LIEBESKIND: Sharav, how do you spell that?

DUBNER: S-H-A-R-A-V.

LIEBESKIND: First name was what?

DUBNER: Yair, Y-A-I-R. And before I left -- I remember this distinctly -- Gary and I were talking and Gary was saying, you know, where is he going to go now? And we talked about this and I said, "Gary, there's one area." I said, "This area of nerve injury, there is no good model. We need a good model of nerve injury. This autotomy model stinks and we really need to get into something -- get a new model that works."

And so he started thinking about that. And just before I left, he said, "Ron, I may have something. I may have something." He said, "I just did a couple of animals." And he described it to me a bit, but you know, "It's just the very beginning, I don't know what it's going to be." Okay?

LIEBESKIND: The rest is history.

DUBNER: A guy who'd been doing autotomy models for a number of years was Ze'ev Seltzer, and I said to Gary before I left, you know, "Don't tell me too much because I want to work with Seltzer. I want us, Seltzer and I, to see if we could come up with a nerve injury model that works." And so I went off to Israel, and that's what I did, Seltzer and I, and we came up with this alternative model, this model where you cut part of the nerve. And the animals developed hyperalgesia slightly different from Gary's CCI model, but basically another good nerve injury model.

LIEBESKIND: What's the CCI stand for? I'm forgetting.

DUBNER: Chronic constriction injury. And so-- And if-- There's no question that the CCI model, Gary Bennett's model, has led to a whole new area of research where people have worked with that model, and they have developed new, other models like the Galveston group.

The Galveston group has picked up on nerve injury; and they developed -- [Jin M.] Chung in that laboratory has developed a model of sectioning nerves that go into the different --

LIEBESKIND: Ganglia?

DUBNER: The ganglia, into the L4, 5 and 6 ganglia. And that model works very well, too. So that -- To get back to my stream of consciousness story, so here we have a whole bunch of models that can demonstrate the plasticity of the nervous system, because we see at all levels, at the molecular level that there are changes in gene expression associated with these models, and we see it at the electrophysiological level. There are changes in the response properties of these neurons. Okay? We see at the neurochemical level, where there are changes in the sensitivity of neurons as well as axons, two different types of neurochemicals under these conditions, so that this is really an extension of this whole idea of neuronal plasticity that began early on.

And that's why I've become so interested in molecular biology, because this is another level at which one can understand how the nervous system responds to this change in signal. And that -- And now we're into that phase of understanding of the nervous system where signal transduction is a major area of importance, where we go from receptor activation into second messenger systems, into changes in gene expression through phosphorylation of transcription factors and their regulation of target genes.

I mean, when I started in neurophysiology, we talked about the convergence of input upon the motor neuron. As a student at Michigan I read -- I have the original copy of the book -- of [John Carew] Eccles' book on the physiology of the nerve cell, which if you see it, you'll -- I've underlined almost every word in the book. It was really a fascination for me to understand how this processing took place in terms of convergence of synaptic input. Well, now we're talking about multiple transcription factors acting in the promoter region of individual genes, and how these genes may be turned on under different situations, by different circumstances. So it's a whole 'nother level of, in a sense, interaction and convergence, and that's why it excites me.

LIEBESKIND: I think I can anticipate your answer to this question, which just comes to my mind. You know, a number of people seem to feel the tension between molecular neuroscience and systems. Let's stop this for a second. [TAPE RECORDER OFF]

DUBNER: Well, you say that I'm self-analytic, that I can enunciate my vision and that I'm aware, self-aware of myself. I wasn't always this way. It's taken time to develop and I must say I owe a lot of this to Mary Ann, because Mary Ann being a clinical psychologist, and being involved in being able to express one's feelings, and express who you are, and how you feel about different things -- I've had to do that throughout my marriage.

LIEBESKIND: She's forced that on you.

DUBNER: She's forced me to do that and I think it's helped a great deal for me to develop my own thinking about myself and where I came from, where I am now, and maybe where I'd like to go in the future.

LIEBESKIND: That's great, and I'm sure that's true. [he laughs] I get the same from Julia, who is not a psychologist, who's been shrunk a bit -- She demands that kind of --

DUBNER: Yeah, Mary Ann demands that, too. I have to express those feelings. I can't just keep them inside, and not talk about yourself, and not say what's on your mind. Because if you don't say it, your behavior shows it.

LIEBESKIND: Exactly.

## RONALD DUBNER INTERVIEW

### TAPE THREE, SIDE ONE

LIEBESKIND: Let's see. Here we go. Yeah, we're on. Okay, you ready for a question?

DUBNER: Sure, why don't you ask some questions now?

LIEBESKIND: All right.

DUBNER: I think I've sort of gone through my stream of consciousness.

LIEBESKIND: I know. You know, just before the phone rang, I was in the middle of trying to phrase a question and then Janeti called and we didn't get back to it. I started by saying I think I know how you're going to answer it, but let's see. A lot of people seem to feel a tension between molecular neuroscience and systems neuroscience, as if the good old days are gone, now everything's molecular, and it doesn't mean anything. How would you answer that? I mean, you seem to have integrated molecular --

DUBNER: There is a tension. The tension does exist. And I think that it's very difficult for molecular biologists to understand systems approaches, and the reverse is equally as true. Systems people don't understand molecular biology. I think it's remarkable that the Society for Neuroscience has been able to stay as one society with such diversity of research from very molecular to very integrative systems neuroscience.

One of the things that Harold Varmus at NIH is trying to do, he's trying to develop groups, interest groups in each particular area. So the neuroscience group divided. It divided into the molecular biologists where Ron McKay and Michael Brownstein are sort of -- were responsible for that. And then the systems group where Bob Wurtz and Bob Desmond from NIMH [National Institute of Mental Health] have been leading that. I think it's a mistake, and I have -- When we've had meetings I've gone to both meetings and I have repeatedly said, "I think we ought to bring these groups together. We ought to try to make molecular biology more understandable to the systems people and vice versa." But I've gotten nowhere. I've gotten nowhere. They don't want to hear about it. I think that Bob Wurtz would be interested, but the molecular people, they're the ones that I think are -- I think if you gave some tutorials in molecular biology to systems people and let them get some glimpse of what's going on, I think they would be receptive, but I don't think the molecular people want to --

LIEBESKIND: Why is that?

DUBNER: -- retool themselves and train themselves in systems neuroscience.

LIEBESKIND: Why is that? What do you think that's all about?



DUBNER: I think it has to do with the state of the art today, and that is molecular biology is king, and therefore, when you're king, why do you want to waste your time learning about what the other people are doing?

LIEBESKIND: Common matters, when you're royalty.

DUBNER: Yeah. And it is king. It has been king. At least at NIH, molecular biology has been king for quite a while. And now we have a Nobel Prize-winning NIH director who is not really changing that. I think he may give lip service to systems areas, and he may give lip service to clinical areas, but there's no question that he views science from a very molecular point of view, and that he sees that the advances in the future are going to mainly come from this approach.

I agree with that to some extent. I agree that the most -- In my opinion, the most important things we've done clinically have been due to our understanding of what's going on in the basic neuroscience area related to pain, our own research as well as other people's research. But at the same time, you've got to be able to make the transition. You've got to be able to say that these are questions that we now need to address in a more systems way both in animals as well as in humans. And I don't think the molecular people are willing to make that transition, make that jump.

LIEBESKIND: So you've answered my question by talking about the world at large, but I think really what I meant was more Ron Dubner and --

DUBNER: Ron Dubner, what question?

LIEBESKIND: Well, this question of the tension between these poles, and what I thought --

DUBNER: There's no tension for me. I can --

LIEBESKIND: That's what I'm trying to say.

DUBNER: I can work equally well in both camps. I can feel comfortable.

LIEBESKIND: Exactly. You feel comfortable in both and you use the --

DUBNER: But I am not a molecular biologist. I know systems neuroscience better than I know molecular biology. But I know enough about molecular biology that I can read the literature, and I can understand the work. I'd have difficulty going into the lab doing the experiments, but I'd even have difficulty going to the lab today doing my electrophysiology experiments. I don't work at the bench and I haven't for over, you know, ten -- at least going back into the middle eighties, you know. When I came back from Israel, I really didn't work at the bench after that.

LIEBESKIND: With that crowd of people you have I couldn't imagine how you could, you know. I don't know how you could do that.

DUBNER: Well, sometimes I regret it.

LIEBESKIND: Not to mention your other responsibilities.

DUBNER: Sometimes I regret not being able to do that. But it -- You can't.

LIEBESKIND: For someone who likes to carve chalk.

DUBNER: You cannot do it; you cannot run a program of this diversity and size and spend your time half of the week at the bench. It just can't be done.

LIEBESKIND: I agree.

DUBNER: So you make decisions in your life on where you're going to be.

LIEBESKIND: Yeah. What have been your experiences as the co-editor of *Pain*? Now, you took this position over, what, two years ago?

DUBNER: No, it's now four years.

LIEBESKIND: Four years ago.

DUBNER: It's four years that I've been doing this. That's been a very satisfying experience for me. I made that decision that I would like to do that. And --

LIEBESKIND: Tell me about that.

DUBNER: Well, I mean, you know, Pat had decided that he wanted to have a co-editor and --

LIEBESKIND: Why did he feel he --

DUBNER: He came to me as one of the people that he was thinking about to be a co-editor.

LIEBESKIND: Why did he feel he needed one? That's one of my questions.

DUBNER: I think he wanted to cut back. I think Pat wanted to cut back. He wanted to have more time to do other things, so he wanted to have someone to share the responsibilities. So he wanted an American. His idea was to have an American office and a London office. And I was one of the people he was thinking about. And at that time I also had the opportunity to think about running for president of IASP. And people were approaching me about that, and he was approaching me about becoming the co-editor, and I had to make a decision because I couldn't do both. I couldn't run--

LIEBESKIND: Why?

DUBNER: I couldn't become president of the IASP and also be the co-editor of *Pain*. There would be much, much too much responsibility.

LIEBESKIND: Too much to do, you felt?

DUBNER: And too much centralization of the society's -- Too much control in the hands of one individual of the society. So there was no way that that should ever happen. And so I made the decision, and my decision was that I wanted to be co-editor of the journal *Pain*. I saw that as that I could probably -- Well, that I could do more for IASP in that position than I possibly could as the president.

LIEBESKIND: And over a longer period of time.

DUBNER: And over a longer period of time. Because I felt that more could be done with the journal. I felt that, you know, Pat had done a wonderful job up to then -- and he still does a wonderful job -- but that the influx of new ideas and new approaches was important. And so a lot of the new things that have taken place in the journal have been at my -- not that Pat hasn't been part of it; he's been part of it the whole time --but, you know, like changing the cover and changing some of the other things that we've done, it's been that, you know, I've been the one who's pushed these. And I think they've helped the journal. The journal is doing extremely well. The journal is, no question, the leading academic journal in the field. It has a high level of acceptance in the whole field of neuroscience. It's one of the higher impact journals in the field of neuroscience.

LIEBESKIND: In terms of citings? Citation analysis?

DUBNER: Citations, yes. And I personally think -- and other people have said this to me -- that the papers that are published have improved over the years. I'm not saying that's just me. I'm not saying that. But I think that this has happened, and I think in some ways I've contributed to that, and so I'm very pleased with that. And Pat and I get along very, very well. We have no problems. We each run our separate offices and it works out extremely well. It's surprising how well it works out, having two --

LIEBESKIND: Where would you two need to make some joint decisions?

DUBNER: Well, on some of these policies about like I talked about changing the format and changing the cover. We discussed it.

LIEBESKIND: How about just in terms of number of articles or something? I don't know. I mean, what if -- I mean, just to make a ridiculous example, what if, you know, he started accepting gazillions of articles and was crowding out the --

DUBNER: Well, we do work very closely on that. Every six months we evaluate our acceptance rate to make sure that we're staying within the same range. We're pretty close. He actually --

LIEBESKIND: Does he use European reviewers and you use -- ?

DUBNER: No, we use the same -- There's a group of associate editors, and we each -- We always use one reviewer as an associate editor, and then we choose someone else for the other review. So we always have a common reviewer in both offices all the time. So that, I think, leads to some --

LIEBESKIND: And how about if you were to pick a reviewer? Would you look first to an American because your papers are coming from America, or might you just as readily go to England, or France, or Germany, or Scandinavia?

DUBNER: Well, I must say the geographic part of it is not the most important thing for me. I choose reviewers on the basis that they write good reviews.

LIEBESKIND: How about Pat? As far as you know, does he do the same?

DUBNER: I don't know. I don't know. I mean, we're both forced to choose one reviewer that is common to both offices.

LIEBESKIND: That's right.

DUBNER: The second reviewer, I don't know what his criteria are. I've never really --

LIEBESKIND: There's certainly no policy, is what you're saying.

DUBNER: There's no policy. There's no policy.

LIEBESKIND: -- that says that the American office should choose American reviewers.

DUBNER: That's right. And my policy is to choose people who are critical, write good reviews, and who send them back in a reasonable amount of time. I drop people -- I don't send requests for reviews to people who take a long time to review them.

LIEBESKIND: Do you keep a sort of an analysis of reviewers in the office?

DUBNER: Well, I keep it in my head.

LIEBESKIND: Oh, just in your head?

DUBNER: I could do it on-- I have all the information, but it's obvious.

LIEBESKIND: You know.

DUBNER: You know who's responding quickly and who's not. You know who's critical and who's not critical. It's actually a rather simple job to do. I would say 90 percent of the time is the -- Well, that's the secret, is picking the reviewers. If you pick the reviewers based on certain criteria, that you know they're knowledgeable in the area, you know they'll give you a good critique, it turns out 90 percent of the time the two reviewers say the same thing. It's almost

unbelievable. It's so rare that I get a divergence of views on a manuscript; rare. It makes it very simple because --

LIEBESKIND: That's very comforting.

DUBNER: It is. And also it's a question of -- And not only -- Yeah, and they rate them about the same level, too, and so it's very simple to decide whether a manuscript should be accepted or not. It doesn't take a lot of work to do that.

LIEBESKIND: Are you finding that it's any -- Has there been any change in the ease with which you can get people to do reviews for you? Have -- Are people still as cooperative on that as they used to be?

DUBNER: Yes.

LIEBESKIND: You haven't observed any change in that respect?

DUBNER: No, I haven't observed any change. What I'd like to institute -- and I've talked to Pat about this, and we've done this a little bit -- I want to change the reviewers more often, because the idea being that what you'd like to do is to let people who review for you know that if they review for *Pain*, eventually they're going to be on the editorial staff. They're going to be on that masthead. I think that's important to people.

LIEBESKIND: Yes, it is; extremely important. I think there should be a lot more turnover in editorial boards in general than there is.

DUBNER: Yeah, I agree. I agree.

LIEBESKIND: I mean, I was on *Brain Research* for many years and was asked finally to rotate off. And that seemed very appropriate to me, you know, and I think they should do more of that with other journals.

DUBNER: And I think the editors should rotate off. I mean, I have Louisa and John Loeser that they should decide whether they want new editors of this journal as we approach the new time period for the new contract.

LIEBESKIND: How long have you been on now?

DUBNER: Four years.

LIEBESKIND: Four years, well.

DUBNER: So another year, five years, that's not an unreasonable time to be an editor.

LIEBESKIND: No, it's certainly a reasonable time to look at it again.

DUBNER: And I don't want -- That's right. I'd like to stay on, but I think the society should make a decision on that. There should not be --

LIEBESKIND: That's right. There should be a term and it should be looked at.

DUBNER: It should not be automatic that one stays on as the editor.

LIEBESKIND: Exactly, exactly. Okay, good. Well, that's -- Would you consider running for the presidency of IASP sometime down the road if -- after you've given up the editorship, or by that time might it be too late?

DUBNER: Well, John, you're getting into a question of longevity, because if you look upon it now, if I was to run -- If I was to stay on, let's say, to be editor for another four years, okay, or five years, that means that I wouldn't run for president -- Well, maybe I'd run for president in '96, but then I wouldn't become president until '99, right?

LIEBESKIND: That's right. So you're getting up there in the sixties and pushing seventy.

DUBNER: In the sixties, yeah. I don't know whether I would really want to take on that responsibility at that point. Though we have had presidents of IASP who've been that old. I think Ulf [Lindblom] was in that range and --

LIEBESKIND: Ainsley [Iggo] was pushing it. He just turned seventy, Ainsley.

DUBNER: Yeah, but he was president when? How long ago was he president?

LIEBESKIND: Ten years ago.

DUBNER: Ten years ago. So he was about my age now when he became president.

LIEBESKIND: Right, that's right.

DUBNER: So if I didn't become president for six more years, I'd be sixty-six. So I --

LIEBESKIND: But it would certainly be a position that would be a position that would be attractive to you under circumstances if you could take it. I mean, obviously --

DUBNER: Yeah, under the proper circumstances, yes, I'd be interested in it.

LIEBESKIND: I mean, you were given quite a -- I mean, a hell of a choice. I mean, those are two incredible positions.

DUBNER: They were two incredible positions. I was pleased that I could have that choice. That was very nice.

LIEBESKIND: Absolutely.

DUBNER: I don't know. As I was trying to say -- Maybe it wasn't very clear, but that would be a challenge, to be the president of IASP. I could tell you some of my ideas on where IASP should go. I don't think they'd be very acceptable to most people.

LIEBESKIND: Really?

DUBNER: Yeah.

LIEBESKIND: Tell me! I'd like to hear them.

DUBNER: Well, I think that in some ways IASP has outlived its usefulness. And that is because we now have such strong national chapters in the pain area. But I think IASP should move more into being the alliance group, like there are other --

LIEBESKIND: Like a federation.

DUBNER: A federation. An international federation that brings together the domestic societies, and doesn't have all of these individual programs of its own, and maybe does a lot more of things like awarding money for traveling lectureships, for fellowships for people to come to the federation meeting that takes place every three years, rather than spending its money on its own committees. It should maybe change its direction. Maybe it should just focus on publication and awarding fellowships to people to come to meetings, which would be educational.

LIEBESKIND: Just to argue with you, but there are certain -- I would think most of the committees -- In disagreement with what you just said, that most of the committees of IASP are international in nature. That is to say that their purview is of an international -- Not their make-up, naturally. That'd -- But I mean, just for example, this history task force that Loeser set up. I mean, you wouldn't want a history task force that was simply national, or the taxonomy. We had the new taxonomy book. I mean, shouldn't that have international representation to the people who were creating that taxonomy?

DUBNER: Yeah, but you don't need to have -- You could still do that by setting up a committee that was international that did a publication. In other words, I think -- And I don't know -- There are people who probably won't -- who would agree with this to some extent. In other words, the IASP should focus its attention more on publications and generating materials: books, pamphlets, both at the lay level as well as at the scientific level.

LIEBESKIND: Now that's certainly an area that they have been remiss in. That was the whole IPF [International Pain Foundation] thing, you know.

DUBNER: I think one of the most important -- Yeah, one of the most important things that we need to do in the pain field -- and this is getting away from this topic a little bit -- is that I am firmly convinced that the only -- the best way to change practicing approaches by physicians, and dentists and nurses, is to have their patients require it of them, insist that they do things. And the only way that patients can learn what to tell their doctors they want them to do is if

they're educated about it. So I want to educate the public. And we've talked about this, and I've talked about this with the May Day [Foundation] --

LIEBESKIND: Right, Fenella [Rouse].

DUBNER: Fenella. And the best way to do this is if the person -- someone is going to have surgery and they go to their doctor and they say, "Look, I read in this bulletin published by this International Association for the Study of Pain that I should not have any pain after my surgery, that I should be free of pain, that there are now approaches that I don't need to feel pain, and that I shouldn't be concerned about taking any opioids, because if I'm a healthy person and I want to not feel any pain, I shouldn't worry that I'm going to become addicted to this drug, because I won't become addicted to one, or two, or three administrations of this drug."

LIEBESKIND: "And that if you, doctor, don't know how to manage my pain, would you please call in a consultant?"

DUBNER: "Would you call in somebody or would you put me in touch with somebody who will manage my pain?"

LIEBESKIND: That's fabulous. I love it.

DUBNER: And I think -- I don't know if we could do it ourselves, but certainly someone needs to do that. I think the time is ripe for that. I think we have enough knowledge, enough approaches for clinical management today, that we need to insist that doctors learn how to treat pain problems. And of course, with what we know about the nervous system and the insidiousness and the pathophysiology that can result from persistent pain. It's absolutely essential that people learn this and reduce or eliminate pain whenever possible.

LIEBESKIND: You know, naturally, I agree with you 100 percent.

DUBNER: Yeah, I'm talking to the converted, but I wanted to --

LIEBESKIND: It just seems to me that what's -- You know, your focus here is on education of the public.

DUBNER: Yes.

LIEBESKIND: And it's what so many other people seem to fear. Like, "Oh no, that's much too big. We shouldn't aspire to try and do that." And that's what Fenella Rouse has said, you know, that, "Oh, yeah, May Day is not there. Oh, no, we're not going to get into that because that's too big a task. We can't deal with it." And it seems to me that's exactly why it should be dealt with.

DUBNER: Well, let's think back to our beloved leader who passed away. When he -- In 1972, someone said to him, "How the hell are you going to bring together all these basic and clinical scientists? They don't speak the same language. What are you wasting your time for, trying to



have a multidisciplinary society that brings together all of these people?" I mean, that's one of the things that's not in your editorial.

LIEBESKIND: That it --

DUBNER: That we need an initiative to educate the public about needless pain.

LIEBESKIND: Oh, no, it's very much in there.

DUBNER: Is it?

LIEBESKIND: Oh, yeah. There are several of those ten items that are focused on public education.

DUBNER: Are they?

LIEBESKIND: Yeah.

DUBNER: I'll have to reread them, okay? In light of what we've said. I will. You have plenty of copies here for me to read.

LIEBESKIND: I certainly do. [he laughs] No, you're just forgetting. There are several.

DUBNER: Yeah, I'm forgetting. I'm forgetting. Obviously, you're right and I'm wrong on this. Yeah.

LIEBESKIND: At John Bonica's home the day of his funeral, when I walked in, one of the first people I saw was Linda Bonica. And I can't remember who was with me. I think Louisa was and I don't think you or John Loeser were. You'd already --

DUBNER: We arrived later.

LIEBESKIND: Yeah, yeah. And Linda said at that time -- We were talking about the IASP or something, and she said, "You people got the best of him." And what I understood that to mean was not that we bested him, not that we -- You know. But that we got the best there was of John Bonica. We got his full attention in a way that the family did not, you know. And I think she was expressing some -- You know, and we've sensed that in the other children and some of the remarks that John Bonica made -- Johnny Bonica made at the funeral and so forth. And you and I have talked about this a little bit.

How about Ron Dubner? How have you managed, in your life, to balance your work? You've obviously worked very hard. How do you manage to balance your home life and your work life?

DUBNER: Well, I don't know what my children would say. I don't know what John Bonica would say if he was asked this question, you know, but I think I've done a pretty good job of balancing my life. I feel that I have given a lot to my children, that my children have been a very important part of my life -- I feel that I have been a father to my children. I've been there for

them when they, you know, they needed me. Granted -- I think it's not only being there; it's also personality. I mean, I'm not the easiest person. I have my own prejudices and I can make demands, and I'm sure just looking at --

LIEBESKIND: I'm shocked to hear you say this, Ron. I thought you were just a complete pushover, myself. [both laugh]

DUBNER: Yeah, yeah, yeah. And I'm sure that has had an effect on my children that they -- I mean, just viewing my life and what I've done, must in some way influence, you know, their thinking in terms of my own work ethic and my devotion to my work. But when I look back over time, I think very early on I made a decision that I needed to give space for my -- I had to have space for my family. It wasn't there originally. I mean, I know when I was at Michigan working on my degree, and you commented, finished in three years. Well, you only finish in three years if you were working quite a bit of the time.

LIEBESKIND: And you had -- Susan was born at that time.

DUBNER: Susan, before we went to Michigan, and Andy was born at Michigan. In fact, I can remember it was a snowy evening, taking Mary Ann to the hospital to give birth to Andrew, my second child. And I said to her -- And I was in the midst of studying for my prelim exams. And I said to her, "Well, you really are going to have a baby, aren't you?" [he laughs]

LIEBESKIND: When was Susan born?

DUBNER: Susan was born in 1960, and Andy in '63, and Julie in '66.

LIEBESKIND: And Julie in '66.

DUBNER: So when we came back -- Actually, about the time -- When Mary Ann went back to school -- I mean, obviously she couldn't have done that without my cooperation. It was about that -- I mean, she went to get her Ph.D. It was at that time that I --

LIEBESKIND: When was that again now?

DUBNER: Sixty-six.

LIEBESKIND: She started back in '66.

DUBNER: She started back on her Ph.D. in -- it must have been in '65.

LIEBESKIND: Yeah, before Julie was born.

DUBNER: Yeah, before Julie was born.

LIEBESKIND: Where did she do that? Here in Washington?

DUBNER: At the University of Maryland.

LIEBESKIND: At Maryland. In clinical psychology?

DUBNER: In clinical psychology. And so, I mean, I began to take a much more major role in taking care of my children. And I never regretted it. I spent much more time with them.

LIEBESKIND: They were still young at that time.

DUBNER: And they were still pretty young, and we did a lot of things together. As I said, I used to make dinners for them, you know, two or three nights a week. And then on the weekends I used to work -- Early on in my career, I used to work six days a week. I dropped back to five and half, and then, ultimately, I stopped going in on the weekends. I mean, there came a point where I said, "I need to have my weekends. I'll work twelve hours a day, but come the weekend, you know, I'll work at home if I need to do something, if I have some time for just sitting around." But I wanted to spend this with my family.

And all of that came about in the late sixties, and then certainly when we went to London. As I told you, every time they were on vacation, and even more so than that -- We had an *au pair* who lived with us and Mary Ann and I made side trips. Sometimes the *au pair* -- There were certain weekends that she had to take care of the children, and those weekends we would head off to Paris or some other short trip for the, you know, three day weekend. So, I'm sure I'm not --

LIEBESKIND: Your kids are all over the country now. Susan is the one in Sacramento?

DUBNER: Yes.

LIEBESKIND: And she's the one that just got married here.

DUBNER: Right.

LIEBESKIND: And what is she -- ?

DUBNER: That was a wonderful wedding. John, if you turn around and you look beyond the pool, we had this magnificent tent set up there.

LIEBESKIND: The *chuppah*.

DUBNER: It wasn't a *chuppah*, it was a tent.

LIEBESKIND: A tent, all right.

DUBNER: It was a real tent. And that's where we had the reception. And then to the right of that, going down just to the right, just -- Do you see where the woodpile is?

LIEBESKIND: Yes.

DUBNER: Just beyond the woodpile we set up chairs and that's where the ceremony was.

LIEBESKIND: Oh, I see.

DUBNER: And it was a magnificent fall day. The sun -- The sky was perfectly blue. It was clear all day, and this little -- The dryness came into the air, the humidity went down. And she wanted to be married at sunset, and she was married at sunset. It was a magnificent sunset.

LIEBESKIND: Isn't that lovely?

DUBNER: So it was a lovely wedding and everyone had a wonderful time. And we had a very nice -- It was her choice; it was a band called Little Red and the Renegades. It's New Orleans-style music. You know what I'm talking about?

LIEBESKIND: Yes, sure.

DUBNER: And what do they call it again? I'm losing the word?

LIEBESKIND: Cajun?

DUBNER: No, it wasn't. It's like Cajun, but it's not called Cajun. They use -- They play with one of these washboards, and they --

LIEBESKIND: It's not zydeco, is it?

DUBNER: Zydeco.

LIEBESKIND: Zydeco.

DUBNER: That's what it is; it's zydeco music. And they're a zydeco band that's local here in this area, in the Washington, D.C. area.

LIEBESKIND: I see.

DUBNER: And we hired them to come out and play. They were great. It was wonderful.

LIEBESKIND: It must have been very nice.

DUBNER: They had a sound -- They had a country sound, a real country sound, but also a rock sound. It was a combination. So people were dancing the whole evening. It was beautiful.

LIEBESKIND: Now, she's the one who lives in Sacramento?

DUBNER: She lives in Sacramento, and her husband -- He's originally -- Well, he's not from this area. It turns out that his parents and his two brothers live in this area now. But his father was in the military and so they traveled a lot. And once he went off to college -- he went to

college at Humboldt State, which is in California -- and that's the last time he lived with his parents, and that's many years ago. And so they've given some thought about whether they want to stay out in California or come back, but I think they're going to stay there because they both like California. They just --

LIEBESKIND: What does she do? What does Susan do?

DUBNER: She's a counseling psychologist and he's a civil engineer. And they both really like the California environment and the lifestyle.

LIEBESKIND: And then Andy's the one in Minnesota.

DUBNER: He's the one that's, you know, more like his dad in terms of this driving force of work. He went through college and then he went for his Ph.D. right away and got a Ph.D. at MIT. And then --

LIEBESKIND: What's his Ph.D. in again?

DUBNER: His Ph.D.'s in material sciences. It's very much related to chemical engineering and electrical engineering.

LIEBESKIND: Not chalk carving?

DUBNER: No, not at all. And then his wife, she got her Ph.D. two years after he did, from MIT, and that's when they looked for a job together. And they were hired by the 3M company, and that's why they're out there.

LIEBESKIND: And Julie? The baby?

DUBNER: Julie is -- The baby is a writer, and a copy editor for *Scholastic Magazine*. Remember *Scholastic Magazine*?

LIEBESKIND: Yeah.

DUBNER: Yeah, she does a lot of the copy for *Scholastic Magazine* in New York City.

LIEBESKIND: She lives in the city?

DUBNER: She lives in Jersey City, but she works in New York City.

LIEBESKIND: Is she single? Married?

DUBNER: She's single. She has a friend. So that's the Dubner family. They're scattered all over.

LIEBESKIND: Right.

DUBNER: They seem happy. But Mary Ann and I, we brought them up to be independent and to do what was important for them. I'd like them to be --

LIEBESKIND: Obviously, you and Mary Ann, I mean, have lived together successfully all these years. It worked out with the two careers, and she has her career and you have yours, and you share in the responsibilities and everything.

DUBNER: That's right and we -- This is our place together. During the week we both work hard, long hours, but on the weekends we come here, and spend time together, and enjoy this place together.

LIEBESKIND: We've talked a little off tape, let's talk at least a little bit on tape, as much as you're willing, about what the future holds. You know, I mean, here you've got this beautiful estate and your vineyard, which is growing. Are you going to hang up the electrodes one of these days and tend your vineyard?

DUBNER: One of these days, but not too soon. I think that --

LIEBESKIND: You're what? You just turned sixty, is that right?

DUBNER: I haven't yet. I will in a few months.

LIEBESKIND: In a few months you turn sixty.

DUBNER: And I'm not ready to stop working. I enjoy my work a great deal. I feel strong; I feel that I have the strength to continue doing my work and that I would be bored just spending full time here. Even though it is idyllic, after a while I think I would become bored. I need to have something to stimulate me, to keep me interested and excited.

LIEBESKIND: What are your outside interests? How do you play?

DUBNER: My playing is really related to these kinds of activities. I mean, it's related to here, and also in town going to the theater, going to plays, out to dinner. Those kinds of activities in town. Here, it's the outdoors. You know, the swimming in the summer, and we walk all the time when we're here.

LIEBESKIND: And working on the place.

DUBNER: And working around this place.

LIEBESKIND: That's a tremendous responsibility.

DUBNER: I mean, there's a lot to do here.

LIEBESKIND: You don't have sleep-in help here, I see. [he laughs]

DUBNER: No. And so maintaining this place looking the way it is, is a considerable amount of effort. But I enjoy it. I really enjoy getting out there with my grapes. I enjoy getting out there and helping Mary Ann plant the flowers, and keep them looking good, and fertilizing, and taking care of the grass.

LIEBESKIND: How many days a week are you spending out here now, would you say?

DUBNER: We spend -- During the summer we spend at least two days. We usually come on Fridays and leave on Sunday or Monday morning during the summer. During the rest of the year we spend -- We usually are here every weekend, but during the other times of the year we may just come for a day and spend a day in town. So it's not something where we're here the full weekend. This time of the year, with the pool open and the opportunity --

LIEBESKIND: Right. It replaces going to the theater and restaurants, as it were, that you might do.

DUBNER: That's right. We don't do much of that during the summer, right, around here. What's nice about this area, as you'll see tonight when we go to dinner, there are some lovely little restaurants in this area.

LIEBESKIND: Yeah.

DUBNER: And we enjoy them.

LIEBESKIND: And probably as you explore more, you'll learn more about different things out here and so forth.

DUBNER: That's right, yeah. But we really love the city. We love Washington and I don't think we could ever give up Washington totally. So I think the future for us is to always spend some time in Washington. And I don't know what -- You know, I think if I could find a new -- If there was a -- Certainly, I'm challenged in my present work, and I certainly am going to continue doing this for a while. I don't know how long. As far as other challenges, if some other challenge came along that was exciting and --

LIEBESKIND: There's a possibility at Maryland, you know, to be a department thing.

DUBNER: Yeah, yeah. And, you know, something like the directorship of NIDR, which I put - - I don't know whether you know, I was invited to submit my resume and application, which I am doing. I don't have much -- I think they'll go to someone from the outside, but I think they --

LIEBESKIND: Are you keeping your fingers crossed that it will? [he laughs]

DUBNER: Well, I don't really know. You know, I am talking to a lot of people about what -- Here again, when you talk about a challenge -- I mean, the challenge here is, as I said before, I feel in some ways I'm coming back to my roots in dentistry. And is there something that I could do, if I was to by some stretch of the imagination be asked to take that position, is there

something that I could do that would be important, that I could really -- that would challenge me for the future?

And I think dentistry is at an important crossroads today. Dentistry is no more a two-disease profession. Caries and periodontal disease have been reduced to a level that dentistry will not exist in the future just on the basis of those two diseases. So dentistry has to move into a new avenue. It has to become more -- The dentist -- And also with the health management care system changing, the dentist needs to become the primary practitioner of the face and mouth for the future. That's where the future of dentistry, I think, is.

I don't know whether I can make any contribution in that area for the future. But if there was a possibility, if I thought there was a possibility, then I might take the position if it was offered. But that would be really -- That would quite a new challenge.

LIEBESKIND: That would be awesome.

DUBNER: That would be an awesome challenge, yeah. In some sense, even -- As you mentioned, I see the opportunity at the University of Maryland in a similar fashion, as an opportunity where I could impart a philosophy that I have developed within my program at NIDR to a dental school in terms of this integration of basic science and clinical science, and make medicine a part of the dental curriculum, so that the dental students are trained to be health care practitioners of the face and mouth area with a strong background in basic science and clinical science. So I see that as an exciting challenge, too.

LIEBESKIND: One of my best friends, who I'm going to see tomorrow, is a product of that dental school.

DUBNER: Oh, really?

LIEBESKIND: He's a very prominent oral surgeon. He's head of the American Oral Surgery Society, or whatever it's called.

DUBNER: Oh, really? What's his name?

LIEBESKIND: Matskin. Michael Matskin.

DUBNER: I don't know him. No.

LIEBESKIND: A very nice guy.

DUBNER: So these are challenges that might interest me. They would certainly -- They would keep me from spending more time in my idyllic atmosphere here, but if the challenge was such, I would take on the challenge.

LIEBESKIND: Meanwhile, there's no present pressure to evict you from NIH?



DUBNER: No, no. In fact there --

LIEBESKIND: There were issues about that at one time, I remember.

DUBNER: They're being very nice. You know, there are changes taking place in terms of the personnel system, and I need to change into a new personnel system. And they're working on that, and they're trying to get me the best pay situation that I can get under that. So I don't feel any urgency to leave the NIDR. So we'll see. We'll see what happens over time. I don't know. I mean, I -- If you asked me the question, John, what would I really want to do --

LIEBESKIND: Okay, right. Ron, what would you really want to do?

DUBNER: I don't know if I'll let you publish this, but if you asked me what I would really like to do two, three years from now, I would say that I would like to be able to spend two or three days a week working on my profession, the pain field, working for the journal, doing consulting work, helping people develop research programs, helping them to get research support, grant support, and spend the rest of the time writing here, or doing the other things, taking care of my farm. That I think --

LIEBESKIND: Writing your book.

DUBNER: As I said, writing my book. You know, if I could work that out, that would be probably what I'd really want to do. But that's not easy to work out.

LIEBESKIND: That consulting thing --

DUBNER: It's the consulting part that's very difficult, to get the stability to something like that. Because just to have the journal wouldn't be enough. Just to work on the journal wouldn't be enough. So I would need to have some part time position at some department somewhere, or very part time-ish positions at a couple of departments, anesthesiology departments, and so on.

LIEBESKIND: How about something like Howard where you have an affiliation? Would they have you do some teaching or something?

DUBNER: They're not -- The opportunity isn't there. There might be opportunities at other places. And, actually, I'm pursuing these in a very, shall I say, in a steadfast, deliberate fashion. Okay? [he laughs] But that's what I probably would like to do most because --

LIEBESKIND: Well, how about that book? What shape would that take? What would that be all about?

DUBNER: That book would --

LIEBESKIND: We talked a little bit about it off tape.

DUBNER: It would be about the stream of consciousness. I would bring together how all this research, how it all interrelates, and what's the commonalities of this research, and how it gives us insight into brain function.

LIEBESKIND: The review paper of the Dubner laboratory.

DUBNER: Yeah, but with the personal touch. With the personal touch with the people, talking about all the people, and all the people I know.

LIEBESKIND: It'd be a little like Sol Snyder's book or something in that vein.

DUBNER: Yes, yes. It would bring in the people.

LIEBESKIND: That's good. I think that would be fascinating.

DUBNER: I don't know how fascinating it would be, but --

LIEBESKIND: Well, this has the potential for being fascinating.

DUBNER: But then, it would also be my philosophy. It's my philosophy on how to, you know, of doing research. We talked about that. We talked about this broad program and the breadth, the importance of the breadth, the importance of -- In contrast to Sol, I would say it's important if you have a program that has breadth, you've got to have leadership from different people. You can't be a jack of all trades; you end up a master of none. You've got to have people that you can rely on in some ways. And so it's a whole different approach. But what you give up for that is that it's all not recognized as coming from you. You share the glory and the recognition with a number of other people. That gets back to the different scientists. Many scientists don't want to share the recognition. That's one of the reasons for being a scientist.

LIEBESKIND: That's right. People have different motives for bringing them to the field.

DUBNER: Right.

LIEBESKIND: Different things drive them. Ron, when you think ahead, when historians will look back on this half century, what do you think -- This is a question that I think we talked about when we were swimming or something. What do you think are going to be the really prominent features in the landscape of pain scholarship that they're going to think about when they say, "Oh, yeah, back in the latter half of the twentieth century, the way the pain field was, was like this. Here are the real big issues," or something like that. Can you think of a few big issues like that that they would -- ?

DUBNER: Issues that we've already accomplished?

LIEBESKIND: Well, or not. Yeah. I mean, what's gong to endure about the efforts of all of us in the pain field? What's going to stay up there in people's minds?

DUBNER: What's going to endure are the research accomplishments, the research advances. And there are certain research advances that we can both point to that are clearly major, major accomplishments. It has to do with the identification of the, you know, the nociceptor system starting from the receptors and onward, okay? It has to do with descending modulatory systems, the work that you have done, and the work that's been done by other people on the opioid pathways and the other chemical mediators. And it has to do also with this hyperexcitability of the nervous system produced by the persistence of pain. I think those are major basic research accomplishments that impact upon our understanding of pain in general, and also on our understanding of chronic pain, persistent pain.

LIEBESKIND: And of the brain.

DUBNER: And of the brain, absolutely.

LIEBESKIND: To quote Ron Dubner.

DUBNER: And I think clinically -- I think in the clinical realm, I would hope that what they would say about this half of the century is that it was the people in this pain field who eliminated pain and eliminated fear of taking medication to control pain. I think that's an important accomplishment. If we can do that, if they could say at the end of this century that we did this, and that we educated -- that we developed a cadre of health practitioners that went out there and were not afraid of eliminating pain in their patients, then we would have accomplished a major, major advance in health care management. And with that, John, turn off the tape. [he laughs]

LIEBESKIND: Just about. You know, I've got -- I asked you who else I should interview. We talked about that a little bit. You gave me a very good idea in mentioning [Vernon] Mountcastle, which I think is a fascinating, fascinating thought.

DUBNER: I think you ought to interview Ulf. It may not be an easy interview, but I think it's very important.

LIEBESKIND: He's on my list, yeah.

DUBNER: He's an important contributor.

LIEBESKIND: Yeah, and you mentioned Manfred.

DUBNER: And Manfred Zimmermann, I think.

LIEBESKIND: That would be interesting, too. I have --

DUBNER: Interesting from a different --

## RONALD DUBNER INTERVIEW

### TAPE THREE, SIDE TWO

LIEBESKIND: We were talking about who else to interview. You mentioned Jean-Marie [Besson] and, so forth, these are all people I've thought about.

DUBNER: You haven't mentioned anybody -- Well, you might want to interview Michael Cousins.

LIEBESKIND: Yeah.

DUBNER: Have you thought about Michael?

LIEBESKIND: Yeah. It's so costly to go out there, I haven't really given it a lot of consideration.

DUBNER: Though he does come in occasionally. You could arrange a time to have him talk to you for a few hours here.

LIEBESKIND: Yes, he does. Yeah, I could arrange a time. Yeah.

DUBNER: Japan -- It'd be difficult, but I don't know -- Certainly, there's a strong contingent of Japanese people involved in this field.

LIEBESKIND: Sato, I suppose, would be one of the major ones now, wouldn't he?

DUBNER: A basic science person would be Sato, but he's so difficult to talk to. Very difficult.

LIEBESKIND: Concerning this interview, what reflections do you have on it? Do you feel it was successful? Were the questions appropriate?

DUBNER: Yeah.

LIEBESKIND: Are there things I left out?

DUBNER: No, you didn't leave out anything. I guess I didn't leave out anything.

LIEBESKIND: I feel we covered a lot of territory here.

DUBNER: I just spoke what was on my mind. You sort of put some questions in along the way. But I think I said everything that I'd like to say. Yeah, I think I really have.

LIEBESKIND: No doubt other things will occur to you later or to me later, and luckily we're both going to live for another forty years, so we have time to -- [both laugh]

DUBNER: Yeah, sure.

LIEBESKIND: -- time to get back together again.

DUBNER: Well, you put this in the framework of, this is our legacy, but I would hope that this information could be used for the present, not just for the distant future when we're six feet under. I mean, I would think that there's information there -- I mean, when you transcribe all of these, I think there's a book here.

LIEBESKIND: Oh, there's no doubt about it. There's no doubt about it. And I think what's --

DUBNER: And you see, what you could do from this is, you'll have many -- there'll be many leads. If you could work with someone who is a writer, who is interested in writing this story of the history of pain, as you mentioned this French woman, she would take leads from this and pursue them. She would go back to people and say, "Well, what's the relation between x and y? How did Larry Kruger get to spend time working with Mountcastle?" And he also spent time working with Albe-Fessard, you know, Madame Fessard, and he spent time working with -- You know, I mean, there -- from the Yale group, he was there. So he was on all -- What do they call it? Someone who's in all camps.

LIEBESKIND: Yeah, yeah.

DUBNER: I mean, you know, how does that come about?

LIEBESKIND: A camp follower, I think. [he laughs]

DUBNER: And Ed Perl, you know, where does Ed Perl come into this picture? Where does he originate?

LIEBESKIND: Where does Ainsley Iggo come into this picture with his C-fiber recordings? I've always --

DUBNER: He was the first one.

LIEBESKIND: Yeah?

DUBNER: He was one of the first people to record from nociceptors. He just didn't do it as well as Ed Perl did.

LIEBESKIND: But Ed Perl generally gets the credit for having --

DUBNER: Because Ainsley didn't do enough of it. It wasn't -- It was anecdotal kinds of data. It wasn't a thorough analysis of a class of fibers that had certain properties. So Ainsley never got --

LIEBESKIND: Because even [Yngve] Zotterman claims to have done it years before that.

DUBNER: Zotterman was the first -- Zotterman had something there, yeah. In '36 Zotterman used to get up-- He used to get up every time and show his little slide, you know, his slide of 1936 showing these, you know, blips on the screen represented what he thought were c-fibers, and they were.

LIEBESKIND: Yeah, probably so.

DUBNER: So Zotterman -- He's a great old guy. Yeah, it's been a very good career. It's been very special.

LIEBESKIND: When you attend a funeral like John Bonica's then it makes you think about things like this.

DUBNER: That's right, that's right.

LIEBESKIND: You know, this is in a sense a very ideal time for you and me to have this interview, because one is naturally reflective.

DUBNER: It is like -- In a sense, it's almost like the end of an era with John Bonica's death, because he was the founder of this society. He was the founder of the whole field, okay? Alright?

LIEBESKIND: The field, the field, wasn't he, yeah?

DUBNER: So the founder dies. That puts you into a new chapter.

LIEBESKIND: Right.

DUBNER: And that's what we need to be thinking about. I think it's appropriate. You know, in a sense I like the idea that your editorial is coming out now, because it is -- it focuses on future initiatives. So the next chapter -- What is our next chapter? And so I think that's important.

LIEBESKIND: Which I think we're both saying, in replacement for the IPF, the now-dead IPF, is still something that's focusing on the public, on public education. I like that comment of yours very much. I mean, that's what -- You know, we've got to get those people, those patients to say, "Doctor, I'm going to be having surgery. What are you going to do about? You know, and I've read about this from the IASP," something, something. You know, I like that a lot.

DUBNER: Well, if we had a National Institute of Pain, you know, then a certain amount of the funding would go into an information office which would develop materials that would be important for the public. There isn't any such institute.

LIEBESKIND: But there will be.

DUBNER: Yes. I doubt it, but you know, when you think about it --

LIEBESKIND: As soon as my editorial comes out. [he laughs]

DUBNER: When you think about it -- I think I said this to you. I've said this to other people over this past weekend, and that is that if you look in the *Society for Neuroscience Abstract Book* each year, you find that there are almost as many sessions on pain as there are on vision.

LIEBESKIND: Is that right? Is that bloody right?

DUBNER: And is there a National Eye Institute? There is a National Eye Institute.

LIEBESKIND: Yes, there is. Isn't that fascinating? Wow, keep saying that. That's a really good thing to say.

DUBNER: I'll have to double check it this year again, but I have checked it a couple of years in a row and --

LIEBESKIND: Hey, we could have half as many and it would be impressive.

DUBNER: It's more than half as many, I can tell you that. It's really quite impressive.

LIEBESKIND: Isn't that something?

DUBNER: It is. That's why I -- That was one of the reasons I was absolutely compelled to come out to that funeral, because I feel the way you do, that this man --

LIEBESKIND: -- this man did it!

DUBNER: This man was the founder of all this. And look what he did. No one really understands that. Very few people appreciate it.

LIEBESKIND: I agree with you.

DUBNER: They don't appreciate the extent of his accomplishments.

LIEBESKIND: I thought of an analogy.

DUBNER: And what made me so -- Well, it didn't make me angry, but what disappointed me was that there weren't more of our colleagues who saw fit to take a part of time out of their lives and pay tribute to him in his passing.

LIEBESKIND: I agree.

DUBNER: That was -- That saddened me a little bit. But --

LIEBESKIND: I'm glad John wasn't there to see it. Let's put it that way. [he laughs] He would have been pissed.

DUBNER: He would have been pissed, is right. But that's the way it is. That's the way people are. You know, I don't -- Though there are -- I mean, there are funerals where everybody turns out for. Why do people turn out for one funeral and not for another? I don't know the answer to that. This was not an easy funeral to turn out for.

LIEBESKIND: No, it's a bad time of year with the holiday and so forth.

DUBNER: It's a bad time of the year. It's off in this god-forsaken part of the country. And so it wasn't easy for people to be there. Like I spoke to Allan, you know, on the phone the other day, and I told him I just came back from John Bonica's funeral. He said, "Yeah, I wanted to go but I never got any information. All they did was send me this letter about that he died, and I was waiting to hear about when the funeral was. And then all of a sudden I heard the funeral took place." Well, the funeral took place a week later. If you really wanted to go, you should have made some inquiries along the way to find out when it was. But I understand why. He's writing a grant, a grant that's due this coming week, and so he's very busy. So it's hard. It's hard to take time out from other responsibilities, other things to do.

LIEBESKIND: Maybe, as you were saying on the drive out here, we need some other opportunities for people to pay their tribute. If they can't get to the funeral, maybe they'd like to write something. I think that's an excellent, excellent idea. Ronny, I think the interview's over. What do you think?

DUBNER: Yeah, I think it's over. Let's turn it off. I thought we turned it off already. Oh, no, we didn't.

LIEBESKIND: Let's stop. Thank you. The interview is over.

**END OF INTERVIEW**