

John C. Liebeskind History of Pain Collection

**Oral History Interview
with
Ronald Melzack**

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Biographical Sketch

Ronald Melzack was born in 1929 in Montreal. He graduated from McGill University in 1950 and in 1954 received his Ph.D. in physiological psychology from McGill, under the tutelage of Donald Hebb. He did postdoctoral work at the University of Oregon (1954-57), with William K. Livingston; and held teaching positions at the University of London (1957-58), the University of Pisa (1958-59), and at the Massachusetts Institute of Technology (1959-67), before accepting a full professorship at McGill in 1967. He has been the E.P. Taylor Professor at McGill since 1986. Dr. Melzack is best known for his pathbreaking 1965 paper with Patrick Wall (“Pain mechanisms: a new theory”), which introduced the famous “gate control” theory. Although modified and in part refuted since, the theory remains a unifying concept of the pain studies field. With Warren Torgerson, he developed the *McGill Pain Questionnaire*, widely used in clinical evaluation. Dr. Melzack has also done many important studies of phantom limb and labor pain, and of the mechanisms of opioid analgesia. He was a founding member of the International Association for the Study of Pain (IASP) and served as its President from 1984-87.

Interview History

Dr. Foley was interviewed in her office at McGill University in Montreal, Quebec, Canada by John C. Liebeskind on October 16, 1993. The interview lasted for approximately 3.0 hours. The transcript was audit-edited by Marcia Meldrum and reviewed by Dr. Melzack 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 recordings, consisting of two (2) 90-minute audiotapes, are in the Library holdings and are 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, beginning with Melzack’s childhood and youth in Montreal; continuing with his education under Donald Hebb at McGill University; early experiments with restricted dogs; postgraduate training at the University of Chicago and the University of Oregon; clinical exposure and exploration of pain pathways with William Livingston; his work at the University of London and at the Massachusetts Institute of technology (MIT); animal behavior experiments with Nikolaas Tinbergen; publication of the gate control theory; work at McGill; development of neural matrix theory; and experiences as a member of the International Association for the Study of Pain (IASP). Major topics of interest include Melzack’s family background; theoretical models of pain; interactions with pain patients; work with students, including Kenneth Casey and John O’Keefe; relationship with Patrick Wall and their development of the gate control theory; relationship with Warren Torgerson and their development of the McGill Pain Questionnaire; work with John Loeser on phantom body pain; the formation of IASP; Melzack’s experiences as IASP president; collection of Eskimo folktales; and reminiscences of Hebb, Livingston, Patrick Wall, and John Bonica.

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 interviews with Patrick D. Wall, Louisa Jones, and John Loeser; Melzack correspondence in the William K. Livingston Papers (Manuscript Collection no. 136); Melzack correspondence in the John J. Bonica Papers (Manuscript Collection no. 118); chapter 21 of Livingston's autobiographical monograph, *Pain and Suffering* (Howard L. Fields, Ed.; Seattle: IASP Press, 1998); and records of the International Association for the Study of Pain (Manuscript Collection no. 124).

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Ronald Melzack, PhD, FRCS

Psychologist

RONALD MELZACK INTERVIEW

TAPE ONE, SIDE ONE

JOHN LIEBESKIND: I will note for the record that today is October 16, and we are in the office, in the Stuart Biological Sciences Building, Room N8/5, with Professor Ronald Melzack, my good friend. Ron, this is a delight and as you know, we are here to talk about you and the incredible, incomparable contribution that you have made in the field of pain. My goal is to kind of interweave issues of who you are as a person and how that has been expressed in your work. I must say, of all the people that I think of in the field of pain, from my knowing you all these years, I think you more than anyone show this, manifest this interweaving of the person and the passion and the ideas. This has been so clear throughout the history of your work, and I'm sure that will be reflected in this interview. Anyway, I wanted to start, just to get it rolling, by asking you to tell me what factors you could identify early in your life that prepared you for or oriented you toward in any way the field of pain. What would you single out? What early factors might have led you in this direction?

RONALD MELZACK: Well, nothing happened early in life that I know of that got me interested in pain, especially as such, but certainly I was a very insecure kid who came from a Jewish family that was not religious. My father had studied to be a rabbi in Poland and then began to ask questions at bar mitzvah time and was so unhappy that he left home, went back a couple of times, got kicked out by his father, and eventually emigrated to Canada.

LIEBESKIND: How old was he when he came here?

MELZACK: Oh, he must have been about 17 or 18, really very young, and learned the needle trade, which is what most of these people did, he went to work in a factory, and his great dream was always to own a business of his own. He tried several and failed and finally my oldest brother, who was then 13, gave him the idea of opening up a bookshop. It was the 13-year-old who actually bought the books, rented the store, did everything and got a bookshop going that later became a very big family business.

LIEBESKIND: What was his name, this brother?

MELZACK: This brother was Jack.

LIEBESKIND: I've heard you speak of Louie.

MELZACK: Louie is about two years younger than Jack, and after a couple of years, Jack decided to leave the book business, even though it was not losing money and was even making some money -- my father was still continuing to work in the factory -- but my brother then told Louie, who was then 13, that the best thing for him to do would be to go into this little book business. It looked like he could make a livelihood out of that, and he did. He went on and eventually had hundreds, literally hundreds, of stores and became a multimillionaire. It was a big chain called Classics Bookshops.

LIEBESKIND: Oh, it was a whole chain. Do they still exist?

MELZACK: No, they were sold to W.H. Smith and so my brother is really very well-to-do. He now lives in Toronto with a house in Florida -- you know, all the typical things. He is 79 years old and doing very well, enjoying his life.

LIEBESKIND: This is Louie.

MELZACK: This is Louie, that's right. Poor Jack died at the age of 26. He was living in Toronto, got pneumonia and died tragically. The police seem to have thought that he was drunk, but in fact he had pneumonia and was very sick and delirious and they threw him in jail, and at the last minute realized how sick he was, took him to the hospital, but they couldn't help him and he died. It was a great tragedy in the family which had a terrible impact on me.

LIEBESKIND: I can imagine. How old were you then?

MELZACK: I was 9.

LIEBESKIND: He was 26 and you were 9.

MELZACK: I was 9 at that time. I used to be a terrible stutterer when I was a kid, and I still do have stuttering jags from time to time.

LIEBESKIND: I never noticed.

MELZACK: Yeah, I know -- most people are always sort of surprised, but I do stutter, and I learned that I stutter most when I try to read something in public, so I don't read. That's how I got to lecture.

LIEBESKIND: That's why you're such a good lecturer.

MELZACK: That sure helped -- that was a contribution. I found that I had to know the stuff before I went on, and then just simply talk it to people. You do the same thing, John. You are a wonderful lecturer.

LIEBESKIND: I read.

MELZACK: You don't read.

LIEBESKIND: I read.

MELZACK: Well, not the ones I've heard you give.

LIEBESKIND: Sometimes I read.

MELZACK: Well, sometimes I read chunks of things. I feel confident enough now that I'm able also to read chunks of things during a lecture, but a lecture really has to be, to me, spoken to the class. Anyway. So there was this great insecurity, great sense of insecurity. My father -- I had friends who were all having their bar mitzvahs and I wasn't going to have a bar mitzvah, so I was busily searching for meaning and understanding, trying, looking for meaning and value in life generally. Read a book called *The Martyrdom of Man* by Winwood Reade [1838-1875], a very remarkable book in which he -- this was written in the 1870s, John, and Winwood Reade had been a *London Times* correspondent in Africa. And it's essentially a comparative history of religion, and he ends up with a humanist faith in humanity, that humanity as a single entity has enormous potential and all those godly virtues that we all aspire to, and that had a great impact on me. I read that when I was about 13 or 14. My brother Louie was smart enough to give it to me to read, perhaps seeing that I was struggling, and then I went through high school. It might be interesting to recount also that I had -- that was called Strathcona Academy, and there were two famous high schools in Montreal at that time. One was Baron Byng.

LIEBESKIND: Baron Byng?

MELZACK: Baron Byng [1862-1935, commander of the Canadian Army in WWI] was one of the great people in World War I, and even pre-World War I times, and Baron Byng High School is the place where the lower class and lower middle class Jews went; so Mordecai Richler [Canadian novelist 1931-] went there, and half the Jewish population that are now the lawyers and doctors and judges and whatnot of Montreal and Canada even went to Baron Byng. But then Strathcona Academy had the Jews that were a little upwardly mobile and moved to Outremont from the Plateau -- what is now known as the Plateau in Montreal, which is east of Mt. Royal, which you saw yesterday -- with that move they also had a new high school, and that was called Strathcona Academy.

LIEBESKIND: Strathcona.

MELZACK: That's right. Lord Strathcona [1820-1914] is the guy who built the Canadian Pacific Railway.

LIEBESKIND: I see.

MELZACK: And also gave a ton of money to McGill, so the college for women, the women's residence, is named after him. He did a tremendous amount for McGill. But at Strathcona, David Hubel [Nobel Laureate in Physiology or Medicine 1981 for his work on the visual cortex] was a student. He was an American whose father worked with the American consulate here in Montreal, and so he went there. I knew him only very, very casually, so I knew he was there, but that didn't mean anything until much later, when we actually became friendly, because he married the woman that was my laboratory partner in honors psychology at McGill. Her name is Ruth Izzard. Both lovely, lovely people. Very good friends with other people at Strathcona, but Strathcona did not have a particular impact on me. My marks were just middling. Then when the final exams were written, the matriculation, I astounded everyone, including myself, by getting the highest marks in my class. [Both laugh] I don't know why, but I did. I crammed like hell, and I learned that if you cram, you can do all right.

LIEBESKIND: You were identified early on as an underachiever.

MELZACK: That's right. So I went to McGill and began to take courses, hoping again to find some meaning, structure in the universe for me, you know --

LIEBESKIND: At McGill, you didn't have to board there, you could live at home, right?

MELZACK: No, interestingly enough, when I went to McGill it was 1946, and that was just when the veterans were returning home from World War II, so McGill couldn't possibly handle all these people. So McGill was given an Air Force base in St. Jean, Quebec, which is about a 25 mile ride out of town, and there were Air Force barracks, so for the first two years at McGill, I in fact lived in these Air Force barracks.

LIEBESKIND: Is that right?

MELZACK: Yeah, would come home for the weekends, take the Friday night bus home and the Sunday night bus back to Dawson College. And I did miserably, I very nearly flunked out of college. I was just miserable being away from home; I was made all the more miserable because I had a cousin who survived the concentration camps, who was a brilliant mathematician, and [had] an enormous sense of aggression and anger toward everything, but it was directed mostly at me, because I had the kind of life he really wanted to have. It's not that he hated me, this was just an angry person, but he could focus that anger particularly on me.

LIEBESKIND: So he had come to this country after the war?

MELZACK: My father brought him over here, went to great trouble, you know, through the Red Cross, and all the shenanigans that one had to do to bring in a displaced person. But he learned mathematics from mathematicians who died in the concentration camps. And Alex went on -- we subsequently became friends and Alex got his PhD at MIT in mathematics and he worked at Bell Labs and he became a professor of math at UBC [University of British Columbia] and that's where he is now, at UBC, married and two children and has had an excellent career.

LIEBESKIND: He worked through his problems.

MELZACK: To whatever extent one can work through such problems, he did. I think he was enormously helped when he decided, in the last eight, ten, years, to write a biography of what happened to him in the concentration camps.

LIEBESKIND: Did he?

MELZACK: It was never published, but just writing it, I think, was a tremendous catharsis for him.

LIEBESKIND: Getting it off his chest, yeah.

MELZACK: So then I took a course in psychology from a fellow called Herbert Lansdell, and that was the first time --

LIEBESKIND: Herbert Lansdell?

MELZACK: Yeah. He's at NIH -- he was at NIH [Dr. Lansdell (1922-2000) was at NINDS 1958-96].

LIEBESKIND: I know Herb. Gee.

MELZACK: He got his PhD here with Don Hebb.

LIEBESKIND: Yeah, but he must have -- I think of him as being a contemporary of mine. Is he that much older?

MELZACK: Yeah.

LIEBESKIND: Wow. I talk to him every once in a while. I didn't realize he was here [at McGill].

MELZACK: Yeah, he's a couple of years older than I am, which would make him maybe 66 or something. But he certainly looks young. So Herb Lansdell taught me my first course in psychology, and that was the first time I felt I was studying something that interested me in college, you know -- really interested me, and I got an A, which was amazing, considering that I was nearly flunking out of everything else. Physics and chemistry, I was falling asleep in all the lectures and I was just a disaster. And then the third year was spent at McGill. And here is where I met the great Donald Hebb [1904-1985, pioneer theorist of the cognitive revolution in psychology]. And I decided to apply for honors psychology, since it was a topic I felt I liked, and I took a course of Hebb, lectures with Hebb; and then in your final year of your honors thesis, you do an experiment, and I asked Hebb if I could be his student and he gave me an experiment to do on curiosity in the rat, which at that time was utterly new. This is 1949, and thus this was the height of Hullian, Skinnerian psychology [schools following the radical behaviorist B. F. Skinner and the more moderate Clark L. Hull].

LIEBESKIND: That must be about when his big book was coming out, wasn't it? *The Organization of Behavior*?

MELZACK: In 1949, that's right. But you know, we all read it and we all thought it was interesting, but we didn't know what a tremendous impact it was going to have.

LIEBESKIND: What a bombshell that was.

MELZACK: That's what it was all right. So Hebb just couldn't have been a more wonderful person for me to work with at that time. He had that capacity to treat students as equals. If you were an undergraduate you still called him "Dr. Hebb", but the minute I became a graduate student, he was "Don" and insisted on it -- it was hard for us to do that. At the same time there

was always that distance. There was no question as to who was the student and who was the professor. But also this great warmth and understanding and almost fatherliness in the sense of giving advice when he thought it would be useful, but not too much, not sitting you down to give you advice, but just a couple of sentences here and there at the right time that would practically change the course of your life -- you know, amazing, a truly amazing guy. So I did my bachelor's thesis with Hebb and got second-class honors, which was very good. That's what they were called in those days -- now, it's simply first-class honors and honors -- then it was really called second-class honors, and that's what I got, and very proud of it at the time, I remember.

LIEBESKIND: Your folks must have been pleased.

MELZACK: They were certainly very pleased.

LIEBESKIND: That you had come out of the academic doldrums.

MELZACK: Only my mother and father were pleased. My brother was not happy -- he thought I should go into the business, you see -- Louie, he wanted me to go into the bookstore. But I definitely did not want to do that, because Louie has a very strong personality and I thought I would never survive the strength of that personality -- I would simply be a flunky to Louie all my life, which is what happened to one of his sons. His sons went through the same sort of battle I did because he was -- you know, self-made millionaires can be tough people to deal with. Louie and I are friends because I did not go into his business.

LIEBESKIND: You have a sister also? You mentioned her.

MELZACK: A sister as well, yes, that's right. She's 12 years older. There were four children, plus several that died very young or stillborn or what have you, which was typical of most families at that time.

LIEBESKIND: So there were four children. So you were the youngest.

MELZACK: So I was the baby, with much affection lavished on me. But grew up, and I would say, John, that between the ages of about -- if I had to characterize my life between the ages of 15 and 25 or thereabouts, it would be a life in which I was almost always depressed, to some extent. I was a depressed, very depressed kid. Really searching for something to do.

LIEBESKIND: But you were sort of gradually -- it looks like over that period you were kind of gradually coming out of that.

MELZACK: There is no question, I was.

LIEBESKIND: Doing better and better. You were kind of bootstrapping it.

MELZACK: That's right. No question, I was.

LIEBESKIND: By the end of your college career you had honors and so forth.

MELZACK: That's right. And then I applied to graduate school. My family wanted -- if I wasn't going to go into business, I should at least go into medicine or something like that.

LIEBESKIND: Did your parents think it was okay what you were doing at this point?

MELZACK: They didn't understand what I was doing. They didn't know. As long as I was going to college, and going to college was a good thing --

LIEBESKIND: They didn't care. So you didn't get any pressure from them to go into the bookstore.

MELZACK: One way or the other.

LIEBESKIND: That was only from Louie.

MELZACK: That was only from Louie, that's right. In fact, several years after I got my PhD, Louie began to open -- he learned the key to opening a bookstore that really could make money and he began to open them by the dozens practically, and he phoned me up and said "Why don't you give up all that stuff you're doing, playing with rats and cats and crap like that and come into the book business and do something important?"

LIEBESKIND: "You'll be rich." I'm sure you would have been.

MELZACK: Might well have been, that's right. Not, not in the sense --

LIEBESKIND: Poor in spirit.

MELZACK: That's right. So what happened, the turning point, was that after I did my masters thesis, which was on irrational fears in the dog -- these are fears that are not learned, they are fears that simply evolve, fears of things like statues of animals, fears of skulls; and it's not even fear as much as a diffuse emotional excitement, which was puzzling and fascinating to me, and I have come back to it in my older age now, as I am thinking about other problems.

LIEBESKIND: These were inborn, they were just born with these.

MELZACK: That's right -- it's fears of that which is different and unexpected and unusual. And that brings you to prejudice, of course. I mean, if you are born in a family full of white people who tend to eat certain things and speak the language in a certain way, then anyone who differs from that is an outsider, is different -- I don't think we learn to be distrustful, we simply are, and I think that's the way we're built. So I thought I might go on with that area. But what happened was that Don Hebb and some students were raising dogs in restriction cages for basically social and perceptual isolation; and when these animals came out of their cages, at first they would freeze, and then they'd become excited and then they became overwhelmingly excited and they would run around the room, any room that they were in.

LIEBESKIND: Scottie dogs.

MELZACK: These were Scotties, that's right.

LIEBESKIND: I remember the *Scientific American* article. You and Scott.

MELZACK: That's right. Scott and I wrote that paper on pain [R Melzack and TJH Scott, TJH. (1957). The effects of early experience on the response to pain. *Journal of Comparative Physiology and Psychology* 50: 155-161.] That one [WR Thompson and R Melzack (1956). Early environment. *Scientific American* 194: 38-42.] was with a fellow called Thompson and he was the guy who was actually looking at intelligence. He was a post-doc working with Hebb, you see, looking at intelligence.

LIEBESKIND: Which Thompson was that?

MELZACK: W.R. was his initials. He unfortunately died. He went to Queen's University [Kingston, Ontario] from here and lived there for many, many years and then he died about 10 years ago. But Hebb was asked to write a paper for *Scientific American* and Hebb, being Hebb, said no, it's my students who did that work; so he asked Bob Thompson and me to write the one on dogs and Woody Herron to write the one on people in restriction who had perceptual hallucinations. So that's how I got to write a *Scientific American* article at that very young age, simply because it was Hebb who was asked to do it.

LIEBESKIND: Something you've -- you must hold the record for the number of *Scientific American* articles.

MELZACK: I must come close to it -- it's really quite amazing. I must come close to it.

LIEBESKIND: How many do you have altogether?

MELZACK: Four. It's really quite extraordinary.

LIEBESKIND: Four, that's really amazing, an amazing record.

MELZACK: Very nice, very nice indeed. So what happened was that as these dogs would run around this room, they would bash their heads on low-lying water pipes, really a thud, you know? And we would be shocked by it, and no evidence that they were in any pain. They would run around our feet and we would try to get out of the way and we'd step on a paw, a tail - - no evidence of pain. I used to be a smoker in those days -- I lit up a match, they'd stick their nose in the match.

LIEBESKIND: I remember that description.

MELZACK: It came out, they'd stick their nose back into the match, they'd back off reflexively -- the reflexes were there. They seemed to feel something, because if you stuck a dissecting

needle through the skin they might give a little yelp, they'd certainly orient toward something happening somewhere; but they'd never get themselves the hell out of the way, like you'd do this to a normally reared dog, which were our control littermates, and you couldn't get them back near you. Well, pain then suddenly became an interesting problem. And I got a phone call one day from Wilder Penfield [pioneering brain surgeon, 1891-1976], which, you know, Wilder Penfield even then was God, or right next to him, and he had heard about my restricted dogs.

LIEBESKIND: He later became an avenue.

MELZACK: That's right, we're on Wilder Penfield Avenue, you're absolutely right. So he wanted to know about the restricted dogs. That thrilled the hell out of me. But --

LIEBESKIND: He was what, a neurologist or a neurosurgeon?

MELZACK: A neurosurgeon. Certainly one of the greatest neurosurgeons of our century, because he did the first really great neurosurgery on epilepsy, and he was the guy who began to map the homunculus on the cortex; and the number of innovations he made in neurosurgery was simply legion and tremendous. A very remarkable man, and with a genuine curiosity. So pain then became very interesting.

LIEBESKIND: So you were a student, and he called you and he wanted to talk to you about these dogs.

MELZACK: Yeah, which amazed me, this famous man phoning me up and asking.

LIEBESKIND: Do you remember going over, did you go over to his office?

MELZACK: No, we just talked on the telephone. But I subsequently eventually met him personally. But people in physiological psychology used to go over to the [Montreal] Neurological Institute to take their courses in neuroanatomy and neurophysiology -- the anatomy from a guy called [Francis] McNaughton and neurophysiology from Herbert Jasper [1906-1999, longtime director of Electroencephalography at MNI], who was a psychologist and a great friend of your friend Donald Lindsley [1908-2003, co-founder of UCLA Brain Research Institute], right? And [he] went into EEG work and then Penfield told him that if he really wanted to have a future in the field of EEG, he had better get a medical degree, so Jasper went back to school, got a medical degree and then came on at the Neuro. And Jasper played a very important role in my life, because it was he who suggested after my PhD that I might -- I went to see him to ask him who I could work with to learn physiology, and he said "Why don't you work with Bill Livingston, he'd be wonderful."

LIEBESKIND: He knew that because of your interest in pain and Livingston's work in pain.

MELZACK: That's right. Bill Livingston [William K. Livingston, early pain theorist and researcher, 1892-1966] was a personal friend -- they both came from Oregon, you see -- they were both personal friends. And Livingston had written what I thought then to be the most important book in the field of pain [*Pain Mechanisms*, 1943]. Well, it became apparent to me

that the field of pain would allow me to do a whole lot of things. Number one, it would give me a model of all different perceptual processes. So I had friends here who were looking at vision and hearing and everything. Also I had gotten into graduate school at McGill and that was not supposed to be. Hebb thought by fiat that no one should accept their own undergraduates as graduate students; so he insisted I go away for a year, and I went to the University of Chicago.

LIEBESKIND: While you were in graduate school?

MELZACK: While I was in graduate school. And I went to work with Dewey Neff [William DeWayne Neff, 1912-2002], who was the auditory man, and there I met some extraordinary people. Kluver, for example -- I took a seminar with Heinrich Kluver [1897-1979]. I took a seminar with Roger Sperry [1913-1994, Nobel Laureate 1981 for his famous work differentiating the functions of the left and right brain], who would never talk about anything. That was the time when he came up with his mind-body paper in *American Scientist* and just absolutely refused to discuss it. We all gave papers, and whenever we asked him questions about anything he did, he said, "Well, why don't you read this or that or something?" It was very interesting. Dewey Neff was a delightful guy to work with. It was interesting, I had never seen a Skinnerian in action and there met a guy called Howard Hunt, who was a great Skinnerian guy. But what I came to realize was, number one, I thought the field of psychology had gone in completely the wrong kind of direction, with its emphasis on stimulus and stimuli and responses, its S-R and its kind of learning.

LIEBESKIND: This is what year now?

MELZACK: We're talking, we're in the '50s here.

LIEBESKIND: The early '50s.

MELZACK: Yeah, early and mid '50s now. And I felt that there is, however, a psychology which is a Hebbian kind of psychology, where you can speculate about the brain and what it does and the brain, after all, is what makes us all be what we are. But I had no sympathy whatever for the standard psychology; in fact, I really loathed it and moved far away from it.

LIEBESKIND: Well, can we look at that for a second? There's meaning there, I think. I want to see if we can get at that. The standard psychology, the sort of black box psychology -- I mean, they wanted to ignore the intervening, what intervened between stimulus and response.

MELZACK: Right.

LIEBESKIND: Like Skinner saying, I don't want to know, I don't want to think about it. I understand there's a brain there, but I don't want to think about it. We don't need to. All we have to do is show the lawfulness of the relationships between stimulus and response.

MELZACK: Exactly.

LIEBESKIND: And Hebbian psychology was exactly the opposite. There was a profound interest in this mediator, and how the brain, and so forth and so on. It seems to me that -- I want your opinion on this -- but it seems to me that, you know, we talk about an interest in ideas, as you and I were talking about yesterday, which so characterizes you, you're a great idea person, and that seems to me what is embodied in Hebbian.

MELZACK: That's right. That's absolutely true.

LIEBESKIND: And the rest is just sort of, it's like plumbing or it's like being an electrician or something.

MELZACK: It's straight through. Right, right.

LIEBESKIND: You know, connect this to that and don't worry about how that works.

MELZACK: That's right. It was the model of a telephone switchboard system, as Hebb used to say. That was the idea.

LIEBESKIND: Which of course a little later in this conversation I am sure is going to apply specifically to the world of pain.

MELZACK: Oh yes, oh yes, absolutely.

LIEBESKIND: As we talk about specificity and a gate that has a brain and descending controls, we'll come to that. Isn't it really the same idea, that which made you a Hebbian as opposed to a Skinnerian is that which made you a --

MELZACK: Right on, John, you are absolutely right. That is exactly so. And Hebb wrote a textbook, in addition to this famous book, he wrote a textbook of psychology. And in there he took -- I was unhappy with that book. He gave it to me to read; I was then a post-doc. So just to complete that story, I did research with some of these restricted dogs, and then when I was in Chicago, I designed an extremely elegant experiment with rats, and some rats were raised in restriction cages and others in a free environment, a park-like thing where they could run around and go through activity wheels and whatnot. Some animals got shocked early in life and others got shocked late in life -- what came out of that was bupkus -- nothing. [Both laugh] It was so complicated -- I mean, if I had had a computer -- I spent a year, practically, analyzing the stupid data on that thing and it all came out zip.

Anyway, Hebb said, "Look, write a really good introduction to your thesis and with your three dogs or four dogs -- whatever I had -- and put in your rat study." He said, "It's a monumental failure, but nevertheless it shows you can do an experiment; I think it will pass an external examiner. And it did. But he really put me through the ropes in writing that introduction. And he really made me come to grips with writing about ideas. He said, "It's one thing to talk about ideas -- you can shoot the breeze with your buddies in the lab all you want -- you sit down and start describing what you mean by something in ideas, and it's hard."

LIEBESKIND: And linking it to others, and the actual work that's been done, and so forth.

MELZACK: That's right. And Hebb was famous for this, you'd give him something that you'd struggled with for three months, and he'd read two pages, and he'd say, "I don't know what the hell you're talking about;" and he'd give it right back to you. So he never told you where to go, he just said, "This is not it;" and I'm not sure that he knew where, how to go, he just knew this wasn't it. You hadn't come to grips with the issues. And finally, obviously, I did come to grips with the issues, and it became the basis of a tremendous amount of my thinking. Which also leads me to recognize, to the recognition that I, certainly many others have recognized, that what you learn as a graduate student is very -- has an impact on you for the rest of your life. Very often you work on the same problem for the rest of your life, that you worked on as a graduate student. So you have to be, one must be very careful, in guiding graduate students into a field that they're going to be excited by and turned on by.

Well, what everybody interpreted the dog work to mean was that these dogs did not feel pain, you have to learn to feel pain. And I knew that just couldn't be true. I even wrote the paper that way because I was almost compelled to and these dogs did not seem to feel pain; but I knew that they could feel pain, because in their home cages, they would respond to electric shock, but when they came out, somehow they would ignore it. And I came to the realization that the brain somehow had to receive information about what was going on in the outside world, but that was not the basis of a perception. The brain received this information, it evaluated that information, and then it would send messages down to some gating area, as I would call it now, where it would let the stuff in or keep it out.

Well, my aim was to do electrophysiological work with restricted dogs, and so I wanted to learn physiology. So far all I had done was what went under the name of classical comparative psychology. And that was when I went to see Jasper: "Who should I work with?" And he said "My friend Billy Livingston in Portland, Oregon, is a great surgeon, he's the chairman of the department of surgery. He has a laboratory where he is trying to do research on pain, he's convinced there is not one pain pathway but many -- you electrically stimulate the tooth pulp and you trace where things are going." And anyway, Livingston accepted me and I won a Commonwealth Fund fellowship to work with Livingston. And once again, I couldn't have worked with a more terrific guy.

LIEBESKIND: Now hold on, I want to interrupt you here. I know your work with Livingston for several years was a big turning point for you, and a very important influence. So before we get into that, I want to go back to something you said a little while ago, which is -- here you were under the spell of this great Don Hebb and thinking in Hebbian terms, which was so consistent with your own way of thinking and the way you wanted to think, and your emphasis and interest in ideas and so forth. And you get put into this project with the dogs and notice that they are reacting differently to pain. And all of a sudden this word, you know, here we are, we're doing this, this is a pain interview, all of a sudden, we have this word pain. I want to find out as much as I can from you now about, before we go on to your work in Oregon, what did you think of, what was known about pain at that time. What did you know? I mean, when you said pain, is that, I mean, there wasn't anything on that.

MELZACK: Nothing.

LIEBESKIND: You could have gone to your shelf, you wouldn't have found anything -- very little; there's this book by Livingston, I guess.

MELZACK: Yes, that's right.

LIEBESKIND: That must have been out. Did you then go read it at that point?

MELZACK: Oh yes. Yes, that was when I read it, when I was a graduate student.

LIEBESKIND: You said, "I'd better find out what's going on in the world of pain."

MELZACK: Absolutely.

LIEBESKIND: And what was your impression?

MELZACK: And he [Livingston] was several pages of my introduction to my PhD thesis. Pain was still an intellectual exercise at that time, though. It was not pain as pain, it was pain as a sensory system, as an emotional system, a motivational system, a reinforcing system, if you will, and I realized there were all these problems. Here on one hand you have people looking at pain from a psychophysical point of view, right? You give a painful shock and you get people rating how much pain do they feel, and can you draw a curve, a psychophysical curve. On the other hand you have people using the same kind of shock as a reinforcer, a negative reinforcer, so there was now a motivational affective component to it. I wasn't using those words at that time, but I was aware of them, that there were many dimensions to this thing that I was looking at. But it was still an intellectual exercise.

I still was very depressed, you know, pretty depressed, but nevertheless it was interesting enough that I thought I had to pursue it and would take a physiological route to try to find some answers. And the real passion for the field of pain came when I was working with Livingston. Now, I think it's important in terms of ideas and the history of ideas that Hebb wrote this textbook -- we mentioned it briefly before.

LIEBESKIND: Yes, that's right, and you said you didn't approve of it.

MELZACK: That's right. And I came back for summer holidays, to visit the family and all. And Don Hebb being --

LIEBESKIND: Back from where now?

MELZACK: Back from either Chicago or Portland, Oregon, or wherever, mostly Portland at that time, when he was working on the textbook. He would give me various chapters of the book to read, as he did to all of his graduate students. And I was very unhappy with that book. What he did was to try to bring everything together in a stimulus-response rubric. And so we had stimuli going into a brain and there there would be a holding mechanism, memory mechanisms, anticipation, attentional mechanisms, all of that; but it was all in a circle -- or a box, whatever

you want to call it, but he drew it as a circle, with an S going in at the bottom and another little R coming out the other end. And suddenly all those wonderful ideas that were best left unsaid, you know, or left vague, as they were in *The Organization of Behavior*. He suddenly put in little pictures. And I didn't like those little pictures.

And I told him I didn't much like some of his stuff on pain, and he had a bit on pain in there, and he was upset. I found that he had stuff on pain in his *Organization of Behavior* that I thought was wonderful -- pain as a disruptive influence, as an emotional disruption -- and here suddenly pain was back in practically the stimulus-response view.

LIEBESKIND: Was he kind of yielding to pressure, at that time, to formulate things in S-R terms?

MELZACK: Well, what he was trying to do -- number one, he was always trying to integrate information. Number two, he wanted to -- he felt that if he was going to sell his ideas on attention and memory and all these things that no other psychologist was talking about at the time, or incidental learning that he was interested in, all of these things -- that he would have to put it in such a way that the Skinnerian-trained psychologist would be even willing to read his book and use his book as a textbook, so that students could get exposure to physiological psychology at that very tender time in their life when they are introductory psychology students. So he was doing it for the right reasons, I just didn't like what it was, the way it was coming out. So we'll come back to this.

Or maybe if we don't come back to this, just let me say that I think it was a step backward in his thinking and I wasn't sure what it was that was bothering me about it, but now I am able to put my finger on it. He lost that sense of time, that we are not automata and we are not input-output machines, and he had come right back to the input-output machine kind of idea; and to me, life is continuous action, it's continuous perception; and what I do now is influenced not only by what happened in the past, but my dreams of what's going to happen generations from now. It's concern about life long after my death. And there is no place for that in this S-R box-like concept.

Well, so while I was at Livingston's laboratory, the guy who taught me was a fellow called David Kerr, and he had worked with a guy called Hagbarth, and they had discovered that if you electrically stimulate the reticular formation, you block the flow of impulses through the spinal cord somewhere. [KE Hagbarth and DIB Kerr (1954). Central influences on spinal afferent conduction. *Journal of Neurophysiology*, 17: 295-307.] So David Kerr, a bright guy, fun, much fun, a good sense of humor, he taught me how to open cats' heads and how to put electrodes in and how to read an oscilloscope, you know, all the basics of electrophysiology of the 1950s, the middle 1950s. But what we found, using Livingston's idea of stimulating the tooth pulp, was that we found at least five pathways that were involved and they were going, some through the central grey, the central tegmental tract, the spinothalamic tract and so on, the medial lemniscus and what not, and the reticular formation. [DIB Kerr, FP Haugen, and R Melzack (1955). Responses evoked in the brainstem by tooth stimulation. *American Journal of Physiology* 183: 253-258.]

LIEBESKIND: Yes, I remember that famous paper.

MELZACK: And the next step -- so that was what I learned. David Kerr then went away and I remained the only fellow working at Livingston's lab. I then mapped where those impulses went at the cortex, found a somatosensory area separate from what was the traditional one. Then I thought the next step would be to really make lesions -- you know, being a psychologist, that's what you did, you made lesions -- make lesions in different brain areas; and [I] made lesions and generally cats responded less to pain, or there was no change in their response. And I had developed a couple of rather clever tests of where pins would come through holes in the floor -- the cats could jump over a barrier to get away from them, or suddenly a heating element like a toaster element would make them jump over a barrier, but those that sat and the fur would sizzle -- they never burned actually, but the fur would sizzle and you could smell it -- you knew there was something wrong.

Then there were cats that overresponded. I didn't know what the hell to make of that. But I had a wonderful technician. He was simply a technician -- he was there to prepare the cat for me, get it ready for me, he anesthetized it so that when I came in it was already zonked out and ready for me to do the surgery. John Friend was his name, and he became a friend, a genuine social friend; he said, "You know, those are like Margaret Kennard's cats."

Well, what's meant by that? Margaret Kennard [1899-1976, a pioneer in the study of brain damage and functional recovery] was a neurologist who came and worked with Livingston for a while, and she put alumina cream on the cats' spinal cord, and these animals became hyperesthetic -- they showed allodynia, all the traditional referred pain, all the clinical symptoms -- which nobody, by the way, has been able to repeat afterwards, amazingly -- she seemed to apply the alumina cream in some amazing way that no one could subsequently confirm. But in any case, it was John who said, "Ron, those look like Kennard's cats." Well, I said, "John, I'm interested in cats feeling less pain, not more pain." But when we then did the histology on these animals, the animals that showed this overresponse all had lesions in that lower central grey and sort of lateral central tegmental tract area. And so the conclusion of that paper is that there is a continuous tonic inhibitory control over what comes in.

Suddenly things began to fall into place for me. The restricted dogs. Suddenly I could understand how information could go up to the cortex through very rapidly conducting fibers. Those could be the dorsal column fibers that carried precise information that alerts memory, activates memory mechanism and memory stores; so that you could now explain why Pavlov's dogs that got shock and food in one paw, would learn to salivate to that, and when they got a shock with no food to the other paw, they would scream and try to escape out of the conditioning harness. Because the input that went in had to be evaluated and localized before the perception occurred. That had to be at some fairly low area. So suddenly I see fibers going up to the brain, fibers coming down from the brain, and doing something at some low area. But what is astonishing about all this, John -- so I discovered this, I wrote this paper, I had, I was very skeptical of my own data. [R Melzack, WA Stotler, and WK Livingston (1958). Effects of discrete brainstem lesions in cats on perception of noxious stimulation. *Journal of Neurophysiology* 21: 353-367.]

LIEBESKIND: This is Melzack, Stotler and Livingston?

MELZACK: That's right.

LIEBESKIND: '58?

MELZACK: '58, that's right.

LIEBESKIND: Who was Stotler?

MELZACK: Stotler was an anatomist, and a superb anatomist. He was an anatomist, in fact, who had worked with Papez, the famous Papez [James W. Papez of Cornell, 1883-1958]. So it was he, in fact, who, with David Kerr, worked out all these different pathways. David was recording from them physiologically, but it was Stotler who said, "That is the central tegmental tract," which happened to be a very special interest of his when he was a student of Papez. So anyway things, so these things were falling into place for me; but then I took a break from the field of pain and I went to England and Italy.

But before I go into that, let me tell you something else very important. Livingston had a pain clinic just down the road from John Bonica's pain clinic. John Bonica then was in Tacoma [Washington], Livingston was in Portland, Oregon, and I am sure it is only for reasons -- Livingston had met Bonica, I believe. But Bonica was a lowly anesthesiologist and in those days it was a real totem pole thing. Your surgeons resided at the top of the totem pole; your anesthetist was down at the bottom of the totem pole. And even though Bonica was writing great stuff that fit, and Bonica was quoting Livingston left and right and obviously very influenced by him, Livingston would not acknowledge Bonica. And somehow I never got to know Bonica -- I mean, it was really Livingston's feelings about him that brushed off on me and I never -- stupidly -- drove up to Tacoma. I used to drive up to Seattle very often, living in Portland. I used to roam all over the place in my old Buick, my ancient Buick, up there. So I never got to meet John Bonica, which is awful. But Livingston had the pain clinic. And maybe he copied the idea from John Bonica's -- I am sure John had the very first pain clinic. [*Ed. Note: Bonica and Livingston opened multidisciplinary pain clinics independently at about the same time in 1947.*] But whatever, whoever, Livingston had this pain clinic. And it was there that one day he said to me -- I was supposed to go work with Livingston for one year, I ended up with three years.

LIEBESKIND: Three years?

MELZACK: Yeah. So coming into my second year, though, he said, "You know, you could work on cats from now till the cows come home, but you got to see people in pain. From now on, you start coming to our pain clinic every Tuesday afternoon." So I started going to the pain clinic.

LIEBESKIND: He had a pain clinic and they were --

MELZACK: Yeah, everything.

LIEBESKIND: It was multidisciplinary?

MELZACK: Multidisciplinary in the sense that, yes, it really was. It was multidisciplinary in the sense that there was Livingston the surgeon, primarily a peripheral nerve surgeon at that time, Fred Haugen the anesthetist, doing general anesthesia and head of the division of anesthesia within the department of surgery, so Livingston was really his boss. I think there were exceedingly few, if any at all, departments of anesthesia in those days. They were just beginning to emerge. There was a general surgeon called Clair Peterson, another surgeon -- I've forgotten his name. Anyway, so there were -- oh, a psychiatrist called Gerhart Haugen who was Fred Haugen's brother, who came to these things regularly, because we had a seminar first, you see, where everyone would talk about what interested them in the field of pain, strictly a pain seminar.

LIEBESKIND: So it really was a pain group.

MELZACK: A pain group.

LIEBESKIND: And it was interdisciplinary, by God.

MELZACK: Absolutely, and we're talking 1954 to 1957 here.

LIEBESKIND: Did you have the feeling at that time that this had just begun, or had this been going on for a while?

MELZACK: It had been going on for about a year before I got there -- oh yes, it had certainly been going on.

LIEBESKIND: You can trace it back to at least '53 or something?

MELZACK: At least to that, that's right. So *there*, one of the first patients I met was Mrs. Hull.

LIEBESKIND: I've heard you talk about her, yeah, sure.

MELZACK: And that poor lady was an amputee, she had diabetes, she had developed gangrene, lost one leg, and that's when I got to first know her; and then she used to come in on a fairly regular basis. Livingston had the idea of giving people intravenous procaine, a slow drip of intravenous procaine -- which helped some people. It helped, seemed to help her for a while anyway, briefly, you know, hours, days, but that was fantastic, given the horrendous pain these people were having. Then she lost her other leg and now the pain was horrible. She was a very well-read, intelligent lady and she began to describe the different pains -- they were shooting, they were cramping, they were this, that, you know -- palpitating, throbbing. I began to write the words down. I began to feel there was something in these words that was important. That was the beginning of the McGill Pain Questionnaire. But by the time I left Portland, I --

TAPE ONE, SIDE TWO

LIEBESKIND: OK, you were saying something about Jack Brookhart?

MELZACK: Right. Jack Brookhart [John H. Brookhart, 1913-95, who conducted many studies on the relationship of brain mechanisms to motor neuronal output], was the chairman of the physiology department [at Oregon, 1953-79]. And he too used to come to these seminars. He didn't come to clinic, but to the seminars. And he was very fond of Livingston, who was very fond of him.

LIEBESKIND: He's a very famous guy, Brookhart.

MELZACK: Yeah, he really was. So Brookhart had just had a sabbatical leave and he'd worked with [Giuseppe] Moruzzi [(1910-86), who collaborated with Horace W. Magoun (1907-91), at the Institute of Neurology at Northwestern in 1948, on the famous EEG study of the reticular formation] in Pisa. And he came back saying that Moruzzi believes that the field is moving in the direction of trying to relate brain and behavior, and he would like to have a psychologist there. And so, by gum, he said, "So you write to Moruzzi and he'll expect a letter from you." Moruzzi wrote back saying that he would love to have me come spend a year as a post-doc with him, but he couldn't take me the next year, but the year after. That would be 1958-59, sort of. So what am I going to do in the middle, in the intervening time. Amazingly, a letter came from a guy, well, from a friend here first, Dalbir Bindra, who later was chairman of our department [at McGill], but I had known D.B. for a long time. And Bindra had gone to London to fill in for a job that had become vacant when the chairman left to go to the United States. His name was Roger Russell. He was an American.

LIEBESKIND: I know Roger.

MELZACK: He was chairman at University College, London, after Sir Cyril Burt [1883-1950], in fact. So when Roger Russell decided to go to Indiana, there was a slot open. Dalbir Bindra was there for a year and they still couldn't find anybody, so they asked him if he knew anyone who could fill in. And he'd heard that I, this business of mine, you know, I got a year to do nothing in, and so, I got a letter, would I come as a lecturer, a temporary, an assistant, whatever they call them, a visiting lecturer? So I went there as a visiting lecturer, working, and I don't think there was anybody in the physiological field in that department. So I had to get space to do physiological work somewhere, and somebody said, "Go see Brian Boycott." Now Brian Boycott [Brian B. Boycott 1924-2000, zoologist and neurobiologist, at UCL 1952-1970] was very famous, had become famous working with J. Z. Young on octopus brains and vision and so forth. I had just read an article by Brian Boycott in *Scientific American*, in fact, on the function of God knows what, lobes in the octopus brain.

LIEBESKIND: Read it at that time.

MELZACK: Read it around that time, yeah. So I went to see Brian, and he said, "Oh, sure, I'll help you out." He was a lovely guy. "I'll help you out and I'll get you some space at Regents

Park Zoo.” He was in the zoology department. So I had a room now in Regents Park Zoo in the reptile building. And right next door to me was Desmond Morris [1928-], of chimpanzee painting fame and *The Naked Ape* and all that. And I told Desmond Morris that I would like to do some work on the hawk-goose shape. Remember [Nikolaas] Tinbergen did a study -- Tinbergen and [Konrad] Lorenz -- a famous study [Lorenz (1903-89) and Tinbergen (1907-88) shared the Nobel in 1973 for their studies of animal behavior]. You fly a piece of cardboard in such a way -- it’s cut out in a way so that it looks like there’s a long neck and a short tail and it looks like a goose in flight.

LIEBESKIND: I can visualize it. I know just what you mean.

MELZACK: That’s right; but if you turn it around and fly it in the other direction, so it’s got a short neck and a long tail, it looks like a hawk and animals are afraid of it. So I thought, that’s like an irrational fear -- now we’re coming back to my master’s. I keep coming back to everything I’ve done before, John, you’ll find. So I thought I would like to raise birds in isolation and show that if you raise them with an animal that’s like their mother and father, with a long neck and a short tail, and suddenly, for the first time ever, they see a thing with a short neck and a long tail, they’ll be scared, right? In other words, the unusual and the strange will frighten them. And if you raise, if you fly the other thing - - if you raise them with a hawklike figure, when you show them a gooselike figure, that will scare them. So he said, Desmond Morris said, “I know exactly who will help you. Niko Tinbergen would just love that kind of an experiment.” So he put me in touch with Tinbergen, who used to come down to London quite often from Oxford -- I think Oxford --

LIEBESKIND: Cambridge.

MELZACK: Cambridge, Cambridge. So Tinbergen would come down. First I was going to do pheasants and we got pheasant eggs and the pheasants all died in no time flat. So I switched, he said, “Switch to mallard ducks, they are very hardy” -- because I got them right out of, you had to get them right out of the egg, the chipping stage. So I remember taking a train somewhere up -- I don’t know where -- and coming down fast as hell. I had everything arranged, so each egg was put in an incubator; and then as the teeth, the little chipping tooth was coming through, they would then go into a little cylinder, a cardboard cylinder, just a piece of cardboard that I put a piece of Scotch tape around. So the animal came out all by itself until it dried off in the incubator. Then it went into a little long box where now it was going to live. These animals virtually all survived; Tinbergen was right, they are very hardy animals. And then came the great day of presenting these things flying overhead.

LIEBESKIND: They were raised with one or the other of these figures.

MELZACK: One or the other, and then I had two control groups raised with nothing. The upshot of it all was it didn’t make a goddamned difference what they were raised with. But the animals that had been raised with nothing were very upset by things; they would really flap their wings and make quack-quack noises and I don’t know what all.

LIEBESKIND: If it was the hawk shape?

MELZACK: It didn't -- No. The guys that were raised with hawk shape or goose shape, it didn't matter. When you turned it around, nothing happened. But the animals raised with nothing, they were afraid of both of them.

LIEBESKIND: Of both?

MELZACK: Both. But, there really seemed to be more quacking and stuff to the hawk shape than to the goose shape, which shocked me. It meant it was built into the bloody brain.

LIEBESKIND: Right. Early experience wasn't doing it, it was built in.

MELZACK: Built in. Well, I didn't know what -- I used to then collect my data very carefully and keep my data and happily I kept it when I went to Pisa and there were guys there who were working on sensory input that goes to the reticular formation and sensory input that goes directly to the visual area. And I thought, maybe what's happening, what happened with the ducks is that the ducks that were raised with something were oriented. We would write down everything, you see. I had two students who worked with me -- Elizabeth Pinnock and I forget the name of the other guy -- we're talking way back here. And I go back to the data and, damn it, when the things were presented overhead, there was more interest if they saw a hawk shape, there was a greater tendency to keep looking up in the air than if they were presented with a goose shape. These were in the animals that had been now presented with animals, with things before, you see? So somehow the emotion had habituated out -- but there was orientation.

LIEBESKIND: But they did orient more.

MELZACK: Yeah.

LIEBESKIND: Which you hadn't seen in the first go-through.

MELZACK: Hadn't seen it, so I really didn't know. So that was why -- so I learned a very important thing, how important it is to hold onto data. So and then, even more astoundingly, if the thing was, if I presented them at the horizon sort of, coming around the room at horizon level, they oriented more at the goose shape than at the hawk shape. So I concluded from that that your parents tend to fly in -- geese and ducks fly in at an angle, from horizon level, whereas hawks swoop.

LIEBESKIND: The bad guys.

MELZACK: The bad guys swoop. So that maybe played a role. Anyway, that was great fun. So that was a couple of very interesting papers and I put that aside. And now the problem of finding a job. So I wanted to come back to Canada, because I love Canada, it's a great country, and my parents were here and it just, you know -- it was not possible to come back to McGill, they were all booked up. So I wrote to, I think, every chairman of every department of psychology right across the country and there was just no slot for me. Lots of letters that said "I'd love to have you, you've got just the kind of ..." I had had five years of postdoctoral type

training at that time, in physiology, psychology -- so all of them were sort of “next year we might be able to fit you in.”

LIEBESKIND: What year are we talking here now?

MELZACK: Now we're talking 1959, looking for a job for 1959. Well, my friend, good old buddy, Dalbir Bindra, heard from a friend of his, Davis Howes, that they were looking for a physiological psychologist at MIT.

LIEBESKIND: Came through again.

MELZACK: So he said, “Ron, you've got nothing to lose, why don't you apply.” So I wrote out a CV, I sent a CV and a description of everything I was doing to Davis Howes, who was the head of the search committee for this little group that comprised John Swets, Dave Green, Roger Brown and Davis Howes, so really quite an elegant little group. I never heard from them. And then one day, just within a week of leaving, I got a letter saying I had been chosen. So I couldn't believe it. I had already written to my father saying, “Can you support me, can I live at home with you and ma? I don't have a job;” which was a terrible bitter pill.

LIEBESKIND: You were thinking about that bookstore.

MELZACK: Yeah, that's right, that's right. And I had written to Hebb saying, “Could you let me have a little bit of laboratory space? Because it looks like I won't have a job, but I can live at home and if you can provide me with money to buy rats, I'll be fine.” And he said, “Oh, sure.” So there I was, set up. But anyway, along came this job offer. So I went to MIT and once again I was in a department that had no physiological psychologist and I had to go somewhere else to find space. So Davis Howes said, “There's a great guy in biology -- there was no physiology section within the biology department at MIT -- a great guy called Patrick Wall.”

LIEBESKIND: Patrick Wall.

MELZACK: He said, “Why don't you ask him if he might dig up some space for you?” So I went to see Pat Wall, and he'd known of my work. Pat reads voraciously, everything. And I had given a couple of talks on the BBC Third Program -- I was invited to when I was in London -- and those were both published in a magazine called *The Listener*, that used to be people's talks on the BBC, and book reviews and all that sort of thing. So Pat knew my thinking pretty well. And it turned out that it was very congenial to, or very much like what he was thinking. Well, Pat is wonderful, he'll go out of his way to help people -- he does, absolutely. He's got many marvelous qualities and one of those qualities is, if he likes you, he'll do almost anything for you.

So he said, “Well, there's no place in biology as such, but there are people in food technology, right next door to biology, and they are wasting that space doing the stupidest goddamn experiments you've ever heard of in your life. They've got a ton of money and they're working for NASA, and they're looking at the impact of cans of food landing on the moon.” They had a kind of catapult that looked like it came out of *Hagar the Horrible* comic strip], you know. And they would put cans of food and they'd slap it against brick walls or stone walls to see if the cans

dented, came apart, or whatever the hell happened to them, the contents were examined. Anyway, he got me some space there. [both laugh]

So I immediately got some dogs, some purebred beagles, and began to raise them in restriction and then implanted electrodes. And now I was really into this business of there being a rapidly conducting pathway, and that's what's happening. It's not that these animals have to learn to feel pain. What their brain has to do is to select what is important and what is not important. And so I was constantly thinking of rapidly going up, activating memories, anticipation, all this, and that goes down, again, rapidly conducting descending fibers that can open or close a gate, without using that word at that time. Then at some stage, so I used to see Pat -- from time to time I'd just drop in on him. He was usually, he was always experimenting. Pat's just an inveterate and he can't stop experimenting. He still does his own rats. When I phoned him yesterday, he was in the middle of a rat. We talked and he said, "Ron, the 10 minutes of stimulation is up. I've got to get off the phone." So I said, "That's fine."

So in the course of our talking I said to Pat, "You know, you and I think a lot alike about a lot of things. Why don't we write a paper together?" So we wrote a paper that was published in *Brain* in 1962. And we struggled with that paper, putting it all together, presenting a pattern theory format to practically everything, because we were certainly both dead against the Cartesian S-R straight through one-to-one psychophysical bullshit.

LIEBESKIND: That was Wall and Melzack?

MELZACK: That was Melzack and Wall. It was sort of my idea and we initiated it and it was certainly jointly done all the way through and Pat's contribution was tremendous. And I think it was I who had the idea, in fact, of putting it all out -- the time when it became feasible -- we were writing and writing and writing and writing and writing; when it became feasible was when it dawned on me that if we set it out as a series of propositions or statements, and then provided data to support the statements, like you need more than one impulse, you need patterning, and that sort of thing. So the paper really came along fine and was sent to *Brain* and the editor of *Brain* was Lord Brain [Sir Walter Russell Brain, 1895-1966], who signed his letters "Brain" -- and he was very pleased with it and he accepted it and it was published. I got a couple letters from Don Hebb -- a letter from Don Hebb saying "Atta boy" -- and I think three people read the paper and that was the end of it.

In the meantime -- so I still -- now I got involved in pain. Really in every way -- not just merely intellectually, but Mrs. Hull and her pain -- now the passion is there. Now I suddenly realized that by an incredible fluke, I had stumbled into an area that I could make an intellectual life out of. That gave my life the meaning that I wanted in life and was useful and it was a contribution and I would do something good with my life, or I could do something good. That sense of guilt that goes along with depression was always with me. I didn't know what I was guilty of, but I was always guilty. And suddenly even that could take some of that guilt away, assuage that guilt, if I were doing this good thing to help people somehow, or that would or could help people. So I was now really well into the field of pain -- gut and head involved. And at one point I suggested to Pat that we try another paper in which we take that first one, that was a good paper but never was read by anybody, and had no impact on anything whatever, and deal

exclusively with the problem of pain. So Pat thought that was a good idea. And at that point I was offered a job here at McGill. The chairman of our department came down to Boston and offered me this job.

LIEBESKIND: Who was that?

MELZACK: His name is Ed Webster. He was an industrial psychologist, a very, very nice, decent guy, and he was a very good chairman for this department after Donald Hebb. In fact, he was Don Hebb's choice, it was as simple as that. There was another guy who wanted to be chairman, who did later become chairman, but Don Hebb thought that the guy to be chairman now was this industrial psychologist, Ed Webster. And now we're in the 1960s and now suddenly money is being pumped into universities like crazy, because the Russians have scared the shit out of the United States by sending up a Sputnik with a dog in it. And suddenly there's money available through the Armed Forces and everything. So I got involved in applying for some of that money -- I got an Air Force grant when I was at MIT. So here I was in the field of pain and suggesting to Pat -- I'm just bringing this back to where we were -- so suggesting to Pat that we write a paper on pain, but being offered the job in Montreal. However, what was very nice was this Air Force grant I had, that allowed me to work on the restricted dogs, allowed me also to have a graduate student. And I had a graduate student who did a master's in electrical engineering. And his boss was Walter Rosenblith [(1913-88), MIT provost 1971-80, who pioneered the use of computers and mathematical models in studying the brain]. And Walter Rosenblith was nice enough to let him work with me on a very biological/psychological problem. And so his name was Steve Burns, a lovely --

LIEBESKIND: Burns?

MELZACK: Burns, and he's still at MIT. I don't know what laboratory he's in. I don't think he ever became an -- went on to academic staff, but he's certainly on staff in one of their big laboratories. Anyway, I flew to Boston once or twice a week to work with Steve and I'd usually stay overnight with Pat. In fact, I'd usually bring a bottle of whiskey of some kind, so that's how we started, we'd argue like crazy. Pat does not like the brain. He's not comfortable with the brain, he doesn't like working with it -- it's fine, it's there, he knows it's there. Pat reads philosophy voraciously -- I mean, he reads Hegel and Kant and everybody; but nevertheless, in science he is a spinal cord physiologist and he thinks like a spinal cord physiologist, and he can't get away from that. So we've always fought and argued, but there has always been a fundamental respect there between the two of us, so that no matter how mad we used to get at each other, we'd always be friends, and that friendship has survived all these decades.

So I now had left, we began to write this paper and sending back, drafts back and forth -- I'd bring them down, we would argue, and so on. And then at some stage, we began to organize the paper into components, the main, the gate control theory got invented, the upwardly going thing I called the central control trigger, because it activated central control processes in the brain. The action system was known as the action demand system, and there is in fact a paper that is an earlier version -- Pat was invited to a physiology meeting in Tokyo, and they wanted a paper. It was published in some very obscure -- well, it's not so obscure, it's the IEEE journal or

something, but that's where it was published. So that was one of the earlier versions. So it's fun to have a published earlier version, one of many, many, earlier versions of that gate theory paper.

Anyway, I suggested that we really aim for the top and try *Science* and see what the hell happens. The worst that will happen is to get rejected. It got accepted. We were astounded. Well, so, then you know the rest, because some people loved it and most people hated it and suddenly physiologists got to work to try to show that we were wrong because it represented --

LIEBESKIND: Oh, I think, surely, most people loved it and a few people hated it, wouldn't you say?

MELZACK: Well, no, it was really -- most physiologists hated it because it was a real -- suddenly -- Well, there were some physiologists who loved it too because suddenly it meant that you could start looking in the substantia gelatinosa, you could start looking for modulation -- they had seen modulation in various systems. Certainly the work of Magoun and Moruzzi had set the stage for modulation, so to that extent, yes -- but mostly the people who were becoming interested in the pain field did not like it. Didn't like it a bit. But the psychologists loved it.

LIEBESKIND: Before you get into the reaction here -- This is obviously an important part of our interview here. Before we pursue the reaction, can we talk just a little more about you and Pat and the devising of these ideas and so forth? You've spoken generally about it. How about the term "gate"? Now that wasn't in the title; it was just used in the body of the paper.

MELZACK: That's right.

LIEBESKIND: But where did that come from? Is that a, was that out of -- ? That's a sort of an electrical engineering term almost, isn't it?

MELZACK: Yes, that's exactly right. And if we were not at MIT, I'm not sure we would have used the "gate", the word, but we used a lot of engineering type terms in there, trying to keep it as simple as possible, so it really evolved. And "gating", the engineers were talking about "gating" all over the place, all those computer guys were talking about it, and computers were big anyway.

LIEBESKIND: So the word was around.

MELZACK: The word was absolutely around.

LIEBESKIND: Do you remember when you first settled on that, was that, can you remember how that actually worked out?

MELZACK: Well, my recollection is that one night, I was writing downstairs in my study room right next to where you are, you know, and the word "gate" came out in the course of writing, and I felt, that's it. And I wrote out, "gate", "gate control", and all that sort of thing, and it stayed on in subsequent drafts, Pat didn't change it. Pat and I would change each other's stuff, but that stayed. The "action demand system" got simplified, I think it was Pat who coined that

term; that got, I took out the “demand”, I didn’t think that was needed. “Central control”, I think that was more complicated, it had the word “cognitive” or something in it, Pat took that out and it simply became “central control.” I had a bigger brain box, he made it smaller. [both laugh] Soon the gate was overwhelmingly large, but that was fine. But all the basics were there, and we could both live with it. The large and small fiber stuff, that definitely was Pat’s work.

LIEBESKIND: That derives back to Noordenbos, right?

MELZACK: And that goes back -- Oh yeah. Oh, we should really bring Noordenbos into this picture, because there would be no gate theory at all if it weren’t for Noordenbos [Willem Noordenbos, 1910-1990]. I mean, my God, how could I have left him out? Because that’s really what made me go to Pat and say, “Let’s do a paper on pain,” was reading Noordenbos’ paper, and Noordenbos’ book.

LIEBESKIND: How did you find it?

MELZACK: OK, what happened is really funny. There’s a guy called Padmakar [P.] Lele [(1927-98), pioneer in the use of ultrasound to shrink tumors], who’s an anatomist, who was at Harvard. And he knew Bill Noordenbos, because Pat Lele had worked with Waddell and Sinclair and those guys at Oxford. Pat Lele and his wife became friendly with Lucy [Melzack’s wife] and me. And one night he invited us to a party at his house, because a neurosurgeon from Holland was coming to visit that he had known. He used to come, the surgeon came quite often to Waddell’s place, a real pattern type guy. And would we come? Well, Lucy said, “I’ll stay home with Laurie,” who was a little baby then, “you go to the party.” So I went to the party. And Frank Ervin was at that party. And Noordenbos was brought by Bill Sweet [William H. Sweet, (1910-2001), chief of neurosurgery at Massachusetts General Hospital 1961-77 and president of the American Pain Society 1981-82] and I was introduced to him, we shook hands, and that was it. Then before I knew it, Noordenbos and Bill Sweet were gone. And I said “Who was that guy?” and it was Frank Ervin who said, “Ronnie (or Ron, I forget what he called me then), he wrote a terrific book on pain -- you mean you haven’t read it?” And this was 1961 and Noordenbos’ book [*Pain*] was ‘59. And I hadn’t read it -- never heard of it. So he said, “It’s a book -- when you read that book, it’s the book you will wish you had written yourself.” He said, “It’s a perfect book.”

LIEBESKIND: Frank understood that.

MELZACK: Frank Ervin understood that. Did you know that Frank is at McGill now? He’s been at McGill for years [Ervin, a neuro-forensic psychiatrist, is still at McGill as of 2005].

LIEBESKIND: Yes, I think I did know that. He was at UCLA for a while.

MELZACK: Yes, that’s right. He’s a very interesting guy, Frank. Bright as hell. Anyway -- whom I never see, interestingly. I would have thought I would become friendly with him, but, well, he’s hardly ever here. He’s always with his vervet monkeys down somewhere or he’s somewhere. Anyway, the next day Lucy and Laurie in her little carriage and I went to Harvard Square, thinking amongst all those bookshops we’d find Noordenbos’ little book on pain, right?

Published by Elsevier Press? Couldn't find it. Finally one guy was smart enough to say well look, there's a bookstore right near Boston University -- try it, they may have it. So we got in our car and we drive there -- they had it. So I bring it home. It's a little skinny book. And that night I started to read it and I read well into the night -- couldn't stop. Went to sleep and finished it the next day, excited as hell. Because suddenly, shit, it dawned on me, he's got a large fiber and a small fiber going into the spinal cord. All I've got to do is take that large fiber and draw a collateral going up to a brain which then --

LIEBESKIND: You were already thinking of a fast system?

MELZACK: Yes, that's it. This is it. And then you would have a descending down thing where he's got his large and small fibers converging on each other. And we've got a theory. And that's how it happened.

LIEBESKIND: Now the theory came out -- I mean, my thinking on it -- you may disagree -- is that the way it was written in the '65 *Science* article, there was a lot of emphasis on the afferent large fiber-small fiber interaction. It mentioned, but not as much emphasis on, the descending control, the central control mechanism.

MELZACK: Yeah.

LIEBESKIND: But I wonder if that was in any sense a reflection of what you said before, Pat's focus on the spinal cord.

MELZACK: Oh, it was, it was.

LIEBESKIND: I mean, did you guys argue about the degree to which --

MELZACK: We argued about everything. We argued about absolutely everything there was to argue about. We argued about everything. And I mean, he really -- if it was going to be happening -- he didn't like the descending inhibitory business very much. He just sort of -- he tolerated it. That's how we write. At a certain point Pat says OK -- I mean, he just gives up, and I give up too. So at some point, we both gave up fighting and what there was -- we trusted each other that we wouldn't put back in or take out -- that it would stay the way we had left it at the last draft kind of thing, and that's what we published. We published several other papers together -- little experiments and things -- but that's really been the way we worked and the way we wrote *The Challenge of Pain*, and the way we wrote -- What happened there is -- well, I don't know, I'm going ahead of myself. Anyway, so the gate theory came out and there was lots of flak. But what was interesting is that it later became a citation classic.

LIEBESKIND: The most influential article in the field of pain.

MELZACK: Yeah, it was, that's right. It was number eight, among the papers published in the 1960s, it was number eight in the neuro, in the basic science -- I forget what -- the premedical sciences, that's what it was -- very high citation [count]. But when we wrote the little thing, they ask you to write a little story to go with it, and what I put in it was the fact that it took a very

long time to catch on -- so the editor of that [*Citation Classics*], a guy called [Eugene] Garfield, decided to take another paper written by [David] Baltimore [virologist b. 1938, president of the California Institute of Technology 1997-], who later won the Nobel Prize [1975], which had roughly the same number of citations, and take a look at it by year. So what happened was that Baltimore's had its peak citations within the first three years of his, the paper's publication and it came down gradually over the next 15. Ours *grew* for the next 10, 12, years before it reached its peak, and then started to come down. So it really took that goddamn long.

LIEBESKIND: This was growth with the field of pain. It pulled the field of pain with it, didn't it?

MELZACK: Yeah. It did.

LIEBESKIND: I don't know how many people could say this, I am sure there are many, but I can say very honestly it was that paper that brought me into the field.

MELZACK: It brought Ken Casey into the field, that's right.

LIEBESKIND: I read that paper as I came to UCLA. So naturally the more people it brings into the field, the more they're influenced by it, they're going to start quoting it as their work goes along, well, that's going to take some time.

MELZACK: That's right.

LIEBESKIND: Plus all the clinicians who were extremely influenced by it, weren't they?

MELZACK: That's right. So now here I am at McGill, and then I get Ken Casey [b. 1935, Chief of the Neurology Service at the Ann Arbor VA Medical Center 1980-, president of the American Pain Society 1984-85] to join me. Ken Casey applied to work with me as a post-doc. He was married, had a couple of children, he had worked with Paul MacLean [(1913-1993), Chief of the Laboratory of Brain Evolution and Behavior at NIMH 1971-85], he had worked with, oh dear, what is his name, done a lot of work, Towe, Arnold Towe [1927-2002, Professor of Physiology and Biophysics at the University of Washington 1953-83], he worked with Towe. He's from Seattle, Ken is. He was really born elsewhere, but his father was the head of a prison in the Washington state system. And Ken went to school and put himself through school basically by working in logging and trucking. And Ken and I became very good friends and colleagues almost instantly.

LIEBESKIND: He's a sweet guy.

MELZACK: He's wonderful. And Jean [Casey], who became a great friend too, so when Lucy's mother was sick, we left the kids with Jean. Well, what happened was that I wanted to take the gate theory further up into the brain.

LIEBESKIND: Right. Pat wasn't the guy to do that with.

MELZACK: No. One of the most horrible days of my life -- I mean, there aren't many days in your life you can say are among the most horrible days of your life. It's not one when you're told you are sick or anything, those must be very special days. But it was a day in which I proposed to Pat the model that really Casey and I proposed, wrote in the paper. I showed it to Pat and I said, "This is what you and I must do now." Well, he was angry and bitter and he didn't want any goddamn part of this, and I was all wrong and I was leading the field astray and -- wow, I got drunk that night, I'll tell you.

LIEBESKIND: You were starting to tell me, and I held you off a little bit because I knew it would come up again today, a kind of similar story with respect to the cingulum work.

MELZACK: Absolutely -- that comes later.

LIEBESKIND: Where he again -- so he has these blind spots, Pat, doesn't he?

MELZACK: To him -- it's not merely blind -- it's not like "I'm not interested" -- it is no goddamn good, it is killing the field, it is the *enemy*. And the guys working on that are the enemy.

LIEBESKIND: What is that all about? Why would he get so excited?

MELZACK: You may recall -- I know this is all on the record, but I think Pat's lighting into Ken Casey after one of those meetings that Jean-Marie Besson [French physiologist and pharmacologist, IASP President 1996-99, and co-founder of the Société Française de la Douleur] had -- there was some meeting that Jean-Marie Besson had where Pat really lashed into Ken Casey. And I'm sure that's part of the bitterness. Anyway, Pat didn't want to do this paper. So I turned to Ken. Ken didn't know that I had asked Pat -- I'm still not sure that he knows it. But, so I asked Ken if he would join me.

LIEBESKIND: He was a postdoc with you at this time.

MELZACK: He was a post-doc with me, we were working on temperature changes in the brain -- changes in metabolic activity in the brain, because I was looking for something slower than these goddamn fast nerve impulses that were zooming back and forth. Something had to be going on more slowly; in real time. And anyway, so Ken and I did about three or four papers. He was with me for two years. He wanted to stay a third year and I said, "No, Ken, it is time for you to find a job and be on your own." He was just too good. And he was also older, he'd worked with Towe, he had spent a couple of years working with MacLean, Paul MacLean.

LIEBESKIND: He was a medical student, he had been in medicine.

MELZACK: That's right, but he was going to be a physiologist. So he went to Michigan. He got a job at Michigan and, of course, that's where he has been ever since. While he was at Michigan, he decided that he wanted to become a neurologist, which he had decided before he didn't want to become -- he now changed his mind. He became a neurologist and he is, as you know, a famous guy and you're going to interview him, I'm sure, if you haven't. Anyway, so

Ken -- that was a wonderful period. That's now since 1967-68, that paper was published in '68 or something, with Ken, in a symposium book, which was a funny place for it to be published, but that's where we published it. Because now these ideas were becoming -- well, anyway, let's not worry about that.

Also, when I was at MIT, there was another guy I met that was very, very important, and that was Warren Torgerson [Warren S. Torgerson 1924-, professor emeritus of psychology at Johns Hopkins]. I didn't know what to do with those pain words. I had done an experiment and all that came out of it was that the more pain people have, the more words they use, and I knew also that the words fell into groups like hot, warm, burning, searing, that sort of thing; palpitating, throbbing, beating, pounding. There were obvious groups, but I didn't know what you'd do with it.

Now at MIT, when I was at MIT, the psychology department had a thing they called the Pretzel Club. So we ate pretzels and drank beer every second Thursday night or something -- I've forgotten how it went -- but the guys from Lincoln Labs and other places - psychologists from a variety of different parts of MIT that were really far out -- they were special laboratories working on government money -- these guys would come in and join us. And one of those guys was Warren Torgerson. And we each gave our little spiels and Warren Torgerson gave his talk on multidimensional scaling. I'd never heard of multidimensional scaling. In fact, I hate statistics. If anything, I used to run from statistics. But here suddenly is a statistician who really had a technique that looked like it could do the job. So I went to Torgie and said, "Would you do this work with me on these pain words?" And he said, "Well, let me think about it," and after a while he said, "Yeah, that would be fun." So we began to do these studies. Now, what happened --

LIEBESKIND: You know, I have to make a comment here. It suddenly strikes me that you have had incredible success in your career interesting other people in working with you.

MELZACK: I have.

LIEBESKIND: And to your advantage, and obviously very much to their advantage.

MELZACK: Absolutely.

LIEBESKIND: And that's not so easy. A lot of people -- certainly I have had difficulty going to -- I mean, not with my students, but going [to] outside -- people, "Oh, I'm too busy, I'm doing this and that"; but you've had very good success with that. I think that's a hallmark of your career.

MELZACK: Yes, very good success. And it's been flukey -- some of it has been flukey, some of it has been -- I mean, the fact that, it's to Warren Torgerson's great credit that he said, "Yes, I'd be interested in this problem," because up to that time he'd been doing most of his work with squiggles. I mean, quite literally, he would make squiggles on a blackboard and people would have to say which squiggle was most like another squiggle or least like another squiggle, and now suddenly there were words, so he found that a very challenging problem that was right up

his area. Maybe that was my lucky fluke. Or maybe that is a talent that I figure this problem will interest this guy, you see. I choose the right guy.

But anyway, Torgie is a perfectionist. So we kept doing experiments, and then when I came to McGill I was still doing experiments, and no matter, every draft, Torgie would find something wrong, unlike Pat and I, where we would come to a draw and we'd say, "OK, we've sweated through this enough, this is how we'll publish it." Torgie always had one more experiment to do. And finally one day I said, "Torgie, either you accept this draft, or this paper will -- I'm just going to publish this goddamn paper." So he made some changes and said, "OK, I can live with this paper." And I said, I'm never going to write another paper with Torgie -- he's going to drive me crazy. And that's why, all the work afterwards he has not been a colleague.

But that first paper on the language of pain is the classic paper on that thing [Melzack R and Torgerson WS. On the language of pain. *Anesthesiology* 34 (1971): 50-59.] That's the big one. And Torgie gets enormous credit for that. But afterwards it just couldn't be. So the McGill -- I called it the McGill Pain Questionnaire -- with no names -- I didn't want my name on it. Other people put my name on it, they called it the McGill Melzack Pain Questionnaire. That was never, ever, ever, my doing. It was always the McGill Pain Questionnaire, with the understanding that it was based on the paper I did with Torgie. Well, that thing began to catch on. It caught on very quickly.

And around that time I wanted, I was looking for a test for pain in animals that would be different, that would be more like the human pain test, that would not test just a tail flick or a jump from a hot plate, but more.

LIEBESKIND: Again, something more in real time, something slower.

MELZACK: Right. So I had a bright student. I had two first graduate students, they were both chosen for me by Don Hebb and Ed Webster. One of them was John O'Keefe. And I gave him a paper written by a guy called -- not Sokolov -- He was a Russian Jewish physiologist who published a paper in *Science*, amazingly enough, in which he injected turpentine under the skin of an animal and produced pain. I gave the thing to John for his masters thesis. He switched from turpentine to formalin. He devised the numbering system, and he showed that --

LIEBESKIND: John O'Keefe?

MELZACK: John O'Keefe. Who later became famous -- O'Keefe and Lynn Nadel, for their hippocampus book, *The Hippocampus as a Cognitive Map* [Oxford University Press, 1978]. John is now a Fellow of the Royal Society of London. And John's job was not only to devise the test, but to make a lesion of the dorsal column, because I was convinced that animals will overrespond to pain. They did, but not enough to John's liking. John was a very meticulous guy, is a very meticulous guy, and he wouldn't publish the paper -- no part of it. Then when he went on to do his PhD thesis research, he became very independent of what I was doing. He was not interested in pain anymore, he was interested in the reticular formation and began to look at reticular formation cells and inputs to them of all kinds, and he got his PhD and left. So that never got published.

It was only later, in 1976, that I had another couple of students, David Dubuisson and Steven Dennis, and they took that formalin test and did all the work with rats, repeated it with cats, and then did it with rats, and they published the paper. They asked me to put my name on it, they wanted to put my name on it -- and Steve Dennis was looking for a job just desperately. The job market had collapsed. And I thought that it's much better if there's a paper by Dubuisson and Dennis, rather than simply Dubuisson et al. And I had had enough papers. So I said no, we'll just publish it as Dubuisson and Dennis, so Dennis' name is highlighted [Dubuisson D and Dennis SG. The formalin test: a quantitative study of the analgesic effects of morphine, meperidine, and brain stem stimulation in rats and cats. *Pain* 4 (1977): 161-74.] He still never got that job and he's left the field of psychology entirely and he is now working in Seattle. He'd worked with Bonica. I'd sent him to work with Bonica for a year. It's a long story and I won't go into the whole thing. Steve is one of the brightest students I have ever had, an extraordinary talent, highly imaginative, a real understanding of biochemistry and chemical structure. He came up with a whole theory of chemistry, did his bachelor's thesis at MIT, and his master's and Ph.D. with Tony Yaksh [Professor of Anesthesiology at University of California, San Diego] in California. Anyway, so that's how the formalin test came out that way.

But to come back now to the early 1970s, so now we're moving up in time, there was a paper by David Reynolds, a famous paper, which never caught on, published in *Science*. People read it and forgot about it. He wrote this book, in which he published more of this stuff, on how you stimulate the central [periaqueductal] grey, and nothing happens [if you then apply a painful stimulus]. And then along came this terrific paper by John Liebeskind and his students, and that was, that did it, that really convinced people [Mayer DJ, Wolfle TL, Akil H, Carder B and Liebeskind JC. Analgesia from electrical stimulation in the brainstem of the rat. *Science* 174 (1971): 1351-54.] And the times were right, and I am a firm believer in the *Zeitgeist* theory of history. And that was a fabulous paper, John. But what is so interesting is that -- to me it's interesting -- I never did that obvious, it was almost an obvious experiment. In 1958, with the Stotler and Livingston paper, arguing that the central gray, that the central tegmental tract, exerts an tonic inhibitory effect, you would think I would have had the brains to stick an electrode in there and try to soup up the inhibition, right? I never did. It never entered my head. I left the field, went to work with ducks, and then in Moruzzi's laboratory, I worked on other things.

LIEBESKIND: Maybe if you'd stayed on, if you'd had a job there or something, you know.

MELZACK: If I'd had a job there, I might have. Might have entered my head, but it never entered my head. I came -- when I got my job, the first thing I did was to work with restricted dogs, but I never did that obvious experiment. Isn't it fascinating, though? That's one of the fascinating things.

LIEBESKIND: Thank God. Thank God for me!

MELZACK: That's right.

LIEBESKIND: I wouldn't be here today. [both laugh]

MELZACK: Oh, you'd have done some other wonderful work, John. You've done lots of wonderful things. But that was great. And you went about it the right way and your characteristic, really imaginative, work and logic -- there's a wonderful logic to what you do, John, and the sequence of papers as they come out. So, you know, looking at the cells and the way you -- stimulating the PAG [periaqueductal gray] would influence the transmission in the dorsal horn -- I mean, everything. And then the naloxone paper -- there was just a wonderful logic to all of that and that's a beautiful story that you are going to tell.

LIEBESKIND: If I were going to be interviewed, I would tell how the gate theory and your work had influenced us, you know that, so anyway, that's for another time.

MELZACK: That's for another time and that's the story for you to tell. That's a fabulous story, John. That was a great breakthrough because that work of yours in the early '70s, the whole period, all those papers between 1971 and 1976, that gave rise to that whole enkephalin story, everything, that endorphin story.

LIEBESKIND: Again, that was the timing issue.

MELZACK: It all happened all at the same -- it was amazing, an amazing time. That's when we had that wonderful second, I think it was the second, Pain Congress in 1979, and the electricity of that, the excitement of that Congress --

LIEBESKIND: That's right, yes, the one here in Montreal.

MELZACK: We've never had another Congress like it. It was a combination of the times, when we all thought that we were at the threshold of discovering how pain worked and the great molecule.

RONALD MELZACK

TAPE TWO, SIDE ONE

LIEBESKIND: One of the questions I had -- maybe I can just pop this one in now, sort of moving forward chronologically -- we're into the 1970s. I'd like to get your reminiscences, as Pat Wall called them, bibulous reminiscences -- "we'll have a drink and we'll have some bibulous reminiscences" -- of the Issaquah meeting. Now that was a watershed event. What are your recollections of the circumstances of that meeting?

MELZACK: Well, my recollection is that I thought it was wonderful, I was overwhelmed at the number of people -- that's when I really realized --

LIEBESKIND: We got a field here!

MELZACK: Yeah. Astounding. And realized that I'd never really fully appreciated John Bonica and also knew why; it was Livingston's attitude that obviously I had imbibed.

LIEBESKIND: Was this the first time you met John Bonica?

MELZACK: Yeah, that's right. First time I met him, to my knowledge. I may have met him before then, because he may well have come here, because he was a friend of Philip Bromage, who was head of anesthesia [at McGill]. I think maybe I did meet him before. He came and gave a talk. But that's all. But there at that meeting, I realized that he may have a very large ego, but -- Pat Wall put it very well, he is on the side of the gods.

LIEBESKIND: That's right.

MELZACK: And I felt that was absolutely right. This guy -- there is a field of pain that is bigger than himself and he really is just devoted to these ideas of multidisciplinary pain clinics. He understood the gate theory, he understood that thing with Ken Casey, he knew the impact and the implications; and I am not sure that the gate theory would have had the impact it did, if John Bonica had not realized how clinically meaningful and important that all was. So that was good. But one other thing happened there that leads us really into the next part of the story. And that is, John Bonica asked me to talk about two things and give two talks. One of the talks was on phantom limbs. I had been interested always, intrigued with Mrs. Hull's phantom limbs. And I had written a paper on phantom limbs that got published in *Anesthesiology*, arguing a central basis, so if there is any theme that runs through whatever the hell I've been doing -- as I realize, looking back at it, it's I'm always keeping my eye on the brain.

LIEBESKIND: The brain, that's it.

MELZACK: The brain is there. And so during that talk I got up and said -- the first talk was on psychological factors. You may remember I showed a film on trephination and a guy fainted.

LIEBESKIND: I'll never forget that. He did more than faint, I think he had some sort of coronary accident. He was sick, I think.

MELZACK: He was sick, yeah. Poor guy.

LIEBESKIND: Scary.

MELZACK: And he said to me afterwards that he just couldn't understand it because he cuts animals' heads open and he sees all that blood, he sees skulls, he just couldn't believe this happened. But you know, it is an innate response, by some humans anyway, to blood, just plain seeing blood, can't stand the sight of blood. Strong soldiers faint. Anyway, so when I gave the talk on phantom limb pain, I said what we need is to show that you cannot explain these phenomena on the basis of peripheral factors. It's generated by the brain. And I said that what we need are observations by neurosurgeons, who have gone in and verified complete, total, spinal cord lesions. And I said -- but up to that point, I gave a logical argument as to why it couldn't be peripheral factors, that peripheral factors most certainly played a role, they modulated, they did all sorts of things that contributed; but they were not the major cause of phantom limbs and phantom limb pain. That simply had to originate in the brain.

Anyway, afterwards, John Loeser comes up to me, a young John Loeser [b. 1935, IASP President 1993-96], we're talking 20 years ago. John is now 55, he was then 35 or something like that. And this young John Loeser tells me that he's got better than just simply verification of total spinal cord lesions. He as a neurosurgeon has gone in and removed a couple of segments of spinal cord on the basis that maybe some fibers had escaped. So you have nothing to lose -- you've got a paraplegic or a quadriplegic, you lose nothing by taking out some spinal cord. And so he's done that and at the same time, in some of the patients he had blocked the sympathetic ganglia on both sides so there couldn't be afferent input going through there, and there was still a phantom body and there was phantom body pain. Well, that thrilled the bejesus out of me.

So I said, "John, how'd you like to get together and work on this?" And he told me another thing. He said, "Neurosurgeons normally don't talk about these things." He said that operation is called a cordectomy -- I don't know whether he coined the term or somebody else did -- but he said it's a cordectomy. He said, "If you ask around nicely, you'll find that almost every neurosurgeon has tried a couple." So I come back to Montreal and our neurosurgeon, Joe Stratford, who is the head of our pain clinic, I asked him if he had heard of this at all, and he said, "Oh yes, Ron, I've done a couple." And I said, "Could we have your cases?" and he gave us his cases. John had three. We used Joe Stratford's cases. I wrote to [C. S.] McCarty, whose drawing of different lesions of the pathway -- McCarty is a neurosurgeon at the Mayo Clinic who did the picture for his residents in which he shows all the places that they're going to cut in order to abolish the pain pathway, cut the pain pathway, and cure pain -- which never worked, of course. [Melzack and Wall used the picture in the 1965 gate control paper.] I'm not sure that McCarty ever realized that I wasn't doing him a favor by showing that picture, that I was using him as an illustration of what's bad, not what's good. But he was always very nice. So yes, he did a bunch too and he sent me one of his recent patients.

So John Loeser and I therefore were able to write a paper that we published in *Pain* [Loeser JD and Melzack R. Phantom body pain in paraplegics: evidence for a central “pattern generating mechanism” for pain. *Pain* 4 (1978):195-210] that Pat hated, and gave us great trouble over. He didn't like it and he went so far as to write a paper with several other guys in Israel on phantom limb pain, and the whole discussion was obviously written by Pat, saying the very same things that were in his letters to me, about why this is all horse manure and I am misleading the whole goddamn field of pain again. It's a great friendship, this -- it's really an extraordinary friendship. [both laugh] So finally he published it. And that paper never had any impact. It was very interesting and I've thought about it a lot.

LIEBESKIND: You're speaking about the paper with John Loeser?

MELZACK: With John Loeser on phantom limbs and the implications of it and the idea of a pattern generating mechanism which must be somewhere above the level --

LIEBESKIND: The one on phantom body pain.

MELZACK: Phantom body pain, right. And people -- I began to realize that people are not ready for the brain somehow. They're just not ready to think of the brain, they're not ready to get away from stimulus-response ideas. And I wasn't sure that I was ever going to be able to do anything about it except -- or ever come up with any theory that was of any consequence that would allow me -- you can't just say what John and I said, because it's not enough. The pattern generating mechanism is very nebulous and it doesn't really give you a rack to hang your coat on or anything.

So that was 1976, so we're now getting into a time when I was beginning to feel like I had just about done everything there was in the field of pain, can't take it up any farther into the brain than I've taken it. I am right in the brain, I'm there, nobody gives a damn about it all. And it was at that time then that Louisa [Jones, IASP Executive Director] said that the guys in the Executive wondered if I would be interested in running for President of IASP. And I figured, well, I think probably the scientific aspect of my life has come to an end, you know, not entirely, but -- I could coast along with a whole lot of ideas that I'd had before, because I didn't think I'd get any more ideas coming along. And it's true, there have not been any more ideas. But that pattern generating mechanism idea gave rise to a lot, as I'll tell you in a minute and as you know. So I let my name stand and got elected and so that's the whole business of being the President; but while that was going on, I got the idea of a neuromatrix.

Suddenly I began to see how the hell one could conceive of this thing. I had Weinstein and Sersen's information and papers showing that people born without a limb have a phantom, so it says that's built in [Weinstein S and Sersen SA. Phantoms in cases of congenital absence of limbs. *Neurology* 11 (1961): 905-911.] I realized that obviously what we feel has got to be built in. You don't learn to feel the quality of pain. You don't learn to feel the quality of itch or quality of tickle, you don't learn to see blue; that's built into our system that we are going to see these things or feel these things. So the idea of a neuromatrix -- and then it was long after I had the idea, it dawned on me that what I've got is Donald Hebb's cell assembly, except that I've got it the other way around. He had to build his up very gradually by learning, with synaptic

connections forming, very gradually. And what I was proposing was that the whole cell assembly, or neuromatrix, is built in and then it gets whittled down, it gets shaved down, it gets sculpted, as it were, by the sensory inputs. So there are too many cells in the neuromatrix and it gets sculpted down to form our own unique particular body. And suddenly I realized I had the basis of a theory. Well, how do you get at the neural matrix? And so then I thought -- well, I don't know whether I thought of it or I had read an article, because certainly it was done before I had done it -- but I thought that if we injected lidocaine locally into parts of the brain, we might see, we might interrupt the neuromatrix and begin to get ideas of the circuitry involved.

So I had a post-doc student called Andy Tasker [now Associate Dean of Graduate Studies and Research at the Atlantic Veterinary College, University of Prince Edward Island] with me then, and we did the hypothalamus, the lateral hypothalamus. So we got pain blocked when the lidocaine was injected into the lateral hypothalamus, but not into the medial hypothalamus, which is very, very interesting. And then I had this very bright student, Anthony Vaccarino [now in the Department of Psychology at the University of New Orleans], and he was looking for a project, and I said, "Anthony, I've got a hunch and it's based on good solid clinical information that's been around for a long, long time, that the cingulum is going to play, ought to play a critical role in pain. Why don't you take a look at that?" And of course Anthony took off and flew -- as we both know with our graduate students, the problem becomes *their* problem. *They* begin to think of it as their problem and they begin to do wonderful things with it.

LIEBESKIND: That's when you know you've succeeded.

MELZACK: That's right. So it was Anthony who then realized -- came up with the idea of putting in -- always using the formalin test, but putting in the lidocaine either before the formalin is injected or just after the formalin is injected, suddenly finding that if you put it in after the formalin is injected, the animals still feel pain -- which means that the formalin set up a central process that now is persisting, in spite of a second injection of lidocaine of the paw, that must be preventing all input from coming in. Or even injection of the spinal cord where you've got a total block of spinal input.

LIEBESKIND: Once you've got that input there the first time, it sets something up, that continues and it doesn't matter what you do.

MELZACK: That's right. And Livingston would have loved that, because that's exactly what Livingston was trying to show. But of course that stuff has now become the plasticity work. It fits right into that. So we're moving very rapidly up into the present times, John, so we're going to come back to all your questions. But you've got Terry Coderre here, working on plasticity [Associate Professor of Anesthesia at McGill], but now getting down to it at the molecular level, looking at NMDA receptors, looking at excitatory amino acids, and C-fos and all those things I don't really understand, but he's an absolute hotshot at; and then Joel Katz, an extremely bright guy, very interested in phantom limbs [now holder of the Canada Research Chair in Health Psychology at York University in Toronto]. I tried to talk him out of working in the field, that aspect of it, because I didn't think it would get him a PhD. I told him it was a great idea but I wasn't so sure it would get him a PhD. But he worked on memories, phantom limb memories, which at first Pat hated, gave him a horrible time and Joel had to fight with Pat to get that thing

published. It got published, and now Pat loves that paper, because it fits into the plasticity story, which he feels is one of the most important things that he has done, that work that he did with Clifford Woolf [now Kitz Professor of Anesthesia Research at Harvard] showing long-term changes in the spinal cord. [Cook AJ, Woolf CJ, Wall PD and McMahon SB. Dynamic receptive field plasticity in rat spinal cord dorsal horn following C-primary afferent input. *Nature* 325 (1987): 151-53.] And then so it was the lidocaine studies, and that work is still going on.

And then there was another aspect -- I became interested in the effects of morphine on pain. Now that goes back further. That goes back to my student Frances Abbott [now in the Department of Psychiatry at McGill] who was with me, and it was really her interest in working on opiates more than my own interest, I must say, that started that work with animals, but I did have an interest, because what happened with opiates -- we talked about emotional gut interests playing a role -- my brother-in-law, my sister's husband, died of cancer very painfully. His doctor had said that she couldn't give him more morphine because he had become addicted to it. And then after a while she realized that was a foolish thing to say; but she said that he would become so physiologically adapted to it that it wouldn't work when it was really needed, and I believed that story.

Then about a year later, a year after Harry died in very considerable pain, I met Balfour Mount, who was a wonderful young guy who worked with Cicely Saunders [b. 1918, founded the modern hospice movement with the opening of St. Christopher's Hospice in South East London in 1967] and started the palliative care service. So Bal started the palliative care service [at Royal Victoria Hospital at McGill, the first such service in Canada]. What happened was that one evening, the dean of medicine organized a little refresher course. I think it was the dean of medicine, if it wasn't the dean of medicine, it was some other guy responsible for continuing education, something like that. And he had me to open it up and give a general story of pain and where we're at, and then on the program was Bal Mount, Balfour [M.] Mount. And I'd heard of him but I'd never met him.

Well, I heard him talk and I was so overwhelmed by what he had to say and what he suggested in terms of palliative care and no need to suffer, and you give morphine in the right amount and you'll abolish pain in most people. I went up and I said, "Look, I don't know what we can do together. I know we can do something together, though. The first thought that comes to mind is that, simply, you've got a palliative care service, I've got a McGill Pain Questionnaire to measure pain with, let's see if the morphine is more effective in your palliative care service than it is on the other wards, the other services."

LIEBESKIND: What year was this?

MELZACK: That now was 1976 -- around 1975. So we did that and we showed in fact that people do have -- that a given amount of morphine has more of an impact on the palliative care service than it does [on other services], and the same amount of morphine given, because the amounts were the same given on the other services, where you didn't happen to have the tender loving care and concern, and the family being able to visit, and the dog being able to come, and so on. So I was all prepared, I had done studies on the lack of good pain control; but that was really to a very large extent just, "Let's do something with the McGill Pain Questionnaire, it's a

toy, it's wonderful, let's have fun with it," you know. A student, I owe it entirely to this student, who said, "Let's take a look at labor pain."

LIEBESKIND: Had you been interested in that before?

MELZACK: No, I had not been interested in labor pain. But she said, "Let's use the McGill Pain Questionnaire to measure labor pain."

LIEBESKIND: And who was that?

MELZACK: Her name is Cynthia Shuan. So I got one of my friends to get permission in a hospital, which was a major labor hospital in Montreal, a major hospital for birthing ladies, to give the McGill Pain Questionnaire, and she got such high scores I didn't believe it. I really didn't believe it. And I'm horrified to think that I gave her a B in that course; it was a terrible thing to do. She is terrific. She has since gone on and done wonderful things and wrote me a book with a beautiful inscription and everything. I felt terrible when I got it. But obviously it didn't bother her. But I thought she must really have goofed things up by getting these horrendous pain scores. How can pain be so bad, you know? And so it was years later that another girl came along, saying that she has a friend who was doing a residency in obstetrics and she'd like to do her McGill honors thesis on labor pain. I said, "Well, I'd love you to do it, go ahead." She got the same high scores. Anyway, by then we were off and running.

And I suddenly came to realize that labor pain makes a wonderful model of acute pain because it has a definite beginning, it's got a definite ending, you can measure things, you can look at antecedents, you can look at the size of the mother, the size of the baby, you can look at everything. And we still don't understand why -- we still can only account for about half of the variance of pain scores in labor pain, but that's OK, it's turning out to be a very good thing, and being taken up by nurses and physical therapists and it's great.

Well, what happened was that the idea of the neuromatrix made me feel that there's got to be an equivalent of phantom limb pain, or phantom limbs, in the visual and auditory modes. So I had a student, Jeffrey Schultz, and suggested to him for a PhD thesis topic that he look at visual hallucinations in people who have lost their vision, a substantial amount of vision. And of course there's a lot of this. I didn't even know that there's a thing called a Charles Bonnet syndrome, named after Charles Bonnet [Swiss zoologist (1720-1793)], and it's very well known. Then in the auditory field, suddenly I realized that tinnitus is obviously an auditory form of the same thing. You hear noises. And I did not know that there are formed auditory hallucinations by absolutely certifiably sane people who hear music.

LIEBESKIND: What kind? Formed, you said?

MELZACK: Formed auditory hallucinations, it's called. And they hear usually music, sometimes it's language, but most commonly it is music in fact. There happened to be a psychiatrist at Montreal General Hospital who gave a talk on this, with the name of Cervantes, who refuses to write up a paper. There's an instance where -- I've never been able to get this

guy to do it. He keeps wanting to, but he's never been able to get involved in it. So I have finally given up on this guy. So I don't always succeed.

LIEBESKIND: But this goes back to Penfield almost, with his being able to cause these recollections of music by stimulating the auditory cortex.

MELZACK: Absolutely.

LIEBESKIND: He just dipped right into the matrix.

MELZACK: Yeah. Well, I've really brought us up to pain as of now. OK, we're working on the neural matrix idea in a variety of ways.

LIEBESKIND: You're planning a book in this area?

MELZACK: So let me tell you about this. So that really brings me to another thing entirely. And that is -- do you want to talk about it now, or do you want to raise questions?

LIEBESKIND: Go ahead, no, by all means.

MELZACK: OK. This won't take very long, actually, John, because the story is quite simple. What happened was that way back in 1968 or so, I had some very bright graduate students, one of whom was Karel Gijbbers, who's [now in the Department of Psychology] at Sterling University in Scotland, came from England to work with Don Hebb. Don asked me to take him on as a graduate student, which I did, and Karel started up a little seminar and one of the guys talked about aggression and gave the Lorenz story of aggression and it's all built in and it's an instinct -- which I found absolutely terrible and did not want to believe. And so I thought I really had better take a look at this literature on aggression. And I did and I thought I could write a useful book, by just writing a book on human aggression that would give a more balanced picture. But in the meantime other guys came out refuting Konrad Lorenz's aggression as an instinct; and the guys came out saying that aggression is learned. And I must tell you that when I read the books that [argued that] aggression is learned, they sounded just as insipid and wrong as the guys who said aggression is an instinct.

Well, I began to write this book and on my second sabbatical leave, most of it was spent writing the aggression book. I finished the book, put it away, picked it up the next summer, went back to teaching -- picked it up the next summer -- always carrying on my pain research, of course -- and I thought it sounded like just crap. So I worked on it again and worked on it some more and finally decided it was hopeless. And then, when the neuromatrix idea came up, I suddenly began to see aggression as something I could deal with. Aggression in terms of innately built-in mechanisms that are modifiable by experience, but not just saying those words. I mean, to say something is built-in and changed by experience is nothing, it's not a theory, it's a statement of the obvious. But showing how that could happen -- how our neuromatrix can develop the basis for perception of our in-group and how inputs that do not conform to the in-group as a result of seeing out-group people or animals or what have you, would now trigger off the fire alarm.

LIEBESKIND: This goes back to all these innate fears that you've been working on.

MELZACK: Exactly.

LIEBESKIND: Tying everything together.

MELZACK: It ties it all together. So I am at this stage, as of October 16, 1993, I have got a draft of -- I think it is 12 chapters of a book, with three more to go. Among that last three is a chapter on pain, which I dread writing, because it's going to be a new theory of pain, and I don't know how good or bad I can make it, but I think I know what I want to say. I do know what I want to say, but saying it convincingly, I know, is not going to be easy at all, and I hope to have it done in a year or so. Right now I'm so busy with proposal writing and stuff like that, but I'll get back to it in January, and spend till the next sabbatical on it, which is next September or so, and hopefully have most of it done, or all of it done, hopefully. I may yet read the thing and feel it's no damn good and not publish it, and it's entirely conceivable. I've written two or three books like that and just never done anything with them. And I'm not sorry at all. People have published books like it and they've not been a contribution; and I figure if it's not going to be a contribution, to hell with it.

LIEBESKIND: You don't need another book.

MELZACK: You don't need another book, no. Gerard once wrote a very good article in *Science* on science being like a signal noise thing. There's so goddamn much noise in science these days you can hardly pick up out a signal.

LIEBESKIND: Ralph Gerard?

MELZACK: Ralph Gerard [1900-1974; former graduate dean at UC Irvine], yeah. And I remember being very impressed.

LIEBESKIND: Did you know him in Chicago?

MELZACK: No, he's a guy I did not meet, no, never met him.

LIEBESKIND: He was on my doctoral committee at Michigan, he subsequently moved to Michigan.

MELZACK: Is that right? He must have been a very impressive man, though.

LIEBESKIND: Oh, was he a character.

MELZACK: Really? God, he seems to have known everything -- an extraordinarily erudite guy. I gather that he could sort of pick a topic and then read everything in it and understand it and digest it and write something really intelligent. I've not read a stupid thing by this man ever. They're all worthwhile, they're all worth reading. A brilliant guy. Well, John, I think that really brings me up to where we're at. I can't think of --

LIEBESKIND: Well, that's a wonderful account, and all that with one question. I said, "How did it get started?" and you told me your life.

MELZACK: Yeah, right.

LIEBESKIND: Well, I came with a whole bunch of questions, and we've actually at least touched on most of them.

[PAUSE FOR BREAK]

LIEBESKIND: Ron, you touched very briefly, mentioned only the fact that you were elected President of the IASP.

MELZACK: Yeah.

LIEBESKIND: I view that as a very significant event. I view the whole IASP and the history of pain as a very significant thing. As you reflect back on your years as President -- I know that you were President-elect and then you were President and then past President -- as you look back on that epoch, what do you see as the most memorable events or occurrences or themes that came out of that? Was that satisfying to you, did you feel that it advanced the field of pain to have worked -- that's a lot of work you did -- was it worthwhile, do you feel?

MELZACK: I think it was very worthwhile and it was a wonderful time in my life. I think, as I look back on it, the nicest part of it, John, was all our wonderful friendships, that you and I have had with all that group. You've been as much a part of it as I have. And so you can't meet a nicer bunch of guys than the Bonicas and the [Ronald] Dubners [Chair of Biomedical Sciences at the University of Maryland Dental School, former editor-in-chief of *Pain*] of this world. These are all terrific people, like cousins. And they're all dedicated, marvelous people. You know, Kathy Foley [Professor of Neurology at Memorial Sloan-Kettering Cancer Center, past president of the American Pain Society], all of them. I mean, there might be some -- as President, I must say, I really locked horns with people over issues, but always realized I was dealing with people at a really intellectual sort of a level and these were things that were honest disagreements on things. But boy, you look around at any [IASP] Council at any time that we were on -- we were on most of them together -- we had wonderful people. So it was these meetings, and we were talking about that just last night, weren't we, when you were saying our lives were sort of regulated by the Council meetings.

LIEBESKIND: They were the signposts, the mileposts.

MELZACK: That's right, the meetings about the forthcoming Congresses, the Congress itself, the meetings after the Congress -- everything. They were marvelous.

LIEBESKIND: You know, some people scorned the whole activity of involvement in societies and that sort and view it -- oh well, that's kind of administrative work and it takes me away from

the lab or something; it's not meaningful, it doesn't really do anything, it's just social or something. But I don't agree, and I suspect you don't either.

MELZACK: I don't agree.

LIEBESKIND: I think that these meetings have a tremendous effect on the field. They promote the professional development of the field. They inspire young people, they bring attention to issues and to the public. I really think they are very meaningful.

MELZACK: I think they are very important, just the way I think that what we do as university professors when we are on committees is very important. I think that it's a part of that intellectual atmosphere that we are in, that intellectual endeavor; and I think that's damned important -- that it's not only to do your own research, but to provide the opportunity to do research to bright young people or to your colleagues or whatever you have.

LIEBESKIND: I don't know who of us would be willing to look back on a career and feel satisfied if they haven't influenced anyone, if they'd been just talking into the breeze.

MELZACK: Right.

LIEBESKIND: I should have put it on tape, but I will mention now briefly this very stirring experience that I told you about last night, where David Mulder [Head of Thoracic Surgery at McGill] spoke at this meeting last weekend and talked about how the work that they've done has revolutionized the way trainees are taught, surgical trainees at the hospital here, to appreciate postoperative pain as a problem and deal with it. He just traced all this right back to you and how you came into his office one day or whatever, his service, and talked to him about pain and wanting to do something, and it had a tremendous impact on his life. I view the IASP -- that's just one way in which we have an impact on the world, on ourselves, the other members, but also on the world through publicity and so forth and so on, and I think it can be very important.

MELZACK: Very. And something we all shared and we all enjoyed -- remember? If there's anything, if I've left any impact on or impression on IASP -- which has a life all of its own, it exists all by itself -- it's that when I became President, no one had, up to that time anyway, given much thought to the initial kinds of committee structure that we had. And what I did was to spend the first year doing nothing but thinking about what kinds of committees do we need to have for an IASP that is going to grow. And I came up with -- now I can't remember what the hell the committees were that I came up with, but I came up with a whole new committee structure. I tried to also develop mechanisms for us on the IASP Executive and the Council to have more contact with the Presidents of chapters and things.

And I started something that -- actually, there was one single meeting of the President of the IASP with the Presidents of chapters. I think the only time was when I was President. I think everyone agreed it was a wonderful idea, and the Presidents of chapters came away thrilled that they were recognized and they were not just the Presidents of chapters but part of a bigger thing, and for whatever reason it never was carried on. I think it wasn't carried on, mostly simply because there's just too much going on, and you can't bring everyone together. Your meetings

are jam packed full. People go there to learn, primarily. But in any case, I revamped the whole committee structure, made it much larger and set the stage for Mike Cousins [Michael Cousins, Head of Anaesthesia and Pain Management at Royal North Shore Hospital in Sydney, Australia, IASP President 1987-1990] then to come in and really start making it grow. I sort of felt that Mike made it grow on the base that I had developed, in part with Mike, who was very much a part of all of this.

LIEBESKIND: He was the President-Elect while you were President, so you worked closely with him.

MELZACK: He was the President-elect, that's right. I did a lot of work very closely with him at that time.

LIEBESKIND: There was an issue, wasn't there, at that time about where a subsequent meeting was going to be held and with the Japanese chapter and so forth.

MELZACK: Oh God. Oh my heavens. Do you want me to mention this?

LIEBESKIND: Yeah, how did that transpire?

MELZACK: Oh dear, what happened was really sad because we had a Japanese member on the Council -- I won't mention names -- and he was obviously a very ambitious guy. And what had happened in Japan -- I was totally unaware of it -- was that some pharmaceutical company had set up a laboratory to develop a particular drug, and then that laboratory went so slowly that they set up another laboratory to develop the same drug. And these two laboratories became bitter enemies. And this guy was affiliated, was from one of them. And the other guys --

So the very first night of my being President, Lucy and I went out and we bought a big pot of Colonel Sanders Kentucky Fried Chicken, with enough chicken for five people and French fries. I was exhausted, I was simply exhausted from what had gone on, and we had eaten our supper and watching television, just relaxing. Telephone rings, it is Louisa Jones saying, "Ron, we have a real problem. Better come down and talk to these Japanese people." I came down, I talked to these Japanese people and I -- *es mir nicht geworn* is the expression in Jewish, which means I felt like throwing up. I thought, oh Jesus, what a way to start your presidency, with this strife and hatred. And these guys were mad. I mean, normally you don't see angry Japanese guys. This Japanese little group that came to see me told me in no uncertain terms that if I were to go ahead with Japan [for the 1987 IASP World Congress], they were going to fight this man.

LIEBESKIND: Having the meeting held in Japan?

MELZACK: Held in Japan.

LIEBESKIND: As organized by this one guy.

MELZACK: Right. Well, in the meantime, I had to -- I don't know these guys, I mean, maybe they're misleading me. So I phoned Ron Dubner and got him in and said, "Ron, come down and

talk to Louisa and me.” And he starts talking to these guys, he knows these guys, and they’re for real, and he’s telling us, this is really what happened. And this is the background of it and these guys will fight it. So then I came back to Montreal, to Canada, thinking what the hell am I going to do?

LIEBESKIND: Because it had all been decided on already. The Council said the next meeting would be in Japan.

MELZACK: Yes, we went out there, sure.

LIEBESKIND: And this one Council member that we’re not mentioning -- he was going to be in charge of the meeting.

MELZACK: That’s right.

LIEBESKIND: He was going to be running it and there was all this opposition within his own country.

MELZACK: Not only going to be running it, but apparently he himself was looked upon as a real egotistical -- he was doing this for self-glory. And so I realized that we just couldn’t have this. So what are we going to do? And then I remembered that there was really one guy who desperately wanted to have the Congress [in his country] and made a wonderful submission at the same time that the Japanese made that submission where the Japanese won, and that was Manfred Zimmermann [now Professor Emeritus at University of Heidelberg]. And I had developed a very considerable respect for Manfred Zimmermann over the years. First at Council, I thought Manfred Zimmermann was entirely an egotistically-oriented guy and came to realize that he too has his many, many good qualities and [is] sincere and concerned about the field of pain and everything else. Ego might be in there, but so are a lot of good things.

And so I phoned Manfred and said, “Manfred, I’ve got a hell of a problem and I am turning to you to bail me out. Would you consider holding the meeting?” And he said, “Well, let me think about it and I’ll get back to you.” He got back to me about two or three days later and he said, “Ron, I’ll be happy to take it on.” And boy was I grateful! And then he ran into trouble. Because now, there he was at Heidelberg, and there was no convention center there that could hold us, a meeting as big as ours would be, in Heidelberg, or anywhere near it. I thought Munich; Munich was by then already, all the big places were already booked by then.

LIEBESKIND: Cause this is now short latency, we’re talking now less than three years.

MELZACK: Less than three years. And by then, nothing big enough was available there. So the only place one could turn to was Hamburg, and in Hamburg there’s a guy with a very large ego, called Burkhardt Bromm [Director of the Physiological Institute of the University of Hamburg]. And Burkhardt Bromm was going to now be on Manfred’s team and he’d be the man on the spot, and he wanted to be *the* man on the spot. Soon I found myself in the middle; but anyway, that was handleable, and Manfred did a very, very good job, he was just, you know, I think the German character is such; and when I told Bromm that no, you cannot be the President

of the Congress, there is no such thing as a President of the Congress, and that the guy in charge of this is the Council member because there's got to be a guy responsible to Council. And so that all worked out. I think we had as good a Congress as we could have had, given the circumstances, but wow.

LIEBESKIND: It was a good Congress.

MELZACK: Yes, it was a good Congress. But you know, John, my feeling is that, to use an expression that you stated a couple of months back when we met, we were talking about what's happening in the field of pain and neuroscience generally and you said, "Ron, I guess you can sum it up by saying we are learning more and more about less and less." And I have the feeling that this is what is happening. We are learning more and more and we are losing sight of the big picture, and I find myself very concerned about it. And now all my lectures practically, not just to my students, but lectures where I go out to big organizations, is to try to bring back that big picture, that look, you know, we are all excited as all hell about all this technology and what it is letting us do. But look, we don't help most people with back pain, with headache, with reflex sympathetic dystrophy, with trigeminal neuralgia except -- certainly very few with atypical facial pain, most myofascial pains we are still not helping, except on a temporary basis. So keep in mind that we still haven't conquered the problems. And people are still not using the right amounts of morphine, not for children and certainly not for adolescents and all that. You know, my President's address was "The Tragedy of Needless Pain", and that also brought you and me together in a very real way.

LIEBESKIND: Yes, absolutely. We tried to get this foundation [the International Pain Foundation] going.

MELZACK: And I think that's a very important thing for us to mention, because that was a great dream to develop a foundation that would raise the funds to help people from third world countries learn about pain and I still think we don't really know what went wrong where. But maybe it's the international aspect of it, and that is that you really need -- people need to feel they are giving their money to something that they can understand, not something nebulous, and when you think of the International Red Cross, well, there really is a Canadian Red Cross and an American Red Cross and no matter whatever that international organization is, things are collected at a local level. We used to talk about that a lot, of how the local group would give money to the international -- remember, we used to talk? And then unfortunately it went wrong.

LIEBESKIND: We started in the wrong direction. We started with the international. The international should have been built up as a federation of existing national organizations.

MELZACK: I think that's the way it should have gone.

LIEBESKIND: Well, there were lots of problems with that.

MELZACK: But really, think of all the successes -- there were great successes too.

LIEBESKIND: Well, I think it's still a good idea.

MELZACK: It's a hell of a good idea. It's a wonderful idea.

LIEBESKIND: We need to find the right people at the right level to do it.

MELZACK: You know what, John, I foresee in the future --

LIEBESKIND: Yeah, somebody'll pick it up.

MELZACK: When there is a strong American Pain Society Foundation and British Foundation and there is a Canadian Foundation -- when they begin to have monies and begin to start becoming concerned about what to do with those monies, they will start making use of the IASP facilities of evaluating who should get the money, where should they go and all those things. And I bet you that as long as the IPF remains alive, in any context, by itself or within IASP, it's going to be alive. It's there, with a mission. The mission was -- remember, you came up with wonderful mission statements and I think we had great goals. So I think it will live again. The phoenix will rise out of the ashes, I am convinced. I am convinced it will happen.

LIEBESKIND: Let me take you back to Portland. You and your wonderful wife Lucy are two of the funniest people and tell some of the great stories. And I remember a story that you told years ago. I don't remember the details, but it was about Livingston. It's just a silly story, but I wonder if you would put it on tape.

MELZACK: Sure.

LIEBESKIND: Sometime when he got on a bus.

MELZACK: That's it.

LIEBESKIND: Tell that story, would you? Because it would give us an idea of who he was and what kind of a man he was. That hasn't come out yet, really.

MELZACK: Yeah. Livingston was a very large, very warm man with a great sense of humor, marvelous sense of humor, but also absolutely dedicated to the field of pain -- worshipped Weir Mitchell, knew him practically by heart. Livingston was a man of many talents. He was at one time the wrestling champion of the Pacific Northwest. Another time, he was a fencing champion.

LIEBESKIND: He didn't beat John Bonica at wrestling, did he? Two wrestlers!

MELZACK: No, no, he was not on the professional circuit -- that's very interesting, isn't it. Archery champion -- I mean, whatever this guy did he became the best in the area. And so ---

RONALD MELZACK

TAPE TWO, SIDE TWO

MELZACK: So Livingston was made chairman of surgery at the University of Oregon Medical School. And naturally it meant that he had to be involved in medical issues of the whole Multnomah County area, the whole city [Portland], basically. And so he would have quite a few meetings downtown. The University of Oregon Medical School is on the top of Sam Jackson Park hill. So the easiest way to get down was not to take the car, but in fact for him to simply hop on a bus going down and take a bus back. And some of those meetings were Tuesday mornings. And he would have to come back to the Tuesday pain seminar, right -- that started at 12 o'clock, where we would munch our sandwiches for half an hour and then somebody would talk about research for half an hour or an hour, and then we would go to the pain clinic.

So on one of these days he came in all huffing and puffing and red in the face and sort of laughing and embarrassed looking, and he said, "I've just got to tell you the funniest goddamn story you guys ever heard." So what happened was that he was finished with the meeting with the committee that he was on downtown, and then suddenly realized that before he gets on the bus to go up to the medical school, he'd better go and pee. So he runs in to pee and he realizes that he's a bit late and runs out and then there's a bus, with a line, and people are getting onto the bus in the line. He gets in line, he gets on the bus and he could barely squeeze in. He's a big guy with a big potbelly that really stuck out. And so he was on the lowest step and just above him, on the step above him was a lady and the step above that was the floor of the bus. Anyway the bus driver was telling people to move back, to the back of the bus and get their money out, so Livingston put his hand in his pocket to get his change to put in for the bus ride, when he suddenly realized he'd forgot to zip up his fly.

So he holds on and surreptitiously, very quietly, zips up his fly and then people start to move toward the back of the bus and, hot damn, the lady's dress, the hem of her dress, the lady standing on the step above him, had her dress caught in his fly, in the zipper of his fly. And he was pulling the zipper to try to get it to move down and get the dress free and she was pulling her dress, mad as hell at him, and she was yelling at him and he was telling her, please calm down, he'll get his fly undone and everyone looked at him like he was a dirty old man. Eventually she gave a pull on her dress and a chunk of cloth tore off. And he said, "Look, how the hell am I going to explain this back home? What's Ruthie going to say to that?" As this piece of polka dot dress is caught in his fly. So we all laughed like hell. That really was a wonderful and funny, a marvelous story.

LIEBESKIND: That's one of the great stories. Gives you a sense of the man, too.

MELZACK: Oh, he was wonderful. A wonderful guy.

LIEBESKIND: Great sense of humor, he had, too.

MELZACK: Oh yes. He was up, oh, 4:30 every morning, would make himself toast and bacon and coffee. Then he'd sit down and he'd write from 5, 5:30 till 7, when there were rounds, every day. He had a mound of written paper, John -- you know, I'm talking about a book that I may never finish -- he had a book that he'd worked on like hell [*Pain and Suffering*, published by IASP Press in 1998]. He must have had about two feet of paper, written and rewritten and rewritten and rewritten, looking for that theory of pain that he was never going to find. I think that was one of the great disappointments in his life. But he had a lot of enormous pleasures.

LIEBESKIND: I am sure he would have taken great pride in what became of his post-doc, Ron Melzack. He would probably have enjoyed that.

MELZACK: Yeah, I think he would have been very pleased indeed. He would have been pleased to see that, yeah.

LIEBESKIND: Continuing with the theme of how mentors influence mentees and what happens to them, I want to ask you about a different theme that didn't come up in your talking about your work. But there's certainly a great focus of interest in the field of pain in the last bunch of years on children's pain.

MELZACK: Yes.

LIEBESKIND: When I think of that, you know, try to trace back in my mind, where did that come from? The first person that I can think of who was doing work in that area was Mary Ellen Jeans [now Executive Director of the Canadian Nurses Association].

MELZACK: That's absolutely right.

LIEBESKIND: Who must have done that early work with you.

MELZACK: Yes.

LIEBESKIND: So is that true? What are the antecedents of that? Where did that come from? How did she happen to work on that problem?

MELZACK: That really was her baby, yeah. Her PhD thesis with me was a transcutaneous electrical nerve stimulation study to show that it was really more effective than a placebo effect and it was a very good thesis. But what happened -- she tells me what happened -- is that she was invited to a meeting of nurses and asked to talk about pain and she thought it would be very interesting -- she was scudding around for what topic she should talk about, and thought, gee, no one talks about pain in children. And as a nurse, she saw such a hell of a lot of it. So that is absolutely her baby. And she says she now finds it kind of embarrassing, because she wrote that one paper, which reviewed whatever literature there was, pointed it out as a major problem area; and she says now everyone cites that paper and they keep inviting her to give the keynote speech, and she says she doesn't know that field, she doesn't follow that field anymore.

LIEBESKIND: That just opened up, didn't it? I mean, Sunny Anand [Kawaljeet S. Anand, Professor of Critical Care at the University of Arkansas] made these great revelations.

MELZACK: Terrific, terrific stuff. What a field. The trouble is, John, that it hasn't moved nearly as much as it should. I mean, you'd think that -- this is 10 years now or 15 years we've been screaming, give more medication, give more morphine, it won't turn people into an addict. And here is our joint student, Anthony Vaccarino, just recently writing a paper, showing that animals that get morphine when they are in pain do not need increasing doses and don't get addicted. And we are still having to show that. For all of David Mulder's talk and everybody else's, people are still undermedicated.

LIEBESKIND: Goes slowly.

MELZACK: Very slowly.

LIEBESKIND: You know, I don't have this as a question, but it seems to me it is the key question in the field -- why? Why does it go slowly? What is the impediment? I don't know if you and I have talked about this foundation that's gotten started, a private foundation -- some old lady left them 13 million dollars and so forth -- it's called the Mayday Fund.

MELZACK: No.

LIEBESKIND: We talked about that?

MELZACK: No, I've never heard of it.

LIEBESKIND: We should. They're about two years old and they've got over 13 million dollars, they're located in New York.

MELZACK: I'm sorry, you did. Did you introduce us to the lawyer?

LIEBESKIND: You've met her. That's it, Fenella Rouse [former Executive Director of the Mayday Fund]. Of course, I should have thought of that.

MELZACK: I'm sorry, you did tell me, sure.

LIEBESKIND: Their mission is -- they have a beautifully written statement -- and, I mean, we could have written that for the IPF, but we didn't write it so succinctly. They really focus in on this very issue we are talking about now. They say it's the gap between current knowledge and current practice and here's all this knowledge and yet it's not being put into practice. How can we fill this gap, how can we cross this chasm? What are -- and they talk about in their literature -- the impediments to crossing this, the impediments to learning, to understanding, what are they, how can we address them, how can we identify them, what can we do about them?

MELZACK: Sure.

LIEBESKIND: What are your thoughts on that? Why is it that people -- I mean, doctors are not monsters. They are decent people -- many of them are very focused on the welfare of their patients -- why would they be reluctant to believe that just something as simple as postoperative pain is a problem and it's a problem they could do something about? Simply, easily, safely. Why are they reluctant to believe that? Or with cancer pain. Why are they reluctant to believe that opiate drugs can be used safely and effectively throughout the course of the patient's -- What's the impediment there? Do you have any feel for that, Ron?

MELZACK: No. I've thought about it, naturally, but I don't understand it. You know, you were the first person ever to use the word opiophobia -- you went to a meeting and somebody --

LIEBESKIND: No, no, somebody else.

MELZACK: No, you were the first person to ever use it -- the first time I'd ever heard it, you said it, but I'd never heard the word before, but you told me that you heard it at a meeting in Houston or something. And then, you know, Stratford Hill -- is that his name?

LIEBESKIND: Stratton Hill [C. Stratton Hill, now Professor Emeritus of Medicine at the University of Texas M.D. Anderson Cancer Center].

MELZACK: Stratton Hill, right, says that this is an irrational behavior and it is irrational. And I don't know what the answer is. I don't know why. I do know that the law enforcement agencies certainly must play a role in this, because I think people are scared as hell of being accused of using too much of a drug that's going to -- but then you get the same irrational feelings among patients themselves. So I know at our pain clinic at the Montreal General, we sometimes have to fight with patients. I mean, we are very liberal in our use of drugs -- we don't hand it out to everybody, but we do, when we have a patient in severe pain and nothing else has worked, we say we'd like to put you on MS Contin. And they say, oh good, what is it? Well, it's opium, it's a slow-release opium, morphine, and it will help you. No, I wouldn't want that, I don't want to take morphine, my God, I'll turn into a junkie, I'll turn into an addict. And we end up fighting with these patients. And they don't want it. And so it's insane, it's absurd.

LIEBESKIND: You know, the first time I ever heard that story was you telling it, and you made a very interesting point. You were telling about your brother-in-law, Harry, that you were speaking of earlier in this interview, and how you tried to convince him to take more morphine, and he saw that as emblematic, symbolic of his death.

MELZACK: That was another patient, actually.

LIEBESKIND: Oh, that wasn't your brother-in-law?

MELZACK: No, but that new patient is mentioned in the same paragraph, almost the same breath, that I talked about Harry.

LIEBESKIND: Tell me that story, because that was a very dramatic story.

MELZACK: Oh, I describe Harry and just point out that I wished that more were given to him; but then I say that the problem does not lie exclusively with the physician, that really some patients are to blame. And so this was a patient whose cancer tissue was actually pushing its way through his skin. He had had cancer of the rectum and his whole lower abdomen was full of cancer obviously and was pushing its way through his skin. It didn't look ugly, by the way, John -- I thought it was going to look horrible. It didn't look horrible. It looked like normal healthy skin, except it was obviously deadly. Anyway, this guy said, "My friends will all think that I'm a junkie. They will not respect me and I will not respect myself and I would rather have the pain and I want to die working, a working man." And his wife was saying, "Sweetheart, you don't let me sleep, you don't let the children sleep, you're making us miserable." And he said, "No, that's the way it's going to be." Well, we ended up giving him another kind of drug which was --

LIEBESKIND: But there was another one. There was another theme in there that had to do that it represented for the patient that he was near death, that he knew that he would take morphine only when he was near death and he didn't want to start because that would indicate that, it would be emblematic.

MELZACK: Right. Oh, I'm sure there must be some of that too.

LIEBESKIND: I remember you telling some story like that.

MELZACK: There must be some of that too. There must certainly be.

LIEBESKIND: So the problem is, as you say, it's not just with the health provider, it's with the patient as well.

MELZACK: It runs right through our whole culture. And Stratton Hill is the only guy I've ever read who has really looked at it that way, as a cultural phenomenon, but God knows what the roots are. It would be very important to have a sociologist, anthropologist, I don't know what -- the problem doesn't lie with us, it really lies with sociology and culture.

LIEBESKIND: Well, I was just going to say, sociology, social psychology, where you're dealing with attitudes.

MELZACK: That's right.

LIEBESKIND: And how resistant they are to change.

MELZACK: Incredible.

LIEBESKIND: Whether it's an attitude about racial prejudice, or something of this sort -- how do you get racists to live in harmony next to one another? How do you -- Maybe it goes back to the matrix, dammit, if we're born with these things, you know.

MELZACK: Yes, yes, absolutely.

LIEBESKIND: Could fit in there somewhere. Ron, the next is a very broad question. I don't know how you want to deal with it, but what I have here is how has your work in the field of pain affected your life in general? How have you -- has it been stressful for you, is it pure pleasure, how has it affected your living with a woman, your wonderful wife Lucy, how has it affected home life and so forth?

MELZACK: I'll tell you that, for one thing, I feel lucky as all hell to have found a topic that is intellectually challenging and satisfying, and at the same time has got practical value. There's so few things that you can do that with, that touch on both of those. And then the second one is, we have all, you and me and others, have started this field, and so we have become a family. And that has been enormously satisfying to us, to our spouses, to all our families, right? So I just think that has enriched our lives enormously. Again, I can't think -- there are people working in the field of vision [that] do have their own groups and everything, but I feel somehow what we have in our group, working in the field of pain, is we really have pioneered something new and it's been wonderful. We all like each other, we all get along well, and so that has been a tremendously enriching experience. I feel blessed to have stumbled into this fascinating and important field.

LIEBESKIND: When you look back on it, when we first got started, in Issaquah or wherever you want to mark the beginning of the field, there certainly weren't very many other examples, if there were any, of fields that were so interdisciplinary, that brought together -- You know, meetings were all -- the psychologists got together, the physiologists got together, or whatever -- they didn't get together with each other. But here was a group that focused on an idea, a concept, one that related to nursing and dentistry and acupuncture and psychology and anatomy and anesthesiology and all these things. And so it created these strange bedfellows.

MELZACK: That's right.

LIEBESKIND: And we all jumped into that bed together. And there are not many -- I don't think even today there are so many other examples like that.

MELZACK: I don't think there are.

LIEBESKIND: And I think that's really the great part of our field.

MELZACK: Yeah, it is. That's right.

LIEBESKIND: What makes it so interesting when we talk about this family that we are, it's such a diverse family. If we were all the same, you know -- if we were all psychologists or something, it would be a hell of a lot less fun.

MELZACK: That's right.

LIEBESKIND: Pursuing this a little more, has it -- I mean, you work hard. It can be a strain at times.

MELZACK: Sure, it is.

LIEBESKIND: But how do you manage that? You also seem, maybe more than anyone else I know in this field, so in balance. You have such broad interests within the field itself, you're doing this kind of work, doing that, you obviously have found time to think and reflect within the field; and then outside the field, you're also -- you've been referred to as a Renaissance man, you are -- You've written books for children, preserving Eskimo folk tales and so forth and so on. What's the secret of your success?

MELZACK: The secret of that, I would say, is simply curiosity, one. I guess it's the good fortune to like literature and music and here I stumbled on, in the case of the Eskimo legends, onto a whole literature that was literally lost; and is lost once again, I feel. It was lost for a long time, because these stories were all in the anthropological journals and stuck away in the stacks, because they were published in the 1870s and '80s, or 1910s or something -- and nobody was reading them. Nobody was telling them to anybody -- they are wonderful stories. I published those books of mine. Other people published some as well, but now the emphasis has come onto recounting the stories only as they are told by the indigenous people themselves.

Now the fact is that the original story doesn't make a good story. Eskimos, for example, did not need to have a beginning or an ending to a story, but our kids do. So if you are going to tell them in a way that our kids would enjoy them, you had to retell the story. And so these indigenously told tales are just not having any impact at all. They are published by small presses and nobody reads them. So it's kind of sad. And when I resubmitted some of my stories to another publisher, they said, "Sorry, we just don't take these anymore, they're delightful, but we're not interested."

LIEBESKIND: We're now purists.

MELZACK: That's right. But you'll be interested to know that one of the stories from one of the books, called "The Woman Who Raised a Bear as Her Son", won a prize. The story was used as a half-hour cartoon feature, and it's shown virtually every Christmas in Canada and in one of the cable television companies in the States, and last year it won the prize as the best cartoon.

LIEBESKIND: Is that right? This is based on one of the stories that you --

MELZACK: Yeah. On television, one of my stories, which made me feel very good. And I still get about two or three requests to use the stories in collections of tales for children, and that thrills me to bits. I love it. I love it.

LIEBESKIND: Isn't that great. That's great.

MELZACK: And then there are still some more children's stories that I want to tell, so, John, when I retire, I'm going to do that.

LIEBESKIND: Are you going to retire?

MELZACK: Well, I plan to, yeah.

LIEBESKIND: Do you have to? You were saying last night, no, you don't.

MELZACK: I don't have to, no, but I just feel that the time has come for me to retire. I can see myself working for another couple of years, but that's all. I think 68 would be a great time to retire.

LIEBESKIND: How old are you now? You're 63?

MELZACK: I'm now 64, and I'm going to be 65 next July, and then -- if I work a couple of years after that, that surely is as much as I -- then I really would love to relax and travel. And I can give up all this. That may seem a strange thing for me to say, because I'm so very much involved in what I do; but I can also see myself walking away from it and thinking, I've done my share, it's been fun, and God knows there's a lot of, there's a whole new generation of bright, bright young people.

LIEBESKIND: Well, again, you're someone who has all these outside interests and could think and write still in the field of pain if you wanted, in the privacy of your own boudoir.

MELZACK: Sure, if I wanted to.

LIEBESKIND: You could read and write more and so forth. You wouldn't have to have a laboratory going.

MELZACK: No, I don't need a laboratory.

LIEBESKIND: Or a teaching obligation, or be on any faculty committees. You could just do that as a retired emeritus professor.

MELZACK: I think there's still a tremendous need for a good book for the layman, to tell you the truth.

LIEBESKIND: I think you're right.

MELZACK: That's one that you ought to try, John.

LIEBESKIND: Well, that's in my mind.

MELZACK: I think you ought to really try it.

LIEBESKIND: It's one of the things I'm thinking of -- again, that's a few years down the road.

MELZACK: Well, you know, I think if you were to orient -- one possibility would be for you to use some of this wonderful material you are collecting and maybe even tell the story around the

personalities of people involved in the field. That would make it readable and you would get the point across when you tell Cicely Saunders' story and Bal Mount's story or all these, boy, John Bonica's story; and every one of them has made one particular kind of a very important contribution. You could get the tale across that way.

LIEBESKIND: I think you're right. I think that's a terrific idea. Ron, just a few more questions, we're drawing to a close here, but there are a few more I just want to throw at you. What are the traits in a scientist that you personally most esteem? When you think of other scientists, what are they, who are they, if you would be willing to name some of the ones?

MELZACK: Imaginativeness.

LIEBESKIND: Imagination.

MELZACK: Imagination is to me all of it. So when somebody -- I read a letter of recommendation where the person is described as, oh, picks up techniques rapidly -- that doesn't impress me at all, everybody can pick up techniques more or less rapidly; so you don't pick it up so rapidly, what the hell's the difference. But when they have that character -- and it can't be acquired, sadly. You can't go and say to somebody, you know, exercise your imagination. Be imaginative. It's what you look for, it's one of those innate qualities of people and we have seen that among our friends and I prize that above all other qualities.

Now naturally, as you just said, if you don't write down your idea in a way so that your colleagues can understand what you are saying, you might just as well never have had the experience or the idea. You've got to express it, and that requires actually that you've got to be conscientious enough to make yourself, force yourself to sit down and write, which most of us hate to do. So you now have imaginativeness and, if you want to get the answer to a question you've raised, which nobody else seems to have raised in quite that way, you've got to learn ways to examine it. You've got to pick up technology and you've got to communicate it. But imagination is there as numero uno in qualities.

LIEBESKIND: So I suppose if I asked you the converse, you might give the converse, as if I said what are the traits in a scientist that you dislike the most, maybe it would be lack of imagination.

MELZACK: Yeah.

LIEBESKIND: That would certainly be up there, wouldn't it?

MELZACK: That's right. And it's true. I go to talks, I go to colloquia here -- I stopped going to most of them. But I used to go to them, I used to fall asleep -- within the first 10 minutes I'd be fast asleep, except -- that's to say, most -- but there were guys that really kept me awake. There were guys with really imaginative, some of them way crazy, outlandish ideas, but by golly, new ideas, a new way of looking at a problem, looking at a field. But you know, somebody, a hell of a good guy from the United States -- his name is Singer -- he's a biochemist, I think, and I think he wrote a paper called "Ideas: An Endangered Species?"

Something like that. Scientific ideas, an endangered species. And so it's not only our own field, John, it's rampant throughout the whole, I think, biological science field. The whole emphasis is on technology these days. Who can build a bigger cyclotron.

LIEBESKIND: Do you have a thought that this difference among scientists, the ones who are interested, who are excited by ideas, who are imaginative, as you say, vs. the ones who are more technically oriented, who are not imaginative -- do you feel that that reflects some traits, some personality traits that must be consistent within an individual. Is it reasonable that someone who is, let's say, imaginative in science would be dull and unimaginative as a human being?

MELZACK: Yeah. I've met people like that.

LIEBESKIND: Have you?

MELZACK: Yes.

LIEBESKIND: Whose imagination comes out in one domain, but not --

MELZACK: But not others. And they're just outright dull, uninteresting people. Now, where you see this -- it always comes as a shock -- is when you meet somebody who is a well-known artist, let us say -- a painter artist, or a well-known novelist with tremendous insights. And then you meet them at a party and you talk to them. Or you may even meet them at some more pleasant, convivial place, and it turns out you are talking to a perfectly dull bugger who somehow -- the muse hits him when he is down sitting at a table with paper and pencil, but Jesus, not a sign of any other aspect of that personality reflecting this marvelous scientific quality, or whatever it is, or artistic quality. It is entirely possible that in that movie *Amadeus*, it's possible that Mozart might really have been that sort of a prick that was shown in that movie.

LIEBESKIND: Right. But get him in front of the piano --

MELZACK: That's right, sit in front of a piece of paper and this guy's a genius. It touches you right to the core of your soul, and yet an absolute -- it's strange, isn't it?

LIEBESKIND: There's a different twist on this I'm not quite getting -- I'm not quite able to ask my question in the right way. But I'm wondering if people who -- let's take it from the other end -- people who are unimaginative in their science -- is that reflective of some aspect of their personality -- that they're afraid to let go of their imagination.

MELZACK: Oh, I think that's true.

LIEBESKIND: They could be imaginative, maybe, or more imaginative, but they are too conservative, they're too nervous, they might be wrong. I think of someone like, just to take Pat Wall as an example -- now I think of him as someone who is very imaginative.

MELZACK: He is.

LIEBESKIND: And creative and willing to put himself out there at the end of a limb on something. He can be outrageous, maybe, even overstate something, let people take a shot at it, and he likes to have a fight and so forth and so on. Obviously, you are very much the same way, but I'm taking it away from you to put it on someone else here. And then I think of someone -- maybe Ed Perl [Edward R. Perl, who traced the nociceptive C-fiber pathway, Professor of Physiology at the University of North Carolina], who is different in that respect, who is much more conservative and holds his scientific cards closer to his chest and doesn't want to --

MELZACK: Sure. That's a good way to describe it.

LIEBESKIND: You wonder if there isn't a personality factor here, if there isn't a dimension --

MELZACK: Oh, I think there is.

LIEBESKIND: That has to do with toilet training or upbringing in some way.

MELZACK: Well, you know, Hebb used to say that you could never force anyone to have an idea, to be imaginative. All you can do is provide that person with a sense of freedom in your laboratory and make that person feel that it's natural and wonderful to have ideas and even if they're silly ideas, but you've got ideas, that's a rare quality, and encourage people with ideas to come out with them. I'm sure you have, as I have, met graduate students who arrive here and are sort of stunned when you and I sit down and are ready to toss off ideas of our own to them, and then eager to hear theirs. I mean, no one had ever done that to them. They were all told, in fact, shut up. This is the way you do science. You go into a laboratory and you use tools. You don't sit and think. And what science is about is thinking.

LIEBESKIND: All right. One more. Do you have any favorite examples in the field of pain of blind alleys, of things that you or someone else, some other theme that people have gone down, have pursued that just hasn't led us anywhere? Would you identify any themes of that sort?

MELZACK: Well, I think the endorphin-enkephalin story seems to be a blind alley, but I don't know. Somebody may yet [discover more], so it's hard to say.

LIEBESKIND: Certainly hasn't solved the problems.

MELZACK: No, that tremendous excitement and anticipation certainly never -- and then I think that we are letting ourselves in for the same thing with all this NMDA and C-fos and everything else, where it turns out that we're talking about basic biological mechanisms that play a -- that simply reveal how the brain functions overall as living tissue, but it's got nothing in particular to tell us about pain. But there again, some guy may find some kind of a molecule that really stops up calcium channels and stops pain, I don't know. But yeah, I've certainly seen that. Oh, I think the whole psychophysics and pain field -- I think that [Wilbert E.] Fordyce's operant conditioning approach -- it has changed so radically as to become very good, but it's no longer Fordyce's old operant conditioning. [Fordyce, b. 1923, is now Professor Emeritus of Rehabilitation Medicine at the University of Washington.] It has now added to it everything, it's

now become a multidimensional thing and you take into account the fact that behavior can be manipulated and some people have to learn to live with their pain.

LIEBESKIND: What started out as Skinnerian has become cognitive and it's everything else just glommed on there.

MELZACK: Sure. So I think that his original thought was a blind alley, but there, look at that, it became, now that we're talking about it now and thinking about it, it became a very rich alley. Somehow the little stream opened up into a bigger one. I used to speak ill of his contribution, but I think very highly of him now and realize that he's made a very major contribution to the field. He's a very nice guy, too, and his wife is a lovely person. I like them both. And I'm glad I feel that way.

LIEBESKIND: Who else should I interview? We started to talk about that, and you mentioned Bal Mount right here in Montreal.

MELZACK: Yes, he'd be a wonderful person to talk to. He had cancer, that's how it all started, so it would make it a very personal story. And I'm thinking in terms of you telling the tale. If you were to tell the story of pain from a personality point of view, his is dramatic, because he had just finished his residency in urology, surgical urology, and was showering one day and felt a lump on his testicle, went right to the guy he did his residency with, who immediately removed that testicle. He was able to have children afterwards, but once you have cancer, you are marked. So the first thing he wanted to know is, "How do people die?" And that's what made him go to Cicely Saunders' place [St. Christopher's Hospice]. So that's quite a terrific story. So next time you're here, John, we'll have another smoked meat sandwich at Schwartz's -- so there's more smoked meat in your future. I'm gazing into my crystal ball. You have to come back and interview Bal. Who else is there in town here?

LIEBESKIND: Think with me some more about some of the -- as you think about the field of pain and kind of focal ideas, these different aspects of the field that have gotten so much interest and have changed the way we think about pain and so forth, who are the key players that you see?

MELZACK: Right. Do you plan to interview Harold Merskey [Professor Emeritus of Psychiatry at the University of Western Ontario and the leader of the original IASP Taxonomy Task Force]?

LIEBESKIND: I do. I've talked to him.

MELZACK: He'd be a very good person. He has a sense of history himself, as you know; he's a very historically oriented sort of guy, so he'd be good to talk to. Ken Casey is down on your list, I'm sure. A guy like Crawford, what's his name, Crawford Clark; there's a guy who went down a blind alley.

LIEBESKIND: Yeah, speaking of blind alleys, signal detection.

MELZACK: The last guy in the world I'd want to talk to. But the same thing is true for Donald Price [Professor of Oral and Maxillofacial Surgery at the University of Florida]. The same thing is true, unfortunately, for your old student who I thought had great promise, David Mayer [Professor Emeritus of Anesthesiology at the Medical College of Virginia] who never did a goddamn thing afterwards, after he worked with you, he never did. You brought out the best in him. I don't know, has Huda Akil done anything [Akil is now Gardner Quarten Professor of Neurosciences at the University of Michigan]? Oh, is [Hans] Kosterlitz [(1903-96), discoverer, with John Hughes at the University of Aberdeen, of the first endogenous opioids] still around?

LIEBESKIND: He's still alive. I don't know how well he is. He's had a series of strokes, as Pat Wall knows.

MELZACK: Oh has he? That's too bad.

LIEBESKIND: He was in Los Angeles one time and had a stroke when Pat was out there and Pat flew him home. That was years ago, oh yeah, that was quite a story.

MELZACK: Oh gosh, I remember hearing about that, that's right. I remember being, I was at that meeting, I was there when Pat flew him home.

LIEBESKIND: Oh, *that* was the meeting. That was the meeting that [Lawrence] Kruger [Professor Emeritus of Neuroanatomy at UCLA] and I did, I think that was that same time. So that's a long time ago. Oh, he's quite ancient now.

MELZACK: OK, but he might have some interesting reminiscences, although I'm not sure. I did notice, though, that John Hughes, who was his student, is now an eminent professor at Cambridge or something.

LIEBESKIND: Is that right?

MELZACK: I don't know what he would remember. I think these would be very localized sorts of things.

LIEBESKIND: Do you think I should interview Ed Perl?

MELZACK: Oh sure. Definitely.

LIEBESKIND: Ainsley Iggo [Professor of Veterinary Physiology at the University of Edinburgh, first to describe the C-fibers; IASP President 1981-94]?

MELZACK: Absolutely.

LIEBESKIND: They go way back, these two, certainly.

MELZACK: Yeah, but they're part of that history of psychology that we mustn't leave out. Exerting more influence now than one ever dreamed, but the field has gone backwards. Ainsley, I gather, is quite sick.

LIEBESKIND: I understand, yeah. I saw him in Paris, he looked well enough. Do you, as you look back on this interview, have any suggestions for me? I mean, were there any questions that were particularly interesting or any that were particularly uninteresting, or any advice you would give me in this process, as I continue to do this?

MELZACK: I think you are doing an absolutely terrific job. You made me feel that you were absolutely interested in everything I was saying.

LIEBESKIND: Well, I was. That was easy.

MELZACK: Well, OK. I think that's the first, that's it. Those times that I've been interviewed on the radio, for example, whatever -- it's always been that the guy I'm talking to is really interested; and suddenly my nervousness goes and everything else, and I open up and I just talk. So I think you're doing the right thing, John. I think all those questions were well thought out. They were intelligent and relevant.

LIEBESKIND: I try not to put too much of myself in this, but every once in a while I jump in and make a little speech. Maybe I'd do better if I didn't do that as much.

MELZACK: No, no, I think you're doing it just right. It must vary from person to person that you interview, I'm sure, but I think you're doing very well. If I can think of anybody else that would be interesting to talk to, I think it might be fun one day -- why don't you go to Japan and talk to some of these guys and find out what the hell really happened with that, just as a side issue of pain, as an interesting historical note -- that would be intriguing. I mean, I'd be really curious to know -- how the hell did that happen?

LIEBESKIND: Well, we are now going to have a [IASP Council] meeting over there, aren't we?

MELZACK: Is that right?

LIEBESKIND: I think that's established now for down the road. I'm making that up, or after Chicago, isn't that the next?

MELZACK: After Chicago?

LIEBESKIND: After Chicago is Japan, isn't that right? Seems to me I heard that.

MELZACK: Wonderful, I'm all for it. I'd love that. I think that would be great. Who else would be good to talk to? I think Mike Cousins would be very good to talk to.

LIEBESKIND: I'm interested in the science, but I'm also interested in the clinical, in the specialties, the clinical aspects.

MELZACK: Having spoken to John Bonica, I think Mike could bring you up from where John Bonica started. I will tell you, this is not for the record, but there was a time when I wanted to bring John Bonica back to the Council sort of. And Mike didn't say no, but what he said was such that the young guys like him would all feel like we are taking a major step backwards. But I think it was Mike himself who then took the major step of creating the honorary, the Founding President role and making sure that John constantly gets invited. But when I suggested something very similar to that -- so things changed. I think Mike mellowed and grew up. Didn't see himself as the young Turk who was going to overthrow all those stupid old ideas or whatever the hell he was trying to fight. I don't know what battles he was fighting then.

LIEBESKIND: You know, John Bonica has been such a dominant person, and he's such a dominant personality that I can be sympathetic with the feeling.

MELZACK: Is there anyone in Europe, I wonder? I can't think of anyone who played a major role other than Jean-Marie Besson, who might summarize the whole thing.

LIEBESKIND: I thought about the possibility of interviewing my old post-doc mentor, Denise Albe-Fessard. I'm not sure how easy that would be for me in French or for her in English, so that might be a good example of where it would be good to get somebody else to do it. But I don't know if she is talking to any of the French people there.

MELZACK: I think you could do a wonderful interview; I think her English is bloody well good enough. You could have her talk in both if you wanted. You can always translate it afterwards or get one of the people to translate it. But they're all still in awe of her. You can see, [Gisele] Guilbaud and Besson [both students of Madame Fessard] -- I mean, they adore her and they hate her -- so goddamned ambivalent.

LIEBESKIND: They adore her, but well, she won't let them get close.

MELZACK: I mean, it's fascinating to watch -- I never even knew of this business until this last meeting. The whole thing came out with that City Hall meeting, when there was Guilbaud, Gisele, saying that she had waited for Albe-Fessard to show up, so that she could take her to the elevator and Albe-Fessard just walked right past her and marched up the steps and how hurt she was. God, you could see the hurt on her face.

LIEBESKIND: It's been going on for years like that, with all of her former students.

MELZACK: It's absurd. I would have said, I never want to talk to that bitch again, and I wouldn't have let myself in -- Isn't that amazing that she can do that.

LIEBESKIND: Just to be blunt.

MELZACK: Yeah.

LIEBESKIND: Well, it's some sort of weirdness, I don't know what that's all about.

MELZACK: Weird all right. Very strange.

LIEBESKIND: Have we captured Ron Melzack the man?

MELZACK: I would like to think so. The only thing that might be said is that Ron Melzack is a psychologist above all. And that in the field of pain, I've always -- yes, I'm very proud of being a psychologist, and feel it is the only field that might have allowed me to get as broad as I got. And that physiologists were always, had their nose down to the nitty gritty of things, whereas in psychology we were allowed to think and expand and relate pain to --

LIEBESKIND: Look through the funnel in the other direction.

MELZACK: Start looking at visual hallucinations and auditory hallucinations -- and so I feel very much like I'm coming back to psychology, never going to lose the roots in pain, but growing up into something. What will be interesting to see, if this book ever gets written, is whether the larger field of psychology is going to give a sweet damn about what I have to say, but we'll find out.

LIEBESKIND: Well, they certainly admired Hebb at the time, but they admire him a lot more now than they did then.

MELZACK: So maybe sometime down the line -- that's right. Ben [Liebeskind's son] or Ben's grandson or something like that will pick up a book.

LIEBESKIND: That's why you mustn't throw this book away if you decide you don't like it.

MELZACK: No, this one I'm going to publish. I've even signed, I have a contract with Oxford University Press, because I'm definitely going to do it, whatever comes of it, whatever it is, I told Lucy, this is it.

LIEBESKIND: I have another question in here, and we can end with this one. I don't know how you are going to get at it, but I still want to understand your passion. I don't know anyone else who expresses that passion, who feels it, obviously feels it in the same way that you do. Maybe John Bonica is the only other person. You talk to him and he starts talking about some of these issues where he feels so strongly, and tears well up in his eyes, as you know. I'm sure you've seen this. And you're the other one that I would single out like that, who is such an effective orator in this respect -- I mean, it obviously is not just lines that you are orating, it comes from within. Where does it come from?

MELZACK: I have no idea, John. I don't know. All I know is that my brother tells me, whenever my mother used to cry, I'd start to cry too. I don't know if that has any bearing at all on it, but my oldest brother, Jack, when he left home, brought great grief to the family. He changed his religion and all sorts of things.

LIEBESKIND: This is the one that died young.

MELZACK: He died young, he was the genius who started the bookstores, never had much of an education, had to educate himself; but whenever my mother would be talking to Jack on the phone and she'd start to cry, Louie said I would always start to cry right along with her. I don't know -- I have no idea except, and I'm not even aware of it, except that I'm obviously -- except people tell me this, like what you have told me.

LIEBESKIND: Some of our colleagues, I can't imagine them standing up and talking with this kind of passion. Not to name names, but you know, the majority of them. As a field, I think, we are kind of passionate, but a lot of the individuals you can't imagine them speaking from the heart in this way.

MELZACK: I guess I can only say that it's a talent I was born with and I'm grateful for, but shouldn't be given credit for, because I didn't do anything to have it.

LIEBESKIND: You didn't work to get it.

MELZACK: Didn't work to get it, that's right, it just happened to be there.

LIEBESKIND: Part of that neuromatrix.

MELZACK: That's it. One of the dimensions of personality, our incredibly complicated human personality.

END OF INTERVIEW