

Lewis P. Lipsitt

- Born: 6/28/1929 in New Bedford, MA
- Spouse: Edna Duchin Lipsitt
- B.A. Liberal Arts (1950) University of Chicago, M.S. Psychology (1952) University of Massachusetts, Ph.D (1957) Child Psychology



Major Employment:

- United States Air Force - 1952-54, Clinical Psychologist
- Brown University - 1957-Present, Professor of Psychology (1966-1996), Professor of Human Development (1994-1996), Professor - Research - of Psychology (1996-Present), Professor Emeritus of Psychology and Medical Science (1996-Present), Professor Emeritus of Human Development (1996-Present)
- The Lee Salk Family Center, Wiley House (1993-Present) National Co-Director

Major Areas of Work:

- Infant sensory and learning process
- Long-term development
- Autism
- Crib death

SRCD Affiliation:

- Member since 1957

SRCD ORAL HISTORY INTERVIEW

Lewis P. Lipsitt

Department of Psychology, Brown University

Interviewed by Andy Meltzoff
Providence, Rhode Island
April 7, 1998

Meltzoff: Doing Lew Lipsitt, I'm in his home in Providence, Rhode Island and, Lew, what's the address here as well?

Lipsitt: 63 Boylston Avenue, Providence Rhode Island 02906.

Meltzoff: Lew and I are talking on April 7th on Tuesday in the afternoon, after having a nice Greek lunch in a Greek restaurant and a couple days after coming back from ICIS which was—where was it this time?

Lipsitt: In Atlanta, remember? It was just two days ago.

Meltzoff: Right, that's right—48 hours ago! Interestingly, this is 1998 and it's April 7th, 1998 and Lew organized the first large ICIS conference in Providence in 1976.

Lipsitt: No, the first ICIS conference was in 1978.

Meltzoff: In April presumably.

Lipsitt: The most recent one before the one that we've just come from was in Providence. That was the 10th anniversary meeting of ICIS, in 1996. Edna and I—Edna, my wife—and I organized that conference as well as the very first one which occurred 18 years ago in Providence as well.

Meltzoff: So the very first one, which would be twenty years ago from now, right, or eighteen?

Lipsitt: Twenty years ago this date.

Meltzoff: Just this date, we are very close to the twenty-year anniversary.

Lipsitt: Yes.

Meltzoff: The very first one.

Lipsitt: It's very exciting.

Meltzoff: It's an exciting time for infancy and you played a huge role which we will get to when we finish talking today, a huge role in this revolution that's happened over the last twenty years in infancy research. So I think that will become clear in this interview about what an impact your research and your students have had, but if we start a little bit with ICIS, I remember that I went to the first meeting and this conference we just came from had about 1200 people. Do you remember, I don't, do you remember anything about the size of the first one? Do you have any idea of that first meeting twenty years ago in Providence about how many people there were?

Lipsitt: I should know that, but I would have to go back to my records to find out. I think perhaps 300 or 400 people.

Meltzoff: 300 or 400, yeah and that itself started from, had its own developmental roots. So can you tell us a little bit about where the organization ICIS came from, I heard it started off as CRI.

Lipsitt: Yes, CRI standing for, I believe Correspondents on Research in Infancy. CRI and, actually I appreciate your compliment in which you attribute to me important things at the outset, but there were a number of people back then who were very involved in getting child psychologists, particularly those interested in infancy, together, child psychologists and some pediatricians and psychiatrists as well. For example, Jerry Kagan and Bill Kessen were involved in this. Hawley Parmelee, Wendell Jeffrey who was then doing some infancy research and Frances Graham and there were a number of people who had laboratories on infancy at that time, 20 years ago and actually more than 20 years ago. It was 20 years ago that the CRI organization evolved into the ISIS organization as we know it today—ISIS standing for International Society for Infant Studies.

Meltzoff: CRI was a way for the few researchers in the country, actually of having colleagues they could be in touch with. Well, presumably you were not sending e-mail back and forth to each other.

Lipsitt: No there was no e-mail at the time.

Meltzoff: It depends what year somebody hears this tape, either e-mail will be the fancy modern technology or they'll be listening in the year 2010 and saying, had they used e-mail back in 1998, that old-fashioned way?

Lipsitt: There was U.S. Mail and most of us who were doing infancy research were really and truly in touch with each other, we all knew each other and had a good time exchanging ideas. We would talk about infancy when we went to SRCD and participate in symposia and give lectures on infant behavior and development which eventually (Infant Behavior and Development) became the name of the journal that eventually the International Society for Infant Studies was a collaborator in.

Meltzoff: Right, we will definitely come back to that later in this interview and talk about how you founded that journal and the Advances series and so forth.

Lipsitt: When we were just CRI we were a very small society. We weren't a society at all; we were not incorporated in any way for sure. Different ones of us took responsibility for keeping the mailing list and organizing where we would be. For example, one year Hawley Parmelee organized us at UCLA. I remember that Marshall Haith was the keeper of the records at one time. Everyone was just willing to pitch in and help get us together, perhaps every year or two.

Meltzoff: Did you ever meet, actually I don't know this, did you meet in Providence before there was a formal group called ISIS while you were just CRI?

Lipsitt: I don't recall that we met in Providence until the occasion when CRI transformed itself into ISIS.

Meltzoff: Gave birth.

Lipsitt: Gave birth to ISIS and that just twenty years ago as we have said.

Meltzoff: Now I heard the group, the CRI group, either got together at the beginning of the meeting or the day before, the small group that had been the parents of ISIS got together.

Lipsitt: Yes, we had the meeting that terminated us right here in this room in this house twenty years ago. It was rather like an opening of the ICIS meeting only it was CRI members only who were invited by Edna and myself to come here and enjoy the evening which we all did very much, and we gloated over, so to speak, what we had become, the beginning of ISIS.

Meltzoff: It was a birth, not a funeral.

Lipsitt: Yes, it was a celebratory event.

Meltzoff: Yes, well I think that you and the others definitely have something to be proud about. Infancy research now has its own journal and we hear we will soon have more journals on infancy and before you started this organization it really wasn't a separate discipline with its own journal that was contributing uniquely to the field, it's made a huge impact. So let's go back if we can and reverse developmental order now to talk about the origins of Lew Lipsitt and then we will, after talking about intellectual history and your own birth, come back up to your research and the changing face of infancy in this country. But if we can go back to the beginning so listeners can hear some interesting information about that. Do you have anything you want to say about your family, your father, your mother or early background in childhood?

Lipsitt: Gosh, maybe I could begin by quoting Danny Kaye. I was born at the age of practically nothing at all!

Meltzoff: This is true!

Lipsitt: Anyway I was born in New Bedford, Massachusetts. My father was a lawyer, and my mother was a musician. I was born in 1929, and I grew up for the first part of my childhood in New Bedford, Mass. where my father had a law practice and when I was nine we moved to Marion, Mass. where I grew up the rest of the way. I was in the fourth grade when we moved to Marion, and I went to Sippican School in Marion. Of interest perhaps is the fact that today Edna and I and our children have a cottage there. Our children, Mark and Ann, and we own a cottage in Marion and go there whenever we can for vacation.

Meltzoff: Excuse me again, you moved when you were what age to Marion?

Lipsitt: Age nine.

Meltzoff: Age nine, moving back to the same town. You must be well known in that town.

Lipsitt: Well, the Lipsitts are well known in Marion for sure because my father was a well-known and rather outspoken man, and he made his voice heard at town meetings, for example, and he was instrumental in bringing about some significant changes in the way in which the town did things. For example, as a Boy Scout leader, not a scout master but as a board member he was instrumental in bringing in black children. There had been a rather annoying segregation of kids in town up to that point, and so he was instrumental in that as well as some other progressive activities of this small New England town. It was an extremely conservative place at the time with a rather exclusive prep school to which, as it turned out, all five of the Lipsitt boys, I and my four siblings, went for our high school education. We were the first Jewish family in the town of Marion and as such received a few insults. Letters and signs on the door, for example. My father saved all of them and after he died we found the envelope with those words on them and he had labeled the envelope, The Marion Ku Klux Klan. There was no Ku Klux Klan in Marion but there certainly were people who were inimical to the idea of the first Jews moving into town. However, we weathered the storm and in fact I would say that we became a respected clan of Lipsitts in Marion. We were very active in scouting; four of the five Lipsitt siblings became Eagle Scouts.

Meltzoff: I saw a picture of you upstairs, was that a picture of you as an Eagle Scout?

Lipsitt: Indeed, the day perhaps I became an Eagle Scout.

Meltzoff: Well, there's a picture which maybe the Archive will someday have of young Lew Lipsitt with a band across his chest filled with badges, and it looks like the adult Lew.

Lipsitt: Merit badges. I was 15 years old.

Meltzoff: Merit badges, and your other brothers were Eagle Scouts?

Lipsitt: Yes, three of my brothers became Eagle Scouts. We were also known for other things including our musicality. My mother saw to it that we were all musical in the sense that we all played an instrument, and we sometimes played in public concerts together, my mother playing the piano, my oldest brother Paul playing the cello, my next oldest brother, Don, playing the clarinet and sax, and I the violin.

Meltzoff: There's another picture in your study upstairs of a very little Lew Lipsitt, I'm sure before your performing age, holding a violin, I don't know how old you are upstairs with your violin, but it's a young child.

Lipsitt: Well, maybe you can't believe it, but I was in kindergarten at that time and wearing the velvet suit that I wore to play in my first concert.

Meltzoff: Is that right.

Lipsitt: At about age 5 ½ in the school auditorium where I went to kindergarten in New Bedford, the Clarence Cook School.

Meltzoff: So you had early experiences of performing in front of people, being on stage much like a professor!

Lipsitt: It's probably no accident that today I stand up in front of people and do things the way a professor has to, because I and my brothers all were encouraged to perform musically and each of us eventually had a talent in music, nothing professional but nonetheless very enjoyable. We all enjoy music today and enjoy even playing now and then. I still play the violin a bit, and I play the clarinet as well.

Meltzoff: Well, your living room has some lovely musical instruments in there. I don't know where they are from but they're around in your living room.

Lipsitt: Yes, you've probably noticed that I collect string instruments. Most people I associate with nowadays think that I mean a yo-yo when I say a string instrument when in fact my interest in string instruments began with the violin.

Meltzoff: But you stayed on strings ever since.

Lipsitt: I've stayed on strings, except for the times when I play the clarinet.

Meltzoff: Okay, so Lew can you tell us a little bit more about early experiences in school and what schooling was like.

Lipsitt: Well I went to grades four to nine at the Sippican School, growing up in Marion, following which for my last three years of high school I went to Tabor Academy which was a prep school in Marion with a military association. It was an honorary naval academy at the time and we wore uniforms, and we learned all about working around the water and around boats. Marion is a sea-side community, a beautiful little community about twelve miles from New Bedford. My father continued to practice law in New Bedford and commuted every day to New Bedford and came home at the end of practically every day with a basket or a box or a bag full of groceries. My father did all of the grocery shopping and most of the cooking in our family. He was an unusual mentor and father in that regard. In this little town of Marion was this prep school that all five Lipsitt boys went to. The lady who founded the school, Elizabeth Tabor, left in her will instructions that so long as there was no public high school in the town of Marion, townies, as we were called, could go to that school tuition free. So all five of us brothers benefited from that educational experience at Tabor and all five of us eventually went on to college and most of us to graduate school as well.

Meltzoff: Yes, I read in an autobiography of yours that's published in a book called *The History of Developmental Psychology in Autobiography* by Dennis Thompson and John Hogan that you have quite a number of degrees among the brothers. Is that right, Masters and Ph.Ds?

Lipsitt: Yes, that's true. My oldest brother Paul was a lawyer first. He went to Boston University Law School which was the school of my father, and eventually did post graduate work at the University of Michigan. Eventually he became a psychology graduate student at the University of Chicago, I think seven or eight years after he was already in law practice with my father. As he described his situation at that time before he got his Ph.D. in psychology, he said that he found himself in the law practice wanting to help people seeking a divorce with their problems more than to help them get a divorce. So he decided that he was basically and fundamentally more of a psychologist. He went on to become a forensic psychologist, and he and I have just finished co-authoring a paper on juvenile delinquency and the courts.

Meltzoff: Is that right, Lipsitt and Lipsitt?

Lipsitt: Lipsitt and Lipsitt.

Meltzoff: I haven't seen that one, that's quite interesting.

Lipsitt: It's not yet published it will be in a book edited by Florence Kaslow on family functioning.

Meltzoff: Family functioning is a good place to have a paper by Lipsitt and Lipsitt!

Lipsitt: Exactly, that's a nice idea. Now, my second oldest brother --- I have to interrupt to say that three of us were born blink, blink, blink one right after the other, very close together. I think we figured out that my mother had three children in about thirty months. The second oldest, Don, is a psychiatrist. Don is chief psychiatrist at Mt. Auburn Hospital in Cambridge and affiliated with Harvard University. Paul is also, as a matter of fact, affiliated with Harvard University and is a consulting clinical psychologist at Boston University in the Health Service there. Don got his first degree at NYU and then he took a Master's Degree in psychology at Boston University. That's important because he eventually helped me find my way into psychology. Then Don went to the University of Vermont where he got his medical degree and eventually did further training, post M.D. training, in Washington with the United States Public Health Service. I'm the third one, and I've also got a few degrees. I got my bachelor's degree at the University of Chicago in 1950 and my Master's degree at the University of Mass in Amherst in 1952, after which I went into the service. I went into the US Air Force and worked as a clinical psychologist. My Master's degree was in clinical and social psychology. I was in the military for two years and that turned out to be a seminal experience in terms of heading me in the direction of experimental child psychology. I can explain that a little bit later but it was a very important period of my life, not only because I had just been married to Edna, and she helped me find my way in the world in many ways, but also because I decided during my Air Force military experience in San Antonio, Texas, that I really wanted to be a child psychologist. I wanted to find out how it is people grew up, how it is that they got the problems that they had. I was more interested in doing that than testing them to see what kinds of problems they had once they were adults.

Meltzoff: I see so it's interesting Lew most of us think of you now as working with infants and young children but in the military you were working with adults and you were busy doing what, labeling or classifying these adults, not looking at developmental origins but finding out how to classify their psychiatric conditions?

Lipsitt: Well, I was doing what clinical psychologists did for the most part in those days, namely psychometric testing. I administered intelligence tests, the MMPI, I don't know how many hundreds of MMPI's I must have administered. I had lots of experience doing TATs and Rorschachs as well.

Meltzoff: So Piaget and you both started off doing intelligence tests, huh?

Lipsitt: Well, there you go.

Meltzoff: There you go, these are the roots of developmental psychology, classify either children or adults, and you say how did they get that way.

Lipsitt: Yes, I think that I was interested in process much more than I was interested in labels for what people had eventually achieved. I was much more interested in how people became intelligent than I was interested in finding out what their intelligence level was as an adult. But I did all this psychometric testing including TATs and Rorschach's, and I just had a wonderful experience because I saw a wide array of patients and I feel that I came to know the field that clinical psychologists deal with. I worked at that time with Milton B. Jensen who was a psychologist from Kentucky who was then a Lieutenant Colonel in the Air Force. I got very good experience working with him. He had written a paper that I admired very much, called "Our Undeveloped Mental Resources," and it was all about some of the low achievement Air Force enrollees who ordinarily would have been kicked out of the Air Force because they were of low intelligence, but Jensen found a way to convince the people at the induction base, the Lackland Air Force Base, to capitalize upon the talents that those airmen had. He had an influence on me in connection with my interest in processes of development and remediation. In addition, I worked with Jolly West, Louis J. West, who was until recently the Chief of Psychiatry and the Head of the Neuropsychiatric Institute at UCLA. I've kept in touch with him ever since. In fact, I had the honor and the opportunity to invite him back to be a participant in a symposium that I planned

when later in life I was at NIH and produced a book with my friend Leonard Mitnick on *Stress and Coping* and *Self-Regulatory Behavior in Infants and Adolescence*. West came to Bethesda at that time and participated in the symposium with me. It's been a lifelong contact with him, a very important one for me. Anyway I was in the service and in communication with Dave Palermo who had been a colleague of mine, a peer, a friend at the University of Mass. at Amherst. When I went into the service, Dave went to the University of Iowa to the Institute for Child Development or the Iowa Child Welfare Research Station, as it was called then.

Meltzoff: "The Station".

Lipsitt: The Station, and he was writing to me about the wonderful training that he was getting there, and eventually I decided that's where I wanted to go, too. So when I was discharged at the end of two years of military experience in 1954 I went to Iowa and ended up getting my Ph.D. there in child psychology in 1957.

Meltzoff: That's jumping ahead a bit to where you are doing your Ph.D, we still have to do your undergraduate days and we're still back here in Mass at the moment.

Lipsitt: And I even have to tell you about two more brothers.

Meltzoff: Yes, I was going to say what we're up to the third Lipsitt boy being born. I know we are getting to two more boys, but let me just ask, what was it like with your mother being around six men I suppose, five brothers and a dad.

Lipsitt: Yes, it was quite a life. To give you an idea of the sort of thing that went on in my family, we used to chase each other around the house, different combinations of us, not my father, of course.

Meltzoff: Little chimpanzees chasing around and wrestling.

Lipsitt: When I was very young I was running through a doorway and one of my brothers, it's very practical that I have forgotten which brother it was, slammed the door in front of me and I put my arm right through a window, so that I carry with me today a scar on my wrist. That was incurred at that time and it was of course, as these things happen, when my mother and father were out of the house. So a doctor was summoned to the house. This was in fact when we had a summer cottage in Mattapoisett just before we moved to Marion, and the doctor came to the house and sewed that up. Later when we moved to Marion it seems as though the negative reinforcement associated with my injury didn't work too well. We continued chasing each other around the house in Marion, where we had a wonderful house for doing this, because you could run down the front steps and through the entire length of the house. It was an old house where one room after another was added on to the rest of the house as was done years ago in New England, and so we could run down the front stairs, through the entire length of the house, through the kitchen and up another stairway at the back of the house and through the entire second floor. So we had, really, a track right in our own house.

Meltzoff: Well a treadmill for the little hamsters, the five hamsters! So your mom would just say, why don't you run around the treadmill as a way of letting off steam.

Lipsitt: Well, the way we would do it was to slam the door in back of us as we would run, you see, and this would create a tremendous racket in the house so another seminal event in my life was my father taking all the doors off of all of the rooms in the house except for the bathrooms and the master bedroom. The fourth Lipsitt son was born seven years after me, after we moved from New Bedford to Marion.

Meltzoff: Quite a gap.

Lipsitt: Quite a gap and I have definite recollections of this because I was baby Lou up until this point even though I was seven. People in my family, I hope affectionately, continued to call me “Baby Lou” long after I was a baby. When this new baby came into the house, as I think back on it now, he was not a threat to me at all. In fact I was very nurturing toward him. I took him on a bit as my personal responsibility.

Meltzoff: Yes, actually I was thinking, without any evidence that this was perhaps an experience that again led to interest in child development because there’s a picture upstairs in your study of you holding your younger brother who was seven years younger than you. It’s a very sweet picture of a boy being quite nurturing toward a younger child. So it is as if you had an interest in young kids or maybe your family, I gather that your uncle was a pediatrician, and so perhaps men being interested in growing children was not looked askance at, as it might have been in other families, but as something to value.

Lipsitt: Yes, I think that there is something to be said about how you just put it. In any event I was not criticized in any way for taking a strong interest in my young brother’s development, which I did and we’ve stayed close throughout our growing up period. Cy, Cyrus is his name, is now the assistant curator at the Lowell Textile Museum in Lowell Mass. He graduated from RISD, the Rhode Island School of Design and has been a commercial artist. This is interesting because Peter, next born, five years after Cy, is also an artist. He’s a sculptor and does heavy works in iron and other heavy materials. He graduated from Brandeis University and then got a Master in Fine Arts degree at Yale. So there was quite an age span of children in our family. We figured out that my mother and father, although we didn’t know my mother’s real age until she died at the age of almost 90, were both 33 when they married, and our mother had babies until she was about 49 years of age. Quite unusual.

Meltzoff: You knew when her birthday was but you didn’t know the particular age.

Lipsitt: That’s right. We knew the date of her birthday but not the year she was born. She was not just quiet, she was adamantly superstitious, about her age. So she never told anybody, even doctors, her age and when we asked her why she withheld this information she said, I don’t want people to know how old I am because I don’t feel as old as I am and moreover I’m afraid that when I go into the hospital for care now and then as I must that they might not take as good care of me if they know my age as they would knowing that I was a bit younger! So she said to heck with it, I don’t need to tell them my age and she didn’t.

Meltzoff: But did she live to, what you find out was the ripe old age once you found out her true age.

Lipsitt: Yes, she was almost 90 when she died in 1981. My father preceded her in death in 1973 by eight years. He was age 81 at the time. They did not get married until they were 33, both of them were 33, it was very unusual at the time. Then, especially unusual that my mother had babies all the way to, I think, age 49.

Meltzoff: That’s incredible really, especially at that time. Now your birth has an interesting story associated, again might be stretching a little bit but when we talked about early experiences that might affect your later career. Can you tell us a little bit about your birth, not that you remember but the story handed down in your family?

Lipsitt: Well, of course, I was there!

Meltzoff: Yes, you were there!

Lipsitt: But the story about my birth I heard from my mother and father some years later, I never knew this as a child, but when I became an adult I learned that I had been a precipitous delivery. I was born without anyone attending me, or at least closely to me, in the hospital, St. Luke’s Hospital in New

Bedford. It seems as though a nurse simply chanced by, my mother was heavily anesthetized, and the nurse looked under the sheets and there I was!

Meltzoff: You had arrived and to this day you have not been late for meetings!

Lipsitt: To this day, yes I am punctual and I'm resilient, I think. My precipitous delivery was followed by a successful pediatric examination by my uncle who was the chief pediatrician in that hospital.

Meltzoff: Was he off duty during this birth?

Lipsitt: Apparently he was not around but my father and the obstetrician were in the next room, perhaps having a cigar or something like that, and so it was the nurse that came and informed the doctor that there I was and he might like to come immediately.

Meltzoff: Your father had another baby son, the third one at that time.

Lipsitt: Yes.

Meltzoff: Well let's move from this very early period of your life which does sound like it was, you have fond memories of it. I mean the twinkle in your eye when you speak about it makes it seem and look like it was quite a happy childhood playing with your brothers in Massachusetts those years ago. Sounds quite nice.

Lipsitt: Yes, I think for the most part I had a happy childhood. I was a rather introspective kid. I was always wondering why I did things as I did and wondered this about other people. I think I was so to speak a natural born psychologist in that for as long as I can remember I know that I was always quizzical about why somebody did what he or she did or why our family pet, Blackie, the cocker spaniel, of which we had several, did what he did. Several Blackies in succession. So I was ready made I think, I was primed to be a psychologist and perhaps a developmental psychologist.

Meltzoff: Well, let's move on to the slightly older years when you went off to college and talk about experiences at the University of Chicago where you were an undergraduate. The University of Chicago has this wonderful reputation actually for undergraduate education and general arts education. Could you tell us a little bit about what it was like when you were there?

Lipsitt: Well, I'm just delighted that I went to the University of Chicago. I feel that I got a wonderful education there. It was the time of Robert Hutchins and the Great Books program at Chicago, and it was the time also when as an undergraduate you didn't specialize, you didn't concentrate in anything, so I have a Bachelor's degree in Liberal Arts, as the degree was called in those days. You could go through the University of Chicago as fast as you were capable of doing so, so I graduated in three years. That was no big deal, by the way, because there were students there who went through in two years, you know, exempting themselves from various courses, because they already knew enough about the subject.

Meltzoff: But it sounds like an experience to savor so maybe three years was a good idea.

Lipsitt: Well, it was a wonderful three years, and I must say that in my third year when I really didn't know, having not concentrated in anything, what it was that I was going to do after Chicago, or after my undergraduate work. I saw a sign on campus that someone named D. O. Hebb was going to be speaking in the Psychology Department that afternoon so I think I played hooky from my job which was being a waiter at the Quadrangle Club, the University of Chicago faculty club. I was also a dishwasher and a phone operator, I did many things at the University of Chicago Quadrangle Club, and I enjoy going back there today to see that old environment where I was happily employed working in part my way through college. Anyway I saw this sign saying that Dr. Hebb of McGill University would be talking that afternoon. I don't even remember the title of the talk, but I know that I cleared my calendar to go

and hear him. He began his lecture in this amphitheater by saying that he had also been a student at the University of Chicago earlier in his career and he was a student, I think he said of economics, although it may have been political science, and he said that he got this injury or disease—I think it was polio but I'm not sure—that resulted in his having a severe limp, and he said once he recovered from the injury or from the operation that was necessary, he found that whenever he walked on Michigan Avenue in Chicago in front of all of these plate glass windows and looked over in the windows and saw himself limping along, he could not believe that that was him. This absolutely fascinated me, because here was this sophisticated man who was—today I suppose we would say that he was in some kind of denial, but he might be insulted by that kind of a label put on it—he thought that it was a perceptual problem, and that it had to do with his experience of himself up until the point...

Meltzoff: The role of early experience, building what we call body schema.

Lipsitt: Very good, very good. The role of early experience and that absolutely captured my imagination. I was too shy at the time to go and talk to him about how much I enjoyed his lecture, but years later I had the opportunity to tell him. I was president of Division 7, the Developmental Psychology Division at APA, and Dr. Hebb came in to receive the G. Stanley Hall award, and so I was able to sit next to him and I was able to tell him about how much he had influenced my life without his even knowing it. He said yes, he could remember, he didn't remember exactly that experience of telling the story at that particular lecture, but he remembered telling that story to a number of people and perhaps even in lecture form earlier in his life, and he was delighted to find that he had something to do with me going into Child Psychology. That was flattering.

Meltzoff: Yes, it's flattering.

Lipsitt: Flattering for me, I mean.

Meltzoff: It's a nice example of sort of things coming full circle, coming around. Here you had an experience as an undergraduate but then became president of the Developmental Psychology Division in the same year that he got the award, you were able to be with this person. That's quite nice and another thing that that reminds me of is you actually got another full circle experience in that you got an award from the University of Chicago, I understand, some years ago.

Lipsitt: Very few years ago. It was on the occasion of my 45th anniversary from when I got my degree. That is it was my 45th reunion. I had never been to a reunion before in my life, and I went to that 45th reunion, thankfully, and got a Lifetime Achievement Award from the University of Chicago. I'm very, very proud of it.

Meltzoff: How'd that make you feel coming back to where you were an undergraduate and now coming back as sort of an award winner for a Lifetime Achievement?

Lipsitt: It was really wonderful. It was wonderful sitting in the Rockefeller Chapel which is just a beautiful, beautiful place where graduations are held and where other significant events occur and sitting up front there. And as I looked out into the audience and saw Edna there, I just got tears in my eyes.

Meltzoff: Well, that's certainly an experience to go back to where you were an undergraduate all those years ago.

Lipsitt: Yes, and so I've got my 50th, I think I'll go back for my 50th. I think I'll go have dinner at the Quadrangle Club.

Meltzoff: It's hard to see what follows a Lifetime Achievement Award, but maybe they will come up with something.

Lipsitt: But that reminds me that I went also to my very first reunion from Tabor Academy in 1997. That was my 50th reunion, the 50th anniversary of my graduating from high school and that was a very interesting event, too, seeing people I grew up with and went to school with, many for the first time in 50 years.

Meltzoff: I'll bet that was pretty powerful too. So who else was on the faculty at the University of Chicago who might have influenced you to be interested in psychology. You went to the lecture of Hebb, was there anybody else on the faculty.

Lipsitt: Yes, and I went with a friend of mine who was doing some graduate studies in the biological sciences division to a course that was being taught by Eckhart Hess. I honestly don't know how much of an influence that was, but obviously it was an indication that I was beginning to be interested in psychology that I chose to go and sit in on some lectures and they were impressive lectures. But I really had very little to do with the graduate program or with the people in psychology as an undergraduate. I had heard lectures by Bruno Bettelheim and had had a number of sociologists including Ernest Burgess as a teacher and David Reisman, but I think it was not until a little bit later that my interest in psychology was really solidifying and that was due in part to my moving to Boston and living with my brother Don. He was then working on a Master's degree in psychology. I at first sat in on some psychology courses that he was taking, and then I enrolled in some psychology courses at Boston University and decided, yeah this is what I like, and I applied to the University of Mass at Amherst and for a graduate stipend and got one and enrolled there the next year.

Meltzoff: And that was in psychology, I mean at that point you were a psychologist or an aspiring psychologist.

Lipsitt: Yes, I enrolled in the Master's program in Psychology at the University of Mass—there was not Ph.D program at the time—and I became a research assistant to Claude Neet and Robert Feldman who were then doing studies that I found absolutely fascinating on frustration and fixation modeled after the work of N.R.F. Meier at the University of Michigan. I found that again it was process that fascinated me. I found it fascinating that frustrating experiences could produce in rats a state that I was familiar with from my clinical studies. The effect in the rats was a state of catatonia.

Meltzoff: Military didn't come until after the master's. So you had seen that with adults?

Lipsitt: Yes, I had seen catatonics as adults in my practicum at Northampton State Hospital, and here these psychologists, Feldman and Neet, were working on the processes by which organisms can become catatonic.

Meltzoff: So you got involved in an experiment that somehow was going to connect up with your experience with adult humans, but it had to do with rats and a process that's related to something you saw in the Air Force later as well. So this sounds like a good story about how to connect all that, you sure it connects?

Lipsitt: Oh, I'm sure that it does connect. You see what we did was to frustrate rats in using a particular protocol, and the purpose of following this protocol was to get the catatonic response from the rats and then do some interventions with the rats to see what kinds of therapies, as it were, were effective in breaking the fixated responses that these rats developed as a consequence of the frustration that they experienced.

Meltzoff: Sounds like a fundable NIH project.

Lipsitt: In fact the originator of the protocol did get some interesting award for his devising this procedure which had so much potential for helping disturbed people. To talk about the protocol very briefly, we would first present the rats on a Lashley jumping stand with a solvable problem like go to the white and not to the black card and the white and black cards would be alternated from left to

right and the rats would learn that. It's an easy task for rats to learn. They would jump from the jumping stand to the correct window and behind the correct window would be a reinforcer for them. Once they had learned this response we would then change the stimulus arrangements so that they would get reinforced only 50% of the time no matter what they did. Having gotten 100% reinforcement for a period of time before, they would get 50% reinforcement because whenever they jumped to the window that they thought was correct like the black window that had been previously correct, half the time they would get through the window, but the other half of the time they would bump their nose and fall into a net. That was the nature of the Lashley jumping stand. With a number of trials of that sort, the rats began to refuse to jump, and they would roll up like little balls of fur, and they would be as if inactive, unresponsive, even when the task was once again arranged to be solvable. Eventually we would get them to jump anyway, just by flicking their tail so they would jump now to the stimuli now rearranged into a solvable problem, but none the less they would continue their fixation. I guess I didn't mention what the fixation was, it was usually a position fixation. Once the frustration was signaled by position fixation so they would be jumping, left, left, left, left no matter whether the white or the black stimulus was there and getting reinforced only half the time. So then in the third phase we would rearrange the stimuli so it was now a solvable problem. Yet they would maintain their fixation. It was as if reinforcement would no longer work for them. So they would maintain their fixation until we did some other kind of intervention such as putting up a runway so they didn't have to jump any longer, they could walk instead and they would then relearn. Once we changed the response they would then...

Meltzoff: But it was not as quick as the initial learning is that right, I mean it was really like a therapeutic experience for the rats where you were bringing them out, is that right? It was not as fast as the initially learning.

Lipsitt: I don't think it was as fast, no they maintain the fixation for some time afterwards. What I'm remembering right now about it also is that electroshock therapy worked as well as this forced runway treatment. Perhaps the particulars are not as important as this, what is important or what was important to me was my becoming astounded at the extreme effect of experience on these rats. That is to say that they actually developed an organismic diagnosable mental disorder as a consequence of the frustrating experience. I've never done any other research like that since. I mean I turned my attention to children and gentler kinds of situations with children for the most part since then, but that had a profound effect on me in that it just absolutely convinced me of the importance of experience on behavior and life outcomes, and I've had that interest ever since.

Meltzoff: Right, so that emphasizes the power of early learning or learning experiences on what we see later if we come in and don't examine the origins of those states.

Lipsitt: Exactly.

Meltzoff: That was your Master's?

Lipsitt: That was process again. Well, I can't say that that was my Master's thesis, because I did something completely different for my Master's thesis. While I was research assistant to Professors Neet and Feldman, I was doing my Master's thesis with Theodore Vallance, a wonderful man who is now an Emeritus Professor at Penn State University. My Master's thesis was in social psychology having to do with a concept that Floyd Allport had called teleonomic trends, having to do with the consistencies of people's behavior in diverse social situations. So my Master's thesis was actually, and my first publication was, on the role of teleonomic trends in private and group related situations.

Meltzoff: Now I'm jumping, because I don't know this literature very well but the concept of teleonomic trends sounds to me a little bit on the nature side and your experience with rats was on the nurture side, on the power of learning experience. Is that the concept of teleonomic trends the idea that people are going to tend to have a certain behavior pattern but depending on the environment it would be expressed in a different way.

Lipsitt: Yes, it's about the role of context, about the role of context in human behavior.

Meltzoff: But people's tendencies though, doesn't that come prior to the experience that they tend to go in one direction or another and they manifest themselves in different ways depending on the context?

Lipsitt: Yes, that's a very astute observation on your part. It is a trait orientation to expect that there are teleonomic trends as if they are in-built in a person, however they got there, but it also equally honors the role of context in the manifestation of those traits. That too is a theme that you're helping me integrate with my entire life plan!

Meltzoff: It is something very much that you have been studying, and it shows up in different ways. Today we might think of temperament or stability of personality, etc., etc., but coming out in different ways depending on the environment, which again shows the power of context or the environment.

Lipsitt: Yes.

Meltzoff: So your degree or your own investigation was on that topic?

Lipsitt: That's right, that was my Master's degree, and it was after that training which was in social and clinical. I did a clinical practicum at Northampton State Hospital in Northampton, near the town of Amherst and got some really good clinical experience in that way. So when I went into the service shortly thereafter, I had had some clinical experience that the military people that were running the psychiatric clinic at Lackland Air Force Base found relevant and therefore they took me in. Actually it was a personnel officer at Lackland that saw my records and took me over there and said to them that they might like to interview me and see whether they could use my talents at that base. Another thing that happened to me during the very same period of time, I went through basic training at Lackland Air Force base, and the squadron leader one morning marched us all out in front of the barracks and he said, "I want to see how educated you all are." "How many here have graduated from high school," and a certain number of hands went up. Then, how many here have graduated from a two-year college, and a few hands went up. How many from a four-year college, and a couple of hands went up. How many of you have done work beyond college, and only my hand went up. He said I want you to report to building 34 (or whatever it was), and they will tell you what to do next. I went to building 34, and I found that it was a bowling alley, and they were going to use me to keep the arithmetic, to keep the score cards that were accruing that day in some kind of a tournament that was going on at that time. So that's what my education did for me in basic training and until I was assigned to work as a psychologist.

Meltzoff: But it sounds like it also led you to doing these evaluations of the personnel, because you had a Master's degree at the time.

Lipsitt: Yes.

Meltzoff: I mean they then pointed you in that direction as well for your military experience.

Lipsitt: Yes, I did psychometrics and I wrote up reports and I conferred with other clinical personnel, psychiatrists and psychiatric social workers. Lackland Air Force base, the hospital there at that time had the largest psychiatric clinic in the Air Force so that a wide variety of patients came through there. I think that I had experience in testing and talking to a wider variety of patients than most clinicians have in the course of many, many years—even a couple of murderers.

Meltzoff: So it's a compacted intense training, basically during those years.

Lipsitt: It was supervised work by Colonel Jensen in part and in part by Jolly (Louis J.) West who was a very good teacher, even as a young man, which he was then. He is a celebrated psychiatrist today, and he himself, I think, had a Master's degree in psychology. He definitely was an expert in the MMPI. So I had wonderful experience in interpreting MMPI records.

Meltzoff: So from that experience then after the Master's, somehow you ended up at Iowa which as all of us know looking at history of psychology was a really hot bed of psychology and experimental psychology. Many quite influential people came out of Iowa during those years. So what was the experience there like, and why did it lead to such productive people, what were they doing right? What was your experience like?

Lipsitt: There was such an assembly of wonderful talent at that time. In the Institute of Child Development were Boyd McCandless, Charlie Spiker, Alfredo Castaneda, who was known at that time as Alfred Castaneda but later lived better with his Hispanic background than he did during that period of time. We were fully assimilated into the curriculum of Psychology Department courses, and so on, even though we were ultimately to get our degree from the Institute of Child Development in experimental child psychology. We had Kenneth Spence, Judson Brown, Don Lewis, Gustav Bergmann as teachers. It was just a wonderfully generative place. The ideas that were going on around there at the time were very exciting. It was very much a logical positivist environment that I found completely compatible with my training at Chicago. I think that I picked up a lot of that sort of orientation as an undergraduate so I came there believing in the importance of operational definitions and the importance of replication and the importance of setting up hypotheses and testing them empirically and so on.

Meltzoff: But it's still an odd combination that you have and you've carried with you about an interest in clinical psychology and personality, but then also this emphasis on experimental psychology learning and the kind of things that were going on in Iowa. I mean it's a combination one can see working, but it's not an entirely common one. It cuts across experimental psychology and clinical psychology for instance.

Lipsitt: Well, I've never given up that combination of interests. I've maintained an interest in clinical psychology throughout my career. I've been studying infants who were born at risk to find out more about their developmental outcomes. I've edited a book on risk-taking and self-regulatory behavior which is in part clinical and in part research. I see the fields as fused anyway. I don't think that one should be doing clinical interventions that are not adequately tested empirically, and I believe that one should derive hypotheses about why people behave as they do, even in clinical situations, from the experimental literature, from the research literature. I'm a borderline person I think.

Meltzoff: Living between the two disciplines.

Lipsitt: Always have been.

Meltzoff: You're left-handed as well.

Lipsitt: Well, that's right.

Meltzoff: Do things a little bit differently, see things from a different angle. No, but that is an interesting combination of things that does mark your career as somebody that looks at clinical issues and applied issues sometimes, as we will get to later. Things that you do with sudden infant death syndrome and so forth are applied issues. But you like to look at it from, in some sense, from experimental point of view or to get relevant data.

Lipsitt: I think that I've always had this biomedical and psychobiological orientation. My uncle whom I mentioned previously as having been a pediatrician in the hospital where I was born—he was my father's brother—had rather selected me, and I've never really figured out exactly why me, had

selected me it seems to perhaps succeed him as a pediatrician in New Bedford, but I never really took to that. Nor did I enjoy biology courses and biology thinking as much as psychological thinking and so I think I did. I don't know what he would say today if he were alive, but I think he would see the connection between what he was trying to help me be and what I did become. He lived long enough for me to be able to show him several of my publications.

Meltzoff: Well in child development so in some rather large way...

Lipsitt: I have to tell you a story about publications and relatives. Edna had a wonderful uncle named Edward Brill who lived up in Saranac Lake. He had gone there as a young man because of his medical condition. He had TB earlier in life and that's where there were a lot of people who went for the fresh air and for who knows what, and we used to go visit him every year. When the *Experimental Child Psychology* text came out that Hayne Reese and I edited, a beautiful book, I mean beautiful. I'm saying it was an attractive book with a wonderful dust jacket and everything. Edna and I decided to take a copy of it up to her bachelor uncle at his country place at Saranac Lake and, of course, at some point in the evening, our first evening there, I pulled out the book and handed it to him and said, "Ed here's what I've been up to in my career." He took the book in his hands, and he sat down in his accustomed soft leather seat, and he started leafing through it, and he seemed to me for a non-psychologist to be spending an inordinate amount of time with it. Sort of touching it, feeling it, sort of testing the binding and looking at a few pages and reading and then reading some more and so on. Without saying a word after I don't know how long it was, but I think it was a half or three-quarters of an hour he got up from the chair with the book, and he passed it back to me and he said, "You know something, they're never going to make a Hollywood movie out of it."

Meltzoff: And they did not!

Lipsitt: And they didn't, by golly.

Meltzoff: Iowa was this rich environment, there's one gap here I'm not quite sure of, you enrolled in a program that was having to do with child development, explicitly in child development in Iowa.

Lipsitt: Experimental child psychology.

Meltzoff: With Boyd and so forth. So how is it that you came from the master's experience and knew in particular that you wanted to do child development, what was that transition?

Lipsitt: Well, that's what took hold when I was in the service. Rather than dealing with disturbed adults once they had become disturbed, I knew that I wanted to find out more about how they got to be in that condition. I knew that I wanted to study how it was that people got into the psychological troubles that they did.

Meltzoff: So then perhaps that experience with the rats where you knew experience and learning could play a role, influenced you; maybe your uncle labeling you as a pediatrician, taking care of your younger brother.

Lipsitt: Taking care of my younger brother, there was a confluence of a number of circumstances. One part of this picture would involve this. I said previously that I was a rather contemplative kid; I was from time to time a depressed kid and have continued to be so periodically in my life.

Meltzoff: The kid part or the depressed part?

Lipsitt: I hope a little of both. I hope the kid part as well. Part of what I do for fun is to help me not be depressed like when I start fooling around with yo-yo's, things like that. So I've had this state from time to time, and I think that that was part of the picture too. I knew that I wanted to be somewhere

around the field of behavior change, the understanding of behavior, the understanding of mood and specifically I wanted to understand depression better. I felt that I could understand or I was going to understand depression better by listening to, watching kids growing up than by talking to depressed people once they were depressed. Now I don't know whether that's true or not, there is no control subject, you see, so I'm living with the supposition that it worked. That somehow that this amalgamation of causes, this symphony of causes, was relevant in my becoming what I eventually became.

Meltzoff: Right, the Iowa experience had both, we talked about faculty. and I guess we talked about your classmates who many of them went on to become famous people in the child development field themselves and played a big role in SRCD and so forth.

Lipsitt: Oh, yes there was a wonderful group of colleagues. There was David Palermo and Kathryn Norcross, Hayne Reese, Langdon Longstreth, and Shep White, Fran Horowitz. It was a remarkable era at Iowa, I think, and those people that I just mentioned are the people that were involved in child development studies, and there was a whole other group that were simpatico with us in the experimental psychology program under Spence and Farber and Judson Brown and Don Lewis. It was a wonderful place to be. I regard those as golden years in my career and was amply rewarded when one day Boyd McCandless, a wonderful guy and a great teacher, came down the hallway, he was director of the Station, came down the hallway waving a piece of paper, this was in my last year at Iowa. He was saying as he walked down the hallway, "Anybody want to go to Brown?" and I said what did you say, and he said does anybody want to go to Brown? I have a letter here inquiring as to whether we have a graduate coming out this year that might be interested in a position at Brown. So I said "Yes, I think I do, at least I want to learn about it." He handed me this letter from Harold Schlosberg, the chair of the Department of Psychology at Brown University. The letter was addressed not to Boyd McCandless but to Robert Sears, a former director of the Station. Harold Schlosberg didn't know that Sears had gone to Stanford and now the Iowa station was directed by Boyd McCandless. Anyway I started corresponding with Harold Schlosberg and really liked what I heard. There was a national collaborative perinatal project that was about to get started, they were looking for somebody, maybe two people to come and be the psychologist, research psychologist associated with their project and the more and more I corresponded with Harold the more I realized, especially given his background in experimental psychology and in learning processes, the more I realized I needed to ask him a critical question from me, which was Harold, (I didn't call him Harold at that time I called him Dr. Schlosberg or Professor Schlosberg and it took me almost a year before I could call this venerable psychologist by his first name, which he insisted I do). I said could I come there and establish a learning laboratory for infants, and he wrote back and he said he couldn't be more delighted.

Meltzoff: Now there couldn't have been many specifically infant labs at the time. There were some; there was one at Yale and so forth, but there could not have been very many.

Lipsitt: There were just a few -- Bill Kessen and his lab and Frances Graham doing her infancy research at Washington University in St. Louis at the time, were among them.

Meltzoff: You were doing child development though at Iowa, so we have you working with the adults in the military and then going to a wonderful place in child development and so now, your dissertation was not with infants, it was with children. How old were the kids approximately?

Lipsitt: In my dissertation?

Meltzoff: Yes, in your dissertation.

Lipsitt: They were six, seven and eight.

Meltzoff: Six, seven and eight and the general topic of the dissertation?

Lipsitt: The effects of delayed reward in discrimination learning.

Meltzoff: Delayed reward.

Lipsitt: Delayed reward. I've now named my boat "Delayed Reward".

Meltzoff: That's good, this whole interview was delayed! Speaking of that we should have dessert later, that's delayed reward. So you had been doing things with six, seven and eight year olds what gave you the idea?

Lipsitt: I was doing things with kids that age in my assistantship with Charlie Spiker, I was dealing with preschoolers. I was dealing with kids from three on up during my graduate training at Iowa, the whole three-year period leading to my doctorate. But the only contact I had with infants was through my discussions with Orvis Irwin who was a great infant researcher but not by then doing research with infants at all. By then he was testing older kids who had cerebral palsy and trying to find out about their speech development.

Meltzoff: So the fact that Irwin wrote a chapter may have influenced you. I'm going to take a wild leap here, you haven't brought it up, but when was your first born child? Did that enter into these years, was it after, before?

Lipsitt: Isn't that interesting because I was just about to mention the birth of Mark. We arrived in Iowa City in August of 1954, and Mark was born in February 1955 and so through most of my graduate training we had an infant in the house. Those were joyful days with Mark, and I'm sure that my interest in infancy went up appreciably once we had a child of our own. So I don't find it terribly amazing that I eventually went into infant behavior and development and asked Harold Schlosberg whether he could tolerate an infant behavior laboratory around, whereupon he said that he would be delighted if that came about.

Meltzoff: So then you took off to Providence, did you drive, fly, take a train, e-mail yourself! How did you get to Providence?

Lipsitt: Well, we drove to Providence in 1957.

Meltzoff: '57, little child in the car then.

Lipsitt: With a child in the car. I built a special platform, we didn't have a station wagon at the time. I built a special platform for him to rest and to sleep on in the car and, yes, that was the beginning of our life with children. The fact of the matter is, Ann was born one month after we arrived in Providence. She was born in October 1957.

Meltzoff: So here was the young faculty member, with two kids, beginning a job, no house!

Lipsitt: Edna and I did a daring thing, we bought a house on my one-year instructorship. I thought that I would be kept at Brown for longer than one year, of course, but I actually came to Brown with the promise only of one year as an instructor, that was my contract. My salary was \$5,000 with the likelihood that I could get summer salary the following summer, and we bought a house for \$10,800.00 new, a small house out in Seekonk, Mass. just outside of Providence. That's where our two kids grew up for a number of years until we moved into Providence, purchasing a house here in Providence which was followed by the purchase of another house, this one.

Meltzoff: Which is, by the way, a huge lovely house. A nice New England house, it's said that you have had wonderful parties here and so forth, but we will get onto that another place on the tape maybe.

Lipsitt: We have enjoyed being in this house. This is really where our kids, Ann for most of her formative years and Mark for a large number of his formative years, grew up.

Meltzoff: So you came to Brown, you said you wanted to start an infant lab. The chairman said, sure, wonderful. How did you begin the infant lab, was there a grant, what was it that actually got you off the ground, you have a young man showing up who wanted to start an infant lab, how does that work?

Lipsitt: Well, the National Collaborative Perinatal Project had come to Brown in the year previously, directed by Dr. Glidden Brooks, a pediatrician administrator, and he was paying half of my salary and the Department of Psychology the other half, and the idea was that I was to do my research in connection with the National Collaborative Perinatal Project in return for their supporting half of my salary. They supported half of Judy Rosenblith's salary as well. She came in the same year as I did. Incidentally, I met Judy Rosenblith initially in Iowa City just a few months before coming to Brown, and we both knew that we were both going to be at Brown a few months later. That was an SRCO meeting with about 300 people in attendance in a small hotel in Iowa City. That's where SRCO met in 1957, my last year there. So Judy and I got a chance to talk about our visions, our dreams as to what we wanted to do when we got to Brown and Judy eventually created an interesting modification of the Graham Scale of Neonatal Behavior and Development, and I founded the Infant Sensory and Learning Laboratory as we called it. The idea was for us as well to contribute to the National Collaborative Perinatal Project which we both did in various ways, serving as local consultants to the project that Dr. Brooks had gotten off the ground. Eventually I became director of Brown's involvement in that study and director of the Child Study Center which was the organizing agent of the project, when Dr. Brooks left Brown to go to Ohio to found a medical school there. Before he left Brown he actually founded a medical school at Brown University. He does not get much credit for that because he was not called a dean of the medical school at the time, but he actually founded the initial six-year medical training program that evolved later into the Brown Medical School, which is very highly reputable today and has wonderful facilities and is affiliated with most of the local hospitals. So the laboratory that I started was at the Providence Lying In Hospital, now called the Women and Infants Hospital of Rhode Island. I closed that laboratory several years ago when or just before I went to Washington to become director of the APA Science Directorate. I still have some data that I haven't published from the infant laboratory and, who knows, some day when I get time I may still do a couple of articles on infant learning processes out of that laboratory. In any event, those were prolific times in my life and I had some wonderful students, some wonderful assistants. I worked with both undergraduate and graduate students. I was able to fold them into the infant sensory and learning program and you would recognize some of the people who benefited in their graduate training like Carolyn Rovee-Collier from experience in my laboratory. She did her doctoral dissertation with Trygg Engen who was then a colleague of mine in a number of olfactory studies of newborns, so she fit right in there with her wonderful biological background and her interest in psychophysics.

Meltzoff: Right, you showed me a picture of Carolyn on the wall in the seminar room at Brown earlier this morning, Carolyn is a young graduate student getting her Ph.D. Out of the Brown laboratories came those very early studies on infant learning which were so provocative and really helped changed the conception of infancy. It's hard for us to remember back now that it is 1998, but I remember back to graduate school there was literally a controversy about whether infants could learn in the beginning, whether they could be conditioned and so forth. Can you speak a little bit about that, about what it was like when you were setting up a laboratory and the question really was whether infants could learn at all.

Lipsitt: Actually it was not hard to do because it was a sensory and learning laboratory, and I could do some sensory research on taste and smell at the same time that I was exploring learning processes so that even if the learning process part of the research program failed, the research program would not have failed. So we did these learning studies alongside sensory studies and eventually I think other people became convinced as we were in doing those studies that indeed infants—newborns—are

appreciably affected by the environment around them. They respond to a wide variety of stimuli, they come into the world with all their sensory systems functioning, and they can even learn.

Meltzoff: Did that take a while to communicate to the audience, was it snapped up, how did the scientific community respond? Was there any public reaction? Now, of course, infancy is much in the news. What are the years that we are talking about here?

Lipsitt: 1957.

Meltzoff: '57 to '62, '64 what was that era like?

Lipsitt: I was always very careful when talking about learning processes and I wrote a ...

Meltzoff: So Lew before we were so rudely interrupted we were up to 1962, and we were discussing the laboratory you set up to test sensory perception and learning in newborn infants, so can you tell us about some of the work you did on smell to begin with?

Lipsitt: Well, I was fortunate in having a very nice colleague, Trygg Engen, who was authoritative in the field of olfaction and psychophysics, and so we did some studies with babies of olfactory responsivity including habituation, and, I must say, that one of the exciting aspects of this work for me was in our figuring out a way to separate out sensitization or adaptation to the odorants in order to preclude a physiological or neurological assumption about our results of habituation and recovery. One of the arguments that is sometimes made about habituation is that there is a fatigue effect, for example, the assumption being that when you get recovery following habituation, when you get recovery to a second administered odorant, if that happens to be the sensory modality you are working with, that actually it is due to fact that these are different nerve endings that are now being stimulated and it's not real discrimination but rather differential responding of nerve endings. So Trygg and I adopted for our critical or conclusive test of habituation and dishabituation of recovery, we adopted a task in which we simultaneously habituated the child to two odorants mixed, and then used a single one of the pair, actually each of them singly for different subjects for the recovery trials and what we demonstrated is that olfactory recovery does occur to the single odorant of the admixture of two. So we got habituation over a series of say 10 trials and then recovery or dishabituation when we took the single odorant and administered that without any break in the temporal sequencing of the odorants. So that was, I think, with Trygg's help, that was one of the most elegant studies of sensory process in the laboratory that we did, because what we demonstrated is that the newborn is truly discriminating different odors, not just getting fatigued, neurally fatigued. I must say that during that period of time, that Trygg and I were doing that research, we had a visitor to our laboratory, Aidan McFarlane, a pediatrician from Oxford.

Meltzoff: Well, that's interesting because I knew Aidan at Oxford when he got interested in recognizing mothers by odor. He came back to Oxford and did a study about that, I don't know if you know that study, with breast pads.

Lipsitt: Oh, yes I would say that it's an elegant study that he did. I first met him, I believe at Jerry Bruner's house in Oxford. I think that he was there on the occasion of a party that Jerry had, when I was visiting Oxford and talked to Jerry's group there. Aidan subsequently came to my laboratory, at least I think it was just after that, he came to my laboratory in Providence and saw what Trygg and I were doing with odorants. He thereupon got that very good idea that he might use that differential sensitivity of babies to different odors to examine whether they could discriminate their mothers, identify their mother's odor, from that of other kids' mothers and it was a successful study. He got the effect: Differential head orientation by baby to mother A (his own) versus mother B (some other baby's mother).

Meltzoff: Breast pads of mother A versus mother B or mother A versus a clean breast pad.

Lipsitt: That's right. It was very well done and became an important experiment in the field. I would say that that sort of thing gives me almost as much pleasure as doing those things myself, the fact that he was able to get inspired in my laboratory to do that. I think he then credited the study to his visit to my laboratory.

Meltzoff: Well, that's one of the things when we get a little further on we will talk about, when you and your colleagues were making discoveries about early learning and discrimination in the laboratory. At first it wasn't known whether the sensory modalities themselves worked, and you were instrumental in making those discoveries to show that babies had an ability to learn or make sensory discrimination. Others then took it as sort of tools, your having blazed the trails, they used it as tools to investigate perceptual and cognitive development in other ways.

Lipsitt: Yes, well we went with several different modalities. We also tested newborns for their touch sensitivity using the electro-tactual stimulus and using the procedure that Carl Pfaffmann, my colleague when I was just a young guy on the faculty of Brown, he came over to my laboratory and gave me some advice as to how to do the testing of tactual sensitivity. He in fact loaned me some equipment which I could not afford from my research grants to buy at that time. He loaned me an audio-oscillator so we could use a tone in connection with different kinds of other stimulations such as tone paired with eliciting sucking. He also provided me with a great deal of help and concentration in instrumenting the electro-tactual threshold procedure that Nick Levy and I used in our very earlier—I think it was as early as 1958, maybe we published it in '59—study of tactile sensitivity in male and female newborns. What we found in that instance was that indeed babies are increasingly sensitive to touch over the first four days of life.

We had the luxury in those days of testing the same babies over four days, and one doesn't have that luxury nowadays, because normal neonates go home within 24 or 48 hours today, but we were able to test both males and females over four days and we got a steady, in both boys and girls, decline in threshold or increasing sensitivity to touch over those four days and differential sensitivity in males and females. We documented as part of this study that females were more sensitive to touch than were males. It's debatable as to whether one could call that touch sensitivity or pain sensitivity, it was an electro-tactual stimulus. Kids didn't cry to it, it wasn't pain of that sort, but they did withdraw their leg when it got to be a certain intensity and that was their threshold of responding to electro-tactual stimulus.

Meltzoff: Now what was the measure, were you measuring actual foot or leg withdrawal? Was that how you got the dependent measure?

Lipsitt: Foot withdrawal was a decisive response of the foot backward, pulled toward the torso in response to the electro-tactile stimulus.

Meltzoff: Now was that done by videotape coding or was it done by ...

Lipsitt: No, by inter-observer reliability, two people assessing whether a response had occurred.

Meltzoff: Right.

Lipsitt: So we did touch and we did some auditory work in my laboratory. Alice Leventhal, a graduate student of mine did some work. We never did anything terribly definitive in the auditory modality, but we did start to investigate it and eventually I thought that I had enough going with olfaction and taste stimulation. I didn't ignore the other modalities; I didn't do intensive research in that area. We did some work that was not published having to do with the sensitivity of newborns to the voice, having mothers say things like, "Hello Gwendolyn, are we going home today?" and then we would have a male voice imitate her and look at it, and it was just sort of the beginning of a research program that could have developed in that area with Lucile Newman who is an anthropologist and very interested in infant behavior and who has published on learning disabilities and different aspects of child behavior and

development. But she was diverted to other research causes at the time, so we never fully developed that line of research. Interestingly, however, while we were doing that piloting of procedure in our laboratory, a book publisher came and did some videotaping in my laboratory so that on the videotape it appears almost as if we had a line of research in that area, which we never really did. We did not have a definitive program of research in that area.

Meltzoff: Right, now you mentioned that the threshold changed over four days and one interpretation is the developmental change actually within the infants and you told me earlier that there was another potential interpretation.

Lipsitt: Yes, as a matter of fact it was thought by some obstetricians, even as well as others, that maybe our effect was due in part to the anesthesia that had been administered to the mother, some of which got through to the babies. It's well known today that that does happen and so it was argued by some that this was a possibility. Indeed our study was so early in the existence of my laboratory that almost all babies were in those days subject to anaesthesiological effects, so it still remains a possibility that that's a mechanism that contributed to the insensitivity of the babies in the first day of life and the increasing sensitivity over the first four days. But the effect was such an orderly one, accruing with each and every single day that I personally doubt that it was an anaesthesiological effect but it still remains a possibility that it could be. Today I would want to do a study comparing non-anaesthetized babies, babies whose mothers did not get anaesthesia at birth which one could easily do, because now many babies are born without anaesthesia. That does remind me of an interesting fact of life that when we, Edna and I, first moved to Providence we already had had Mark as our first child, but Ann was not yet born. She was born in October 1957, and we came in August 1957. So when we went to an obstetrician here in Providence, Edna said that she would like to have this child with either no anaesthesia or as little as possible. The doctor nearly kicked her out of his office. He said, I'm the doctor. I decide whether you need anaesthesia or not and how much. That was quite an experience. That would not be possible today, I believe. An obstetrician could not easily say that to a woman who was asking to have as little anaesthesia as possible, but in those days the doctor was in control of the anaesthesia. Edna really wanted to have Ann without anaesthesia, which she had done with Mark in Iowa City, where she entered a program with the obstetrician, of hypnosis for delivery of Mark and although we knew that a hypnotized birth would not be possible in Providence, we thought that at least she might have as little anaesthetic as possible. In any event, Ann was born and Ann was fine. It reminds me to say in that connection, there were a lot of things that were different in our transition from Iowa City to Providence. Even though I became associated with Women and Infants Hospital, where Ann was to be born, I was not allowed to be in the delivery room. But two-and-a-half years earlier when Mark was born, I was allowed to be in the delivery room, in Iowa City.

Meltzoff: You were present at the birth.

Lipsitt: I was present at Mark's birth in Iowa City, so that tells you the difference between a community in which there was a well-established medical school with forward thinking about anaesthesia, forward thinking about parental presence in the delivery room, and another community, Providence, which in those days was, I hesitate to use the word backwards, but it had not caught up to some of the more sophisticated university towns where there were medical schools. In any event, that reminds me to mention about the National Collaborative Perinatal Project which I became involved in from my earliest days at Brown and indeed which I came to Brown to be involved in. It reminds me that Yvonne Brackbill was associated with the Collaborative Project from its very early days, and she did a study on anaesthesiological effects upon offspring and documented, I believe, at both four and seven years of age that those children who were especially heavily anaesthetized—their mothers being anaesthetized—those infants who were born to heavily anaesthetized mothers were lower in IQ than the children of mothers who were either lightly or not at all anaesthetized.

Meltzoff: Not by many points.

Lipsitt: Not by many points—three or four or five IQ points. But she had a large enough sample that the effect was significant, and she wanted to publish the result and felt that it was an empirically sound result and that it was publishable, and some publishers thought that it was publishable as well. But my understanding is that NIH forbid her to publish the results, and she became quite angry over that and eventually published the results in the *New York Times* and the *Washington Post*—on the front page yet—by simply calling a press conference or by delivering the results to them. It was a terrible situation, because I don't think that Yvonne did much work with children after that and certainly not with the Collaborative Project data. I think it was simply an interdisciplinary argument going on there with Yvonne thinking that these behavioral data should be recorded, and the obstetricians and gynecologists feeling that it was not necessary to report that. One of the arguments that they made was that in the days in which the data were collected more anesthesia was administered to the mothers than was administered at the time that Yvonne did the data analyses and wanted to publish them. So the data were not published appropriately as far as I know, at least I don't recall ever seeing them anywhere but in the newspaper. It was kind of a moment in the history of child development.

Meltzoff: Now people actually took off from there and then did study the effects of anesthetics during child birth on development. Either Brazelton or I'm not sure who, but the data, some data, or information about that got out I believe and helped change the practice.

Lipsitt: Definitely changed the practice of heavy anesthetization of mothers and that's been a good effect of applied research in the field of child development and a good effect of the National Collaborative Project.

Meltzoff: On some of the basic research that you did presumably had those kinds of effects as well because people used to say that newborn babies felt no pain, for instance, and the research that you were doing on foot withdrawal presumably was an experimental effect that showed that babies could feel things in the hospital as well as smell, taste, and so forth.

Lipsitt: Yes, now there is a widespread understanding among hospital personnel and particularly surgeons that babies do indeed feel pain and then they cry, that probably means that they can experience considerable pain, but surgery not so long ago was often performed on babies without any anesthesia. It was not malevolence toward babies. In those days they believed that the dangers involved in anesthetizing babies exceeded the benefits. So whenever possible to do some surgery without anesthesia on the babies, it was done.

Meltzoff: Another modality then, and one, maybe one that you were most famous for investigating and a technique that you are famous for developing, had to do with sucking and savoring. When you were investigating infant taste actually and whether they preferred sucrose solutions and what effect that had on infant sucking and so forth. Those were a series of very famous and influential findings, can we talk about that?

Lipsitt: Actually that was a very exciting period in my laboratory when we started doing rather sophisticated polygraphic recordings using the computer to record the inter-burst intervals and the inter-suck intervals and the amplitude of sucking and numbers of sucks per burst and that sort of thing. We had six parameters of sucking behavior that we were looking at all at once, along with heart rate and respiration. We discovered that the sweetness of the fluid for which the baby sucked contingently was an exceedingly important parameter, that the babies behavior was actually highly controlled by the hedonic value, as I put it, of the fluid for which the child was sucking. We already knew that babies suck in bursts and pauses. Among other people, Peter Wolf had documented that babies suck in bursts and pauses, and we embellished his finding by then demonstrating that the burst-and-pause pattern changes appreciably depending on the sweetness of the fluid for which the baby is sucking. Then we found, in brief, our first finding was, that babies slow down their sucking behavior within bursts as the fluid becomes sweeter. As we switched the baby from 5% sucrose to 10% to 15% and then moved the baby from 15 to water, we detected that there were very important changes in the baby's

sucking pattern. They slowed their sucking down with increases in the sweetness of the fluid for which they were sucking.

Meltzoff: On the surface, I know the end of this story, but on the surface that sounds anti-Skinnerian.

Lipsitt: Yes, it was a little bit disconcerting to see that these babies had not read Skinner!

Meltzoff: You were having sweeter liquid and slower sucks, as if they weren't working as hard, okay.

Lipsitt: But we then took a closer look and discovered that the burst-pause pattern was altered by the sweetness of the fluid and that even as the baby slowed down its sucking for the sweeter fluids, they ended up by taking fewer and shorter rest periods between bursts, sucking more times per minute. So they ended up being good Skinnerian organisms in the final analysis! That was very pleasing because it was a very, very orderly finding. I believe that in just about all normal full term babies you can get slower sucking to sweeter tastes but shorter bursts and fewer bursts. So it's lawful behavior and gave us a lot of optimism with regard to the control of sucking behavior by circumstances following that sucking behavior. What we discovered is that the baby controls his own savory input, and I use the term savory advisedly, because we believed that the babies when they slow down their sucking slow down to savor the fluid, to taste the fluid more skillfully. They wallow the stuff around in their mouths and nonetheless end up sucking more times per minute. So they are winning two ways. They are slowing down and enjoying the savory quality of the sugar more and ending up sucking more times per minute to enjoy it more.

Meltzoff: Now, I suppose with today's technology or in the future people who are taking up your research might do two things, one is to get some sort of brain measure of pleasure, I suppose so your prediction is that with the sweeter fluid although they are sucking less rapidly there will be greater pleasure—if you could measure that somehow in brain responsivity.

Lipsitt: I'd love to see that done using MRI techniques.

Meltzoff: Yes, so you have a clear prediction about that and the other one is that on this idea of savoring that somehow that they'd be keeping fluid up near the front of their tongue where the taste receptors are that they'd be bathing their tongue in this liquid more when it's sweet. So if there was any way of looking at where the liquid was kept in their mouth with some new technology, you have a clear prediction about that, someone can test someday.

Lipsitt: But they actually move the stuff to the rear of their mouths, I believe, very, very quickly, because that's the way tasting and swallowing work. Food is moved to the rear of the tongue preparatory to swallowing. But it's true that the sweet taste is rather acute at the front of the tongue. They've got lots of taste buds, and now we know that they not only have taste buds in their tongue, including at the back of the tongue, but they have taste buds all around their mouths. Taste buds can even be found in the cheek. The mouth is designed for pleasure!

Meltzoff: You do have a prediction about savoring and pleasure from the sweetness which with modern technology somebody some year here might be able to chase down that hypothesis, which would be interesting I think. Keeping liquid in the mouth really, that's partly what the babies are trying to do. They are trying to taste it more.

Lipsitt: Yes.

Meltzoff: Now one of the things about that whole time, let's see, we started in the late 50s and where are we up to chronologically now, the mid 60s somewhere, I suppose, by the time we've done these lines of research.

Lipsitt: And by this time I was into the Papousek phenomenon of the conditioned head turning response. Hanus Papousek came up with this terrific paradigm for looking at learned behavior in the very young baby, and Einar Siqueland and I adopted and adapted that procedure that Hans skillfully devised, and we came up with some data which indicated that babies did indeed discriminate between combinations of stimuli. We found that babies would turn their heads over trials increasingly toward a touch-feeding combination and not turn their heads or turn their heads less to a touch-no feeding situation, so that we were able to look at ipsilateral head turning in combination with different couplings of stimuli and showed discrimination. We then showed reversal of that discrimination over about an hour-long period in the laboratory in a suitable number of babies to produce a highly reliable effect. Discriminated operant learning and the ...

Meltzoff: What was the discrimination part?

Lipsitt: A tone/touch combination. One tone/touch combination led to reinforcement and another tone/touch combination led to no reinforcement and under those conditions the babies gradually acquired more ipsilateral head turning to the positive tone/touch combination and less to the one that was essentially ...

Meltzoff: They were discriminating the two tones.

Lipsitt: Oh yes, we capitalized upon the baby's ability to discriminate sounds to do this..

Meltzoff: How were you measuring head turning in those days?

Lipsitt: We had the baby attached to a cap device around the baby's head attached to a rod which went to a needle on a protractor.

Meltzoff: I remember seeing that figure in that picture.

Lipsitt: We were able to establish criteria for a head-turning response, and only when the baby turned his head, whatever it was, maybe 15 degrees, we would then pop the bottle into the baby's mouth when the baby turned in the appropriate direction. That technique that we adopted from Hanus Papousek was a powerful one. We got a very, very profound effect and, I must say, that it was due to Hanus' ingenuity that this procedure ever even got devised, because it's a mixture of respondent and operant learning. The respondent is the ipsilateral head turning to a touch at the cheek, and it became an operant when we then, by protocol, by design, reinforced the baby on those positive trials only for turning his head in the ipsilateral direction so that we were operantly reinforcing elicited head turning. The head turning response was initially an elicited response, by a touch to the cheek. It was operant on top of respondent, and I believe that those kinds of combinations may be among the most powerful learning circumstances that one can devise. Combinations of respondent and operant are very prevalent in everyday behavior. ...

Meltzoff: I don't know if it's a combination but another powerful learning procedure certainly was the one that Carolyn Rovee-Collier developed, mobile conjugate reinforcement. Now that's not a combination in the same way, but it certainly turns out to be an extremely powerful way of conditioning a baby.

Lipsitt: Yes.

Meltzoff: Since Carolyn's played such a large role in the field too, is there anything you can tell us about her early career or your observing her early.

Lipsitt: Actually Carolyn did her research in my laboratory but under the direction of Trygg Engen. For her doctorate dissertation, she did an olfactory psychophysics study with newborns but all the while

she was learning about operant reinforcement, and she and I and others learned I guess at about the same time, I from a visit to the laboratory of Ogen Lindsley. Lindsley had devised a conjugate reinforcement, he may have even named it Conjugate Reinforcement Procedure in which the person who was dealing with humans at the hospital up in Waltham, Mass, and he was using conjugate reinforcement, or reinforcement that was commensurate in amount with the strength of the response that was emitted by the patient so that, for example: To use a water spigot analogy, the more you turn the water spigot, the more water you get, or by using the accelerator on the car, the harder you push the accelerator the faster the car goes. Carolyn instrumented that beautifully with babies, with her own child first incidentally, and became convinced herself that it was a powerful technique, because the conjugate reinforcement tracks the strength of the behavior, immediately and commensurate in amount with the strength of the movement. So if you tie the baby to a mobile rather than just having him lie under the mobile, if you tie the baby to the mobile, so to speak with a piece of thread or a ...

Meltzoff: A ribbon, she sometimes used, ...

Lipsitt: ...a ribbon, the more the baby shakes the limb that is attached to the mobile, the more does the mobile move, and so it's a perfect operant conditioning situation. She found that infants very, very quickly learned to move that limb that happens to be operative in moving the mobile. She has capitalized on that technique to create an absolutely beautiful program of research over a long period of time using the technique to study, not just learning in babies but memory processes. She's used it to tap into the strength of memory of the previous experience and the duration of that memory. She's had a wonderful group of students following suit doing studies using the conjugate reinforcement technique and devising variations of it, and I think that it is one of the most powerful research programs on infancy in the country.

Meltzoff: She has a wonderful program going and, she, like so many others, is capitalizing on basic discoveries that were made through the early 60s about the sensory capabilities and the learning capacities of young kids. So I just want to pause here for a second and ask you what it felt like, really, if you can remember back, what it felt like to be making those front-line discoveries: To be transforming our view of an infant where maybe all the sensory modalities didn't work, and there were arguments about whether newborn babies could learn or not learn, whether they felt pain or didn't. That all got transformed and it got transformed somewhere between 1957 and 1965 or '66. It was changed; it was forever changed, so in an eight-year period or so, a large transformation in our history.

Lipsitt: It felt very good, there were a number of us that were publishing data on newborns. Although psychologists had studied infants previously in the history of our field, psychologists did not have as much access to babies as we had in those days. So there was a burgeoning interest and involvement in the behavior of the newborn child and one-month-olds and two-month-olds and so on. It was a good feeling, because we could tell that we were having an effect upon the public understanding of the capabilities of babies, and we were off and running in developing a solid science of early human behavior, and we haven't stopped.

Meltzoff: That's true! Did it have...? Today infant research—what babies know and when do they know it—are common headlines on *New York Times*, *Time Magazine*, *Newsweek* has a lot of that material. So in the early 60s were you making inroads mostly to the scientific community or was the public picking it up as well? Was there the intense interest in the public early on, did it come later, was it simultaneous? How did that work?

Lipsitt: I would say that at first people were so dazzled by it all. Like they would ask, "Do you really believe babies can do that," and of course I did and other people working with babies believed that they could do that, that all of their sensory modalities were operative at birth if they were normal full term babies. We always tended to recognize the babies who were born at-risk were vulnerable to changes in that scenario, and that such babies born prematurely or born with auditory deficits for one reason or another would, of course, be compromised in certain modalities or in certain of their motoric

behavior. But for the most part, we were convinced that normal full-term babies were operating on their environment and learning from it. We were also convinced that high-risk babies that were being born could be studied with respect to those sensory and learning attributes, and we could find out early in life what the extent of compromise was of the psychological attributes of the child in addition to what the compromise had been physiologically or medically. I must say that early in my career at Brown—when Judy Rosenblith and I began our work at Brown—she and I did some collaborative investigation of Judy’s variation on the Graham Scale, the Graham Test of Neonatal Behavior, and it was on the basis of my knowledge of what Judy was doing with that test eventually, even though I had gone my own way to more sensory and learning process experimental and manipulative research rather than psychometrics, I realized that Judy was collecting a lot of data on one of babies’ responses, as it was called, the Defensive Response to mild respiratory occlusion. There is an item on Frances Graham’s scale as well as on the Brazelton Neonatal Scale that had to do with the baby’s defense against respiratory threats. There was a test item in each of those scales in which a gauze pad or a piece of cellophane would be put over the baby’s nostrils and mouth, not necessarily respiratorily occluding the baby, but so to speak, threatening that with something around the nose and mouth. I began to think a lot about crib death and wondered why SIDS, Sudden Infant Death Syndrome, was still an unsolved problem. I suggested to Judy that perhaps she should take a look—if perhaps she could and indeed she could and did—at the relationship between the baby’s strength of response to that defensive item on the one hand and the occurrence of crib death on the other. Because she had a large N in her study that she was using to standardize her scale and to test the National Collaborative Perinatal Project kids and other babies in the hospital, she was able to look at a very large sample and to discover that there was indeed a relationship. Those kids who came in with a very weak respiratory occlusion reflex, as I have come to call it, were those who were most in jeopardy for crib death between 2 and 4 or 2 and 5 months of age. The fact is that crib death has to do with the death of the baby between 2 and 4 or between 2 and 5 months of age. Ninety-five percent of all crib death babies die in that age range, and so there’s got to be something, I thought, about this immune period from birth to 2 months of age, and there’s got to be something important about the period after 5 months of age as well. So I thought about that a lot and eventually I went back to Myrtle McGraw’s superb data on the development of reflexes in the first year of life, and I found that she had a tripartite system of analysis of babies’ behaviors in the first year of life and that she had found in testing babies reflexes, the swimming reflex, the grasp reflex and others, that babies are born mostly as reflexive creatures and that they eventually go on to a different type of responsivity which she called voluntary. Eventually the reflexes go away, as neurologists have known for a long time, and the behaviors evolve into or transform into voluntary or, as Myrtle called them, deliberate behaviors. This looked to me a lot like the learning processes of the newborn. It looked to me as if the reflex might be there in order to enable the supplantation of that reflex eventually with a learned response that essentially mimics or derives from the initial reflex. In looking more closely at Myrtle McGraw’s data, I also noticed that a third kind of response that she documented was called by her disorganized, disorganized behavior, which started at near zero level, like deliberate behavior did at birth, and climbed up to about 3 months of age with each and every reflex that she studied and then peaked at that period at around 3 months of age and then also waned like the reflexes waned in order to be, as I put it, as I supposed about it, supplanted also by the deliberate, well-organized, learned behavior. Myrtle McGraw’s book, the one about the *Neuromuscular Maturation of the Human Infant*, it’s a classic monograph with some wonderful data and some very brilliant thinking in it. What she found was a period at 2, 3 months of age of disorganized behavior. She supposed that this is neurologically important, and she went back to her anatomy and her physiology books and found—and she was working around a lot of people who were sophisticated in anatomy and neurophysiology—and she found that indeed that’s a period of great myelination of brain tissue and of dendrite proliferation. The torch so to speak is passed right around that period of disorganization, as she called it, from subcortical to cortical structures, and she presumed that that was an important, a crucial, period. I don’t know if she ever used the term critical, but she believed that that was a very important period of transition for babies. I simply took that another step further and I had the pleasure and honor of knowing her and talked to her about this toward the end of her life. For example, I asked her what she really meant by disorganized behavior, and she said that’s when the baby doesn’t know whether he’s supposed to be a reflexive creature or a

learned organism. That was her description of the peak of disorganization, the second stage, after the reflexive period and before the deliberate-behavior period.

Meltzoff: The babies are vulnerable during that time; that was your hypothesis.

Lipsitt: The baby is especially vulnerable during that time, because the reflex which has been saving his life for the first two months of his life is going away or has gone away and in some babies may not yet have been supplanted by learned behavior. So the baby gets into a snit, so to speak, gets into a disorganized type of behavior and perhaps, becoming respiratory occluded, cannot fight his way out of it. So my presumption has been that crib death may be the result of an aberration of learning, a learning disability perhaps based upon failure of the reflex to be strong enough in early life to pass the torch to a learned pattern of behavior.

Meltzoff: How's that work, does that mean that the baby doesn't have enough experience in the reflexive response to learn it, to bring it under deliberate control, is that the idea?

Lipsitt: That would be part of my supposition that some babies—and it's a small number—some babies are born with such weak reflexes or such reflexes that they don't get triggered off as often as they should, so that they self-administer less learned behavior that is eventually so critical in babies to saving their lives when they are threatened with respiratory occlusion...

The American Pediatrics Society has come up with what they call a "Back to Sleep Movement" in which they urge parents and nurses in nurseries and so on to put their children to sleep on their backs or on their sides rather than on their tummies. If the baby is put to sleep on his tummy he is much more likely to roll his head over on top of his nose and mouth and to, as the pediatricians call it, rebreathe the air that is expelled. That's sort of another way of saying that the baby is more likely to smother if placed upon his or her abdomen. All around the world now there is this movement to try to keep babies off their tummies. This hypothesis about this being the cause and preventative of crib death is not universally accepted by pediatricians, but there is enough of a movement favoring that as a hypothesis about crib death that it has really taken hold, and world-wide caretakers of babies are now being cautioned about the possibility that babies would be in jeopardy if they are put to sleep on their tummies. Well, undoubtedly, most babies are not in jeopardy due to crib death, because they have the capability of lifting their faces right up off the mattress or responding vigorously to anything that threatens occlusion, but the universal advice given is for all babies to be placed on their back on the assumption that we don't know which ones are in the greatest jeopardy of crib death. Since this admonition has been put out by the pediatricians and by crib death societies, there has indeed been a decline in crib death, not just in the United States but in a number of places. So there is some presumptive evidence that the position of sleeping and the hazard of smothering is a factor in this.

Meltzoff: There must be data about when crib death is most likely. I mean your hypothesis of it being during this transition period predicts a particular time, and people have done longitudinal studies measuring the reflexes of behavior and seeing that if that maps on to crib death, but there is a synchrony in time when you take root data, is that right?

Lipsitt: That's right, there is actuarial data indicating that the time period between 2 and 4 or between 2 and 5 months is the peak period of crib death. Ninety-five percent of all crib deaths occur between 2 and 5 months of age. The fact that the baby is essentially free of danger for the first two months of life except for the first few days of life which are a period of jeopardy for the human newborn anyway, the fact that they are essentially free of jeopardy in the first couple of months of life supports the hypothesis that this transitional period is deeply implicated in the crib death incidence. So anyway that's been exciting, I have some regrets in this area that I was not able to do more research on crib death myself.

Meltzoff: I gather... Was your hypothesis accepted with open arms?

Lipsitt: Hardly.

Meltzoff: Hardly!

Lipsitt: No I recall I was speaking at NIH very early in the game, the early 1970s, to the neurology group there, and I remember I suggested that I thought that probably we needed to investigate learning problems of babies in order to truly understand the hazard of crib death and the natural history of crib death, and I said that there is the possibility that there is a kind of learning disability going on here whereby the baby is not acquiring, because perhaps of some initial neuromotor inadequacy, the appropriate behavior to save his or her own life by picking the head up off the mattress, and I remember that a pediatrician raised his hand, or maybe he didn't even raise his hand, and he said, "That's ridiculous, babies don't die of behavior," and then he added, I think, "Children don't die of their own behavior" and I said, "My goodness, in my view *most* children who die, die of behavior." Either die due to accidents or due to other kinds of behavioral misadventures, and as you go up the age ladder, I said, you can see that even according to the statistics from the Centers for Disease Control and Prevention, we can see that all the way up to something like age 32. The biggest killers of humans are essentially behavioral: Accidents, suicide, homicide, the consequences of excessive drinking, drug deaths and more; violent behavior, aggressive behavior, and so on. So it's not so wild a dream that even babies can die of some kind of inadequacy of behavior or behavioral aberration or a learning deficiency. I don't find the supposition at all unwarranted, but I think that the public and many professionals dealing with children have difficulty accepting the fact that babies can die of behavior or of learning deficiencies.

That worked against my being able to get a grant funded early in my career when I first had this hypothesis about crib death as a behavioral phenomenon. I submitted an application, and it was shot down by a review committee at NIH for an odd reason. The reason that they gave I'm not sure was the reason they shot it down, but what they said was that it would be unethical to do the study I wanted to do. I wanted to give training to kids in jeopardy. I figured that I would zero in on a population of kids who could be predicted, more than the generality, to be in jeopardy because we know that there are, there are certain factors, even low socioeconomic level is a factor in crib death. Being black is a factor in crib death, being born to a smoking mother, although it's only three out of a thousand babies who died of crib death at the time. Fortunately, one could zero in on a population at risk and perhaps come to three out of a hundred, or something like that, predicted to die of crib death. In principle one could do that by using the Apgar Scores, and then I had proposed to do that and then to take half of them and administer a learning condition which would in effect train the babies in jeopardy, train half of the babies who are in jeopardy to lift their heads up off the mattress and turn their heads into a safe position, and not train the other half for perhaps two to three months. That was to say, to train all one hundred percent of the kids but to delay the training in some of the kids. My hypothesis would be that the kids who were trained between zero and two or two and a half months acquired the appropriate behaviors in order to save their lives during that critical period between two and four months of age. But I wasn't permitted to do that study, because I didn't get it funded. And the reason that they said that they were not funding it was that it would be unethical to do that study. They said "after all, suppose your procedures work. Imagine what the parents who are in the control group of the study, with delayed training, are going to say."

Meltzoff: So they had everybody in the country being in the control group...I mean that's what they are doing by not allowing you to conduct it; everybody's in it.

Lipsitt: That means that everybody is in jeopardy. If you can't explore the conditions that might save some children in order to save a large number of children, then we are going to continue to have our children in jeopardy of crib death. But you know, drug studies are always done by splitting the population, half get the drug and half don't or get placebo, but the reviewing committee couldn't equate this situation with a drug study because it was behavior. I think that there was much more at that time, a kind of built in prejudice of reviewers against behavioral studies with high-risk kids than there is now. I think the situation is much more agreeable nowadays. Crib death was not the only area

in which I explored the importance of behavior and perinatal and other early risk factors. My friend, Lee Salk, called me up one day rather early in my career, I think it was in the mid-60s—I don't remember exactly when it was—and he said you know you have this population of kids in the Brown cohort of the National Collaborative Perinatal Project available to you, and he said I want to tell you about something interesting that I'm finding in my clinical practice. Lee Salk was a very good thinker about human development and infant behavior, and he was a very good clinician as well. He said, in my clinical practice I find that children, adolescents who commit suicide and other adolescents who survive a suicide attempt were frequently victims of some kind of perinatal risk factor, like being born with the cord around the neck or being born prematurely, or of a mother who smoked incessantly and, therefore, the child being born small for dates. And he said because perinatal risk so frequently pops up in the anecdotal data that I'm gathering, I'm wondering whether you'd be willing to collaborate with me in a study in Providence using a collaborative project sample. To make a long story short, I got in touch with the medical examiner to engage his, the medical examiner of Rhode Island, to engage his collaboration in this study. When we started looking, comparing his records of kids who committed suicide with our collaborative study, it turned out that only one, fortunately, of our collaborative sample to that date had committed suicide, so we really didn't have a study. We had no way to do the study that we wanted to do, so Lee, being bright and clever, got back to me and he said, well I wonder if your medical examiner would be willing to go into his files and find all of the kids in the state of Rhode Island over an eight- or ten-year period who committed suicide—adolescents up to age 20 who committed suicide—and enable us to then go back into the records and see what their perinatal histories might have been. Again to make a long story short, the medical examiner liked the whole idea and did reveal to us who the cases were. Using a blind observer going into the hospital records, we eventually did an analysis, a survey as it were, using some 41 or 44 high-risk factors at birth. We did a survey of the kids who had committed suicide along with a survey of the perinatal histories of two control kids for every target kid. For the controls we selected the birth just before the suicide and the birth just after of the same sex and race. So we controlled for season of birth and we controlled for place of birth, we controlled for a number of things, race and sex and so on, and did a comparison of perinatal historical factors in the two controls for each suicide kid, and we found that there were many more perinatal risk factors in the histories of the suicide kids. We eventually published this article in the British journal, *Lancet*, and there then ensued a good deal of publicity where it was picked up by the press.

Meltzoff: Is that an accepted finding now, if there is a correlation between these early factors?

Lipsitt: Very interestingly, unbeknownst to us, there were studies going on of this sort in Sweden at the Karolinska Institute. A Swedish physician named Jacobson, Bertil Jacobson, was doing similar studies with some of his colleagues, one of whom was Karen Nyberg who eventually came here to study and collaborate with me on some research with infants and research on drugs that mothers had and that sort of thing. She, herself, as well as Jacobson, is an authority on perinatal risk factors. She is a nurse midwife who then got a doctorate degree from Karolinska Institute under Bertil Jacobson's tutelage. That team of researchers at the Karolinska Institute has found some pretty unusual, almost scary phenomena, the sort of thing that goes like this. Of the adolescents who commit suicide, the risk factor that was most prominent in their background would coordinate with the manner in which they chose to commit suicide. So if respiratory occlusion was a factor in the perinatal history of the person, he was more likely to hang himself than to take drugs. Whereas if the perinatal hazard had to do with drug ingestion on the part of the mother, the offspring when he or she comes to commit suicide some 16, 18, 19 years later is more likely to be a victim of suicide by drug ingestion. That sort of thing. There isn't nearly enough research available to us today even though there are smatterings of research findings, there isn't nearly enough research today on the prolonged possible effects of perinatal risks of all sorts.

Meltzoff: You'd think with NIH that would be something NIH would be happy to investigate, because it's in some sense, it's retrospective study. I mean the people who tragically commit suicide later in life, it's a matter of getting access to birth records. That would be how you'd do it...

Lipsitt: I've been fortunate in another way in that I am now collaborating with two other scholars using a longitudinal design to look at the follow-up data from kids who were in the National Collaborative Perinatal Project. Those Providence kids are now in their mid-30s, something like 32 years to 37 years of age, and we've got the complete records from birth, indeed from before birth, because their mothers were recruited into the study before the infants were born. And so we've got these extensive data, birth records and neonatal records, 4-month, 8-month, 12-month neurological and pediatric and psychological examinations. We've got psychological data and speech and hearing data at 3 and 4 years of age and all the way up to age 7. So we are in a position in this longitudinal design now to do a thorough neuropsychological follow-up as well as what we call an hour of reminiscence on the part of the subjects, just having them sit and tell us what their lives have been like, what has been critical to them, how they get along with people and so on and we're collecting this kind of data as well as more well-controlled psychometric neuropsychological data. My colleagues in this are Paul Satz who is an eminent neuropsychologist at UCLA in the Neuropsychiatric Institute there and Steve Buka, my honors undergraduate student of 20 years ago who went on to get a doctorate at The Harvard School of Public Health and who is today a psychiatric epidemiologist. So we are using the whole thing, you know DSM for a diagnostic interview and all that sort of thing.

Meltzoff: And that's ongoing now...

Lipsitt: It's ongoing now.

Meltzoff: Is it funded by...

Lipsitt: It's funded by the National Institutes of Health.

Meltzoff: And it's a follow-up for this perinatal...

Lipsitt: For this collaborative perinatal project sample (talking over each other)...

Meltzoff: ...way when you came to Brown to begin with, that was the project you were originally connected with.

Lipsitt: There are certain continuities in my life. I've stayed at the same university for 42 years, and then, on the eve of my retirement a couple of years ago, we got the largest grant that I have ever gotten in my life, but, of course, in collaboration with a lot of other people like Steve Buka and Paul Satz, a one-and-a-half million dollar grant to follow up these children who are products, as it were, of the National Collaborative Perinatal Project. So we are looking at learning disabilities and all kinds of adverse developmental outcomes as well as what the factors seem to be in early history that makes some vulnerable kids resilient and cause other kids to go downhill. We're especially interested, not just in the developmental continuities—the kids that look good early in life and still look good and vice versa—but we are interested especially in those kids that we call developmental surprises that you know look good early in life, have no obvious or detected jeopardy factors early in life but somehow have become troubled later in life. We want to find out what some of the contextual growing up factors are that move some kids as they develop in that direction and by the same token discover those factors that make some kids resilient.

Meltzoff: So you are doing that research now, the research period has hardly ended or slowing down, you are in the middle of your biggest grant.

Lipsitt: I've been as busy in retirement as I ever was before I retired, the only thing that seems to be different in my life is that I don't do stand-up teaching. I still lecture now and then at peoples' request, but I don't do course teaching, and I am very involved in this long-term study and I seem to be doing more traveling than I ever did before. That's in part due to the fact that I don't have teaching obligations, so I can accept more invitations to go lecture or to go to scientific meetings or symposia.

Meltzoff: That sounds exciting. That sounds like it brings together those rats from the Lashley jumping stand in a way where you saw a process by which they came into an atypical statement go good and now you've watched these children, you have data about them early and you are going to see if you can understand the course of life. It's a more complicated process than your giving the rats particular experimental treatments, I mean a whole life has intervened in between the two to turn these adults if they are at risk, or their developmental surprises may have had some experiences like those rats or something where life did not treat them well or it was hard to make sense of life or something.

Lipsitt: Yeah, I think there is a lot there that we are just beginning to understand, and it's in part due to the fusion I think of both clinical and research talents in the field of psychology.

Meltzoff: Well, which was your original interest, all this interest in clinical psychology and social psychology with a leaning towards empiricism and doing experiments and sort of separate out the factors and causes and so forth? So it's interesting, here you finally had a chance to bring it together right after you've retired.

Lipsitt: Yes, I hope so. I hope I can continue to bring things together. This study is now in its third year of four, we now realize that we are going to have to go on for longer than four years in order to truly examine all the relationships that we want to. In fact, it's almost an endless study because our subjects are still growing, they are still moving on ...

Meltzoff: And they are all younger than you are.

Lipsitt: And they are all younger than me so it's a good thing that I've got colleagues who are younger than me like Steve Buka and Paul Satz.

Meltzoff: Right. (tape interrupted)

Okay, Lew well I wanted to just make a shift here from your own research program to different ways that you have influenced the field and the two ways that I can think of has to do with I.B. and D. (*Infant Behavior and Development*) a journal that you founded and the advances in infancy research series, book series and there have been several others. But let's talk about IBAD, that's a journal for infancy that started, when did it start?

Lipsitt: It's now in its twenty-first year.

Meltzoff: Twenty-first year. Though I'm an infancy researcher and I teach a course at the University of Washington called Infant Behavior and Development.

Lipsitt: That's catchy!

Meltzoff: That's a catchy title! Comes right out of your journal but I think in addition to getting courses around the country in infancy which there are more of now than there were twenty-one years ago, IBAD has become a vehicle for some of the best infancy research really, a magnet for that. So tell us a little bit about either the founding of it or your vision about why there was a need and has it fulfilled the need, that sort of thing. It's an important journal at this moment in time.

Lipsitt: I was at one time on the Editorial Board of *Child Development*, as an associate editor very early in my career, still when Bill Martin was the editor of *Child Development*, and I realized as I grew up as a young professor that we were, first, getting more and more studies of infancy coming in, but secondly I realized that there was no real focal outlet for infancy research. So about twenty-three or twenty-four years ago I went to Larry Erlbaum who had become an independent publisher with his own

company, Lawrence Erlbaum and Associates, and I said you know now would be a great time to found a new journal. There are lots of people interested in infancy now and we need a repository of really good psychological research on infant behavior and development, and he said that, yeah he would find that attractive to publish such a journal but he had an agreement with his former boss, Walter Johnson, that he would not publish journals, at least yet in his career, I think that was his reason. In any event he said, why don't you go to Walter Johnson, Larry's former boss and formerly the owner of Academic Press and ask Walter if he would like to publish an infant behavior and development journal, IBAD as we called it. And so I did and Johnson liked the idea very much and took me up on it, and I became the founder and first editor of that journal. Actually Larry is part owner of Ablex and thus was part owner of the journal as well, and he continued to guide my involvement in the publication of that journal for the first two years. Eventually he sold out his shares of Ablex to Walter Johnson, and Johnson became the sole owner of the company. It continued to be published, it just went on and on and when I gave up the editorship...Carolyn and I co-edited it for a time and then after about five years I asked Carolyn if she would like to edit it and I was delighted when she said yes because I was doing too much work and I needed to lighten up a little bit and I needed to keep my own research program going because that's where my greatest investment was and moreover I was involved in some other writing projects as well. So Carolyn thankfully took over as editor and has edited the journal for about fifteen years now. And is just recently resigning, retiring from the editorship and it will go through another transition at this time, indeed it is going through an important transition right now, because the society which eventually came to sponsor that journal, the International Society for Infant Studies, is now going to go its own way and publish its own journal. The Infant Behavior and Development journal was always owned by the publisher and ISIS, the International Society is going to own its own journal and to be the, one might say, the co-owner of it with the publisher, and so there will be a much closer relationship between the publisher and ISIS from here on. The new journal will be called *Infancy* and *IBAD*, as far as I know, will continue to be published. It's a bit of an iffy thing right now following the ICIS meetings.

Meltzoff: We just came a few days ago from that where it was all announced, I'm sure you knew about it earlier but the formation of this new journal called *Infancy* was just recently announced. So although *IBAD* was separate from the Society, ISIS, from a legal point of view, from a practical point of view it was the journal of the Society, we got the abstracts of the meetings through that journal; it was fairly closely tied.

Lipsitt: It was conjoined with the Society. Mr. Johnson actually helped ISIS, which was a very young society, become incorporated, and he supported the society in various ways with clerical help and subscription matters and that sort of thing. It was a mutually beneficial affiliation between the two, I would say some people may disagree, in which the journal sort of belonged to the Society although the Society did not own it, the publisher continued to own it, but the Society benefited from being affiliated with it, drawing in many more members and that sort of thing. The publisher continued to subsidize membership almost until right down until the present time.

Meltzoff: So you edited that for about five or six years, but you served as editor for many other things. In fact as I looked over your vita, which I have in front of me which is in fact forty some pages—a huge document—forty-three pages, I can't tell exactly but it looks like you've been an editor of things for quite a long time, perhaps since you've been an Eagle Scout, I don't know. But you've been an editor of many, many things. You must enjoy editing.

Lipsitt: That's very perceptive of you, I in fact was called Scribe when I was in Scouts as a kid. I've always enjoyed writing, I've always enjoyed editing, and I guess I am a writer as well as an experimental child psychologist and infant developmentalist. So I have always had some writing projects going or editing projects going. I founded *Advances in Infancy Research* initially and then became co-editor with Carolyn Rovee-Collier, and before that I had founded *Advances in Child Development and Behavior* ...

Meltzoff: With your graduate school colleagues.

Lipsitt: With my graduate school mentor, Charlie Spiker. That was a joint endeavor as well and that was published with Academic Press and still is and is currently edited by a colleague of mine in graduate school, Hayne Reese, with whom I have also co-edited a textbook in experimental child psychology, the one I mentioned previously that they will never make a Hollywood movie of. So I'm proud to say I've had long lasting good intense friendships with people, who are wonderful colleagues and have contributed to my career and my life in many, many ways.

Meltzoff: It's a stable life, you've been in the house for how long?

Lipsitt: In this house?

Meltzoff: Yeah, approximately.

Lipsitt: For approximately 33 years.

Meltzoff: And at Brown for approximately?

Lipsitt: At Brown for 42 years.

Meltzoff: And married to Edna for approximately?

Lipsitt: Married to Edna for 45 proud years.

Meltzoff: I mean and friends with Charlie Spiker for a long time and Hayne Reese for a long time, so there's quite a bit of stability in your life. Not a gadfly floating around. Actually the same field, child development for quite a long time, and so forth. That stability or the recognition of your being in this field has certainly come to you in recent years. We should just put onto the tape, I went to an awards ceremony at AAAS, the American Association for the Advancement of Science, for a 1994 award called the Mentor Award, which I thought was a very nice award to get. Can you tell us a little bit about it and its purpose. It wasn't just for your own research, it was for other things.

Lipsitt: Well, I was delighted to get the award especially because it was for, they said, my special attention to minorities and women scientists who were trying to get ahead in the field of behavior science and who might have had some handicaps as a consequence of either being a woman or being a minority person. Things are loosening up considerably now, but there was a time in our field when both women and minorities needed special help in getting ahead academically, and I'm pleased to say that I directed the doctoral dissertation of the first black graduate of the Department of Psychology at Brown, a woman. Unfortunately she died young; she died about four or five years after getting her doctoral degree and while she was teaching at, well she was going to teach at Hampton Institute, now called I think Hampton College. Her name was Mae Williamson Simmons, and she did some of the early infancy work with me, especially that which was about discrimination learning in older kids, not newborns, but kids of preschool age, and helped develop with me some operant discrimination tasks that we used. I still have some of that apparatus, those operant discrimination tasks including an oddity discrimination task, which engaged the interest of Dr. Lattell at West Virginia University. He has an Apparatus Museum that not very many people know about, largely of operant apparatus but as I understand it not exclusively operant apparatus. He has expressed an interest in it, and so he and I are right now trying to work out a way for me to deliver that and several different pieces of apparatus to him. Another writing or editing task is the *Brown University Child and Adolescent Behavior Letter* which I have been working on now for, oh I think...

Meltzoff: Here's a copy, so it's still being published...

Lipsitt: It's still being published here in Providence by a prominent graduate of Brown University who founded this newsletter and a number of other newsletters and other written works to the benefit of Brown University and with the help of Brown University personnel. This Brown University newsletter is currently into volume 14, so I have been editor of it for 14 years now.

Meltzoff: You started that.

Lipsitt: I started that and it comes out monthly and every now and then I publish a Commentary. It so happens that this month, the one that just came out today, is a commentary entitled "Developmental and Destiny Derailed" and it's all about Princess Di and about the interruption of her development, interruption of her career, the tragic interruption of the things that she was trying to do with her life. So I've been busy.

Meltzoff: You not only write in that newsletter, when I look through your vita I notice that you've written editorials and things to newspapers quite often, not just once or twice but you express your opinion in written venues quite a lot.

Lipsitt: Yeah, I've written quite a few op-ed pieces.

Meltzoff: Yeah. Now I don't know if that goes back to early experiences and learning but wasn't it the case that your father was interested—he was an attorney—he was interested in the newspaper or did...

Lipsitt: Absolutely, my father was a newspaper writer in New Bedford, the former New Bedford *Times*, before he went to law school. It was during that period when he was a writer for the newspaper, a reporter for the newspaper, when he decided to become a lawyer and went off to law school, then came back to New Bedford as a lawyer. Also my father was very interested in having his kids involved in writing, and so when I was about 9 or 10 and Don was 11 perhaps and Paul was about 12, he got the three of us to publish a newspaper, a mimeographed newspaper produced right in our own home in Marion.

Meltzoff: Oh, so you were a publisher editor so early in...

Lipsitt: Would you believe it, I was dog editor! And what we did at that time was to produce a mimeographed newspaper of about six or eight pages and my father typed the whole thing up—he was a two-fingered typist like I am. I learned to type from him so early in my life that I don't remember having learned to type, so it must have been quite early, and he typed on a little tiny typewriter in his study, and we all cranked the mimeo machine to get the newspaper out and we'd go and sell it for two cents. My father used to tell people that his three boys financed their trip to the New York World's Fair in 1939-40 by selling this newspaper, producing and selling this newspaper. Of course, it wasn't so. It cost a lot more than we earned from the paper, but it made good copy!

Meltzoff: Probably like Walter Johnson who helped supplement the fees.

Lipsitt: A fantastic exaggeration! Yes, he supplemented it considerably. But indeed we did go in 1939, maybe 1940, the three of us with my father to the New York World's Fair. My mother did not go, because she was by then pregnant with Peter.

Meltzoff: Still not telling anybody her age.

Lipsitt: Still not telling anybody her age and still getting pregnant.

Meltzoff: In any case the editing and publishing has been around in your life and when you look at sort of founding these different journals and book series, it's just something that you are good at and you must enjoy doing because there have been many of them.

Lipsitt: You know there's a little bit that I've left out inadvertently when we were talking about the University of Chicago and my time there. I joined the editorial staff of the Chicago *Maroon* which was the University of Chicago newspaper, and did a few stories.

Meltzoff: As an undergraduate.

Lipsitt: As an undergraduate, and indeed when I was toying around with what I was going to do with my life, and I was wondering and wondering about the possibility of journalism. That possibility was definitely rather strong. I did consider trying to get a job at a newspaper and so to speak working my way up as a writer.

Meltzoff: Well, you've worked your way up pretty far. So the last thing I wanted to talk about was the influence you had on the field that I'm most familiar with is through these developmental journals and books that we just discussed, but slightly earlier in your career and indeed now you were on NIH panels of various sorts. One was some time ago, and you are on a new very current panel about day care, so to just sort of finish this session we are talking about your research contributions can you tell us about those too, NIH connections.

Lipsitt: Well, I was on the experimental psychology study section which was run by the executive secretary at that time, Keith Murray, whom I believe is still at NIH in that capacity. That was a wonderful experience, because I got to associate with and hear research critiques by some of the best experts in the field of experimental psychology. It was a wonderful experience. I think maybe I was on the study section for six years.

Meltzoff: And they did not have separate ones for child, now they have something called HUD I and HUD II. HUD I is for infancy and child research.

Lipsitt: To the best of my recollection they had nothing like that. They might have had a kind of child development study section that worked mostly with NICHD, but I don't think so. They put a lot of the child psychology experimental grants in this study section of experimental psychology.

Meltzoff: And now you are connected to this very large well-known day care project, on the steering committee.

Lipsitt: I'm on the steering committee, I'm chair of the steering committee of the National Early Child Care Project. In this capacity I succeeded Henry Ricciuti and Bettye Caldwell, they were the first two chairs, Henry first and then Bettye, so it was a tough act to follow. But that has been a very good experience. The principal investigators, ten or eleven of them, all meet every two or three months and talk over the progress of the study and lay plans for the future of the study and analyze data and talk about the ways in which we should be talking about the results of the study. A very sharp group of people, including Cathryn Booth and Kathleen McCartney and Jay Belsky and Bob Bradley and Bettye Caldwell, and it's a fun group to be associated with because it is such a bright group of people all putting their shoulders to the wheel to better understand what day care is all about in this country and what the effects of different kinds of day care are on different kinds of children.

Meltzoff: I'm very familiar with the project because it's in the news quite a bit. Is there a final report envisioned? I don't know what its course will be, how long they plan to follow-up the children and that sort of thing.

Lipsitt: Well, we are coming around to the end of what is called Phase II and the group is now preparing to apply for Phase III. It's hoped that it will go on for another four or five years and follow the children up to, I don't know, seven or eight years of age, with some excellent techniques for assessment, some of which have been developed right in the context of this study and others which have been appropriated from other research work in the field.

Meltzoff: Do you think it will have an impact on policy.

Lipsitt: I don't see how it can avoid having an impact. It's been a very expensive study, a study that's occupied hours and hours of people's time researching and writing. It cannot help but come up with some very meaningful data about the effects of day care, child care generally on kids, and it remains to be seen just what the most important findings will be that will come from the study. It's really hard to predict from this point, but it looks from the beginning as though there will be some very important findings from the study having to do with the effects of quality of day care, defining the quality in terms of numbers of care takers, kinds of homes or institutions in which the day care is carried out, and relating all of this to attributes of the children and attributes of the home and of parents.

Meltzoff: Long-term developmental study and again roll over early experience in sort of long-term outcomes the processes by which it happens, something you've been studying for quite some time as we see here.

Lipsitt: I was on the advisory group for that day care study initially, and they appropriated me from the advisory committee to come aboard and be chair of the steering committee.

Meltzoff: I see.

Lipsitt: It's been a challenge.

Meltzoff: Yeah. Let's talk a little bit about some of the institutions that you've worked at and anything you'd like to say about them and their role in child development history.

Lipsitt: Well, I came to Brown directly out of my Ph.D in 1957 and established my infant laboratory at Women and Infants Hospital shortly thereafter, and I immediately composed an application to NIH to start an experimental child psychology training program at Brown. And it went through the process, I think it must have been 1958; we were turned down. We were not granted the money to start the experimental child program. Harold Schlosberg came to me after getting the letter of rejection and he said, "Here's what they said about the application. Lipsitt is too young to do it." So Harold and I conspired to rewrite the application a little bit and to make him director of the experimental child program instead of Lipsitt, whereupon we went through the process and the money was granted and Harold Schlosberg now was nominally the Director of the Experimental Child Psychology Program at Brown, whereupon he handed me all of the papers and said, "Here, you're the director." And so I then became Director of the Experimental Child Psychology Program at Brown, and we started a twenty-year long supported program of training of child psychologists in this department. The department had had a nice history in the field of development, Walter S. Hunter having done research on delayed reaction and other matters like double alternation, and Leonard Carmichael having been there, and there were the studies of J. McV. Hunt from Brown University with Harold Schlosberg and Richard Solomon as co-authors having to do with the long-term effects of early frustration experience on hoarding behavior, of animals in that instance. And so it was a nice legacy that I moved into here at Brown, and we were a happy twenty-year institution of training in child psychology.

Meltzoff: That was a training grant. A training grant of post-docs?

Lipsitt: I think it was all pre-docs and I think it was renewed three times for a total of twenty years after which or by which time the funding had diminished to such a low level, which is what was going on in government-supported training programs at that time, that we simply decided that we would not apply again for another five-year term. But we had trained a number of child developmentalists, child psychologists, and supported people like Barbara Burns and Carolyn Rovee-Collie, and Herb Kaye, and a number of others. I wish I had the list of all the students in front of me. I don't, but I have it in my office, and it will become part of the archival record of my work at Brown University. We had several outstanding faculty members whose support came partly from this program, like Rochel Gelman and

Eleanor Rosch, and they moved on to outstanding careers from there. And in the course of all of this I became in 1966 the director of the Child Study Center. It had previously been called the Institute for Life Sciences, and it was the legacy of Dr. Glidden Brooks, whom I mentioned before, who came to Brown and in fact founded the Brown University medical program while he was running the collaborative perinatal project. I took over the collaborative project operation and named the parent institution or renamed it from the Institute of Life Sciences, renamed it to be the Child Study Center, and I directed that center for the next twenty-five years. After a while the Child Study Center directorship fell to a number of people including Arnie Sameroff, who was here for a while and moved to the University of Michigan, and eventually Bill Damon who was also here for a while and has now moved as of this year to Stanford University. Bill Damon in his time renamed the institute to be the Center for the Study of Human Development and, who knows what it will evolve into from here on, but it was a very productive center. It was an interdisciplinary center that helped sponsor research of other faculty members and rather enjoyed a good reputation on campus as an organizing entity for diverse research programs of child developmentalists and others interested in children.

Meltzoff: Was it only a research center? Did it have any teaching obligations or was all the undergraduate teaching done somewhere else and not connected to this?

Lipsitt: All of the teaching was done elsewhere, such as in education and psychology and in pediatrics. It was not a teaching center, it was a research center and many of the grants, mine and some of those of others, went through the Child Study Center and were administered by the Center.

Meltzoff: You went to the Center for Advanced Study at Stanford, that's one prestigious institution you were associated with. I don't remember the year that you did that, but that was a sabbatical year in Stanford, yeah?

Lipsitt: Yes, that was a joyous year, 1979-1980, when Edna and I moved to Palo Alto, and I was at the Center for the Advanced Study in Behavioral Sciences. I became part of the group that consisted of Michael Rutter, Norman Garnezy, Jerry Kagan, Jerry Patterson, Judy Wallerstein and Julie Segal. And we had a wonderful year in which we did lots of things including a book. That is, we planned a book and wrote chapters for it and a couple of years after 1979/80 the book was published by Wiley, first as a hardcover and then as a paperback. That was a book on stress and coping during the development of children. It was a joyous year because this entire group got along so well at Stanford, and we were most, or all of us, there with spouses, and the spouses got along well, and we still—all the survivors—associate with each other and sort of gloat over that one year of heavenly association among compatible scholarly colleagues and their families.

Meltzoff: And a nice research environment....

Lipsitt: It was a great research environment...

Meltzoff: And that's what people want to do.

Lipsitt: Yes.

Meltzoff: Now, many people have research careers and teaching careers. Some people find a conflict, for some it's complementary; it's a difficult act to balance those. What was the experience like in your own case, teaching Brown undergraduates, teaching graduates, and then working on your research program?

Lipsitt: Well, for all of the years that I was there I taught various courses in child development, one of them was numbered Psych 81, and it was like the introduction to child development. The course that I really enjoyed most innovating and teaching at Brown was Psych 103 which was Research Methods in Child Development or Research Methods in Child Psychology. Much of the content early on I drew from my courses with Charlie Spiker, who was a great teacher and a great organizer of courses, and then I

evolved that, I suppose one might say, into more of my own course over time. One of the features of that course—I must say the students really loved it and still talk to me about their early experience in that course—it was a course mainly for senior, senior concentrators in psychology and every student did a research project in connection with the course. Some of them, right there in Hunter Laboratory, bringing in a few kids to observe in a certain way. Others would go out into the school systems that I had made relationships with and who were cooperative with us. As the child program built and Peter Eimas and Bryan Shepp joined us, Einar Siqueland became a faculty member, having begun his Brown career as a post-doc with me, they would also take on some students from Psych 103 and direct the students' research activities of that semester. Very frequently the projects would evolve into Honors projects. For example, they would do the Psych 103 project, usually an experimental manipulative study, but not always. Tony Davids was also involved, and he was more clinically oriented, usually supervising correlational studies with the students and these projects would often evolve into Honors projects in the second semester of the senior year. Or if the students took my course in their junior year, very frequently they would organize a research project to be conducted as their Honors thesis the next year either with me or with some other faculty member. I must say that I never really had a lot of, never felt a lot of tension between my teaching obligations and my research, because the two of them melded with each other. My graduate students were very frequently my research assistants or my teaching assistants in the courses that I just mentioned, or sometimes my undergraduate students would become my research assistants over at the hospital laboratory. So I think that my teaching and research careers were always quite well melded and didn't conflict much with each other. I never felt that I'm not teaching enough. I never felt I'm not doing research enough. I never felt that either of these academic domains of my life activities were drawing away from the other.

Meltzoff: How did you like the Brown students? Did they seem like a good, bright group?

Lipsitt: The Brown undergraduates have just been phenomenal throughout my career. Some of these kids, I call them kids because I'm not sure how old they are even though some of them are in their forties now, were just wonderful assistants, wonderful research colleagues. I can mention Deborah Kemler, now named Deborah Kemler Nelson. Her name was Deborah Glazer as an undergrad. She came up to me at the recent ICIS meeting, I hadn't seen her in many years...

Meltzoff: This one that we just finished...

Lipsitt: Yes, and I didn't recognize her at first because she didn't use the name that I knew her as when she was an undergraduate. She is Deborah Glazer Kemler Nelson and I remember her warmly. She was a wonderful undergraduate Honors student in Psychology. Dan Ashmead, I'm still in close contact with him. He is a professor today and was my honors undergraduate student. I've previously mentioned Steve Buka, who was a wonderful undergraduate student as well and worked with me in my Newborn Laboratory, and did a published study on grasping behavior in the newborn and now is my colleague in this long-term follow-up of students that we previously studied as newborns. And, again, I should really have the list of names in front of me to remember everybody, but I have just wonderful recollections of the sharp and collegial undergraduates that I worked with and the graduate students as well. So many graduate students contributed to work in my laboratory such as Ted Bosack and others, and I would say that it has been, as I sit here reminiscing about it all with you, a lot of those experiences come back to me and I feel very warmly about them and the relationships that have persevered.

Meltzoff: Has Brown since you've been here for, what did we say forty-something years—forty-two, forty-one years at Brown—do you feel that the institution has changed in a measurable way over this four decades or so.

Lipsitt: Yeah, that's a good question and a rather sensitive question with me, because things have changed at Brown. Things are much more scattered. The field of child development is not well represented anymore at Brown and that's been a consequence I think of the split that occurred such that a number of faculty from the Psychology Department helped found a new department on campus,

the Department of Cognitive Sciences and Linguistics, and as a consequence there are really two sort of large factions on campus now that are interested in child development. So the old gang is rather broken up. I never thought that it was a good idea to found a separate department, and I think that experience over the years has proven that and that there are some inadequacies now in both departments as a consequence of the split. My own feeling about the matter is that cognitive science was always part of behavior science, that cognitive experience and organization of memory and so on is part of psychology. Memory has been one of the hallmarks of the field of psychology. I felt right from the very beginning that there could have been founded, right in the Psychology Department, a cognitive science wing or section or something like that, just as we formed an experimental child psychology section or program within the Psychology Department. That would have been a far better move than to have a complete departmental breakup and split.

Meltzoff: And so do undergraduates now, I'm not familiar how Brown works, do they choose as an undergraduate major or are they only split at the graduate level?

Lipsitt: Split at both levels.

Meltzoff: Even at undergraduate?

Lipsitt: Yes, yes so that students can concentrate in cognitive science and linguistics now and I must say that in ...

Meltzoff: Probably too early on to choose between cognitive science and linguistics versus psychology, probably the undergraduates are just interested in the human behavior and the human mind, and they don't have a clear division between those two at an undergraduate level.

Lipsitt: Yeah, I think that it has had a deleterious effect on undergraduate morale among students that are really interested in psychology more generically and that maybe the programs of each have suffered a bit from the split, and this is due in part to the fact that there's relatively little cross attendance at each other's colloquia, even among faculty, and I think that just by virtue of the fact that there's administrative separation, there's kind of an automatic setup for competition and even hostility. I don't think that it was a good move. I think that it was something that might better not have happened especially at a university that has such a small Psychology Department to begin with.

Meltzoff: Okay, well we are here, actually this is sponsored by SRCD so what were your earliest associations with SRCD and how did you as somebody at Brown relate to that large organization?

Lipsitt: Well, I must say that I am very grateful for SRCD having made it possible for me to do this recording, oral history as we say, and especially for them having appointed you, Andy, as the person who would come and interview me, because, as you know, I've had great respect for your research and your involvement in the field of child psychology forever—for as long as I've known you—right from your earliest days in the field.

Meltzoff: Well, you helped in my earliest days in the field. I'm happy and proud to be here. It took us a while to get together, however. We've been working on this for about two years, and we're finally here. I'm happy to be in Providence having this conversation and actually I've learned quite a bit about the history of the field, so it's helped at least one person, I hope others will be listening. So your early experience with SRCD...

Lipsitt: Well, I must say in addition that I once went to the National Library of Medicine and listened to some of those earliest SRCD recordings that are there, and one of them that I listened to was that of Boyd McCandless, and I followed along with the type script at the same time and got some amusement from the fact that the recorded tape was a bit different from the typed script. The person who had typed the script didn't know some of the vocabulary that Boyd McCandless was using, the type of vocabulary of child development and psychology, and as a consequence some of the words came out

differently than one would expect. But it was really quite an interesting exercise to listen to some of those early tapes.

Meltzoff: Was he alive when you listened to those?

Lipsitt: No, he had died, and he died young, as you know, and that's one of the reasons that I wanted to go and listen to his tape.

Meltzoff: Did you recognize the quality of his voice.

Lipsitt: Oh, yes, it was a good recording. And there was no question but that that was Boyd and that he was very recognizable, and I was touched really by the opportunity to listen to him describe his own history and career development and everything. I first joined SRCD, I guess in Iowa City—at least that was the very first meeting that I went to. And it was in 1957, my last year at Iowa before I came to Brown, and it's hard to believe today, given the membership size and the enormous journalistic effort and monograph output of SRCD nowadays. But there were only about two hundred people at that Iowa City meeting, and we were all able to get into these small meeting rooms in a very small hotel in the middle of Iowa City. But it was there that Bill Martin, then the editor of *Child Development*, came and a number of people from the Fels Institute came there. I believe that Jerry Kagan was still with Fels at the time that he came to Iowa City from there, and it was a fascinating time that we students really enjoyed meeting what we thought were the older people of the field. Some of them were probably not any more than 4 or 5 or 8 years older than we were, but it was a historic event for us.

Meltzoff: Was Robert Fantz coming to SRCD meetings at that time in Iowa City, '57. He was beginning his very influential work around that time.

Lipsitt: I don't recall his being at that meeting, and I don't think I can recall him being at other SRCD meetings, although he probably was. I only remember meeting him on one occasion and that was at a conference in Wisconsin. Perhaps there were actually two meetings, because we first planned the conference and then we had the conference, and I believe that Bob Fantz was at both of those meetings. In any event, he and I are both chapter writers in the volume that came from this. Hanus Papousek also wrote a chapter for that volume, edited by Harriet Rheingold, Eckhart Hess, and Harold Stevenson. But we all knew of the work that young Bob Fantz was doing at that time, and we knew of its importance—of its seminal importance—in the field using the selective visual gaze as a dependent variable. It was a measure of what it is that young children as well as chimps prefer to look at. So he was around and we were enjoying his presence in the field, but I don't think many people got to know him very well.

Meltzoff: Now has SRCD itself changed as an organization, or do you feel that the meetings are different now than they were and what way have you noticed any change in the organization?

Lipsitt: Well, the meetings are very large, of course, very different than the way they were in the beginning. One could almost say that SRCD was an organization of about the size that ISIS was twenty years ago. Each of these organizations once they became solidified and once they began to attract membership at rather an enormous rate have become productive and started having wonderful seminars, symposia, and meetings and started generating further interest in the field. So SRCD has just been a remarkable organization in terms of its services to the membership, services to the field of child development and in terms, I must say now, of organizing the history of the field. SRCD rather joins with the historical archives, the Archives of the History of American Psychology, which John Popplestone and Marian McPherson organized at the University of Akron. These two major organizations are archival organizations and seek to preserve impressions and memories of what the field was like in its heyday and before and after.

Meltzoff: Now some people like large meetings and some people have an aversion to large meetings and force themselves to go. In the case of Bob Fantz, I guess he didn't really like large

meetings and did not force himself to go. What's your feeling about the meetings? Does it energize you, do you get depressed afterwards, do you attend the seminars, do you hang in the hallways and not go to the seminars? How do you relate to these sorts of scientific meetings?

Lipsitt: Well I love these meetings of SRCD and ISIS. When I go there I feel like I'm with my people, these are my two major organizations. Needless to say, APA and APS on the larger scene and as Washington-type organizations and societies, have importance in my life as well. But for sheer collegiality and for learning what is new in the field of child development, ISIS and SRCD are my places. I enjoy going to symposia there and enjoy seeing my friends and colleagues again and former students. I'm energized.

Meltzoff: You like it?

Lipsitt: I like it.

Meltzoff: Yeah, that's a lot of SRCD and ICIS meetings that you have been going to, I mean it's thirty years of these meetings or something if you were there in 1957.

Lipsitt: Let's say 42.

Meltzoff: That's a long time.

Lipsitt: I think I've missed very few SRCD meetings. I know that I missed the one in New Orleans a number of years ago, the one that Myrtle McGraw showed up at and where she was reputed to have approached somebody in the hallway who was looking quizzically at her, and she, Myrtle McGraw, turned to this student, or at least young person, and said, "I know, you thought I was dead!"

Meltzoff: People aren't coming up to you yet and saying that, are they?

Lipsitt: Oh, no!

Meltzoff: Interestingly in this last ICIS meeting that we came from, there was this whole symposium on early learning and conditioning. So the styles come and go, and there were symposia on the very things you were studying in the 50s and 60s, and they are back again. For a while at SRCD and ICIS they were not represented.

Lipsitt: Yeah, I think each time they come back, they come back with enhanced proficiency in methodology, statistical analyses, and sophistication of thought with regard to the processes that they study.

Meltzoff: So you would have some people wonder whether we should continue to have meetings or whether we ought to break apart SRCD into smaller meetings, some people interested in applied, some in basic, maybe social, cognitive, and so forth, but you would, it sounds like, advocate continuing to have these meetings, and you don't mind them at the size they are.

Lipsitt: No, no I hope SRCD continues on pretty much as it is right now, and I think that it has a good chance of doing so with John Hagen serving as the Chief Executive of the Society and organizing the meetings.

Meltzoff: One of the things that I was interested in asking you Lew is how in all these years in the field of Developmental Psychology you felt that your other peers or colleagues at a wonderful university like Brown have treated you in this sense. People who have careers in Physics or English or Chemistry have traditional fields that everybody has understood for a long time. If you say that you are studying chemistry or a chemistry professor everybody knows how to react to what they do. It seems to me over the years of your career, saying that you study child

development, has perhaps in the beginning those in Physics and Chemistry didn't understand exactly what you did and that may have changed or maybe I'm incorrect that it was not misunderstood in 1957. Can you talk about that, in a sense has the field changed, has it been understood by other scholars within university settings in a different way?

Lipsitt: Well, the field has changed, but not so much that I don't recognize it from 1957. That is to say that I think that much of the same subject matter is still of concern to us. We have somewhat different methods and more sophisticated ways of thinking about things, particularly in connection with, for example, the nature/nurture issue. But the nature/nurture issue, which was prominent in our vocabularies and in our speech and in our conversations with colleagues in other fields in 1957 to 1960, that issue is still with us and still calls forth an awful lot of emotional reactivity on the part of people on either side, as it were, of the continuum. I've always been an interactionist; I've always believed that you can't get conditioning out of a dead horse and that the only way in which the environment can impose learning process on an organism is through the prior presence of responses in the repertoire of that organism. So I've, perhaps less than some other people, maybe I should say, perhaps more than some other people, I've not had a great problem with this. I get incensed with people polarizing on this issue of heredity versus the environment, because both are so clearly necessary. In the early part of this decade when I was at the American Psychological Association as science director, the slogan Decade of the Brain was just being born. I talked to a number of people about how it would be very nice if instead of it being called the Decade of the Brain, it was called the Decade of the Brain and Behavior, because, as I said, the brain isn't worth a damn without behavior and experience. That's the interactionists' position I've had for a long time, and I don't think that studies of the brain can do much without consideration of what the environment and experience do to the brain and the nervous system and behavior. By the same token, I don't think it is easy to study environmental effects, experiential effects, without a knowledge of what the organism is capable of organismically. You have to have responses present to get Pavlovian conditioning. For example you have to have a respondent present like Pavlov capitalized upon the salivation response, which is in the animal's repertoire simply in response to food on the tongue. Without that being there at the outset, Pavlov could never have gotten conditioned salivary responses. So, much of my interaction with people in other fields, other disciplines, which you've encouraged me to think about here, is involved in letting them have the lesson I've just recited about the importance of organismic variables, but then I emphasize the great importance of experiential inputs and the fact, startling to some, that more young people die of behavioral misadventures than of all diseases combined. That usually grabs 'em, and they go away understanding that Psychology, behavior science, and behavioral interventions are serious business.

Meltzoff: And so one of the things I asked was whether *colleagues* at Brown—now maybe this hasn't changed so it doesn't strike you—I was just wondering whether in the early history of child psychology when you were a young professor here, did the Physics professors, the English professors you met understand what you were doing within child psychology? Has it always been understood. If not, was it thought to be Home Economics forty years ago. How have you been treated by colleagues in that regard being in this field?

Lipsitt: It's an interesting question, because I guess I don't think about it very frequently as to what colleagues in other fields think about what I am doing. You know, we choose our reinforcers and that's true in our everyday academic scholarly scientific activities as well, and I guess that when I'm formulating a proposition about the way in which babies behave, I'm looking more to the understanding of my colleagues in my own field first and then, if I get it right in my own field and I get some approval for the way I'm thinking about it or researching about it, then I think I know how to talk to a chemist or a physicist about what I'm doing. And I'm something of a public writer, you know I edit the *Brown University Newsletter on Child and Adolescent Behavior*. I write these Op-Ed pieces and so on, I try to be a good communicator to the outside world, by outside I mean to nonpsychologists. Whether I am really successful at this or not, I don't know for sure, but I do know that when I write on behavior and about children's behavior in Op-Ed pieces and so on, I get responses and usually positive feedback from other kinds of people besides psychologists. I have had letters from Classicists about my Op-Ed piece

on Princess Diana, and I've had people from Economics ask me about things that seem like overlaps with their field. And I think there is a great deal of overlap between the field of Economics and the field of Psychology wherein organisms work for things and get certain rewards for doing so and people plan their economic lives in terms of their psychological aspirations and anticipations, and so there is a lot of overlap, and I enjoy talking with Economists about that.

Meltzoff: Early economists, like somebody like Adam Smith who was a philosopher, an economist, and thought a lot about psychology, that framework. It was interesting, he was interested in human behavior in general, given incentives and given how to structure psychology in the society so it would work a certain way.

Lipsitt: Yes, I'd say Adam Smith was an authority on human nature, even if he wasn't a psychologist. He understood motivation, he understood laziness, he understood the human capacity for work, and he understood what drives people to behave as they do.

Meltzoff: Now in terms of how society has reacted and picks up research in child development, we've recently had a state of publicity in *Time* and *Life* and so forth, *New York Times* has carried many pieces. In 1957 to 1962 was the public attention as much as it is now on infancy or has that been something of change.

Lipsitt: I think that the public attention to infancy has been going on for a long time. There was a great deal of publicity surrounding John B. Watson's pronouncements about what one should do in raising one's children, and what one should not do. There was a great deal of publicity surrounding Myrtle McGraw and her studies of twins, and there was publicity from the Gesell Center at Yale. You know we are going back really to the origins of our field in this. Arnold Gesell was actually a student of G. Stanley Hall. He got his Ph.D. with G. Stanley Hall. Many people don't realize that, they know him as an M.D., a pediatrician, and they don't appreciate that he had psychological training as well, and while his orientation was not one that was terribly friendly to the point of view that I myself have had, emphasizing as it did too much in my view the maturational aspect of development and rather overlooking the importance of experience in helping to foster maturation and other aspects of development. Nonetheless, he was there and getting a lot of publicity and getting a lot of publicity for his viewpoints. I've learned about this largely through my associations with Cheiron, which is a historical association, and through being a member of the Division on History of APA. I have historical interest in the field and have found it very interesting and enlightening and inspiring to talk to some of the people that were truly important forces in the history of psychology, like Fred Keller and Myrtle McGraw and Mary Cover-Jones. These people had a lot to say to the public as well as to their colleagues, and I think they're still relevant to the public and I hope we continue to talk to the public.

Meltzoff: I was thinking, in your early career when you are making breakthroughs in learning, I guess the Johnson Administration was busy making policy that in some sense impacted children. So again perhaps, child development was in the newspaper and in the news.

Lipsitt: Yes, you know I think that once you start talking about children, just about everybody gets interested, depending upon of course what you are saying, but nonetheless there's fairly universal interest in child development and cross cultural differences in the way in which children develop and in proficiencies or capacities of the very little child.

Meltzoff: And have you ever experienced yourself being distorted in any horrible way by the press, or do you think all in all it's been, they are the press and we're scientists and it's our job to try and communicate as best we can and sometimes they get it wrong but in general they do a good job. Some people have an aversion to the press, some people like using the press and run to the press quite often. What's your attitude about our scientific relation to those outlets?

Lipsitt: Well you know...

Meltzoff: And I include film, because there have been films about child development as well on television, I mean educational films, classroom films.

Lipsitt: I think that I've probably got a thin skin about being misunderstood about what I say in not just the research articles but I mean in public articles as well. And I can tell you an experience that I had that was one that I have thought about a lot since, which was a little disturbing to me at the time. When I wrote my piece for *Psychology Today* I was writing it in such a way, I thought, that the public would truly understand what I was saying, and I talked in that article about the role of stress in development, the way in which problem-solving behavior could only be inculcated in kids through some kind of stressful conditions in their environment. In order to solve a problem there had to be a perturbation, and I think I used that term "perturbation." And I rather promoted the idea. I think I said that given the way children learn from being stressed, given the healthful aspects of mild stress in their environment, in their lives, and given that stress provides the opportunity to work things through, I conjectured at the end of the article that maybe if a child is seen not to be experiencing enough stress a little bit of it should be imposed upon him or her. And a guy wrote a letter to the editor and in effect said, well don't let Lipsitt get near my kids. The whole idea that anybody would think of imposing a little bit of stress on a child was inimical to that man. So that experience has taught me a lot. It taught me that I must be very careful about the way in which I say things in public and the way in which I must qualify what I am saying, so it won't turn people away rather than turn people toward the idea. I still feel that it's quite important for young children to experience stress in interpersonal relationships and in problem-solving situations and in learning in order to develop really good systems for coping with perturbations.

Meltzoff: Now psychology is an important discipline that we teach in college. We don't teach it as far as I now in very many high schools. What's your opinion about possibly having psychology classes where you learn about human behavior at the high school level, not only college?

Lipsitt: Oh, I think it should be taught in first grade. I think that just as kids are already learning in the first grade about gravity through experience with gravity, and they are learning about chemistry through their acquisition of knowledge that some water is okay to drink and some water is not, some of it is toxic, so also I believe that kids need to learn at a very early age that there is a way of looking at human behavior and a way of appreciating the behavior of another person. A way of thinking about why another person did what he or she did. I think that's for first grade, and I think that it's up to teachers to try to use real life conditions in the classroom, on the playground, or wherever; it's up to them to try to point out to kids that there are regularities, that there are laws of human behavior, just as for other aspects of the world around us.

Meltzoff: I also thought, you know, it's interesting your science fairs... and most parents when they are organizing what science project for the science fair, it's a chemistry experiment or a physics experiment or something like that, and it does strike me that perhaps kids would be interested in understanding laws of human behavior, and it would make a lot of sense because they're playing with children and seeing human behavior around them all the time. But we don't teach our science of behavior, how to do controlled experiments, how to form hypotheses and perform experiments. We don't teach our children how to do that about human behavior. It's the last discipline to be discovered, it's a college discipline. It's very odd. You'd think it would make a lot of sense to teach fifth graders and fourth graders when they do these science fairs, or eighth graders or high school kids.

Lipsitt: I think it's important to teach young kids about causation in the realm of behavior, whether it be the behavior of their pet dog or the behavior of their mother and father or whomever.

Meltzoff: Has this ever come up in any of the organizations? In SRCD or APA where you were in the science directorate, has it come up about bringing psychology to high school or junior high, and what's the reaction?

Lipsitt: Oh yeah, I think that APA has done a lot by way of helping implement courses in high schools and has had, I believe may still have, a newsletter that goes to high school teachers of psychology. Even if the teacher is actually teaching biology, but throwing in a little psychology on the side, that person is eligible to receive that high school newsletter for teachers. Those issues that I've seen are actually of high quality, and I'm sure that by virtue of the fact they provide even some little experiments that the teacher can do with the children or with the high-schoolers, it's serving a very important purpose.

Meltzoff: Or maybe museums like the Exploratorium in San Francisco, or other museums could begin to have even more exhibits about human behavior.

Lipsitt: Again, APA has financed and built an exhibition, "Understanding Ourselves, Understanding Others." And it's a traveling exhibition that is now in its 5th year, 6th year, and components of the exhibition are in the headquarters of AAAS in Washington, the American Association for the Advancement of Science, and at the Smithsonian. Children are taken to that exhibition and get a chance to do some hands on things.

Meltzoff: Well, for the end of the interview, Lew I'd like to just turn to the personal side of things, and there's a couple of things I've always wanted to ask you about

What I was asking Lew is about how personal interests have affected professional and vice versa. One of the things that has always been amusing to talk to you about was your interest in names and the names of people and how they sometimes correspond to those professions the person has. I wanted to ask you if you had some favorite examples of that and then also ask you whether there was some confusion of how to spell your own name when you were young and whether you think that sparked an interest in names on your part. So perhaps you can tell the little story about your name and then talk about the interest in names in general.

Lipsitt: Well, let me do it in a bit of a reverse order.

Meltzoff: Okay.

Lipsitt: In order to make a point about my name.

Meltzoff: Right.

Lipsitt: I've had a fascination with peoples' names and occupations for a long time. It has seemed to me that an inordinate number of people have names that sort of tell what they do for a living or tell what they do for a hobby or preoccupation. And one day I decided to use that element of coincidence in a lecture that I was giving in child development, the very same course I mentioned before, Child Psych 103, Research Methods in Child Psychology. And I was waxing eloquently about the difference between a causal statement on the one hand and a correlational statement on the other, and I said to the students, you know very frequently things will seem as if they are closely related, even causally related, when in fact it's simply the way things are and there is no need or scientific permission for attributing causation to the situation, and I said, take for example peoples' names. Very frequently, I said, you will find that a person will have a name that foretells or suggests what he or she does for a living. I said for example Dr. Fish founded the Rhode Island Oceanographic Institute, and there were a couple of snickers in the class, and I said, it's really true Dr. Fish did and moreover he hired a man named Saila and Dr. Saila immediately hired Mr. Seamann. So I said these three men, Fish, Saila, and Seamann all worked together at the Rhode Island Oceanographic Institute, and by then the students were really perking up, saying gee what kind of a situation is this. And I said, look, moreover, you know Professor Fidler is here at Brown University in the Music Department, and I don't know whether you know this, but Mrs. Record is the lady over in the alumni office that keeps the roster of records on alumni, and I said if you just go across town you will find the AAA, the American Automobile Association of Rhode Island is headed by Mr. Rowles, who is a two-way winner, cars roll and Rolls

Royce. And I said Dr. Hawkes is the head of the Audubon Society of Rhode Island. Well by now, by now, the students were really quiet and wondering what I was going to say next, whereupon a student at the back of the room raised his hand and said, “and Dr. Lipsitt, you study sucking behavior in kids”, and of course that brought the house down, because that was one-upping me, and, as a matter of fact, I had never thought of it. I had been interested in names and occupations for some time, but never really thought of the whole thing as anything more than a coincidence that enabled me to make the point about the difference between a correlational statement and a causal assertion. But from that time forward I’ve actually believed that there’s probably something to it. It happens just too often for there not to be anything to it. I wouldn’t know how to examine the veracity of the hypothesis as to whether there is truly a causal relationship here, but it happens so frequently that there has to be something to it. And when people ask me what the mechanism could possibly be, I say, “Well, if you grow up with a name like Fish, you are constantly being reminded of not only yourself and your family but of fish, fish.”

Meltzoff: It would be hard for somebody to sort of say, Lew, I hate fish, I mean if their last name was Fish, right? I hate those slimy creatures! That’s not likely.

Lipsitt: Right, so because you have it on your mind so much, you become sensitized to any occurrence of it in the literature, any mention of it by anybody else...

Meltzoff: And your friends bring it to your attention.

Lipsitt: So it makes some sense to me that there could be a causal relationship. But it’s just a hobby on my part, to have fun with it, and while I’m having fun with it other people are having fun with me having fun with it. So that for example a reporter from the Canadian Broadcasting Company called me one day and said, “We are just finishing down in Newport doing the trial of...what’s his name, the man that was accused of murdering his wife, Von Bulow, and they said we are just finishing the trial, we’re on our way back to Toronto but we’ve heard about you and your names and occupations and they said have you got a number of instances?” “Oh” I said, “I have three files full of instances.” And they said we’d like to come over, we’d like to interview you at your home” and I said, “look this is just a hobby of mine, it’s not a serious pursuit. I do serious research with infants, and I’d love to have you stay over and come to my laboratory in the morning and do a really nice story about the importance of infant behavior and development and about the work going on at Brown University in infancy.” And they said, “No, no, no. What we really want is the names and occupations stuff, and it will be on tomorrow night if you will allow us to interview you. It will be on just after the news.” Well, the next step was that they did come here, they set up the camera here in the living room. The lady in Toronto interviewed me through an ear piece, and I sat there with my file full of names and occupations and gave them a whole blast of them. And the morning after it all appeared on Canadian Broadcasting Company, the lady from Toronto called me up, and she was laughing so hard she could hardly stop. She said “I wanted to tell you that we’ve gotten many, many calls from people after having heard the story about you, and I want to tell you also that we had a gentleman in the studio, quite unrelatedly, that was to have been interviewed about his work on his occupation immediately after you, but we sent him home without interviewing him and asked him to come back in a week’s time, because his name is Dr. Bird and he’s an ornithologist and we were afraid that he would think that we were making fun of him—coming on right after you. She said that it was just a coincidence, but I’ve always suspected some sneaky scheduler in the TV studio had a hidden agenda.

Meltzoff: So it happens. Well it’s always been fun when I’ve heard you talk about it. You do have many, many examples. But so, just for the record here, there was some confusion about your name to some degree. Sometimes people spell it one way and sometimes they spell it another way to this day and that has origins, I think in your childhood, is that right?

Lipsitt: Well, it is true that my name is frequently misspelled. My name Lipsitt is a variation of a similar name of my grandparents when they came here from Russia and changed their names early or even at Ellis Island, and I’m not sure it was the best transformation to make as long as they were going

to make a change at the time, but that's my name and I and my family all live with that and it's not too bad. The family name was originally Lipschitz, and, growing up, that provided kids with the opportunity to make fun for themselves. When I got older and realized that Ralph Lauren had the same name, I wished my own grandparents had done the same or similar with ours.

Meltzoff: But your first name -- wasn't there one occasion, some people spell it Lew and some Louis, Lew.

Lipsitt: Well, I'm sorry I didn't realize that you were getting at my first name.

Meltzoff: Yeah.

Lipsitt: I have previously mentioned that my mother was heavily anesthetized at my birth. Precipitous delivery—remember? When the nurse came around to ask her what my name was going to be, apparently she was still a bit drugged up and she said, perhaps precipitously, “his name is Louis” and the nurse apparently wrote “Lewis.” And in her groggy state, apparently, my mother assented to it, and somehow I was both Lewis and Louis at various times in my childhood. I was Baby Lou in the family, and especially in my mother's family, for quite some time, but my name on my birth record is Lewis and so my parents decided to keep it that way, even though they had intended for it to be spelled Louis, after my grandfather Louis Paeff. So I am Lewis Paeff Lipsitt after my mother's father.

Meltzoff: So her intention was the “Lou” spelling, but on the legal document it's the “Lew” spelling.

Lipsitt: Yes, so my parents simply went with destiny.

Meltzoff: You were named by the nurse.

Lipsitt: Essentially.

Meltzoff: By the environment.

Lipsitt: I was put under a spell by the nurse.

Meltzoff: By the environment. So let's see if we can conclude with anything else about personal notes. The one that strikes me most after spending these days here with you and also being at conferences is that you're a man that's had a life-long partner that's supported you and you've supported her. And my wife and I have often talked about your relationship with your wife, because the two of you seem to be friends and in love with each other after all these years and that in today's society it's hard to find people married this long no less caring about each other for all this time.

Lipsitt: We've had students come to the house, groups of students, and they begin to ask us questions like how long have you been married, and I think half the students at Brown are from homes where the parents are either separated or divorced, and so they are always quite surprised that we have been married as long as we have been. And they often ask us also how we met, because I usually said something to the students to the effect that I'm one of the luckiest people in world. Somehow I found Edna, and we got married, and we have been happily married ever since. So the students frequently ask, how did you meet, and when Edna and I tell them that we were a blind date, they usually cannot believe that either. But that's true, my brother was dating a young lady who was a good friend of Edna's and was happily having a relationship with... We should bring Edna in to tell this story. She's nearby. Edna said well are there any more Lipsitts like him and this young lady said, yes as a matter of fact there are five of them. So Edna said, “Can you get me a date with one of them?” I think that's how it started, Edna, come in for a few minutes. Edna is actually, you know, Edna Duchin Lipsitt.

Meltzoff: That's right, she doesn't speak on tape much. I've heard that even on answering machines she doesn't speak!

Lipsitt: Andy just asked me a question that you can answer better, Edna. It has to do with how we met, we were a blind date, I've just said into the tape, and it had to do with one of my brothers dating somebody...

Edna Lipsitt: ...dating the friend of my roommate at college.

Meltzoff: So you saw an older brother of Lew's?

Edna Lipsitt: An older brother of Lew's was dating a friend of a friend and then my friend met another brother. They went on vacation, they all went on vacation, and when they came back they said, we met two of the nicest guys, two brothers, and I said if there were two aren't there three, and they said yeah there are five. So I said, well...??

Lew: Edna said "Well?" and they arranged for Edna and me to meet.

Edna Lipsitt: The others eventually broke up, and we stayed together.

Meltzoff: Neither of the other two...

Lew: No, neither of the other relationships lasted.

Meltzoff: And so when was this, it was during graduate, during masters, where in your intellectual history?

Lew: It was during that period right after the University of Chicago, and I was now living in Boston for about six or eight months, and Edna was going to college, at Lesley College, in Cambridge Mass. It was during that period.

Meltzoff: Now is that Teachers'...

Edna Lipsitt: Teachers' College.

Meltzoff: Yeah, Lesley Teachers in Cambridge. I know that...

Lew: That was in 1951.

Meltzoff: So you had your first date in Cambridge Mass?

Lew: Our first date involved our going to Symphony Hall in Boston, to a concert.

Edna Lipsitt: To an opera. You had a free ticket.

Lew: Of course!

Edna Lipsitt: And no money!

Meltzoff: And you married him despite that...

Edna Lipsitt: In spite of that!

Meltzoff: How long after the date were you married, was it several years?

Edna Lipsitt: It was about a year and a half.

Meltzoff: About a year and a half.

Lew: We were married in 1952 on Edna's graduation day from college. I had to go down the aisle and bring her out of graduation so that we would get to our wedding on time.

Edna Lipsitt: Because the graduation was going on, with my sister whispering, "She's about to get married" to everyone.

Meltzoff: Well that was a big day for you graduating college and getting married at the same time.

Edna Lipsitt: Because Lew had to be back to the University of Mass. because of his job, and we were going to run one of the tourist places for the summer to get free housing. We ran a tourist home for the summer.

Lew: In Amherst.

Edna Lipsitt: In Amherst.

Lew: So we met when we were ages 21 and 19 and got married when we were 22 and 20 and Mark was born about three years after that, when we moved into Iowa City from San Antonio, where I had been in the Air Force. He was born during my first year of University of Iowa graduate study.

Meltzoff: And that's the one, you were present for that birth. That was in Iowa and they allowed you to be present.

Lew: Yes, I was in the delivery room, soothing Edna, when Mark was born. It was a momentous experience, wonderful. Then we came to Providence, and Ann was born here, and I wasn't allowed in the delivery room.

Meltzoff: So you've had, in that sense a pretty stable life. Two children, one wife, this house for a long time, a job for forty something...

Edna Lipsitt: This is our third house.

Meltzoff: Yeah, third house, but you've been in this house for quite some time. This is a large beautiful house presumably when you were a full professor you could move in here. I don't know how soon you did, but this is not the house of a young faculty member.

Lipsitt: Shortly after my promotion to full professor. Also we've had one of our cars for about 13 years and the other one for about 10. So we're even stable with regard to automobile ownership.

Meltzoff: And your friendships. Well, I hope we remain friends for a long time. You've made a great contribution to the field as I hope we've illustrated on this tape. You've influenced many, many students, many researchers, influenced my research with seminal research, getting infancy off the ground, and I think there has been a revolution in understanding infancy during your professional career due in large measure to your research, the research of people who you've touched and connected with Brown University in many ways. I think the history of child development has been changed by this work. So thank you very much for your contributions from all of us.

Lipsitt: Well, thank you very much for those very, very kind words. We value our family relationships and we regard you, Andy, as one of our family.

Meltzoff: Well, thank you. At the last meeting of ICIS I was mistaken for your son. Not only do you value me as your son but somebody came up to me as if I was Mark!

Lipsitt: Sure, you know who it was?

Meltzoff: Yeah.

Lipsitt: It was Larry Sayco, the yo-yo man.

Meltzoff: And he started asking me family stories which I didn't know the answer to! He thought I had too much to drink that night. I was very polite and very nice to him; I just couldn't fill in these behind-the-scenes family stories. I think I seemed uninformed.

Lipsitt: He thought it was funny later. Speaking of our children though, Mark and Ann are both in areas relating to mine and Edna's. We are kind of a learning process family. Edna was trained as a teacher, and I being in the field of child development and studying learning processes of kids. We bore two children who are very involved in learning processes themselves. Mark is a professional animal trainer and thoroughly enjoys going to meetings like the Association for Behavior Analysis, where he sometimes talks and sometimes even trains a dog right before their very eyes because he is truly an expert. He is going to the ABA meetings with me at the end of May down in Orlando this year, and he is looking forward to meeting J.P. Scott there. J.P. Scott is one of his idols as are a number of other senior people in the field of psychology particularly animal and human learning. So he is in the field of learning as an animal trainer. He is a very interesting chap. He is now age 43 and our daughter Ann is age 40. Mark is very interesting in that he dropped out of school very early in life and one of the first things he did was to join a circus at the age of 15, I think, although sometimes I think it might have been 14. But he was very young anyway when he joined the circus, with our permission. Many people say, oh did Mark run off to the circus, run away to the circus? And we say, "Oh, no we took him. We gave him permission, and we actually drove him with us to the circus where he joined on and traveled with the circus for many months." Today he's really an expert. We got a legal publication from Maryland wherein his testimony in court about animal behavior was taken as the truth about the way animals behave, and particularly this animal that he was testifying about behaved... So he's got a certain kind of grand accomplishment there—unschooled accomplishment—but none the less a real gift. Ann our daughter went to Lesley College at first and then eventually graduated from Goddard College in Vermont and is a teacher of kids with what we call learning disabilities, but she insists on not using the term. She calls them learning differences or learning distinctions. She is a remarkable teacher. Edna and I enjoy really going up and watching her teach, because she is such a fine and respected teacher of kids and has such sophistication of what kids need. So Edna and I are very proud of our offspring!

Meltzoff: A family that's centered on human behavior and human development and human learning, or learning in general, behavior in general.

Lipsitt: Yeah. Behavior and learning processes.

Meltzoff: Okay.

Lipsitt: Well, thank you very much Andy, it's really been a pleasure talking to you, and you sure do a thorough interview.

Meltzoff: It's been a while. Well thank you very much.