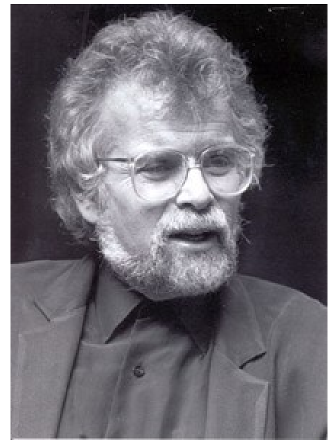
Michael Lewis



- Born January 10, 1937
- B.A. in Sociology (1958) and Ph.D. in Psychology (1962) both at the University of Pennsylvania

Major Employment

- Rutgers University
    - University Distinguished Professor, Graduate School of Applied and Professional Psychology: 2015-present
    - Professor of Social Work: 2015-present
    - Associate of Center for Mathematics, Science, and Computer Education: 2015-present
    - Professor of Biomedical Engineering: 2002-present
    - Professor of Psychology, Graduate School of Education: 2000-present
    - Professor of Psychology, Department of Psychology: 1979-present
- Robert Wood Johnson Medical School
    - Director, Robert Wood Johnson Autism Center: 2006-present
    - University Distinguished Professor, Departments of Pediatrics and Psychiatry: 1993-present
    - Director, Institute for the Study of Child Development, Department of Pediatrics: 1983-present
    - Research Director, Center for Human Development and Developmental Disabilities, Department of Pediatrics, University of Medicine and Dentistry of New Jersey: 1990-2002
    - Professor of Pediatrics and Psychiatry, University of Medicine and Dentistry of New Jersey: 1982-1993
- Clinical Professor of Pediatric Psychology, Columbia University, College of Physicians and Surgeons: 1972-1981

Major Areas of Work

- Sociology, Clinical psychology, Infancy, Attention, Mother/child interaction, Demography

SRCD Affiliation

- Consulting Editor, SRCD Monographs (1972-75) and Child Development (1978-83)
- Member since 1962

SRCD ORAL HISTORY INTERVIEW

Michael Lewis

Interviewed by Marsha Weinraub
April 14, 1993

Weinraub: Describe your family background along with any childhood and adolescent experiences that might be of interest. So let's start with where were you born, where did you grow up and what was your schooling like and what was it like in terms of what happened to you before you went to college? This can be brief because we are going to develop some of these issues.

Lewis: All right, briefly, I was born in 1937 in January—January 10th, ten days shy of being a New Year's baby—to a family that already had a sibling, my sister, who was three and a half years older than I by the name of Barbara. And Barbara plays a very important role in my life. My mother was considerably younger than my father and they were both—I'm first generation American—they were both born in Russia and that in itself is an interesting side of my whole family history which I will get to if we like at some point. So I was born in Brooklyn and we moved at the time of my birth to a rather fancy, that is upper middle class, area of Brooklyn where I was to live in that apartment house for the first 18 years of my life, and perhaps what best characterizes the early part of my life is sickness and death which was the major theme. My mother was not well and probably by the time I was three or four was in serious declining health both from breast cancer and from hypertension and it was indeed the hypertension which killed her when I was eight years old.

Our family then was reduced to my father and my sister and me. We continued to live in the same location. My father didn't remarry. My mother died in 1945 which was right after—actually the war wasn't quite over and there was a large number of displaced persons: women who came to the United States after the war who took jobs as housekeepers. We had a succession of these women who lived in the apartment with us that kept house and who wanted to marry my father. Well, my father didn't marry, so I probably had, between eight and eighteen when he died, probably ten different women, adult women who lived in the house as housekeepers. Now it was a rather small apartment and so we had two bedrooms. I slept on a cot in my father's bedroom all through my adolescence and my sister in a cot in the living room and the housekeeper had the other bedroom. Now this is very surprising, it probably reflects the total disorganization that was taking place in my family because my father was a very successful businessman and we were quite wealthy. Then the society characteristic of my early childhood was that the place where we lived changed in terms of social class and my father could not organize himself to move us out of there. So when I was ready for college all my peers were not college bound. In fact, they were junior hoodlums in a certain sense. I think one of them went to a teachers' college, and I always had a feeling that I didn't belong and I never had a good understanding of the reason. My first reason, I suppose, was that my family was different than the rest. My second, which I held for the longest time, was that I was just smarter than my peers, but I don't think it was that. And my sort of final conclusion as I enter my last part of my life is that it was really a social class difference, that we should have moved out of the neighborhood but we simply didn't. So not only did I have this family background of strange constellation but also my peer relationships were, I wouldn't say lacking, I certainly had good friends all the time, but I didn't belong to the group. I never felt cohesion with the group. That included when the boys went out and had gang bangs or they gambled for a lot of money; I was simply never comfortable with that as a group.

By the time I was 18 I had lost through death all the major figures of my life including my mother and my father, my grandmother, a significant uncle in my life who was critical for my educational goals. I had an uncle, Uncle Morris, who was a physician and a cardiologist. He died at 51 of a massive coronary, but he was the intellectual of the family. When he started to become ill he retired and he taught himself five or six languages which enabled him then to read the great works of literature, in the original. That's what he did in the last five years of his life. So you see I had a rather powerful intellectual model and he was, suddenly—wasn't a father, but he was a father figure and a very important man in my life. My father was college educated, was an engineer, but never practiced it.

Weinraub: He had been trained in Russia?

Lewis: No, he had actually come as a child from Russia. He went to Cornell and graduated in 1915 from engineering in Cornell on a regent scholarship and this really speaks to my whole family background which was one of European educated city cosmopolitan. So, in any event, including the last remaining grandparent who lived nearby, my father, my mother, my uncle, and an aunt or two who were significant figures died and I found myself at 18 going off to the University of Pennsylvania in engineering. Now a word of explanation for engineering is probably in order. I'm a left-handed dyslexic and to compound a strange childhood I was thought to be somewhat slow and my older sister was considered brilliant. She was and is very bright and very talented demonstrably in school. I could never spell. I didn't learn to read really until I was 17 and so did well in some subjects but not terribly well and very badly in all the language-related subjects. It wasn't until I started to take mathematics which wasn't tied to a language—you know those problems in which you have two oranges and they cost 10 cents and a banana costs 8 cents and you give them 25 cents and how much change do you get back? I could never do those because they were language problems. But once I got into algebra, algebra saved me because it turned out that I was very, very good in algebra, but it was too late to affect really the course of my early education. I was taken out of the college-bound curriculum in the sixth grade, when you are 10+ and put in to not the slow class but sort of what was called the commercial class. I could be a bank teller or a shoe salesman but certainly not college material. By the time I took the algebra and started to demonstrate that I could pass it I had already been separated out from the college-bound kids. In high school I was able to recoup but, again, if you'd looked at my record you would have seen a bright dyslexic's record. The term wasn't invented yet so no one knew what it was except it was another peculiarity of my early life. I took math and I took physics and chemistry and in fact did very well in those subjects, but in history, English, anything that was language based, I did terribly. And languages—I took French for seven, eight years still I can understand it and I can do menu French when I go to Paris but certainly can't speak the language. So here I was 18, having deciding that I should go to college and it was agreed upon by everyone that I should go to engineering which would be where my skills were located. Now that was a mistake; it was my father's doing because he was an engineer, and what I should have gone into was some theoretical basic kind of science.

Weinraub: Like theoretical physics?

Lewis: Or physics or mathematics which I clearly had talent for, but I don't think he understood as an advisor, nor did I have a particularly good school advisor to advise me of what to do. So I went off to the University of Pennsylvania's Moore School of Electrical Engineering. Now that was a top, top engineering school and the top floor—this was 1954—the top floor of the Moore School was Univac, the first tube computer that was functioning. It took up the whole floor and of course your hand calculator can do more than that could do. So here I was in engineering.

So I went off to engineering and, fortunate for me in terms of my life career, my father died within my freshman year at school and that essentially did two major things. It simply released me from the obligation of engineering which was really his doing. Secondly, it placed me on my own. In fact, I had been on my own because no one really took much care of me as a child, but I was clearly alone at that point. Financially I had to swim for myself. I had no place to live, and I essentially then had to organize myself. So my father's death really marks the end of my childhood for me and marks, if you will, the moment in which I had to become something because there was nothing left in family and so on to become. I might add that my family was all considerably older and I was the last child, the last cousin, and so much of my family was dead by the time I was 18 and so I was truly orphaned in this sense.

Now I mentioned my sister and my uncle as two important forces for me in terms of intellectual. Well, I've mentioned this uncle as being a scholar in the last part of his life, being trained as a specialist in medicine. My sister became a psychologist. She went to Brooklyn College and Brooklyn College was one of the centers of psychology in the country. You had people like Herman Witkin, Solomon Asch, oh gosh.

Weinraub: Festinger, was he there?

Lewis: Festinger was there. It really turns out—I can't remember all the people but it was really a center and they had a master's program. While doing your BA, for a few students, they could actually start credits toward their master's, and my sister was in that program and was in a program that now has—I mean, I know at least a couple of the people are distinguished psychologists now—Steve Glickman out at Berkley, Salvatore Mattie out at Irvine were class members. So here was another sort of model for me, an educational model that was my sister doing graduate work, in this case in psychology. Now it worked against me for a while because psychology was taken and my sister was just an extraordinarily good student and I simply couldn't compete with her, and so when I decided that engineering wasn't my subject it was not psychology that I went to, although I was clearly psychologically oriented. Shall I go on with this?

Weinraub: Yes, this is a mystery now. How did you move to psychology?

Lewis: Well, what happened was this. Keep in mind this important thing with my math skills because this turns out to be the thing that saves me on three occasions, four occasions. It gets me back into the track of university. It gets me into a good university in engineering and now it is going to affect two more things that happen to me. So here I am, I am in engineering at the University of Pennsylvania and I take an elective which is a humanities course taught by a man named Marvin Bressler who is a sociologist who, interestingly enough, is chairman in the Department of Sociology at Princeton where I end up 25 years ago. I take this course with Marvin and I cannot believe such goings on. I cannot believe people think the kinds of things they think and ask the kinds of questions they ask.

Weinraub: Like what kinds of questions?

Lewis: Questions about why people behave the way they do. Now they were asking it from the perspective of groups in sociology. They weren't asking the individual psychological question but what accounts for group behavior, and I found that absolutely fascinating. I was still an engineer, I'm in my second year now and I'm hating it. I'm hating my peers who are engineers. They are not my kind of people. They are good people; they're just not my kind of people. The thing that clinches it is that we have to take a literature appreciation course—one credit—there must be 30 men, no women in engineering, yet 30 men in this course on literature appreciation taught by a Chinese professor whose English I cannot understand, is incomprehensible. What the task was for this course was that each person was to read a book in the course of the semester and give a book review. Everyone read *1984* and they passed the book review one to the other and so for the entire semester we heard the same book review on *1984* in this course, and meanwhile I am taking this absolutely mind boggling course in—

Weinraub: Sociology.

Lewis: So it turns out it is not too difficult to switch from engineering into liberal arts, which I do at Penn. Now I would never have gotten into Penn liberal arts. One, I couldn't do the languages or the history or the writing, and I'm now forced to take French, for example, as one of the courses I now have to take. Well, I had this professor who I mentioned earlier who is a mainline gentleman and we have to read in class, we're reading literature and we are reading in French. Now I had four years of French in high school. I didn't learn anything, I didn't know anything, so now I'm taking it in college and it's all modern French literature and it's on existentialism, the existentialist writers Camus and Ponty and Sartre and some others. Now I happen to be reading them in English and I'm fascinated by the topic, again the movement that I have toward a psychological sense. Of course I can't read them at all, but I've read them in English. So I make a deal with the kids in the class and with the professor. The deal with the professor is, he says, if you don't bring your text to class you won't get an opportunity to read. Since I can't read or speak French at all I never bring my book and he never calls on me, which is fine with me. For the papers and so on I explain it to my student friends what it's all about in English and they help me translate that into French. So I manage to get through, by the skin of my teeth, all these humanities, but meanwhile I'm taking sociology courses.

Weinraub: Now I just want to go back to the question of the Chinese professor who taught the one credit literature course. You say that was an important influence in some way?

Lewis: No, I'm simply saying that I am amazed with the contrast with engineers who had absolutely no interest in literature, no interest in anything except engineering, and it just clinched. That wasn't my place in the world. So I was able to transfer and I lost no credit, interestingly enough, because in the early years of engineering you take math and physics and those can be applied as liberal arts. So I had satisfied all my requirements except the humanities requirements and the sociology. Now what happens in sociology is that at Penn there happens to be a Center for Demography. One branch of sociology is mathematically oriented and that's where I head for and I work with Dorothy Swain

Thomas, one of the old-time classic demographers. If you knew anything about demography you mentioned Dorothy Swain Thomas you'd be looked as like you worked with a pro. I do a dissertation—

Weinraub: An honors thesis.

Lewis: An honors thesis now; a piece of basic research which I spend my senior year working on and it was on mental disease and migration and it's statistical looking through census and doing all these things. And here I am now suddenly starting to do basic research. I've come into my own. I'm considered a very good student, I'm very good in the mathematics part, and I'm doing sociology and I'm also doing mental disease. So here I am sort of starting to move, I find myself being formed by a set of forces, but you know there is nothing deliberate going on. If you said to me at that point even in my senior year, "Are you headed for an academic career?" I would have said, "I haven't the foggiest idea." I do my honors thesis; it receives wonderful grades and acclaim and so on. They publish it and I graduate university with honors in distinction in sociology. Now what should I do in my life, I have no idea at all. I have several options. I have applied to Penn's Law School and I've been accepted. I've been accepted in the graduate program in sociology at Penn and also at several other places around the country. So here I am, I've graduated, and I have no idea what I should do and the idea of an academic life still hasn't clicked for me. One, I have no model, okay. My sister now has transferred to Penn to work with Eugene Galanter and maybe get a Ph.D. in psychology. Now she only stays one year in Philadelphia, it's the only time in 60 years that she's left the city and she soon goes, which as you will see will be a saving grace. So here I am, I've graduated with honors from a university in liberal arts. Now if you had said that this would occur I would have been—

Weinraub: Your father would have been shocked.

Lewis: Everyone—no one would have sort of expected this of me. I must tell you that I have talked to several of the professors back then and asked them out of curiosity about what I was like then since I was still so much in a formative period. It took me so long to form myself that I really don't have a good sense of myself back then. They all reported to me—and you never know because they know me now—they all reported that they all thought I was exceptionally bright and competent and so on, which was always a surprise to me and that has to be kept in mind. Remember for the early years till probably my mid-teens I was thought of—no more than my mid-teens, when I went off to university—I was thought of as a slow, okay kid at best and stupid at worst. So my sense of myself has never been that I was as competent as I have turned out to be. It was always this idea, am I fooling people, you know is there something surprising them.

Weinraub: Yes, and you didn't hear it from them at Penn while you were in your college years.

Lewis: I haven't heard it and I don't know if it was that they hadn't told it to me or I had been so grounded in this old belief of who I was, at least that part of who I was. So I actually thought of joining the CIA and came rather close. Now you have to understand, this is 1958, the CIA is only formed a few years before and no one has heard of it. I have to tell you this story because it is really quite amusing. To show you the quandary, I don't know what I am going to do now at this point of graduation so I started to look for jobs as another alternative besides law school, besides sociology and graduate school. I started to look at jobs. One of the jobs that the government was advertising was for this organization called CIA. There was no literature or anything about it. So I made an appointment with a recruiter and I go in and I say, "Tell me something about this agency," and he says, "Well, before I tell you I want to ask you a few questions." I say, "Fine." He said, "Well, what is your mother's name?" I said, "She's dead." "Doesn't matter, what's her name?" I said, "Lee. Last name, well, maiden name Cohan, last name Lewis." "Where was she born?" "Russia." "What's your father's name?" "He's dead." "What's his name?" "Bernard Lewis." "Where was he born?" "Russia."
At which point he terminated the interview. It took me years to uncover how I could have failed the interview by answering those four questions. So I didn't get into the CIA, which is probably fortunate because who knows with the kind of problem solving capacities which I subsequently realized I had, I could have been a chief operator in Poland or somewhere.

In any event, I decided that what I wanted to do was to do nothing, and I got a job. The first job I had was—I decided I was really interested in psychology. I was really interested in clinical. My sister wasn't in clinical, she was an experimentalist but she had already dropped out after having a master's and decided not to go on for a Ph.D., which is a sad story for her part, and she went out and started to work in educational psychology and was now essentially out of the field as I envisioned it. So I was interested in psychology, I was interested in clinical psychology, but I hadn't pushed myself. So I decided that I would work for the year. At the same time I decided if I was interested in clinical psychology why don't I start in analysis, which I did start in that year. So I went to work at my first job at a small marketing research company in Philadelphia. This was 35 years ago. It's not the Philadelphia of today, where we sit now, but it was downtown in what was the developed part of downtown. I started in the beginning of the summer. It was a small concern and I actually liked it. It was interesting problem solving and a first look at sex roles. It was at that time that the first book on sex roles and marketing came out; I've forgotten the name of it. We had one company that had—it was the whiskey company the Four Roses, and they realized that for a burly man to go up to a bar and say, "Give me a shot of Four Roses" was not very masculine. So the company changed their name under our tutelage to the Society of Four Roses. The bottle had a shield on it with four roses and so on. So we did a whole lot of these interesting marketing research things. The end of the summer—I had been offered another job at another marketing research firm. At the first marketing firm I was being paid $60.00 a week. The second one offered me $80.00 a week. I went to my boss, who was an old European sociologist interestingly enough, who had formed this marketing company. This was a brand new idea, marketing research. I said I prefer to stay here with him but I had been offered that salary; would he match it? Once again my spelling got me into trouble. As an old European he said, "You know, Michael, you can't spell very well and that's the sign of an educated man. I think that you should probably take the other offer." And so I wasn't fired—I might have been had I stayed on—but in fact left that first job for another job in a much larger firm. Now that larger firm was a true business organization in which you couldn't wear loafers, only laced shoes. In 1958 men wore hats; I didn't wear a hat. It wasn't until 1962 that Kennedy—or 1960—when he didn't wear his hat to Inauguration that broke the back of the Hatter's Association, and men stopped wearing hats. Every day I worked in that organization they asked me where my hat was and I could see that it was terribly hierarchical. I really got a taste of what business was like.

Weinraub: The "corporate world."

Lewis: And I decided that wasn't really for me. This was in the fall. I went to the psych department at Penn and I said to them I'd like to be a psychology major, do graduate work in psychology. Well, they took one look at my record—and this was Jim Diggory at the time.

Weinraub: I know Jim Diggory. [Marsha met him in her first year at Chatham College in 1966.]

Lewis: Oh, yea. Well, this was Jim.

Weinraub: His handlebar mustache.

Lewis: Yes, yes and big eyes. He had a thyroid condition. Obviously, I guess, he is dead now. He married—which is of course a side story—Sylvia. Four of the professors in my graduate class at Penn in psychology married their students in my year, but as I say that's another story. Anyway, I went to Jim and I said I wanted to do graduate work in psychology; I want to be a clinical psychologist. He looked at my record and he laughed because, although my grades in sociology toward the end had become very good, I had those language Ds and I still had the engineering which were Cs and Bs, which were enormously respectable in engineering, but not good for him. He said, "No, with this record you couldn't be admitted." So I said, "Well, that record doesn't reflect what I can do." I said, "I'm really this A student here now and this was the earlier me." He said, "No, those were the harder subjects. You did badly and then on the easy subjects like sociology you did well." So we disagreed for a while. I had admitted myself into the sociology program, I don't know how I did that and worked at the same time, I guess I just was on a roll. I said to him, "Listen, I'm in the graduate program in sociology. What course is your hardest course in the psych department? I'm working this year. I will take the course, and if I do well in it this will be a judgment, an on the job test of whether it was harder versus difficult subjects or whether in fact I've matured to do this." And here is the fourth or fifth and final time that my mathematics helps me. He tells me to take the statistics mathematics course, which is being taught by Bob Bush from Bush and Mosteller; the beginning of mathematics psychology. They are teaching this abstract mathematics course with a little statistics in it.

Weinraub: Perfect, no numbers to confuse you, all abstractions.

Lewis: All abstractions. Okay, all the hardware, the front part of my brain, doesn't function, all the abstract conceptualization, and he throws me into the briar patch. You know there is an old child's story about Br'er Rabbit and the briar patch—you don't remember—but in any event he says, "Take this course and that will be the proof." Well, I take the course while I am working in marketing research and not liking it very much, although there were interesting problems we solved. One of them had to do with the appearance of tampons as a product. It just appeared. Kimberly-Clark that made Kotex was really interested in why their market was being drawn away from them. The whole idea of the sanitary napkin, a necessity versus a sort of a luxurious thing, was coming into play and I did a lot of research on this.

Weinraub: Was this after hours?!

Lewis: No, no during, during hours and I got paid for it! So I took this mathematics course and of course I got an A in it. I just aced the course with no trouble at all and they admitted me. I never took the Miller Analogies. I never took the GREs. I had taken two psychology courses in my undergraduate career but was admitted to graduate school in 1959 in clinical psychology. Now Penn was in the midst of converting from a clinical to an experimental program. Robert Bush had come. Galanter was there. Dick Solomon had arrived. Phil Teitelbaum, Horovitz, and Jameson.

Weinraub: Justin?

Lewis: Well, Justin Aronfreed was just a brand new professor. He didn't even count, and a whole host of other people who became really stars.

Weinraub: Big experimental.

Lewis: Enormous, and it became one of the powerful departments in the country. The year before there had been 20 in clinical and five in experimental. The class that I joined in 1959 had five in clinical and 20 in experimental, and I was one of them.

I started graduate school now, full time. I had an NIMH mental health scholarship, so I didn't have to do TA or RA for someone else, so I could do my own research.

Weinraub: Now how did you get that if you didn't really come in the way other graduate students did?

Lewis: It's unclear, I don't remember it. I remember having it; I think they were available for all graduate students who wanted to do research or something. In any event, I started to take courses in clinical but I soon discovered that I was as interested in the experimental part. The one thing that stands out in my mind in the first year was I was taking a physiological course and it was being taught as a pro-seminar. Dethier and Teitelbaum were teaching about eating behavior. Dethier—Vincent Dethier—was talking about how he had discovered the mechanism for blow-fly eating. It's a fly. He had them on leashes and flying them around Hopkins where he had done the research before going to Penn's Medical School. Teitelbaum talked about the serendipitous finding of discovering that animals which he had bladed in some node in the hypothalamus would starve to death unless you force-fed them, would in fact survive, and they would recover their eating function. They discovered it by giving them a piece of chocolate randomly and discovered that they could eat something very sweet. I was absolutely enthralled. I went to them and I said, “I’m in the clinical program and I’m terribly busy and stuff, but put me to work in your laboratory. I’ll give you ten hours a week or whatever.” I just wanted to discover. So they put me to work cleaning out the rat shit out of the cages. Well, I did this for a week or two. I figured I probably knew more about rat shit than anybody else in the world, but it was certainly not very informative, so I quit, but it started to reflect my interest in physiological.

I guess my graduate school career can be characterized by really a thing which I think then indicates the formed person, the adult. Number one, I was interested in problem solving and so I started to do a lot of research on my own, just doing research all the time. So much so that everyone sort of parted the way as it turns out and just let me go and do it. So much so that, by two years after I had gotten my degree—which I did in three years, which was a record at Penn—I had six or seven publications: a *Psych Bulletin* article and all sorts of things because I immersed myself immediately in research. I took all the required courses and I had essentially three professors. In my first year I had Seymour Feshbach, who’s a distinguished professor out at UCLA in personality in clinical. He was my first clinical advisor. He was doing some interesting research but it wasn’t what I wanted to do, so I did my own research. The research I did was on the effects of social reinforcement of kids and from this I got to meet, within a year, Dick Walters and Jack Gewirtz, who were also working in this area, and so I started to become friends with the people working in it, the professionals. My second year I worked with Bob Cairns. Bob Cairns is also now a distinguished professor now at North Carolina. Bob had his own interests; we actually published a paper together because I did some things. The first paper I ever published, which was in 1961 in a clinical journal.

I got interested in a whole other problem that involved research with animals and rats and it was a motivation problem. If you made rats work hard for some food, reward, would they value it more than if they didn’t work hard for it? Sort of the Puritan Ethic translated into rats. I suddenly found myself running a rat laboratory. They gave me a rat laboratory and I had rats biting my fingers, hanging from my fingers, and here I am doing the clinical program at the same time. Facing patients and going to work at the hospitals, at the local hospitals, and seeing these schizophrenic adults in back wards and VA situations and the rest of the time I’m sort of in the laboratory. I also discovered that research should be fun, that it should be serious but not solemn. So when everyone else’s T-mazes and runways were black, gray, or white I discovered these rats are color blinded, I’m not color blinded, so my mazes were colored. They had the same reflected light from them as a gray maze but it was a light blue which I liked to see better than the rat. So people used to come in and visit my lab and see these crazy color mazes. And of course I got involved in animal research, which I did in my second year.

My third year there was still no developmental program of any sort. Justin Aronfreed was there, but he was really in the clinical program more than in the developmental. Remember that this was 1961 now and it’s beginning to start—development—but it’s not really caught on. I’m starting to read Piaget; there’s Flavell, whose book is I think ’60 [actually, it was 1963]; there is Ben Bloom’s book in ’60 on experience [Bloom, B. S. (1964). *Stability and change in human characteristics*. New York: John Wiley & Sons]; and J. McVicker Hunt’s book in ’61 [*Intelligence and Experience*. J. McV. Hunt. Ronald, New York, 1961]. So I’m beginning to start to read in this and I’m starting to get curious about kids, but at the time I’m in clinical, doing clinical and I’m working with rats. So I decided my third year—but I had been working with kids in other ways: motivation and social reinforcement work with kids. I’d also been doing binary choice research or probabilities in kids and published, now over 30 years ago, a half a dozen papers on binary choice. I knew all the theory and the mathematical stuff, which of course had that old appeal to me. So I had to now think of my dissertation, and the person that I really wanted to work with, who had discovered my work, was Dick Solomon, but he was in the experimental program not in the clinical so he couldn’t be my advisor. But Dick saw that I was doing interesting stuff, or what he thought was interesting, and he gave me RA support so I didn’t have to be in the lab day and night every day with these rats. It turns out, of course, I was studying effort which he had done his dissertation on in 1948. I believe Olds had done his dissertation on the same topic with Dick. Effort had to do with retroactive inhabitation in the old Hullian sense. It had to do with inhabitation, which is exactly what Dick Solomon's major contribution was in psychology around the Principals of Inhibition. So he invited me into his lab where I met Marty [Seligman], who had just come, and Russell Leaf and Bruce Overmier. They were doing all this learning stuff which I wasn't interested in, and I never felt a part of that group. I rarely attended their meetings, but Dick saw me as making a contribution to what he saw as what his laboratory wanted to do. So I decided since I had to work with Justin Aronfreed, and I wasn't interested in moral behavior which was what Justin's interest at the time, I decided to switch from rats to children and I worked with children of different ages on the effects of effort. In fact, I used Jim Olds' dissertation machine, which dispensed chips on the basis of how many turns per chip you could do.

Weinraub: And you used M&Ms, is that right?

Lewis: I used M&Ms as the rewards and so on. So Justin was my advisor, but Dick Solomon was really my advisor, because it was out of his funds and so on that my support was coming. I did my dissertation and, now looking back, I didn't realize it then, but I was invited to give a colloquium in the department. Now no graduate student gave a colloquium, and Penn at that time already had matured in the few years and had become a powerhouse. Now it was only a few years ago that I discovered the colloquium notice that I realized and remembered that they had asked me to talk about this work, and I gave it. Again, I had no feel for what people thought, but I had sort of found what I was good at. What I was good at was really a set of things. One, asking what I hope are good questions and, two, knowing how to answer them.

Weinraub: How to set up the experiment, how to phrase the question.

Lewis: Exactly. So, for example, in my rat studies I had rats with rubber band harnesses that I devised for them and they pulled weights along runways.

Weinraub: That was the effort.

Lewis: That was the effort kind of thing. There was a kind of a flare for what you might call methodology, but it was also in the service of what appeared to be interesting questions. I must tell you that I published both in the animal, like the *Journal of Comparative and Physiological*, all the animal stuff; reviews in the *Psych Bulletin;* and in *Child Development* I published my dissertation with kids. I think it may have received one reference in 32 years. It's not exactly my hit contribution, but nonetheless, it is there to be seen. So in three years I was finished which was an absolute record. What happened was in the summers I did my internship in my clinical and they allowed me to get my Ph.D. in clinical and experimental without a year's internship in clinical. I was still doing analysis, which had now changed more toward training as I was seeing patients and discussing them with my analyst. As an aside, I didn't see him for 25 years and we have now become good friends. He actually refers some cases to me.

Weinraub: Is he still in Philadelphia?

Lewis: He is still in Philadelphia. He stopped being an analyst himself; in fact, he told me just recently that I may have been his last classical analytic patient that he had on the couch and so on.

Weinraub: Was he at the Institute [Pennsylvania Hospital Institute of Psychiatry]?

Lewis: He was at the Institute. It was a few years ago that I was completing this book on shame that they invited me back to give the lecture. I suspected he would be in the audience and, lo and behold, he was and he came over to me and he remembered me. It was really profound. Not only did he remember me but he remembered my wife's name, he remembered my sister's name. Now that was almost 30 years ago, so I figured well maybe he looked it up in his records, but you don't keep records for 30 years. It's just the kind of guy he is. He was both proud of me and at the same time he stood with me and allowed me to receive the attention. It was sort of like I saw him like a father, but it isn't really because no one's father is that way. But it's what our fantasy, the idealized father who essentially marches off together with the son as equals into the sunset having both achieved and feeling good about themselves, each other, and their lives.

So in any event, so I did it in three years, which was at the time and probably still stands as a record, certainly in the clinical program. The only other person I think who did it in three years was Eugene Galanter who was both a student at Penn and had become a professor. Now he is a professor up in Columbia, probably near retirement. So I graduated, I graduated at 25 in three years as a Ph.D. in experimental and clinical and an interest now in developmental and I still didn't know what I was going to do because the idea of academics never hung on me as the Halls of Ivy. I never had that feeling, it never belonged to me. In a certain sense I think that's another important event in my life. That is I think this idea of being a dyslexic, being removed from the track, essentially removed me from the group and if there was something to characterize my career it's been that I've been a part of but never really a complete part of. I've been a member of SRCD for over 30 years. I've been a member of all these organizations, but I—and I would assume have made contributions, but I'm not part of an intimate group. I have friends. I go to all sorts of meetings and all sorts and I've never really had a traditional academic appointment. My life has simply not been traditionally academic. And as I try to explain it now—you know what explanations are worth, they are simply ways of accounting—it's not necessarily true. It's really that because I never thought myself very competent intellectually I really never felt I belonged. And that in contribution with the fact that my mother died early and I was different from other kids and alone and that by 18, again, I had to do it all by myself, I didn't have much support essentially and I guess I am very much of an individual person as opposed to a group person. I say that on one hand; on the other hand I have friends for a lifetime. I've been married and live with the same person for over 30 years, so it's not as if I'm an isolate. I don't see it necessarily as pathological, but I see it as a character characteristic. It's a "here's where I stand, come join me if you like my ideas, if you think I'm doing something useful. I'll join you if I like what you are doing." But my natural posture is not to be part of a group.

Weinraub: And not to be part of the Academy.

Lewis: And certainly not to be part of the Academy. So in a certain sense that presents a kind of struggle, I think, for me in the sense that if you are to receive from the group its rewards, the group has to feel you are a member of it. And I think in a certain sense my standing somewhat apart has the effect of limiting the kind of influence I might have had if I had been more a part of the group. So here I am obviously a senior member in the field, but I don't belong to any. You would have trouble characterizing me as well. What am I? Do I do cognitive? Sure. Do I do attention? Yes. Do I do physiology? Yes. Emotion? Social? So we will get to contributions, but the fact of the matter is you can't find me in a group.

(pause in tape)

So the earliest, as I said, this earliest book is 1704 I believe and it's in Spanish and Hebrew. Published in Amsterdam. So, two things: one, that my family was collecting books 300, almost 400 years ago; and two, it meant that they have to be city for the most part, had sufficient income and were learned. I don't know earlier where they were.

Weinraub: That book was in your father's collection?

Lewis: My aunt's collection. She kept it because my father essentially renounced his background, which I will come to. So the next thing I know that in about 1800 a relative of mine is the Chief Rabbi at Leipzig. And what you are going to see is that my family moved eastward through Europe until at about 1880 when they migrated to the United States. The next record I have is in Krakow and then in Russia, in I believe Kiev.

Weinraub: The migrations were to escape persecution?

Lewis: I don't know. They were city dwellers, they were educated. Many of them were rabbis and they had money because of the book collection. For example, I have a personal Torah, a scroll. I have a full set of the Talmud in Mishna. I have, actually, several books that I've now discovered are quite valuable because they're rare. For example, I have a first edition of a Bible translated from a dual translation of Hebrew and German with plates. It is probably quite valuable, I've heard it is. So they moved in this direction and then my father's family came to United States where my grandfather was also wealthy, learned, and religious.

Weinraub: They were Orthodox Jews.

Lewis: They were Orthodox but not fanatical. To show you what happened, when my father came to this country, when he became old enough, he added *W* to his name. I asked him once what the *W* stood for and he told me Wesley.

Weinraub: Bernard W. Lewis.

Lewis: And Wesley was really exactly correct. My father turned Protestant. I understood none of my roots, which my father totally had given up as he Americanized himself, so it's really this European discovery. But there is no question, in terms of esthetic and interest in orientation, that I've been European and I have always been. When I go to California I feel more foreign than when I'm in Paris or Berlin or Rome. So it's a European family, I'm a first generation American. My son for a while lived in Rome and I thought, my God, this whole line would return.

Weinraub: So that's another aspect of your feeling different, not only in terms of social class, not only in terms of family issues, but also in terms of your father not having a place within a heritage that felt comfortable as you were growing up.

Lewis: Right. This may—I mean, you know, in some sense if I were to introspect, which who knows if this is the occasion for, but I have thought about it in terms of my own career. This standing independently, not necessarily buying into something unless I was really convinced about it again characterizes who I am and who I have been. I wrote on attachment long before there were attachment paradigms. I was interested in the concept, as I've remained, but I've never bought the baggage, what I consider the baggage, of a set theory, of a set methodology. So although I believe in social relations and the idea of attachment, I certainly don't believe that attachment is and only is that thing. And I think that must come from this kind of standing alone kind of position, this kind of being alone.

Weinraub: We left you last emerging from University of Pennsylvania with a Ph.D. in experimental and clinical in hand and, as I remember, your next research job was at Fels Research Institute in Yellow Springs, Ohio. Is that correct?

Lewis: That's right.

Weinraub: How did you get to Fels and what were you studying there?

Lewis: Well, this is what happened. I'm finishing up in three years and I guess I still don't see myself, as I have tried to say it, somehow with a gown in academia. I don't have a good feel for it. It's not that I don't, but I just don't know. But it seems quite clear to me that in higher education scholarship is the payoff. That's what higher education should be about. That's what it was about at Penn when I was a graduate student. All my professors were significant figures not because of what they taught but because of the research that they did. In fact, Penn at that point had very few classes. What they had, in fact, was research courses, so I began to think, well maybe a research job would be the kind of thing I should have. Remember that Justin Aronfreed was my professor and what you probably don't remember was that Justin Aronfreed, Marty Hoffman, and Larry Kohlberg were the three leading people in moral development. Larry, of course, took the Piagetian. Marty Hoffman took to psychoanalytic, and Justin took a kind of learning position. So it was certainly the case that they were all young men struggling as the field was with these kinds of positions. So Justin was well placed. He didn't choose to have a highly visible research career; he was certainly at the beginning in that scene. What happened was, in my senior year, he said to me, "Jerry Kagan is looking to hire a young Ph.D. out at Fels to help him on a new project he wants to start on infancy and attention." Now Jerry had just finished with Harry Moss the Birth to Maturity, the first analysis of the Fels longitudinal data, and had discovered in some sense they had no data on the zero to three period and had discovered that infancy might be important. This is 1961. Jerry had never done any studies with infants. Indeed, he had been mostly in personality and social kinds of development. And so Justin recommended me and I was invited out to Fels in Yellow Springs, Ohio. I went out and within ten minutes of talking, as I recall it, Jerry and I really hit it off. We're emotional, we're enthusiastic, we were excited, and he offered me this job. Now it was kind of interesting—again, this is reconstructed history, who knows, obviously the best guess is—but as I recall I remember saying to Jerry, "I don't think I am going to do very well working for someone," and Jerry, as I recall, said, "Well, I'm certainly not going to work for someone." So we agreed we would do this collaboratively. That was very important for me in part, again, because of this individuality for me, but also I think in part because I was already doing research and no one had been my supervisor. Although I had professors, they weren't Dick Solomon. When I used to go to Dick and say, "Gee, I don't know how to do this, what do you think I should do?" Dick used to say, "Michael you know more about this than I do. You're working in it, what's your guess?" So Dick had this wonderful approach of bringing out the best in a person by making them think about the problem. So I didn't see myself as being a grad research assistant.

Weinraub: Now it wasn't clear to me regarding how Jerry Kagan viewed you. Did he view you not as a person that would work for him but that you would both be collaborators? He didn't feel comfortable having you work for him?

Lewis: Well, it was a bit presumptuous on my part. Jerry was already a very significant player in the field and he had already—this being 1962—had been offered a job as the first professor of development at Harvard. So here I was, wet behind the ears coming out, but I think in all honesty Jerry saw that I was not a run-of-the-mill, you know, kid and likewise he had an appreciation himself for supporting research and young investigators.

Weinraub: So he acknowledged that independence.

Lewis: I think so. You know, how in the world can you 31 years later recall this, but this is my recollection. Our recollection was that we agreed in principle to a collaborative kind of activity. Now it is obviously in my favor as a youngster starting out and he as this seasoned pro that I could convince him. Thinking back over my career and my students it was always the case that it was always their ideas, but I don't make the claim certainly with Jerry. When you get old enough you start to understand when you do it with your own children or your own students, you begin to realize what you've done too. Jerry was, in a certain sense, a very important figure for me along the line. What I had learned about craft was considerable—the craft of doing research, asking problems, and solving problems—but Jerry is also master at that. There is just no question, whatever else you might want to think of his work and his contribution. Jerry has always asked good questions and he's always had novel ways of answering them. So in one sense Jerry showed me that you could do that and live in the world, I mean, you could survive. It is different being a graduate student and being a professional. Jerry was and remains enthusiastic, that's what keeps him alive. I'm enthusiastic and was that way with more energy and more enthusiasm, so I saw that enthusiasm not as a deterrent. In fact, you could be emotionally excited about what you did and that wasn't a negative, which offsets some of the solemnity that many of my colleagues think is a necessary part of science and certainly the science of psychology and developmental psychology. You have to be serious, but your mazes don't have to be gray and black, they can be blue and pink if you like, but you have to know the saturated lights, you have to know all the things that occur. So Jerry offered me the job, and it was a very good job, it was a research job. It wasn't a teaching job and it was at Fels, which I discovered was and remained for most of its career an absolutely essential center for developmental studies in psychology. It had a distinguished group of scientists out there and a distinguished number of people in our field passed through.

Weinraub: Would you like to name some of them?

Lewis: Well, I could, let me just go back. There was Sontag, Les Sontag who was the director for the vast majority of the life of the Institute, followed by Falconer who was in physical growth, but by then the Institute was in its last legs. Supported by the Fels family of Philadelphia, and the thing that did Fels in was that the vast majority of its money through 1965 was foundation money, with grants accounting for a small part. But when it grew, it grew on grant money and then it couldn't sustain itself without grant money so it became soft money and it became difficult. So there was Les Sontag for us. There was Ward Crandall and his wife Virginia Crandall. There was Jerry Kagan, there was John and Bee Lacey. Elliot Valenstein was there. Stanley Garn was there. Elliot and Stanley ended up at the University of Michigan, Elliot in physiological and Stanley in physical growth and genetics. Lee was there as a psychopediatrician before she went back to school and got her Ph.D. and now is at Cornell. I was there. Then after me, Bob McCall and Ross Parke were there and that was toward the end. That is by 1970, I think, it was in the throes of difficulties so it was quite a significant group. It had one of the few ongoing longitudinal studies that were available, and that was remarkable. There should have been a second volume to Birth to Maturity that Virginia Crandall was working on that somehow never got finished. I don't know why, but there were two cohorts that could have been analyzed. Those data exist and it would be wonderful if someone could resurrect it.

Weinraub: Are those the data that Bob McCall was working with?

Lewis: Bob worked with some of that data, that's right. But Bob's first monograph on IQ was really with the early Bayley data which he got from the other longitudinal study, the Berkley Growth Study, which is where Bayley was, and where the Bayley derived from was the test that they had. So I arrived in the fall, September, of 1962 just having finished three years and I simply continued doing what I had done before which was do research. I had some interests of my own which I pursued and continued to publish in binary choice, in social reinforcement, and wrote up and published the studies I had done. I was clinically trained so had 15 hours a week of clinical patients. I had just gotten married to Rhoda who I have been with since. She hadn't finished at Penn her undergraduate, and so she went to Antioch. I had an appointment in Antioch and taught a course. So I taught an undergraduate course, I had ten to fifteen hours' worth of research, I was a newly married person, I did clinical, and I did my research.

Weinraub: What kind of patients were you seeing clinically?

Lewis: I was seeing—remember this is 1962, it's the beginning of the 60s—I was seeing Antioch patients for the most part. So I was treating adolescents with all sorts of problems from psychotic to simply character disorders and neurotic behavior. I was 25, some of the students—it was a five year program—were 22. I, of course, looked like I was 12. I grew a mustache with some attempt to look older but nothing seemed to work. I had a very interesting clinical practice because Antioch had a work-study program and they were on campus for three months and off so you had to think of therapy within a kind of a bundle of a short-term therapy and we worked on short term. I was psychoanalytically driven, having had analysis training and a clinical background and that was the kind of therapy that I practiced. When I came to work at Fels Jerry was interested in attention, which was Lacey's interest, interested in psychological heart rate. John gave us a lab of his and I remember—although I've asked Jerry about it and he doesn't recall, but I think that makes sense why I would and he doesn't—our first subject. We were going to do studies of attention and we were presenting the child with visual stimuli. At that time we delivered it by simply holding up a card which had a bull's eye or a nursing bottle. I still have those stimuli, by the way, they should be archival material. It was our first beginning, and we didn't even get looking data. We simply looked at heart rate and respiration. Jerry knew a little about it, I knew very little about it, but Lacey was mother hen to us and he was always in the lab and telling us what to do and so on. John had a very forceful personality. I remember the first child we had was a farm woman's child, it may have been her seventh child or something. She threw that kid around like it was a sack of potatoes. Well, Jerry had a child, Janet, who is actually now married to Steve Resnick, who is a professor at Yale now and was a student of Jerry's. Jerry wasn't—I don't think in those days he was really work oriented and men didn't have much to do with young children, so he had very little experience with children as well. I remember we held this baby like it was made out of glass. We thought it would break, we didn't quite know what kind of creature it was. Rats I had handled and kindergarten I had handled and college students, but I had no experience with babies at all, just not in my life. So this was a very fragile and delicate thing. Well, it was either this subject or the next subject that did a bradycardia. That is what happens: it's a breath holding response in which if you are looking at respiration it disappears and heart rate plummets down. I forget who was looking at the polygraph at that time, whether Jerry was looking at it and I was running the baby or what. As I recall, Jerry was running it and he came running in to announce to me that he thought we had killed the baby because the baby wasn't breathing and his heart rate almost went to zero. But the child was fine, turned a little blue and then just, you know, just pepped up. The mother wasn't concerned, but we were two very frightened researchers doing this. After playing around a while with a couple of things we hit upon some kind of paradigm where we were looking at auditory and visual attention. The first paper of which I gave at APA in 1963 at a symposium that included myself, Bill Kessen, Lou Lipsitt, and Bob Fantz. Now these were already distinguished workers in the field who were well known and Michael Lewis who was a complete unknown. But, you know, there is a sub story. There is a joke about someone who knows everyone and he is standing next to the Pope and someone says well who's that person standing next to Joe, and it's the Pope. Well, it's the kind of—I think of that only in the context that if you stand amongst giants and you are not one you just simply become it by association. Well, I gave the paper and I could put on my resume—then you used to have an abstract, which would appear in the *American Psychologist* for APA, and so my first paper in press, in publication was an abstract in APA. We then gave the paper at the Merrill-Palmer Conference. Jerry gave the paper and when it was published it was Kagan and Lewis.

Weinraub: Was that 1964?

Lewis: 1965, I believe that was that paper.

Weinraub: Was that the monograph?

Lewis: No, it's that simple paper on studies of attention. It's a long paper in *Merrill-Palmer Quarterly*. It predates Frances Graham and her work on heart rate, because she was doing the same thing with babies in Wisconsin taking off from John Lacey's theory as we were. Lacey's theory, as you might recall, was that heart rate decreased when you paid attention to external events. So we were working strictly within a cognitive domain, we were working on attention, and we developed measures on how to measure heart rate and also fixation. So by 1965 Jerry had been pestered sufficiently by Harvard that he decided to take the professorship. We had started a longitudinal study and I stayed on at Fels until we finished that longitudinal study.

Weinraub: That lasted 28-30 months or so?

Lewis: Yes, the fact of the matter is that nothing much came. We published quite a few papers from the study, but the longitudinal study never got published. So Jerry went off to Harvard in 1965 and here I was now just essentially the investigator with my laboratory doing this. I had already started to get my own research money. I had grant money from NSF and NIMH at the time, and I had grants so I started to focus on infants. The first area that I worked in was perception, attention and physiological which were the leading edge in the infancy literature.

Weinraub: In some ways people could say that infancy research began not only in the Midwest but in Ohio.

Lewis: I think that's right. You had Fantz; I really hold Bob Fantz in part responsible for the surge of interest in infants. The reason was, we had thought of the infant in terms of James' terms of a mass of confusion and an unorganized, unthinking, unperceiving, let's say nonperceiving, organism. What Fantz showed as early as '63, '62—

Weinraub: '57.

Lewis: —1957—that's right—was, in fact, that the infant, even though the visual acuity was somewhat limited, its distance in which it could see well also limited, could in fact see and process and have preferences. The same time I think Bill Kessen was doing the same thing at Yale. But it was Fantz's work that really caught people's imagination. The important thing was that, as a captured audience, infants could be studied because there was something there to study and that was really the heart of it. So in a certain sense my career started in infancy. Infants were serendipitous; I worked with infants because it was a first job and it was around the subject of attention. I must tell you that my work on effort with rats convinced me that the reason why they valued it more was that they paid more attention to it. That was my underlying theoretical explanation of why, when you work hard for something, you value it more and the reason is was that you focused more of your attention on it, and so as the interest in attention was triggered by these old rat studies so was I interested in attention. The idea that there was a physiological part stirred up my old yearning in a certain sense for what I had tried to do in my first year in graduate school which was get interested in physiological.

Weinraub: In your early works in the '60s you talk a lot about the Russian psychologist Sokolov and that seemed to have a very important input into your theorizing in attention and response decrement and why you took that paradigm.

Lewis: Right, I would say that of the contributions that I, and I may be the only one who was tagged, that I made the first—I'm probably, that is—no, I would say the first, not the longest lasting, it's longest lasting because it was the first—was the attention paradigm. What happened was we had presented stimuli to children—we were interested in the contents of the stimuli—but we presented each stimulus to the children seven times or something so a child saw for ten or fifteen seconds an event, and then saw another event, and then the same event and we did this. One day in my office, I remember this, I was graphing the data, and I graphed it by stimuli and I graphed it by the trial that that stimuli was presented. Lo and behold, what I noticed was a response decrement to all the stimuli. I suddenly realized that there was a phenomenon here which was a phenomenon of boredom or inattentiveness. Exactly how I got to Sokolov, whose book was in '63—so you see it's exactly at the right time—I don't remember. I still have the book. But Sokolov talked about it, as a student of Pavlov's, talked about an orienting and a defensive reflex. The orienting reflex contained looking and had physiological changes. I suspect Lacey had known of it and that's where I have found—and so I invented—and I clearly am the first—invented a paradigm for looking at habituation and dishabituation. Now what is not known is—

(pause in tape)

—you know that in science papers, old papers get referenced less and so there is a five years kind of—

Weinraub: You only go back five years.

Lewis: Right. And so few people are going to bother to ever go back, but in the history of the events which this is what this interview is all about, in 1967 I did a paper which is published in the *Journal of Diseases in Children*, a very legitimate journal in pediatrics, a paper on habituation and dishabituation, and differences with children on the basis of their Apgar scores. I found, in fact, that children with less than good Apgar scores showed less habituation and less response recovery. In 1969 I published an SRCD *Monograph* on habituation where we showed that it was clearly a memory phenomenon. I showed that if you varied the inter-trial interval between presentations you could switch the rate of habituation and recovery. It is the first paper showing that the rate of habituation is significantly correlated with IQ at three-and-a-half years and significantly related to other cognitive tasks such as—now I've forgotten exactly what they were, but I do believe they were concept formation tasks. So here there is a monograph in our series in 1969 which clearly marks the habituation/dishabituation paradigm.

Weinraub: And continuity across cognitive changes.

Lewis: And continuity across cognitive changes and that it reflects individual differences even at birth. So what we had—what I had done certainly before the '70s, long before, since the research was done early—was to capitalize on this particular paradigm. Now the paradigm went out of favor for a variety of reasons, replaced by the familiarization/novelty paradigm. The interesting thing in the history of this is that it was never demonstrated that it was not a good paradigm. It simply, in a Kuhnian sense, was replaced by another paradigm. In fact, in a study—this is kind of amusing—I had a graduate student here at Temple—

Weinraub: Kathleen Gerrity.

Lewis: —right—who did a study in which she gave children the two different tasks, the familiarization and the standard habituation/dishabituation that I had used. The data somehow vanished and we were never able to do it, and I never did it again, but what the data showed very clearly, and I remember this, was that there was a 0.4 correlation, quite significant, between rate of habituation and recovery versus familiarization and novelty so that the two things are highly correlated. I know of no study in the literature which ever took both of them, compared the results to both techniques and then compared it to outcome. It's never been done. There were certain problems with it and so on. I must admit we still use it and used it as late as 1981, which is the last paper that I really did in this field, and that one still gets referenced. That's a paper with Jeannie Brooks—probably Jeannie Brooks at that point, and not Brooks-Gunn yet—which appeared in *Intelligence,* although I must say that whenever the thorough reviews are done the '69 monograph is mentioned but never the '67. The '67 simply has vanished so if one looked at the scheme of things you would find that a published paper in '67 on habituation and dishabituation probably marks the beginning, but there were a variety of early investigators: Les Cohen, Bob McCall, they were somewhat later, Fantz himself, and Joe Fagan who got involved with Fantz and Fantz's laboratory.

Weinraub: And that was in the early '70s.

Lewis: Those were ready in the early '70s. So habituation/dishabituation, the use of heart rate and visual attention and the different fixation measures was, I think, my first contribution to the field which hadn't been done before, and each of those two measurement devices and the paradigm were the first contributions that I think I made.

Weinraub: The other contribution that I see as very significant is in '67. You had a paper looking at mother/infant interaction at the same time as Howard Moss had his paper in *Merrill-Palmer Quarterly* also. I think they were both in the same issue.

Lewis: Right.

Weinraub: Studying and observing mother/infant interaction in a real home situation and exploring it behaviorally sounds like something that wasn't very popular at the time.

Lewis: No, and I think you are right. That would be the second thing. In fact, there was a time when I received some accolade for that. I'll tell you how it happened. First we had this paradigm in which we got individual differences in kids' attentive behavior. Now the question was where did it come from? Being one who believed in the effects of environment as opposed to set dispositions, I looked to the environment, and here I think my clinical background, my interest in social and emotional lives, came into play; I looked at the mother and the mother's treatment of the child. Two things resulted from that. One, I think, was recognized more than the other, but two things occurred. The first was I started to observe mother/child interactions in an attempt to find out how you would describe that environment. Here I was very much influenced by the ethologists. Lorenz just won his Nobel award, in close proximity so ethology had become legitimate. I decided an ethological approach was a valid way, so I became an ethologist. The reason why I am sort of pleased with that is that I actually became a member of the International Ethology Society and, I forgot the year, but we were in Parma. I was invited as a guest of the International Society to be one of the three plenary speakers. Lorenz was another one and Robert Hinde was the third. They were, of course, ethologists, and I was invited. At the end of my presentation Lorenz, whose history as a Nazi I hadn't appreciated, got up and asked the audience to make me an honorary member of it, and so it was from the mother/child that my interest in attachment and other things derived. So I found myself, in observing the mother and child, starting to branch now away from simply perception physiology and attention as cognitive physiological variables into the social domain. So I started to devise systems using the way ethologists did—the primate people, for example—had for years, observed what did the mother do, what did the child do, and so on. That's when I started to publish. Of course, as I did so in this paper that you make reference to one of the things I noticed was there were some sex differences that appeared and that was of interest to me. Again I think, again out of my clinical sense or just maybe just an interest in an individual difference, which again was clinical. I have always tried, I hope, not to simply stick with sex difference, qua difference, but essentially to use it as a vehicle to talk about process differences. What I believed was that mothers behaved differently in response to girl children than to boys. In fact, those old papers demonstrated quite convincingly to me that when children vocalized mom's behavior was quite different to a girl child than to a boy child. So I started to watch that and started to move in that direction. The second branch in that was truly a discovery that the important features of the environment, one important feature for sure, was the contingent nature of the environment. And in 1969 I published the generalized expectancy_paper. Now that paper predates a helplessness paper by Seligman by a few years, although I did know of his work and referenced it as an unpublished monograph that he, Mayer, and Solomon were doing at the time. In fact it seemed to me to fit very nicely what I was observing.

Weinraub: Now, for the record, I just want to plug that paper. It's Lewis and Goldberg, 1969 in *Merrill-Palmer Quarterly*. That I think is one of your unsung great papers.

Lewis: Well, I must say again it's probably a highly referenced paper. Indeed, of course, it became important within the attachment framework when the idea of contingent environment was one of the key features that made for secure attachment, or the belief. It was never measured in the way I was doing it because I was doing it as an ethologist. I was looking at when the child emitted a behavior, did or did not the mom emit a behavior? I must say that's been a continuing interest, and we've now done a whole series of papers which I think are even more important. Later papers in terms of working with machines that are contingent and non-contingent to learn about what I think is a fundamental property of certainly the mammalians, including young human infants which is to seek out and pay attention to contingent events. Now this goes back to the old, old literature in psychology long forgotten about, and I've forgotten what it's called at the moment, where two events occur simultaneously, associations are made. It goes back to—I forget what James called it—back in the old learning kind of thing, but the idea is the co-occurrence of things, that is the mammalian—

Weinraub: Thorndike talks about his—oh dear, I forget. I'm having a blank too.

Lewis: Some term, I forget what it is.

Weinraub: Something learning, associative learning, but there is a particular term.

Lewis: That's right but there is a particular term for it and as it turns out to be very powerful. I mentioned all the people—White who had talked about efficacy from the psychiatric point of view. I talked about the little that there was around at the time and talked about this contingency. Again showing—still hanging on to the cognitive—showing that this contingency was related to attention in the habituation/dishabituation paradigm. So I was still doing that, but now had branched out toward the social.

Weinraub: Was that your first foray really into the social?

Lewis: No, I had been moving in that direction with Susan Goldberg in 1969. At that point, she was not even a graduate student. She simply was someone working for me at Fels in the lab. She subsequently, after her personal life altered, went back to graduate school at U of Massachusetts, got her Ph.D., and then went on and has done her things mostly with dysfunctional children.

Weinraub: And premature children.

Lewis: Yes, premature.

Weinraub: But there's the Goldberg and Lewis 1969, *Child Development*.

Lewis: Yes, and that is the sex differences in play behavior, which again starts to mark what was my interest, and now looking at the mother child relationship at a year of age. Remember that's published in 1969, so you have to understand, and I'll come back to it, what has been going on in the social domain that I am now suddenly finding myself a part of. So I start to shift essentially five to ten years after I start off in the cognitive/physiological attention. I now start to find a new audience that is attracting my attention in the social. That may have something to do with the fact that in 1968 we have our first child. My son is born and we leave Fels, which is heavily physiological, and I move to ETS. ETS is educationally oriented. Now I am in an environment which is much more supportive, if you will, of environmental rather than physiological/cognitive kinds of things. But at any event, the ideas had clearly brewed before because they were published at the time that I left. So in the paper, the 1969 paper with Susan, what we found was that at a year of age we found children behaving differently to their mothers in a free-play situation and in a barrier frustration situation.

Weinraub: Now what is so important about this article I think, Michael, is that it has pictures in *Child Development*. You can pick up a *Child Development* in 1993 and you are not going to see pictures. Those pictures were very expressive of sex differences, with a little girl crying, at the middle of the barrier crying and the boy at the side of the barrier clamoring to get around.

Lewis: Right. Now I must tell you that the phenomenon is real. A few years ago, I've forgotten which, one of these talk people did the Human Animal, a series on primetime, Phil Donahue. They came down and they filmed it and we had three girls and three boys lined up for it and figuring that at least one of the six or two of the six would show the kind of thing for TV, it turned out that all six did exactly; the three girls cried at the center of the barrier, the three boys tried to knock it down. Everyone was convinced of the reality, you know, you never get a phenomenon like this where every kid—

Weinraub: Twenty years later!

Lewis: And it was 20 years later! But what goes unnoticed in that paper is that we looked at the mother/child behavior at six months and found and predicted what children did at a year. What we started to look at now was ways of trying to conceptualize that relationship between the mother and child. Now, we used the free play situation and we also used a separation situation. And we used these situations because we were interested in what was now called attachment by everyone. So now let me back up a bit and switch scenes to another area of psychology in development which was going on parallel, also from the '60s, but not in the attention/cognitive/physiological but in the social arena. That's an enduring interest that goes back at least to the turn of the century in modern times of the child's relationship to significant others in its field. You had early studies about children being separated from their mothers, and these were studies by Rene Spitz and Goldfarb of anaclitic depression and hospitalism. These studies had to do with foundling homes. And you also had the Turan studies of McVicker Hunt—

Weinraub: And Wayne Dennis.

Lewis: —and Wayne Dennis. And what you had was from the psychoanalytic tradition the idea that the mother was a significant figure in the child's life. Now what was quite remarkable about that literature was that it also fit in with the ethologists, especially the primatologist, and that included Harry Harlow as a dominant figure. Harry's studies of motherless monkeys and so on in the late '50s was also fitting in to show a kind of universality of the importance of the mother. There were all sorts of people working in that domain. There were people like the Gewirtzs, there were people, as I have already mentioned, like the Harlows, there was Mary Ainsworth and there was Rudy Schaffer, both of whom were working at the Tavistock working with Bowlby who was a psychoanalyst also interested in this question. There were the Robinsons, there was a diverse group of people who were active in animal ethology in psychiatry and in psychology in the child's relations, social relations. There had been a series called the Foss Series.

Weinraub: Four volumes: *The Determinants of Infant Behavior*, Volumes I through IV published in '61, '63, '65, then '69.

Lewis: It started in the late '50s and all the players—Harriet Rheingold, mustn't forget Harriet—all the players trying to ask this question: what was the nature of it, how did you measure it, and was the mother significant?

Weinraub: And the emphasis there was on the early parent/child, really mother/child relationship.

Lewis: Right, but it was unclear. As I say, there were these three questions: how would you measure it, what importance did it have, and did you need the mother, was the mother *the* figure? Now I must tell you, although Spitz in the '40s had argued for the importance of the mother with anaclitic depression and hospitalization being the consequence of losing the mother, by the late '50s the argument was more along the lines that it was the lack of stimulation. It was the lack of what mothers provided rather than the lack of what the mother was that was gaining the upper hand. And in fact there is a Yarrow paper in which he summarizes the work, again no longer referred to, and concludes that we could not possibly tell it was the mother because it's confounded with the nature.

Weinraub: Was that '61 or '69?

Lewis: Yes, I think it's '61. Now understand that that gets translated if you like, especially in your interest, to daycare. Is it the lack of the mother or is it a rotten daycare? And since we have usually a confound, lack of mother is rotten daycare. For most people then you don't know if the child doesn't survive because of its lack—or not doesn't survive, doesn't do well—because of its lack of mom or because of its bad daycare. The argument played out again 34 years later and now is pretty much unknown. You don't find this argument referred to in the literature. So none of us who spend our lives doing this should ever be discouraged to feel, oh, we're not being referenced anymore, because it's the nature of the field.

Weinraub: Well, it was pretty much settled and Michael Rutter did a few summaries of this.

Lewis: The Robinsons have a wonderful—

Weinraub: Their studies are beautiful too, and Chris Heinicke's work. We thought it was resolved, but the ethos pulls it back.

Lewis: Well, Bowlby has an enormous influence, when his *World Health Report* is published.

Weinraub: That was 1951.

Lewis: Yes. He argued that the child doesn't survive if it doesn't have a mother. So he brings the argument back, if you like, to a biological necessity to have the mom. Whatever the theme was it gets picked up again, and we were still struggling with it in the late '60s. Mary Ainsworth is probably the best example of addressing this question in the 1960s. Remember, we had three paradigms depending upon how the child stayed close to the mother. We thought of attachment as following the mother around. That, of course, was my interest when I was studying the child's interaction with the mother in the playroom setting. Ultimately the things that we did [Lewis & Weinraub, 1974] together were we saw that there was a shift from the proximal to the distal kinds of following. From touching to looking as the child sought to make contact over the first few years of life. But in any event, so that was one idea then there was the idea, let's look at stranger anxiety, because the more upset you were the more maybe you were attached to the other. No one was quite sure what was more or less; if you follow the mother more was it because you were more anxious that you would lose her therefore less securely attached or did you follow her more because you were more securely attached? I didn't have an answer.

Weinraub: And the same thing is true with separation distress.

Lewis: The same thing was true with the stranger and then with separation. We didn't know. Mary was simply trying to figure this out like the rest of us, of course, with her 23 subjects, then decided that it was the reunion; it wasn't the separation, it was the reunion. But you and I published the monograph, which was your dissertation, in what '70—

Weinraub: I think it was '77.

Lewis: 1977. So you have to understand that it's not until '68, '69 that the paradigm was hit upon. So this is six years later or so in which we are talking about how the child is going to react to the separation from a cognitive—from what the mother does and sets up the environment. Notice, if you will, in the historical sense that fits much more into the environmental argument rather than this unique bond kind of argument. Moms are not important if that is their exact presence, if they can organize the kid in a proper supportive environment. In our particular case, in what you study, the proper environment was, in fact, has she constructed what the child was to do and attend in her absence. So in a certain sense it had to do with whether the nature of the environment made it easier for the child to sustain itself in her absence.

Weinraub: I hadn't heard of a playroom situation before 1969. When I read about it in *Child Development* I wanted to come study with you. Where did you hear about that from, what made you set up a playroom? Because Ainsworth copies that paradigm when she goes into the laboratory from the field.

Lewis: I think there is an important point here, which has to do with ecological validity. I think we have missed the boat in our concern for ecological validity. What's important in validity of a situation is does it predict to things we are really interested in. If you could do it with marble dropping, if marble dropping predicted some wonderful other behavior in the real world then marble dropping would be a fine paradigm. What you discover is that in the home, for example, there are many different situations and moms behave very differently in different situations in the home. So if it is playtime, her organization of behavior is very different—and we hadn't begun to publish on that, her organization—what she does with the kid is very different when she is on the phone and it's very different when she changes the diaper or puts the kid to bed. So there is no ecologically valid situation per se. We simply decided that it was too difficult, there were too many uncontrollable variables to go into homes. Better let's construct a play situation for the child, and bring the mother and child in. I must tell you that in the beginning the rules about disclosure were such that we didn't have to tell moms that they were being observed. In the early Lewis and Goldberg study watching what moms did, they didn't know we were watching them.

Weinraub: While they were waiting to do the attention paradigm.

Lewis: While they were waiting to do the attention paradigm. What this meant was, I mean, we saw things that you know you couldn't publish. I mean one mom thought she might have an odor so she looked under her arm, touched her armpit, and then smelled it to see. I mean all sorts of very revealing and embarrassing things.

Weinraub: People weren't so sophisticated about one-way mirrors back then.

Lewis: They didn't know about one-way mirrors. So we had this wonderful situation. Here's a playroom with mom and children, we have this vast one-way mirror, and we could videotape what was going on. So it was simply an actualistic setting in playrooms. Now I must say that there were children, at Fels for example, there were playrooms which kids and groups of kids came in with moms in their longitudinal study and they observed them using a Fels checklist, not videotape. So we were a very early user of videotape, again, because of the ethological concerns that I had and was interested in and paid attention to. So we knew we needed videotape to reduce that complex kind of data.

Weinraub: Was the data from the '69 paper videotaped?

Lewis: The '69 paper data was videotaped, yes, yes. So what I find then by 1970, which is essentially a decade into from '60s to '70s and my graduate work which is really in motivation and so on but not in social—well, I did some studies on social reinforcement. So what I find by 1970 I'm becoming increasingly as interested in the social as I am in the cognitive, in the attentional and physiological. So one can now start to see a real divergence in what I'm starting to do. Now in 1970 my second child is born and my second child is an absolute wonder in terms of the fact that she is really one of these unique 10% of these kids who have a difficult temperament, who are really weird in terms of their relationship to the world. Felicia is really very much responsible for a lot of new interests that develop. For example, my whole interest in the self, which has now really dominated 15 years of my life, minimum, maybe 20, is really out of a very simple observation. What I did was I observed that Felicia was terrified of adult strangers at three months of age; that was the first thing. We knew this from our housekeeper who—on Wednesdays the housekeeper would appear; Felicia would scream all day long and we finally discovered it was this strange person in the house. I decided that she was fearful of adult strangers, but her older brother by two-and-a-half years had three year old friends and she greeted them with cooing and aahing and so on, so that was very strange. How in the world could it be that you could be fearful, as everyone reported kids were—which turns out not exactly to be true—of one class of people but not another? So I decided the first study was let's bring that into the laboratory and see what went on and so we brought in adults and we brought in children and we had these adults and children approach younger children, eight months, nine months of age in a stranger approach situation, a paradigm that had been developed by others is what we just used. What we found in the first study was, lo and behold, absolutely correct! Children were fearful of the adults but not fearful of the children. Now, in doing that study I already had an idea that the child might be utilizing something about itself vis-à-vis these social objects. So it would have been the first inkling of a model, a self-model. We did in that study a rather interesting variation. Besides having an adult male and female and children male and female approach, we literally had a mirror approach moving on wheels approach the child and sure enough, the child responds to the image of itself like it responds to another child.

Weinraub: You know, Michael, I want to have the pleasure of saying what it was like to stand behind that one-way mirror with you and, I think, Jeanne as you came to that idea. The adults and the children had all done their approaches, and the approaches had all been counter balanced. You might not remember this, but you said, "A mirror! A mirror! We've got to have a mirror!" And you started, like ranting, behind the one-way mirror, and it's dark in this little observation room, and you said, "Go get a mirror!" I left the room thinking, where am I going to get a mirror? I think I went into the men's room first for some reason, because it might have been closer; I didn't see a mirror. So I went into the ladies' room and there was a big mirror attached to the wall. I yanked it hard, but I couldn't get it off. So I went back into the men's room and I saw there was a little

mirror on the side of the room and I yanked it off and came back to the observation room. , We put the mirror on the floor leaning up against the wall and you put the kid in the carriage, and we wheeled the kid to the mirror. And so it was born—the self study!

Lewis: Yes, and so this idea was born that the kid was interested in the kids in the mirror. Now let me back track because before this point I am now starting to get interested in helping the field in some way and I decide, because ETS gives me some resource, I decide to start a conference. Oh, I know what happened, what happened was this: I was invited to the last Foss meeting in London.

Weinraub: That was in 1969?

Lewis: That was in '68 or something—no, must have been '69. So in this series now Lewis starts to appear. At this place I meet Len Rosenblum who has become not only a colleague but my best and dearest friend. When the Foss Series stops in London I say to Len, "We should be doing this here." Now Len Rosenblum is a primatologist so he can represent that whole primatology interest and, of course, he is interested in social development. So we decide to do this conference, and I get money from ETS where I now get money to, in fact, start this conference series. And so we do the first book, which is occurring at about this time, and this first book was the *Effect of the Infant on Its Caregiver* [Lewis, M. & Rosenblum, L. (Eds.). *The effect of the infant on its caregiver: The Origins of Behavior* (vol.1). New York: Wiley, 1974.]. Now that is a very important book and it's important for one more reason than you probably realize. At the time, the word for parent was *caretaker* and we decided to use the term *caregiver* and changed the way the entire field referred to the parent. So here is a dyslexic who had trouble getting into higher education because he couldn't spell or read changing the word and, in fact, the word ceased to be *caretaker* and became *caregiver*.

Weinraub: In fact the first time Jeannie and I met with you together in your office, I don't know whether you the book was out yet, but you started just ranting and raving it and saying, "It's not caretaker, it's caregiver." And the *its* was also important. You used *its* instead of *his*.

Lewis: And with that we made it gender neutral. In that book, if you stop to think about who contributed for the first time on the developmental literature you would be quite amazed. Berry Brazelton, who was not known. Berry had been doing graduate work with Bruner and with Miller at Harvard, he was a pediatrician.

Weinraub: Which Miller?

Lewis: George Miller at Harvard. One of the earliest papers that he ever published in developmental was in that book. And there was Dan Stern and unknown—I don't have the book, I can't even remember who—so Freiberg who had been in psychiatry but had moved on. Dick Bell who in fact had published a paper earlier on that topic, myself, Leonard—

Weinraub: Ricciuti?

Lewis: Ricciuti? No it wasn't Ricciuti. In any event that became the mother/child interaction book. What happened was that although titling that the child affects the parent, its major impact was that what you had was an interactive situation. That book was published in 1974—

Weinraub: The second one, the *Origins of Fear*, was Volume 2 in the *Origins of Behavior Series* that you and Len Rosenblum published, was also published in 1974.

Lewis: Okay, so you can see now it's 20 years ago and that book was published. Well, it had all the players, or to-be players, and it essentially established the field of mother/child interaction. The next book I did in this series was the *Origins of Fear*, which was published in '74, already beginning to reflect an interest now, as I look back, on not only social but emotional, which is really what had dominated my thinking I think in the last 15 years, but also cognitive; the idea that there was such a thing as social cognitive or emotional cognitive developmental psychology. So there was a new domain of cognitive that needed to be considered that it didn't reside simply as a domain as either psychometrics or traditional kinds of things about learning or cognition. That there was another domain in which learning could take place. So we started these studies, as I say, the first one in which you were already involved, but you were already involved in our attempt to—and so was Jeannie Brooks—at this point in our attempts to try to articulate something about the social life of the child within the context of mother, but we did a father study as well. Maybe the earliest father study; if it isn't <u>the</u> earliest, it's damn close.

Weinraub: There were three father studies published around the same time. There was Milt Kotelchuck's dissertation, Michael Lamb's—I don't know whether that was his dissertation or a project in graduate school, I believe—and then the initial study thing that you did with Peggy Ban at ETS where you had 20 fathers and mothers come to the lab with their infant, one week with one parent, and one or two weeks later with the other parent. [Lewis, M., & Ban, P. (1974). Mothers and fathers, girls and boys: Attachment behavior in the one-year-old. *Merrill-Palmer Quarterly*, *20*(3), 195-204.]

Lewis: Right, and the remarkable thing of course is that we found that they behaved very similarly to them except for this whole idea of proximity. That is, they didn't stay as close to their fathers; they used more visual contact, which, of course, has never fully been explored. That is to say, the old developmental problem: does the essence of the thing have different behaviors associated for, let's say, mothers versus fathers? It's still a bug-a-boo for us because if we want to say something that mothers are different than fathers, do we mean they are different in the essence of the relationship or do we mean they are different in the behavioral manifestations? This is an old problem in development. Are different behaviors representative of the same construct or not?

Weinraub: Well, there's your original discussion of that in 1967 with the Oriental Metaphysician.

Lewis: That's right. In 1967 I did this paper which did receive some attention on this whole notion of relativism. Now, Jerry at the same time published a paper on relativism—maybe a little later—I think it may have been a year or two later, but it was quite clear that when you were studying infants who were developing rapidly the behavior repertoire changed. If you were simply going to say that they were different because the behaviors were different as opposed to say no the underlying thing was the same, but different behaviors became in the service of the same thing, then in fact—or the converse—that is the same behavior became in the service of different things.

Weinraub: I see, your crying example.

Lewis: That's right, so you get all those things which then got labeled as these problems in measurement. It's the Western problem of the difference between essence verses manifestation, which is Plato's Aristotelian argument brought into development. So here we are starting to move really heavily into the social, and the emotional interests us, and the first emotion which anyone paid any attention to was fear because there wasn't much literature on the topic. So fear got us to study, first of all, that the child was fearful of different objects in its environment, which had to mean that there was some cognitive underpinning here, some understanding. One of the things that, as you've just related and I don't remember as clearly as you, is that this idea of a self might have been part of this. The things that are like me are less fearful than things that aren't. Now social psychology—Heider and Kelley and—oh I can't think of his name, he is at Princeton now [Ned Jones]—in fact, developed theories about self, like self and how that, in fact, interested social behavior in adults. So that got added to our considerations.

Here's what's got to be the funniest story in developmental psych I feel, bar none. When I tell this story everyone laughs. We decided, as you recall, we decided that well there are two ways in which adults and children were different, two simple ways. One was height and one was facial configuration. We decided, well, can we distort that, can we somehow mix those up? If so, what will the child do? Of course what occurred to us was a midget, that a small person was the height of the child but the facial configuration of an adult. I've forgotten exactly how it was. I think we said the first person who could locate a midget, ask them to participate in a study of scaring kids could have first authorship. In any event, we found the midget and we repeated the study this time with a midget. What we discovered was in fact that the children were neither happy nor fearful as they would be to an adult or to a child but were surprised! They showed a particular response which I have subsequently learned was a classical surprise face. Now, of course that was fascinating. It distracted us because one, it started to tell us about emotional life and, two, it moved us into, again, social cognition because this meant that the child understood the discrepancy and, in fact, meant that it had some knowledge. Children as young as six and seven months responded with surprise. Now you subsequently did at least one, maybe two papers—I've forgotten—in which you tried variations of the height thing and that was never pursued. [Weinraub, M., & Putney, E. (1978) The effects of height on infants' social responses to unfamiliar persons. *Child Development*, 49, 598-603.] But the general theme, namely that children should be differentially fearful on the basis of features of the environment, is of course an important concept which has never fully been explored and which would be fun to. So we then got together the first book on emotion; okay, 1974, that's almost 20 ago before there is any literature on children's emotion. Any emotion was captured by social, and we did a book on an emotion called *The Origins of Fear* and, again, we captured what I thought was, in fact, people who were serious. Rudy Schaffer, who has moved away from attachment into fear of strangers, but he was now experimenting with cognitive techniques. I must add part of the reason for that was in 1969 he invited me to come and I spend six months at Strathclyde teaching him cognitive techniques and, in fact, had two students of his who became students of mine: one Harry McGurk and the other Stuart Millar.

Weinraub: And he did a '67 monograph on memory. He was one of the students.

Lewis: Exactly, he was one of the students and they essentially did their dissertations with me during this period and they were cognitively-oriented dissertations. Howard Hoffman, who is well known in the imprinting of field literature, had a chapter in the fear volume.

Weinraub: Here at Bryn Mawr.

Lewis: Right, at Bryn Mawr. Other chapters in the 1974 volume on fear were by Alan Sroufe along with his students Everett Waters and Lee Matas. Henry Ricciuti had a chapter, as did Peter Smith from London. There was also Inge Bretherton, a student at that time of Mary Ainsworth, Len Rosenblum, and his associate Stephani Alpert; myself with Jeannie Brooks; Jerry talking about his discrepancy; and Harriet Rheingold, Gordon Bronson, Daniel Stern, and William R. Charlesworth were all discussants. So my interest in emotions is obviously witnessed by this '74 book, but there are of course papers before and after involved in fear primarily. All this, of course, starts to get a little blurry as you know when you try to reconstruct 30 years, but one of the things that the study of the field indicated to me, again Felicia playing an important role, was that in the studies the children seemed very pleased with other children. Well, you know there was at this point this idea that children couldn't really profit from the interaction with other children. Daycare or childcare was essentially after three because the idea was that children had no cognitive capacity. If you took a Piagetian point of view and you took just the general view in home economics and so on it was that children couldn't profit from peer relationships and they didn't seem to be important. After three everyone recognized they were. Well, how were you to explain the great enjoyment children had when they saw other children and their great fear when they saw other adults if there wasn't something in there which essentially said, hey, kids might need other kids? So a postdoc student, Jerry Young, a Canadian who did his dissertation with Theresa Decarie and then came to do a postdoc with me and then returned to Canada—I haven't really kept track of Jerry, his career—we decided to do some peer studies. We did several studies on peer friendship patterns again using an ethological approach of what peers did with each other. We actually did one study in which we took strange children, we paired them off and had the mothers take the children home and at least three times in the next two weeks play together for an hour, and we took a group of children who didn't have that experience and we had a before exposure measure and after three weeks of contact with each other measure. So we had two groups of kids: kids who had been exposed to other kids and kids who had not been exposed to other kids. The idea being could they profit from peer relationships, and sure enough, we got just whopping differences of the kind that you just would expect in friendships. They stayed closer to the peers they knew than the ones they didn't know. They showed very decidedly different patterns after simply three hours over a couple of week's exposure. So we were constructing probably the first infant daycare situation; of course it was family care but we noticed they could profit from it. We didn't publish extensively on peers except a few papers and then sort of perceived that peers were neglected. Now again from an autobiographical point of view, my sister played a very important role in my life and, again, because I didn't have strong parent presence I found that other people served me fairly well, at least I fairly adjusted to compensate, so this whole idea was what was the nature of peers. We essentially did two things: we published some of the peer studies and then had another conference on friendship and peer relationships, the book published in 1975. [Lewis, M., & Rosenblum, L. (Eds.).(1975). *Friendship and peer relations: The origins of behavior*, 4. New York: Wiley.] That book did the same thing that the fear book did, it brought everyone who had been working with peers, that is working with children beside their mothers, working with children in social relations different from the mother together and suddenly in this weight of these people, which also included people who were unknown—Ned Mueller, who for a time was prominent in peer relations. I don't think he is as active anymore.

Weinraub: Was that a chapter with Mueller and Vandell?

Lewis: Yes, that was that chapter and, by the way, Elizabeth Bates. Liz Bates got involved because I called up David McNeil in Chicago and asked him about the language of peers; would he come and talk about language differences between children? He said he couldn't come, but he had a very good, bright graduate student who was finishing up. Liz arrived, and that was her first exposure, as she recalls it—actually, we just spoke recently about it—that was her first exposure to the developmental people. So the series not only had the effect of bringing together topics that were unexplored, thus providing a source for reference, it also brought people together. It gave them cohesion and it also introduced lots of young people who later became prominent in their own right.

Then we started to do, and published with a graduate student at City University in New York, Stephanie Schaffer, a couple of papers which have been picked up in terms of child abuse because we started to look at—and I don't remember when these were done, but probably somewhere in this period—started to look at could peers compensate, could they compensate for what the parents weren't giving in the sense that parents were neglectful or abusive? At this point, what became very clear to me, as the attachment model solidified, was that I could not be in accord with any model which held that simply the mother alone was the primary and unique contribution to the child's social and emotion wellbeing. Peers then was the point which promoted this idea that a social network had to be introduced, that it was a mistake to simply say it was repeating this old error that had gone on in the literature before, which had now promptly been forgotten, that the mother was not unique, that there were other people of importance in the child's life and that they had important roles, and it was peers which were the, if you like, the spearhead of that. Children's relationship to peers was apparently very important and the question was was there a separate system? Now Harlow had argued for a two-effect system—again, that paper has been forgotten of Harry's—but Harry Harlow essentially said the peer system and the mother system were independent parallel systems and that, while they might be mutually influential, they were separate systems. That was, of course, the social network idea. That idea stuck with me. Although Harry had written it himself, he was so overwhelmed by the acclaim of his motherless monkey work that essentially the idea that the mother came first and peer relationships were a consequence of the mother so that it was a sequential process rather than a dual or network kind of process was forgotten. But I, of course, didn't forget Harlow's paper. I don't think Harlow is in the *Origins of Fear,* although Harlow appears later in some of the other more network-oriented books at the end of his life. No, Harlow isn't here, I guess. It's Len Rosenblum who is broadening, although Len is still stuck more with the mother as the tradition and Steve Suomi picks up the peers and becomes prominent. Now what's interesting is that that's in 1975 that the *Friendship and Peer Relationship* is published—I don't know the papers and chapters, I'm just referring to books now. As you know there are 31 in 31 years, so there is a book a year of either written book monograph that I've either written or edited, either one. What happens in 1976 you probably don't even know, but I got some money—being interested now in the peer system—I got some money from the Rockefeller Brothers Foundation. Now that, in itself, is an interesting story. It's not a foundation most of us know about. It was to explore child care and public policy and that, in fact, produced a monograph with Carla Goldman who was at that time working with Walter Emmerick and was interested in child care. So in 1976 we started—there is this volume—

Weinraub: Oh I do remember. It was when I was finishing my dissertation and the two of you started talking about this and it seemed weird then.

Lewis: Right. What emerged from our research was that there were two kinds, historically, of daycare. One was custodial care, which the poor got, and one was educational care, which was very expensive and went to the middle class. I forgot what we did with it but we had all sorts of people involved in it and we produced this monograph, which concluded, out of the peer stuff, that proper daycare would not be bad for children. It would expose them to peers and it would enrich their social lives and so on.

Weinraub: But this would be after the age of one or so.

Lewis: I don't think we even specified. I must tell you there is a magazine article. Time did a feature story of me years later and on the cover of that is a picture of a baby. It's the most popular issue cover that they ever do and whenever they advertise Time that cover appears. They took pictures and I could show you 3-month-olds lying on the floor just barely able to lift their heads, really interacting with one another. I think it's really a mistake, that is from an ethological point of view, ever to believe that children did not have orientation toward, and special feelings about—including cognitive—toward other children. That doesn't mean that they don't need adults, it simply means that it is a parallel system.

Weinraub: This monograph on child care was in which series?

Lewis: No, it was published by ETS who then had a publishing arm and distributed it and, you know, it did not get well played. [Goldman, K. S., & Lewis, M. (1976). *Child care and public policy: A case study*. Princeton, NJ: Educational Testing Service.] It disappeared, as it should have. It was to be replaced by much better and more careful thought and concern. But it really grew out of my idea that there were peers and that kids could go the school, that kids could go to settings with other children. That this wasn't bad for them, that it was enjoyable and that this model that they needed to be with their mom all the time until three was somehow a false model that needed to be changed. So as you can see in 1976—when did we do the social network paper?

Weinraub: It was published in 1977. I guess we worked on it in 1974.

Lewis: 1977 it's published, that's right. [Weinraub, M., Brooks, J., & Lewis, M. (1977). The social network: A reconsideration of the concept of attachment. *Human Development*, 20, 31-47.] Now, as you know you, Marsha Weinraub and Jeannie Brooks-Gunn and I published, or tried to publish—it took us a long time to publish that paper—essentially the outlines of an alternative to attachment which was a social network paper. Fundamentally what it suggested was a set of propositions which suggested that children have relationships with others beside the mother and that we have to understand that relationship in its context, and it was a propositional paper. Now, the remarkable thing is that we tried to publish it in *Child Development* which was at that point simply dominated, as it remains, by attachment theorists who believed in a, what I believe is a, rather narrow point of view. So what we got was always mixed reviews.

Weinraub: In fact, we wrote that in '73 and tried to publish it in '74 and kept getting shot down.

Lewis: Right, and what happened was that we would get mixed reviews always and I do remember the last time we had gotten two or three sets of reviews from *Child Development* and the editor at that time—I forgot who it was—said, "Well, you see, three people have rejected it." Of course, three others had accepted it, and I wrote that back as a final thing and I had all these reviews and Klaus Riegal spent the year—this was earlier—had spent the year at ETS with me. Klaus, believing in interaction and a much more dynamic dialectic environmental approach, also was a little appalled by what has to be linked to a very powerful biological model that it's the mother alone who is the soul and this is evolutionarily the case and it's true for animals and humans and so on. Sort of did account for the diversity that humans were capable of and the effect of experience and environments, total environments. So he was editor of *Human Development* at the time, and I sent him all six reviews and the paper and simply said, "Listen, we can't get this thing published because it's too controversial. Don't send it out to review because if you send it to review you will get the same thing, you'll just get the same thing. So you decide whether it's worthwhile," and he published it. Now the joke, of course, the end of that is that in its time the attachment people all made reference to it. Although they wouldn't dare to allow it to be published in its form but clearly placed me in my sense of things into the commitment toward a boarder perspective of social development than with simply the mother. And so I moved away from attachment. Although if you were to ask what camp I belong in, I would say that I certainly believe that early experiences are important for children, mother included, but not exclusively the case. So you see here, I don't belong in a temperament of biological deterministic or genetic, I belong to an experiential, but not the powerful experiential, namely the attachment people, which is the powerful particular model to the broader world view model of experience. As a consequence of that I then published, edited a whole series of other books and monographs. One called *Perspectives in Interactional Psychology* with Larry Pervin in 1978. Another in '79 in *The Child and Its Family*. In 1981 *The Uncommon Child* and in 1984 *Beyond the Dyad*. All of which I must say were terribly, not overly successful but just moderately, unlike the ones that had mother in which were tremendous sellers. The *Effect of the Infant on its Caregiver* sold 8,000 to 9,000 copies, you know, all time. I might add it's now out of print. So no one was really keyed on social networks, they are a little more but not in the developmental field. In a removed and certainly not in early childhood and infancy, there is no social network modeling going on. Although I have continued to try and Weinraub and Lewis and Lewis and Weinraub did a whole series that moved in that direction and then, of course, you went off in your direction of what you wanted to do, but I, of course, have stayed that way. Now around 1979, possibly even earlier, Candice Feiring came as a postdoc, and she worked on a social network model from the University of Pittsburgh.

Weinraub: Who was she working with?

Lewis: I think she worked with a chap named Taylor who was a social network person. And Candice and I essentially pursued, in the last 14 years or so—it's more than that, I think it is 16 years—pursued the social network modeling kind of work. Much of that, I must say, is peripheral to the central focus of what people are interested in and in the Osofsky handbook, which was my last attempt to create a theoretical argument against attachment as a limited model and more for a broader model, I have sort of said what I have to say. Now in reviews where attachment has now been extended into Phil Shaver's kind of idea in adults and so on, again, I have tried to say, hey, come on, there are lots of other experiences here. Early relationships between men and women are probably as deterministic of what you are going to end up and do as your relationship with your mother or the introduction of the father, simply to complicate two attachments, to complicate a model that simply rests on the mother. I don't mean to belabor the point except to say that it is very, very interesting to me, in a perspective of over 30 years as a serious investigator, that when there are powerful models you cannot undo those models. There is no empirically disproving them; the Kuhnian idea is absolutely correct. There is simply no undoing them. The word attachment is as built in to our idea of human behavior as is our toes. You simply can't undo it.

Weinraub: Have you found that very frustrating or have you begun to accept it?

Lewis: Well, what it did is that it led me to simply say either I have to join or I have to stand in permanent opposition to it, and I simply said neither of those are where I want to go. And what had happened was a new stream of interest entered in for me and that was the stream of emotion starting with the fear and then moving into the self, which for me was a cognitive construct. So before that I did some work with Roy Freedle who was very much involved in language. This was still at ETS where I stayed until 1982, having gone there in 1968—research environment, no teaching tenure, senior position, but again no university, consistent with my being outside of academia. With Roy, who was interested in language and I interested in mother/child interaction, we started to do a whole series of things on early communication and its relationship to language and in 1977 we published, Rosenblum and I again, a book, the fourth in this series called *Interaction, Conversation and the Development of Language*. That truly broke the back both of a Chomskian and of a Skinnerian approach to language and introduced language finally as the outcome of a mother/child communicative framework. Again, if you look at the people who were in that book it is Liz Bates, again, as are all the people in language who included, oh gosh, Lois Bloom.

Weinraub: Was Roberta?

Lewis: Golinkoff, no, is not in it. She was, I think, a little later. Courtney Cazden. So what happened in my career as the postdocs and the graduate students came in, because I had no academic affiliation, they came in either wanting to do the kinds of things that I wanted to do or they came in with skills and capacities which I needed to start to do the kinds of things that I wanted to do. So Lois came with a knowledge of language background and Roy Freedle, who was a colleague at ETS, had it so supplied me with this and we published several papers which had some impact on communication and language, looking at the mother/child interaction. Using those techniques, which I had discovered and we published our monograph in 1977, which is an effect monograph, again, it's the child's upset if you really think of it. I mean, it's called *The Determinants of Children's Response to Separation*, which we know is distress. That fundamentally—here we are 1977 talking about their reaction to a stressful event, the cognitions necessary, the nature of, as I said earlier, of the mother's arranging the child's environment, but it's not attachment. We have a playroom situation and a separation which predates the attachment paradigm which we use for our purposes and not for really the study of attachment.

What happens now is, in 1974 actually, I apply for and get a large grant from the NIMH to study self and self-development. Coming out of the work with the mirror I now get interested and I discover in Gordon Gallup a technique that he used with chimpanzees to detect whether they could recognize themselves in the mirror, which is the Rouge Technique. So I can't make any claim to have been the first, in fact this is always amusing, we gave credit to Gallup who was working with primates. I thought I was the first doing it with children but, in fact, it turns out that there was a dissertation done by Amsterdam which was published in '72; I had already been writing on self-recognition earlier than that, but not earlier than her dissertation which was '68 where she, independently of Gallup, used the same technique, or she claims independently, but actually Gallup had used it before. What I always find amusing is that she did one paper, maybe two, on it, and you can always tell who your friends are in the field by referencing and when someone chooses not to reference my work, but to reference hers I clearly know they are avoiding because clearly this is a major impact in another technique which I essentially introduced into the developmental field. Although she had the paper earlier, it went unnoticed until I started to publish on the self.

I have continued to work on the problem of the self and with it a movement toward emotion. So in 1978, we published another volume in the series called the *Development of Affect* and that is the first book on development of affect in the developmental field. And again, it now starts to capture both the newcomers to the field as well as the peripheral people. The *Development of Affect* brought together people like Cal Izard and then his students. He started to get developmental students but neither Cal nor Ekman were really interested in kids; they were adult emotion people. We started to bring it together. In part I was also influenced by meeting Sylvan Thompkins who spent the year at ETS. We spent one day a week eating lunch together. Sylvan was a raconteur. He was just loads of fun and he was a man who spewed ideas as one breathes. He was very dynamic and I found it fascinating. At that point Jeanette Haviland had come as a postdoc. Jeanette was interested in affect and published, I think, a paper. She published it alone on looking smart which was, I think, a very important paper and which essentially looked at Piaget's use of affect to mark cognitive milestones.

So when the child was surprised that the object disappeared when it went behind a screen and you opened the screen and the object wasn't there, that surprise marked that the kid had memory of it and was surprised that its existence wasn't maintained over removal or a curtain in its way. My own work interest in the emotions combined fear of strangers, physiological research, and we were trying to look at heart rate as an index of measuring emotionalities. So I hadn't given up the physiological attempt to mark fear, but started with Jeanette, who knew facial coding, to get interested in facial coding, that is, to move toward a true affect kind of study in terms of looking at the face. Now I still, of course, was still very much interested in attention and cognitive and was continuing to publish in that, and so did a volume with Gordon Hale at ETS called *Attention and Cognitive Development* where we brought all the attention theorists together and published a book which was well received in terms of attention and cognitive. So that interest still was maintained and what happened was that that interest started to shift toward the measurement, using it not so much as a process but using it as a clinical tool. I had established from '71 on a visiting professorship at Columbia where we started to use the attention and habituation stuff to measure stuff with dysfunctional children. It was there that I met Gail Wassermann who's continued—got her Ph.D. with me and has continued at Columbia and is now a professor. She has continued in the medical school, using this cognitive technique to measure dysfunction and disabilities in children. So also in 1979, after working a decade—well, it wasn't quite a decade, but almost a decade—on the self, Jeannie and I published the *Social Cognition and the Acquisition of Self* which was also a major moment, again, because this was a written book rather than an edited one, my first, which was a statement about the development of self. Now the development of self for us long preceded any of Bowlby's discussion of a model of the self or model of the mother. That was all very vague and no one had much to do with it and, of course, what we discovered was not only a technique but its relationship to other things, and it's probably—I mean, if you mention self you know to mention that book. It stands as a marker. I might also add that it's out of print.

Weinraub: Really?

Lewis: Yes, what happens with publishers is if after a while—and again it was a very good seller. It sold, I don't know, 5, 6,000 copies.

Weinraub: And it is referenced highly and is seen as a major mark in terms of it compounding the whole way of looking at self from a developmental point of view in terms of its early origins.

Lewis: Right. So all my attention and cognitive abilities were really thrust toward, now, this whole idea of using this to measure differences in children as I started to get interested in the topic of differences. And so in '81 and '82 we did a book called *The Uncommon Child* and then another book called *Developmental Disabilities* which I did with Larry Taft who is a professor of pediatrics in which we started to take these techniques on attention distribution and on learning and move them into the field of pediatrics. Now as I had said, I had for a decade a visiting professorship in pediatrics at Columbia and in 1981 ETS made a commitment to eliminate its basic science position and, like Bell Labs at the time, essentially wiped out its basic research. Now having tenure and being a senior member I could have stayed on as Irv Sigel did, but my feeling was that that was enough and I had had a standing offer to move the institute, which I had established at ETS, to move it up to the medical school up at Rutgers; at that time it was called Rutgers Medical School although it's changed its name since.

Weinraub: Now it's Robert Wood Johnson Medical School.

Lewis: It's called Robert Wood Johnson Medical School, but it's too complicated a story to explain what's happened, but it's no longer part of Rutgers. It's a free-standing state medical institution of which there are four or five throughout the state, it being one of those, and the university's name is called the University of Medicine and Dentistry of New Jersey and the particular college is called Robert Wood Johnson Medical School.

Weinraub: The name of your laboratory there is—

Lewis: It's called the Institute for the Study of Child Development. Now it is a unique laboratory because now this is my third job, my last I assure you, and I finally now become a professor in an academic environment but it's not arts and sciences, it's in medicine. So I finally found a home in academics, but it's not in arts and science.

Weinraub: And it's not in engineering either.

Lewis: And it's not in engineering. I do have a professorship in the graduate program at Rutgers but they don't pay my salary and it's not where my tenure is and is established. So you can see from a career point of view, two research institutes and a research professor in a medical school, so I've never gotten to arts and sciences.

Weinraub: Now, Michael, there is lot more I want to do in terms of updating your work, especially the exciting stuff that starts with shame and lying, and get to some of the real nitty gritty emotion stuff. I also want us to talk a little bit about where the field is now and where do you see it going. So I want to ask you one question just to end this one topic. We talked about your thinking and how it's been shaped and what you see as some of your most significant studies, although I may have colored that and I might want to go over that again. Which contributions, says this question, are the most wrong-headed? Are there any of those that you wish you could take back out of the literature, that were distracting?

Lewis: Well, no, the answer is no. I pride myself in the following: my approach to peer relationships turned out to be correct and the attachment as the most important thing is up for grabs. Attention the paradigm has been replaced, but not the idea. In general, I can't say that I don't feel that things that have been done similar to what I have done and the things that I hold dearest to have proven wrong. I don't think I have been wrong. If I have been anything it's been that my feeling about how to have a career is to pursue things that interest you. I have chosen breadth rather than depth. Would I, if I could, do that again? The answer is probably yes. Am I terribly happy doing that? The answer is yes and no, maybe. I think science progresses in two ways. I think it progresses by the leaps of faith in ideas, by creative notions that put things together. Howard Gruber, who is very interested in and I think has a wonderful theory about creativity, argues that the creative human being is the one who skips around. He used Piaget, he is writing his biography which may not survive because Howard is old. But he did one of Darwin which won all the awards to show that Darwin spent his life beating around the bush and the bush was a central core idea which he finally came to and, as I view my life and my career, I think I've been beating around the bush of some central idea, and I'll tell you what I think it is. I think it is about the idea of the self and about the self in relation to others and in relationship to the self. But in beating around you do many different things and if you look you will see that people who found that bush beat around it well and it may turn out, to my sorrow I suspect, that I beat around the bush but I didn't get to that. Now I'm not dumb, the dance isn't over yet and maybe I'll never get to that, and if I don't then I think I will be sorry because I've enjoyed myself. I think I've made contributions but what I didn't do was do 35 studies on the same thing, varying parameter after parameter. I haven't stuck with peer relations and only peer. I haven't stuck with attention and only attention. The fact is very few people do that, stick with a thing, although some stay within a domain. The fact of the matter is I've done physiological work and continue to do it, now working with adrenal cortical and hormonal responses as well as heart rate and respiration, GSR. I've done attention, I've done cognitive studies, although that hasn't really been my focus. I've done social development, emotion.

Weinraub: Temperament.

Lewis: Temperament. If one looks at my CV, one would find me in much of the literature. So, if I were complimentary, I would say I'm a Renaissance man. I have made, I think, influential contributions and helped to shape a field, the field being developed by the psychology, not a single thing. I think that has its cost. People who have different approaches talk about being the dilettante, talk about being this—again, this image of the scientist is, you know, dealing with the particular—this idea of the singular, the detailed kind of thing and that's simply not my style. I should think that a field that doesn't have people that I think I represent is not going to go very far very fast. A field that only had it would also go very far but not do very much. I suspect the field is more like I am than it should be and that's a sorrow. I may have been a bad model in some sense, I don't know. I made a list, with your help in fact, of graduate students and post graduates and colleagues. It's an enormous list of people who I have worked with, they are all the senior members of the community as it is, and I think if I imparted anything besides a zest for life and solving problems it was don't be encumbered by detail. That's good and bad, and so I have some regrets there. I think I went down a lot of avenues. I've done a lot of papers, of which I have one or two that I wouldn't do again and that I now know better not to waste my time, but how do you know where to go when you are going? I've spent my life sort of discovering what it is that I am and also what it is that I am interested in which has to follow—and since I don't belong to a group, it's a self-discovery so I have to try it. I went down this path and I did binary choice research—well, I wasn't wrong, I was right, I said, in fact, humans don't act like adults which goes for the certainty, but they get real joy out of guessing—and correctly—an improbable event. Well, they came to that conclusion, the math model as it was, so I don't know anything; now that sounds fairly presumptuous but, you know, I'm likely to be wrong, but I don't have any real regrets that I did something that's wrong. I mean I think lots of people would say he doesn't know what he's talking about, attachment is the thing. The idea that others are important is true and that would be, I guess, my biggest controversy, but you know it's such an important idea nobody cares what I think about it. So it's hardly that I've done it wrong, no one says what a lousy study, it was wrong. I don't know anyone—that really is presumptuous—I don't know—I have never read—maybe I don't read it, anyone who's said, well, what Lewis found was wrong. I've heard them say, yeah, that's right but I think it should be slightly modified or slightly different. But I wrote in the preface of this last book on shame that Einstein said of Newton that the best one could ask of a powerful theory is that it becomes a limited case of a more powerful theory. Now if Newton became a limited case to a more powerful idea, where am I? So my feeling is, hey, I tried; this is what I believe to be true. I'm serious, if I ever discovered that someone did something with the data that was not legitimate I would be absolutely devastated. I would immediately retract it, I would immediately write, and I'm totally prepared to say, I thought that was a good idea but it's wrong, but you know what, the self and emotion things I only have confirmation on them. I mean what I said at 15 months it emerges, it emerges at 15 months. There is no argument no one can disagree with that. When I say that it's the best measure we have of self-consciousness people say how can it be the only measure we have? I've spent 20 years talking about it; no one's come up with a better one.

Weinraub: What's unusual is that you are someone so tied to research and empirical data who's also so flamboyant with ideas. You take joy in both ideas—theory—and data collection. This is a unique marriage and it has resulted in multiple contributions to our field. But you've always told me that data are just an excuse, something to hang on the theories. It is the theories which are the branches that provide us structure; data serve primarily to decorate these branches. The data give us an opportunity to continue to build those branches. Do you still feel that way?

Lewis: Oh, yes, but you know having gotten old since when we first met and hopefully a little wiser, I—now in much more sophisticated philosophy of science terms. And what we really have is a crude versus a proper argument and the idea of refutation, the idea that there are data that are true is simply the material of one paradigm but not of another. The best example is, of course, what we see in our field, these paradigmatic shifts that I've watched two times occur. One in my measures of attention, which I introduced, being shifted to other measures which don't give any different data. I mean, whenever they do their reviews of data of all the stuff of how habituation and dishabituation are related to IQ, whatever paradigm they do, they find the same .4, .5 correlation, it's the same thing. If I were to write a grant now and say let's do that, they would say, well, that's not a good method but we have no reason for—or for the acceptance of attachment without questioning it or without trying other things. So my feeling is that fundamentally there are ideas of what we believe are true. I used to recommend—it's now out of print—a book by Liam Hudson. He was a professor of education at Edinburgh. I don't know if he is still alive or practicing. He wrote a book called *The Cult of the Fact*.

For a while I wrote a paper or two on this in which the idea that the scientist and what they work on should be somehow separated is this Newtonian idea that you can open the clock and look at it and your observation of it essentially can be independent of the nature of what you are. Whereas the relativity, the quantum mechanics simply says that things are interaction and you can't know everything because in studying it you distort it. I happen to believe in that second paradigm. I happen to believe that if we wanted to give a personality inventory to attachment versus temperament people we would find that they differ in personality characteristics or between genetics, people who have strong views about genetics versus those of environment. That the people who believe in environment are much more humanistically oriented. They are less rule governed, they are much more interested in that people can change and make change. Whereas temperament people believe that it's a disposition, are less likely to believe it. Therefore they are going to be more conservative. They are much more conservative. There is no question that, in fact, you can do these studies and that they have been done to reveal to you a whole literature which you are not familiar with. There is a woman named Ann Roe who published extensively, and for years, demonstrating the differences in scientists or the basis of the things they study and the fields that they are in. For example, in medicine it's well known that the most progressive liberals are pediatricians and psychiatrists. The most conservatives are surgeons.

Weinraub: Does it have to do with their economic status differential?

Lewis: I think it has to do with that, but not entirely. After all, pediatricians make much more than liberal arts people, so it's not how much money. If that was simply a linear function then they should be conservative. No, I don't think so. I think the issue is one of world views. I published a paper, actually it's a chapter, in which I had done a study on sex differences in the literature in the year 1977. I reviewed all papers in *Developmental Psych* and *Child Development* and asked several questions about sex differences and had the characteristic, the gender, of the chief author—didn't have to be the first author if the first author was a beginner, okay. So I knew the sex of the senior author and I looked at a variety of characteristics. One, I looked at the sample size: did it include women and what proportion? Lo and behold, men have authored studies that didn't have women in them. Women had studies that had men and women in them. So, two, what sex differences are reported? Three, were sex differences confirmed? Now, I only report on this last one because I remember it in my head, the other data are there. Men are three times more likely to be correct in their prediction of the sex difference, either that there was none or that there was—or whatever direction—than women scientists. Now are men smarter than women? What do you think is at work? Women scientists are more rule oriented. What's the rule; you state your hypothesis and then you state your results and if the results contradict your hypothesis it still goes in that way. Men peek! They know what the result is, they form their hypothesis on the basis of it! It's either that or women are stupider than men, and that's ridiculous. The other one is so what you have is that the scientist is a part of the process. Our beliefs, our commitments are simply a part of it and we can't escape that. So what do data mean? If you believe something you're going to milk your data, work it more to find it. For example, anyone who doesn't believe in sex differences is likely to take the first run of their data, divide it by the sexes, do a mean test difference, find no difference, forget about it.

Weinraub: Of course.

Lewis: Not the cohesion of the measures, like in correlation matrix for boys and girls separately, they would never do that. The only people who would do that are people who believe there are sex differences and are thus searching for. Now is one group more right or less right than the other? Clearly, the ones that believe in a sex difference are going to try to find it; those who don't believe it are not going to work hard to find it. Ergo, what do data mean, I mean, what do we mean by data? What do you mean when you tell me at the beginning of a paper there are no sex differences? I did an analysis of variance on mean differences, there are no sex differences and then you present correlation matrices or regression analyses and never enter sex into it. Because, after all, with the sample sizes that most in this field work with you don't want another variable in there because you will cut your 20 to 10 in sample sizing, you will be in terrible shape. So the idea is get rid of it unless you want to fight it. Data are not suspect and I simply believe it, as always. One uses data as best one can to make the point. I believe we can take all the data we have on attachment—just take the data, take all the introductions out—just take that raw data and put it together and tell a totally different story.

Weinraub: Let's tell that story in the next chapter. We are up to chapter two I think on April 14, 1993.

(End of interview)