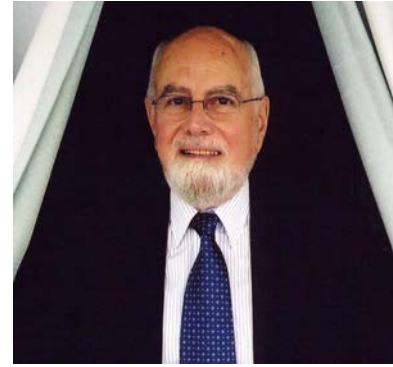


Robert LeVine

- Born 3/27/1932 in New York City
- A.B. (1951) University of Chicago, M.A. in Anthropology (1953) University of Chicago, Ph.D. in Social Anthropology (1958) Harvard University



Major Employment:

- University of Chicago, 1960 - 1976, Assistant Professor to Professor of Human Development, Anthropology, and Psychiatry
- Harvard University, 1983 - 1998, Professor of Anthropology
- Harvard University, 1983 - Present, Roy E. Larson Professor of Education and Human Development

Major Areas of Work:

- Psychological Anthropology

SRCD Affiliation:

- Member since 1987
- Nominations Committee

SRCD Oral History Interview

Robert LeVine

Interviewed by William McKinley Runyan
At Harvard University
July 18, 1994

Runyan: This is an interview with Bob Levine on July 18, 1994 conducted by W.M. Runyan at Harvard Graduate School of Education. Now Bob, let's start with some questions about your family background. Could you please describe your family background along with any childhood and adolescent experiences that may be of interest?

Levine: Well, my family were all eastern European Jews, both parents were born in the United States in New York City. I was born in New York myself in 1932 and my father was an engineer who worked for the City of New York and I spent the first nine years of my life living in New York City and the rest of the time I grew up in Englewood, New Jersey just outside of New York. I lived in a sort of tenement building which was owned by my grandfather. An owner occupied tenement and we lived there too. When I was nine my parents moved out there because my grandfather had a heart attack and my mother was trying to help out take care of him. I grew up in a sort of working class environment of largely working class Italians and Irish and very few people like myself either in terms of social class or in terms of being Jewish. It was a largely Catholic population on the wrong side of the tracks in Englewood, New Jersey which is quite a nice town, we lived in one of the worst parts of it.

Runyan: That's where your grandfather was?

Levine: My grandfather owned this building and he lived there. He had a great impact on me. He had come from Eastern Europe, he didn't really have much of an education but he was a very highly literate man in several languages and he'd been a butcher and had made a lot of money at the time of the First

World War and then kind of retired and he had all kinds of ideological ideas. He was a socialist in spite of owning property and most importantly he was an atheist who was extremely oppose to all forms of religion which he regarded as hypocrisy and so on. So he was the kind of a village atheist type person, but his real identity was this sort of Jewish socialist type identity. Read the Jewish Daily Forward in Yiddish everyday and he had been, long before I was born, part of various Zionist experiments, like a collective settlement in Utah for Jews which he had contributed to and then the organizer of it ran away with all the money so they never got to Utah. But anyway that's the kind of person he was. A little crazy, you know in terms of his left wing orientation but there was a great deal of ideological and political debate in the family and that sort of thing.

Runyan: Did he talk to you about these beliefs much of the time or is this all stuff you figured out later?

Levine: He talked with me and he talked with my father and my father and my uncle would argue with him every weekend about politics. My father had been, for a short period of time in the 30s-this is you know we're talking about the 1930s and 40s here and of course everybody's enormously influenced by the depression and all of that and my father had been a member of the Trotskyist movement in New York for a short period of time. He also had kind of a left wing orientation but not very far left. I mean they were both, he was very anti-Stalinist and then I had an uncle who was sort of a capitalist type and they would have these huge arguments every weekend with my grandfather, my uncle and my father. That had a great impact on me, the life of ideas. There wasn't much else intellectual going on in the family but political ideology was a big thing. So for me the intellectual side was something I developed quite separately from everybody else in the family. My father was an engineer. My grandfather was a retired butcher. My mother was a great book reader, but didn't do anything else.

Runyan: Did she participate in he political debates?

Levine: No, for the most part not.

Runyan: Is any of this related to your later work in political science or is that too much of a leap?

Levine: It's not so much of a leap, I mean I was only in political science at the beginning of my career, you know my first job. But I would say my interest in social science of course comes out of that whole sort of interest in public issues and that kind of thing.

Runyan: What was your schooling like and did you have any early work experience, this is precollege experience here?

Levine: I didn't have any real work experience before I went to college. My schooling was in the public schools of Englewood New Jersey. I was an intellectual kid and I was sort of a maverick or off by myself, somewhat stigmatized, known as the professor and stuff like that.

Runyan: Did you have any brothers or sisters?

Levine: I didn't, I was an only child. My sister had died before I was born from scarlet fever and my mother wasn't able to have a child after me so that was the end of that. My parents were very, I suppose they spoiled me in a lot of ways but there was also a certain amount of strict discipline in the family. I won't go into it.

Runyan: How did you get to University of Chicago?

Levine: Oh yes, there was a cousin in the family, a woman about my mother's age who, I guess she also had the same stereotype of me as an intellectual kid that my classmates did and she came to the house one day with a copy of LIFE magazine that had a big article about the University of Chicago as a place where kids went even before finishing high school and she said this is where Bobby, as I was known in the family, should go and I read that and I said, yes that's right, that's where I should go. But I did complete high school then I went to Chicago.

Runyan: Could you say something about some of your intellectual experiences or influences there?

Levine: No, because I want to go back to high school because I found high school, I mean intellectually pretty non-challenging, the curriculum, even though Dwight Morrow High School in Englewood New Jersey, I have been told, in the late 40s was considered one of the best high schools in the United States. Pat Graham told me that, I didn't find it very challenging. There were two things that were significant for me, one is I joined something called the Foreign Policy Club. Where we discussed issues and we would sort of debate issues every week and after the first year of high school I was elected president of that club so I remained the president for two years, my junior and senior years. This was the time when there really were major issues going on in the world, well I mean there always are but it included the partition of Palestine, the Revolution in China and the formation of the United Nations and we were very involved. We used to go to, the faculty member who ran this a Mrs. Eckerson she was a great mentor of mine-really my most important mentor in high school. She would take us to visit the security council debates in Lake Success and the general assembly debates in Flushing Meadow. This is 1947 and 1948-49 and there were these major debates going on at the United Nations, it was just getting started. It was a very exciting experience. I remember a lot of those things so that was a really important focus for my life, while I was in high school. The other thing was and this is even more important in terms of my career, is that I use to spend a great deal of time in the public library. That's, I always say, that's where I really got my education and that started in junior high school. The junior high school was next door to the public library so somewhere when I was about 13 I started reading Freud, The General Introduction to Psychoanalysis as translated by Brill. So I felt I understood something about psychoanalysis. Then when I was sixteen, which would have been 1948, I discovered two books that had a great impact, one was the book called Frustration and Aggression by Dollard, Doob, Miller, Mowrer and Sears put out by the Yale Institute of Human Relations and the other was much more exciting actually, the book by Kardiner called, Psychological Frontiers of Society, which had been published in 1945 just three years before. So when I read Kardiner I said, this is what I want to do. That is what I really want to do is, it was almost as if I had stumbled upon the very thing that I had thought in my mind before which was that how could you tell Freud's ideas were right unless you looked at different cultures. I don't know where that idea came from but certainly by the time I read the Kardiner book.

Runyan: So you hear that question before you looked at Kardiner or around same time.

Levine: Well, I have to say that I felt it was like a deja vu experience reading Kardiner but I don't know. Maybe my idea came from the Kardiner book but certainly I felt this is it, this is what I would like to do which is to explore the cross-cultural validity of psychoanalytic ideas. Now I was sixteen at that time and from that point on wards that seemed to me what I really wanted to do and I just wanted to figure out a way of doing it.

Runyan: And psychoanalytic ideas felt that important at that time or were they just from reading Brill?

Levine: Well, not from reading Brill's translation of Freud, it was really Kardiner's formulation of, that is, here was this child rearing system and here is the rest of the culture and it was something to do with

the child rearing determinism idea that affected me. I should mention that the following year I applied to colleges and I had gotten into trouble with my advisor, my high school advisor who was a rather conservative man, I wasn't aware of that but he thought that I was sort of a radical socialist because I wore a button for Norman Thomas in the 1948 elections. I wore it to school and so he truly wrote me bad recommendations for college, in which he predicted to these colleges that my future would be as a soapbox orator in Union Square New York. Of course I knew nothing about this until I got turned down by Cornell prior to their receiving my college board exam scores and that made me suspicious that they hadn't even received the college exam scores and by the way, in spite of the fact that I was rather bored by high school I was, I don't know, fifth in my high school class out of I think it was 250 pupils and there were people who were admitted to Cornell all the way down the middle of the distribution so something was obviously wrong. My parents went to see the school principal who discovered about these terrible recommendations and the school principal tried to make up for it by getting me admitted to various colleges, including Columbia where I was interviewed and they asked me about a book I'd read and I said Kardiner's Psychological Frontiers of Society and then they admitted me on the spot on the basis of that. But I only wanted to go to Chicago so I got all my teachers, my teachers who supported me, to write letters to the University of Chicago and they admitted me and gave me an honors scholarship so I was able to go there. My parents were very poor by the way. My father was a civil servant. At the time I went to college I think he was making about four thousand dollars a year, this was 1949.

Runyan: Had either of your parents been to college or trade school?

Levine: Yes, my father had been because he was an engineer but the secret was in the family that he hadn't actually graduated. He went four years but basically flunked certain courses and so therefore never was a graduate, Ohio Northern University in Ada, Ohio. My mother had gone a little bit to Columbia, but anyway neither was an actual college graduate. But they were very much behind my career and they agreed that I should go away to college and they had saved in spite of the fact that they had very little money, they had saved so that I could go to college.

Runyan: They supported this University of Chicago idea?

Levine: Yes, they did. I had tuition paid for by the University of Chicago the whole time I was there and my parents paid my room and board and I supplied, from my work--I forgot I did work in high school and that was from my junior year onwards. I appeared on a quiz program on television, NBC television in the New York area, a program called Americana which was an American History quiz program. I was an American history scholar at our high school and so I was chosen to go on this program and then I was told then I was on it for every other week and then I would be on it every week. But my senior year I was on every week and there would be these us high school kids against a group of celebrities and they would show these skits about American history and we had to tell what they were. The celebrities included James Mitchener, he was one of the leading ones and I can't remember the rest to tell you the truth but Ben Gauer ran the program and it was Firestone Tires that was the sponsor.

Runyan: Who won the contest?

Levine: We kids often won, we were as good as some of these guys but Mitchner was such an obnoxious person, to us kids he was obnoxious. He didn't like us, anyway it was great fun.

Runyan: Maybe this motivated his later interest in his dream, all these historical novels.

Levine: Right, having to put up with these smart-alecky kids. Later you wouldn't have even imagined that you could possibly compete with James Mitchener on history but he wasn't all that fantastic

compared to us. Well it was great fun and I was able to save enough money to finance the first two years of my, maybe it was just the first year, my sort of spending money at college from that.

Runyan: Are you now ready to say a little about intellectual influences at the University of Chicago and college?

Levine: Yes. I arrived at the University of Chicago in September of 1949. We took placement tests and I placed out of, you know many people placed out of a tremendous number of courses so the average for somebody who graduated from high school was that you would get your BA in two years and that would be eight courses. I placed out all but seven courses but one of the courses I placed out of was Social Sciences II the title of which was Culture and Personality designed by David Riesman. So I decided that I would take that course even though I had placed out of it and since I had only seven courses that would make an eighth. So I took that course and by chance, the sections were all taught by faculty members there were no teaching fellows, I was assigned to David Riesman's section and almost immediately I formed a strong relationship with him and he's been the greatest mentor in my life and that started when I was seventeen right in my first month in college, I mean its an amazing thing. You understand David Riesman was a great mentor to a large number of people in the social sciences he came in contact with. He's one of the great mentors of all time, but for me it was tremendously important.

Runyan: He must have just started social sciences relatively recently?

Levine: Yea, I don't know when, he must have started a couple of years before that course and then during the year I was taking the course he published The Lonely Crowd and in fact he was still giving some of the ideas from The Lonely Crowd as lectures in the first part of the course but then in the spring of 1950 it was published so then he became a celebrity and so on later. Then after I got my BA at Chicago, well first of all I have to say that the University of Chicago was the most exciting educational experience I'd ever had. It was like I had died and gone to heaven it was and every course we had started with Plato and Aristotle, whether it was Humanities or Social Sciences or whatever it was always even Physical Science you start with at least Aristotle if not Plato and mostly everybody was taking the same courses together and therefore we would argue about these things in the dormitories. The boy's dormitories had nothing to do with girls because there were very few girls at the University of Chicago, compared to the number of boys. Most of my friends initially were mathematicians and scientists and philosophers so that was really important. Then I got really interested in logical positivism which was not being taught in the college at all, I mean we were just studying Plato and Aristotle. In my third year when I was already a graduate student in Anthropology, I got my BA in two years, I was admitted to the graduate division as a master's student and then I sat in on a course on the Methodological Foundations of Physical Science taught Rudolph Carnap.

Runyan: Who should know something about logical boundaries?

Levine: Oh, this was heaven. This was great, I mean by this time I had actually talked to a number of Carnap's students and I had read enough background stuff in logical positivism to know that he was the great figure in the field and it was wonderful sitting in on his course. You know it was just nice that he let me do that, but I decided not to be a philosopher. I still decided that I wanted to really do social science work following the logical positivist model rather than to be a philosopher of science or philosopher of social science.

Runyan: How did you get into anthropology then, back at this Kardiner?

Levine: Well, I was still thinking about Kardiner. Of course I had studied with David Riesman who inflamed my interest further in culture and personality ideas. Riesman also told me at some point, I went to see him long after I had studied with him and when I was talking to him about where should I really go to study and he said, well there's a new book you should read called Childhood and Society by Erik Erikson. So I went to the University of Chicago Bookstore and bought it and of course that had a great impact too, it was really important. I had never heard of Erikson up until that time. I mean, I was determined to do something in that area. The University of Chicago had wonderful teachers in anthropology, I mean they were fantastic and I studied with Redfield. He was my advisor but I disagreed violently with him. I wanted to write my master's thesis on a comparative study of Shamanism and I wanted to use a logical positivist comparative approach, Redfield was totally opposed to it. He used to encourage students to write papers and then he would give you several pages of typed feedback on your paper and then you could answer his feedback and we had this like exchange of documents, I think I have those papers somewhere.

Runyan: What was he objecting to?

Levine: Redfield was really opposed to formal methodologies. He was much more, I don't know exactly how to call it, but maybe Weberian we would call it now. But he didn't think that formal comparison was the right way to go, he's more historical. He'd want you to see changes in human cultures from a historical perspective and he felt this was some what barbaric to do it a logical positivist way.

Runyan: Was Carnap somebody you could talk to about social sciences then?

Levine: No, Carnap really had nothing to do with social science and poor Carnap, I mean nobody had much to do with him. I mean, he was located in a philosophy department at that point that had been stacked by Aristotelians, by Hutchins and Adler. But Charles Morris was the one apparently who brought Carnap there so he had one person to talk to, no he was cut off from the social scientists that wasn't his thing. Not that there weren't social scientists around that could be classified as logical positivists, I mean for example Otis Dudley Duncan, the demographer was there. I heard some of his lectures, they had a great impact on me and there were others too but I can't think of them right now.

Runyan: Were there any other faculty or students that you wanted to say anything about?

Levine: At Chicago, well, my real advisor, you see Redfield was actually my substitute advisor because Fred Eggan went off to do field work in the pueblos, the pueblo indians in the later part of my second year. I had worked very closely with Eggan, I liked Eggan and he liked me and we got along well. A very quiet man and he wasn't interested in any of the same things I was interested in but he was just a very fine person and I knew him throughout his life. There was a critical thing that happened in my second year at Chicago, my second year as a master's student at Chicago. I went to see Eggan and said, I want to leave this university and go somewhere else because I don't think there's anybody who really cares about culture and personality here and I want to be at a place where I can really study with somebody who really does and he said, oh, why don't you go and see Milton Singer, or maybe it was Riesman who did, I must have made the same complaint to Riesman. But I went to see Milton Singer who had just completed a gigantic critical review of culture and personality and we got into an argument and I mean I thought, well so he thinks it's worth paying attention to but only from a negative viewpoint so that doesn't help. But I've known Milton, and once again it was all friendly etc. but I thought, I want to go somewhere where I can really study and Riesman tried to get me to go into the committee on Human Development so I went to see Bernice Neugarten. I don't think I actually went to see William Lloyd Warner but he would have been a critical person to see, but I didn't I talked to Bernice and I wasn't impressed with what was offered.

Runyan: So at that point they didn't have much of an anthropological or cross-cultural focus.

Levine: Well, it had William Lloyd Warner who was a famous anthropologist. He was in the committee on Human Development and I didn't feel that what he was doing was really psychological anthropology as I thought of it then. We didn't use that term, by the way. I wanted to go to Columbia to study with Kardiner. I told this to Egan, he said, no that's all finished. Don't go, Kardiner has nothing more to do with the Anthropology Department so you shouldn't go there, this is now 1952. He said, where you should go is the Harvard Department of Social Relations. I said, I don't want to go to Harvard, I want to go to Columbia. Well, he said why don't you apply to both places, so I applied to both places and he organized it so that I got a fellowship at Harvard and I didn't get anything at Columbia. So that was it, I had to go to Harvard and of course he arranged it with Clyde Kluckhohn, who was his good friend. So I got a fellowship at Harvard because of Clyde and then when I got to Harvard I discovered that John Whiting was here, I didn't know that. I actually had read his book, Becoming a Kwoma and somehow I thought he was still at Yale, I wasn't aware. He gave a talk which I went to at the 1952 Anthropology meetings, it was on the Freudian Concept of Fixation and I thought that was great and I still didn't completely understand that he was at Harvard. He said he was at the School of Education which didn't make any sense to me and so when I got here as a student I was assigned to Dave Schneider as an advisor and then I discovered that John Whiting was here presiding over Palfrey House, that was the Human Development lab. He had just taken over, Bob Sears had just left in '53, to go to Stanford and John succeeded him as the director of the laboratory of Human Development. So I started spending more and more time with John Whiting during my first year, although my sort of major mentor and role model was Bill Caudill. Caudill was on the faculty, Schneider was my advisor. I also worked with DuBois. By the summer of '54 the Whitings were starting the Six Culture study, really it was John Whiting with Irvin Child and Bill Lambert and they invited me to be a research assistant on that project and I spent a good deal of that summer with the Whitings and the planners and the all field teams.

Runyan: So this would have been the summer of '54?

Levine: Summer of '54. I was doing two things, I was working for Clyde Kluckhohn and Ethel Albert on a bibliography on values and then I was also participating, without pay I guess it was, in the planning of the Six Culture Study, or what became the Six Culture Study, the Five Culture Study. Then in the fall I became a research assistant for the Whitings on the Five Culture Study. I was Bea Whitings research assistant, she was the coordinator of the field work. Bea and I got along pretty well together. John and I always have had a combative relationship and that's the way it's always been.

Runyan: What's that about?

Levine: Well, it's about two things. On his side it's about the fact that the students of his that he really likes are people with mathematical ability and sort of a gift for formal approaches and I was never that way. I was more ethnographic, even though I had this logical positivist approach and I was enthralled by the fact that John Whiting came out of a logical positivist approach too, namely even though he didn't spout philosophy because at Yale under Hull they had been totally in view of logical positivism. After all Clark Hull was the most, you couldn't get any more logical positivist in psychology than Hull and what's more John Whiting had also been at Iowa with Bob Sears and there had met Gustave Bergmann who was an important figure from the actual Vienna Circle. So, I mean Whiting knew what he was talking about in terms of logical positivism but he didn't talk philosophy.

Runyan: He was influenced by Murdock.

Levine: Oh, yes Murdock had been his teacher, absolutely at the Yale Institute, oh yes. That whole Yale group as it turned out was a kind of logical positivist program but you know the Yale Institute of Human Relations was a failure you understand but then it's successes were kind around the edges. It failed in it's larger goals but this one track through it that went from Hull to his students and Murdock and Whiting, that was very successful.

Runyan: How was this logical positivist approach received at Harvard when you came here? Were there people here who were into that, I guess Whiting.

Levine: Well, Whiting but not people centrally in social relations. No, Clyde Kluckhohn didn't have much time for it. Clyde was at that time really interested in linguistics, now by the way, I had a big disappointment when I came to Harvard. In those days you were sent from one professor to another, I was only 21-year-old, I was sent from Fred Eggan to Clyde Kluckhohn. I get here in September of '53, Clyde Kluckhohn has become the director of the Russian Research Center and he handled students, he's not teaching, he's not advising.

Runyan: As a footnote, do you know how that happened, how he became head of the Russian Research Center?

Levine: No, and it still seems a complete anomaly to me but he was a widely admired person within Social Science at Harvard. He was considered by some a young dynamic person who could handle all kinds of things so I don't know, I guess it was Merle Fainsod the political scientist who must have prevailed upon him to run it. I particularly don't understand why he agreed to do it because it didn't have any connections with his normal work. Of course Alex Inkeles was involved in it and Inkeles was a central person in the Social Relations Department, very important. But I don't know, but Clyde was nice enough to say okay I can't teach you but you know I recognize you came here to study with me so every Wednesday night he would have me to his house along with some visiting scholars and one other graduate student. The one other graduate student was Bob Gardner, now the head of the Visual and Environmental Studies at Harvard. He was a graduate student in the Department of Anthropology, don't ask me why Clyde had him come there, I never understood that and the others only understood more or less why I was being invited just because I thought he felt guilty to Fred Eggan essentially but then there were other people who were visiting scholars who were there. Kathleen Gough who had worked in among the Nayars in India and a visiting Indian anthropologist named Srivastava, who's not a well known figure at all. We had a good time and of course, I don't know about the Indian scholar but the rest of us drank enormous amounts of bourbon. That was Clyde's idea of having an evening seminar was to get everyone a glass filled up with bourbon. It was fun but I have no recollection of any conversation that occurred perhaps because of all the alcohol content.

Runyan: How about your interest in psychoanalysis whether there was anybody around here that you connected with.

Levine: Well, yes. Well there were two things, first of all there was Clyde. Clyde Kluckhohn was deeply involved and interested in psychoanalysis. He knew everybody in the field, he had very articulate opinions and I learned a lot from Clyde about what was going on in that sort of area but then you have to understand also we were taught a lot. I mean, we had to take Henry Murray's Problems and Concepts in Clinical Psychology course and the year I took it Gardner Lindzey was running it. Murray was away somewhere and we were taught by various psychoanalysts that included John Spiegel, George Gardner who was a major figure in Boston psychoanalytic circles, he was running the Judge Baker Guidance Center at the time. Alvin Semrad, all these guys came and lectured to us in Murray's course, and of course Robert White, he was developing his notions of effectance motivation at that

time. So that course was heavily psychoanalytic and I learned a lot about it. Dan Levinson was teaching a course too. Psychoanalysis was absolutely a dominant thing around.

Runyan: How about your interest then or thinking about child development through these years.

Levine: Well, what I did, of course as far as I was concerned child development was John Whiting but I knew I didn't have a really good background in child development from Chicago. I'd never taken a course in child psychology. I'd taken introductory psychology and I'd taken a course in psychoanalysis with Bruno Bettelheim which was not a course in psychoanalysis. So I decided I should take John Whiting's undergraduate course on child psychology and that course he taught jointly with Eleanor Maccoby who was, at that time jointly in Social Relations and Palfrey House because she and Bob Sears were running the Newton study at that time and so actually most of the lectures, many more of the lectures, it seems to me, were given by Eleanor Maccoby then by John Whiting. I thought her lectures were extremely informative, I learned a lot. The following year I got to know her very well at Palfrey House. So I would say that I learned most of my child psychology from Eleanor Maccoby and that was really wonderful, she was terrific. I was deeply interested in Hullian learning theory and there was a seminar on learning in the Psychology Department, not the Social Relation Department, the Psychology Department. It was given by Richard Solomon together with three other people including John Whiting, William Verplanck and Eddie Newman were the others and we mastered the literature on the Hullian learning paradigm. The seminar room in the basement of Memorial Hall was located right next to Skinner's lab and Skinner walked by everyday. Skinner would walk by into his lab, never looking inside the seminar room because the hatred between the Hullians and the Skinnarians was so extreme that they didn't basically speak to one another and Skinner would not acknowledge the existence of a seminar on learning that was basically Hullian theory.

Runyan: This seminar didn't do anything with Skinnarian ideas?

Levine: No. Verplanck talked a little bit about it but no, on the whole that was a different thing. So we all had contempt for Skinnarians as the sort of low level people who didn't believe in theory and stuff like that.

Runyan: Did some of that lead to this chapter you later did on behaviorism and psychological anthropology?

Levine: Well, yes behaviorism but you know when I wrote about behaviorism I didn't include Skinnerian. See in those days, that article was published in 1963, in those days to me behaviorism meant Hullian behaviorism. It's funny but that's what we thought, we didn't acknowledge the Skinner approach as anything more than a sort of minor phenomenon.

Runyan: One question I wanted to ask was about the trajectory of your beliefs about behaviorism and psychoanalysis as theoretical systems.

Levine: Well, I can be very concise about that because I went through a major transformation. When I was with John Whiting, when I was a graduate student at Harvard University I was totally convinced that psychoanalysis had to be translated into the language of stimulus response psychology and then out of that could come testable propositions. Then we would go out to different cultures of the world and test those propositions. Then we would revise the theory accordingly and that was the way psychoanalysis would enter into science, through the testing of child development propositions. When I was trained in psychoanalysis at the Chicago Institute from 1962 to 1971 I totally changed my mind about that. The change for me was that, it's funny different people in training in psychoanalysis it has a completely different impact on them. In my case, psychoanalysis made me very deeply aware of the

subjective element and even the phenomenological aspect, that conscious experience was important. The subjectivity was important, that that had been left out by the behaviorists. That behavioristic approaches were very superficial. Psychoanalysis was deep. That meanings were imbedded deeper and so on and so I came out of psychoanalytic training having to try reformulate the way in which psychoanalysis could be translated into research and that's what I tried to do in my 1973 book, Culture Behavior and Personality, is really an effort to sort of go back to psychoanalysis, rethink personality theory and so on in those terms and see if I couldn't find ways of developing a new research program that was empirical and yet took behavior as its subject yet was still searching for unconscious meanings.

Runyan: Could you say a little of it about how you thought about doing it?

Levine: Yes, I was exploring. I said you have to look at all kinds of universal structures, whether it's developmental structures, whether it's the structure of the daily life, I tried circadian rhythms, emotions, universal emotions as a structure on which you could pin these things so I felt that that was one thing you had to have some armature, some basic trans-cultural structures on which you could then relate the traverse meanings from different cultures. That's sort of the major thrust of that book. I also made a very powerful sort of critique of personality tests which was the center piece of the book and in fact the only thing that most psychologists noticed, I said that basically Murray had set things off in the wrong direction by trying to get rid of the intensity of psychoanalytic observation. In my own experience in psychoanalysis, I felt I had learned that what psychoanalysis gave you, that psychoanalysis' greatest strength was it's looking at individuals, spending so much time on them, so you know a diagnosis would be arrived at maybe after two hundred hours in psychoanalysis. Whereas you know academic psychologists would spend about 20 minutes or 2 hours if they were clinicians and it seemed to me that was in itself a great strength of psychoanalysis in terms of the understanding of the person. So I became very hostile to the personality testing tradition and that's what that book has a great deal of and of course that's why I got such negative reviews by psychologists, particularly.

Runyan: In terms of continuity and change in your beliefs as a whole, do you have thoughts about changes and revisions you'd want to make in that theoretical structure at this point?

Levine: Of the 1973 book? Oh, absolutely. Well, some of them are noticed in the second edition, the 1982 edition and I added a section on the Self and Culture, but basically what happened was that over the years I was influenced by two anthropologists. First in Chicago by Victor Turner who the great student of African ritual and I tried to do some of his work when I went back to Africa in the 70s and the other person who influenced me so much was of course Clifford Geertz. Cliff and I had been students together at Harvard although he was ahead of me and he was always rather hostile to me when we were students because he saw me as a student of John Whiting, which was true and as representing what he regarded was the worst kind of logical positivist. But at Chicago, we taught at Chicago in the same department for 10 years and we were part of the committee for the comparative study of new nations together. We saw each other every week at seminars and in the long run I became deeply convinced of Geertzian Symbolic Action approach and of the importance of Max Weber and a meaning oriented system and of ethnography and I got to be much more Geertzian after I got to Harvard mainly because I felt that the psychologists here were still absolutely benighted heathens, Jerry Kagan and so on that they simply didn't understand what culture was and I included the Whittings I mean, we had big arguments. The first year I was here I taught with both Whittings, we taught a course of socialization of the child. It was an undergraduate/graduate and we had like a hundred students I don't know, it was a big course taught in Longfellow 100 and with sections and we had lots of arguments where I was arguing for a deeper Geertzian understanding of culture and cultural meanings and cultural models and they were going by their older more behavioristic approach. I think I convinced them a little bit but anyway I became much more turned toward the Geertzian direction as the years

went on even after writing the '73 book. I mean that's what '82, about '82, I had really become very strongly involved with the Geertzian symbolic action approach.

Runyan: How about from '82 to the present?

Levine: I don't know what to say exactly about changes, I mean I'm still trying to work out what I feel. Okay, let's come back to child development here, that's really the major thing. When I was working on the VanLeer project on human potential with Howard Gardner and Corky White, Iz Scheffler that's when I became convinced that here Howard Gardner, particularly represents somebody who felt that the way that at that time, believe it or not, his real feeling was that the way to go to child development and understanding potentials was to go into the brain. He was tremendously influenced by Norman Geschwind and that was his thing and when I took the opposite approach, or what I felt to be the opposite approach, which was that what we really lack is a social theory of child development. That sociologists had more or less given up on child development, which is amazing, when I was young there were lots of sociologists involved in the child development field and then they sort of phased themselves out of it. So I felt what we really need is a social theory of child development and one that was grounded in comparative knowledge. I mean, I wasn't very happy with the social theories of child development of the past, the Talcott Parsons and so on. I was very well aware of those but I thought we could do better than that and try and mount a new social science in child development which was culturally oriented and very comparative and that's what I have been working on for some years now.

Runyan: So the human conditions book is the first statement of that or one statement of that?

Levine: Right.

Runyan: That's what comes out of the VanLeer project?

Levine: Right, that was my contribution from the VanLeer project and that's very comparative but it took as it's theoretical frame work some of the ideas of Rolf Dahrendorf about life chances which he had derived from Max Weber.

Runyan: What's your perception of how that book was perceived or what influence it's had?

Levine: Well, it took three and a half years for it to get reviewed in American Anthropologist. I mean a lot of people have had this experience and that was a very good review but it sort of fell between areas, you know. The only person who ever got excited about it was a historian at the University of Chicago named John Craig who was using it and he tried to get it reprinted as a paperback which the publisher was uninterested in doing until it sold out all the copies they had printed in hardback, it was a disaster in that respect.

Runyan: I don't know if you know a historian in Berkley, who teaches in social welfare also used it in his own small seminars he did.

Levine: I was aware it was taught at the social welfare school at Berkley.

Runyan: His background as historians so maybe they caught or some that were more interested in the transition between these traditional and modern cultures in a place with different structures (very difficult to understand what was said).

Levine: It was a very historically oriented book and I think I did really go back to Redfield, you know Redfield while I was his student he published a book called, The Primitive World and It's

Transformations which you know I think there's this impact of that kind of Redfield in that book. But I remember what was important for my own thinking and that was yes, you could say the kickoff for sort of developing a social theory background for child development.

Runyan: You may have touched on some of this already but would you say what published or unpublished manuscripts best represent your thinking about child development and which of your studies seem most significant and on the other hand, which seem the most wrong-headed at this point.

Levine: Oh, boy.

Runyan: Should we do this one part at a time, which manuscripts best, you know reflect your thinking about child development? Which of those seem the most significant now?

Levine: I think of things, recent things rather than old things in this respect. The article on enculturation that I wrote for a book called The Self. Dante Cicchetti and Marjorie Beeghly wrote this book on The Self and I did this article on enculturation, which advances a sort of population oriented view and that is certainly one that I feel contains a lot of ideas that I'm still spelling out.

Runyan: What do you mean by population oriented?

Levine: That right, I've tried actually for more than twenty-five years now to articulate the notion that cultural psychology if you want to call it that or cross-cultural studies in human development really should model itself after other kinds of population studies like demography, like epidemiology and so on. What I like about that idea it that your comparing different populations across the world. We know something about what a population consists of and your looking at statistical tendencies of that population against its environmental parameters, or its institutional parameters. That relationship, that's what Alex Inkeles once called the correlations of a state and a rate. A state meaning the sort of general institutional parameters or cultural parameters and a rate meaning the aggregate statistics on individuals in that population. This is what you do in population genetics, in epidemiology, in demography and it seems to me that's what we should be doing and I've tried to argue that point and I think I've gotten stronger and stronger in my arguments for that position.

Runyan: Where did this idea come from? Were you thinking of this in relation to classic critiques of culture and personality studies back in the early 50s or did this idea population thing come from some other source later on, some outside source?

Levine: That's a good question, first of all I was well educated in the social sciences so I mentioned that I had studied with Otis Dudley Duncan, I mean I knew what demography was all about from my time at Chicago. I think that I mentioned Alex Inkeles, the correlation of a state and a rate, that's in an article of his in a book called Sociology Today by Merton, Broom & Cottrell published in 1959 so I know it was in 1960 that I first put down notes about the population approach so maybe I had read the Inkeles article. Also by the way, you mentioned the critiques of the kind of essentializing, as we know call it, of culture and personality days. The best critique ever written of course was by Inkeles and Levinson in Lindzey's Handbook of Social Psychology, their article on National Character. That articulated that position basically although they didn't call it population position but they said, look we are dealing with statistical distributions within. Another thing, I had studied biological anthropology with Sherwood Washburn at Chicago and he made us read William Boyd's Genetics and the Race of Man, that's where I learned about population genetics. Washburn also lectured about that and stuff like that, so I knew about population genetics and gene frequency and it seemed to me that what Inkeles and Levinson were saying was that we should have that kind of frequency distribution data for

behavioral features of the populations. Also Gordon Allport, whom I used directly in my 1973 book was a tremendous impact there because in his book, The Nature of Prejudice, it told about group differences, you know these four different kinds of group differences and there again you have that, that's a kind of population approach too. So those are the things that I think of in the origins of my thinking from a population viewpoint. Then later Don Campbell and I developed that idea in our book on ethnocentrism.

Runyan: Did you have a sense of where this population idea sunk into child development of cross-cultural child development research?

Levine: No, I don't believe it has at all, I don't think it has had an impact. But culture is now fashionable in child development stuff, but I don't think population approach. But I have a new book coming out on child care and enculture where I have, I think, a stronger statement of it in the first chapter of that book and then I'm working on now another book with Catherine Snow where I shall present it again and try to influence more people about using the population approach.

Runyan: What's that book going to be?

Levine: Well, we call it Human Nurture, Social Process in Child Development and it presents what we consider a new environmentalism for the child development field. The focus of it is on communicative development of the child and looking at cultururation as part of communicative development. I will present that population oriented approach as part of a necessity for a communicative approach to development.

Runyan: Do you have any speculations about why this population centered approach and maybe cross-cultural approaches in general haven't affected child development as much as they might have or what prevents that from happening?

Levine: Many things have prevented it from happening. Possibly the most important thing is that child development research is in the hands of psychologists. It's funny when the SRCD was formed and when the journal, Child Development was founded no one expected the child development field would just be so dominated by psychologists. It was thought that it is going to include all of the sciences that deal with child development and children.

Runyan: Are you talking about the 1920s?

Levine: Yes the 20s, certainly the 20s. As it turned out, except for a few peripheral pediatricians and psychiatrists most of the people involved are psychologists and psychologists, I mean have a tendency to look at things in a decontextualized way. They think that unless they are generating universal laws then they are not doing anything worthwhile or they deal with very small samples and they don't want to think about these sort of big aggregate kinds of things. So I think, one part of it is we're are dealing with some ingrained prejudices and assumptions and operating styles of psychologists and that's part of the reason why population studies isn't a very big thing for them, I mean they don't see demography and epidemiology as part of what they are interested in or anything that's related to their own work.

Runyan: Do you see any promising signs in this integration of say, hard science, psychology approaches with population or cultural or historical...

Levine: No, I don't see any promising signs but I think there is an opportunity to make a case now. I think there's a great passion in the child development field for building bridges with neuroscience and molecular genetics because these are fields that have advanced a lot and have a lot of prestige and all

that but I'm trying to argue for a different connection between soft and hard science, if you will that is more modest. It isn't something that is going to create great breakthroughs as one always expects from DNA research or something like that or suddenly discovering the brain you know and stuff like that but rather that taking as given that we are a social science and it's going to be messy and that a lot of what we are looking at is statistical and it's never going to be so perfectly nailed down and that we have models for that in other sciences. Let's look at them and biomedical science is often a population approach which I think is extremely relevant and useful and it is an appropriate scientific approach for people when you are dealing with phenomenon that are statistically distributed in different environments and different habitats and so on.

Runyan: I guess do you see that part way between universal law stuff and the ethnographic one culture at a time approach to child rearing or child development?

Levine: Yes, and it permits both ethnographic depth and a certain amount of comparative rigor, cross-cultural.

Runyan: Do you want to deal with the question of which of your own approach seem most wrong-headed?

Levine: Which of the approaches...

Runyan: Of your own work I guess?

Levine: Oh, of my own.

Runyan: Yes.

Levine: Well, I once wrote a book about achievement motivation in Nigerian adolescence which I have disavowed. Unfortunately it's a book that a lot of people read and you know for years people would know me in relation to that book, it was very hard to tell them, look I don't believe what I found there anymore. I did a critique of that book in my *Culture, Behavior, Personality* and then I did an even more severe critique in an article I wrote somewhere else about Nigeria. Basically, I felt that I had jumped to conclusions on the basis of very flimsy evidence and I hadn't ruled out alternative explanations of what I was claiming of the data.

Runyan: And you became more skeptical of projective tests or of TAT, things over the time?

Levine: Definitely, that was building up to that, yes. That's why in the '73 book that is when I expressed the skepticism.

Runyan: Now, Bob could you say something about what political and social events may have influenced your research and writing?

Levine: I think that I should say first of all that I've always believed in ethical neutrality. I am not, I mean in the sense that I do not go along with the idea that child development research or any of the kinds of social research I'm involved in should have a political orientation. I think there is a point to try to be impartial and to transcend partisan political viewpoints and all that sort of thing and I don't think that the post modern kind of perspective that says that well everything is political is valid or is the right way to approach these things. I think there is a place for science apart from politics. However, I'm also very preoccupied that in the child development field there is a kind of child development movement since the late 19th century, or even earlier that we're really all part of a romantic movement

concerning childhood and caring about children and so on and redefining the parent/child relationship. Which has to be acknowledged, that's one thing. Now then there is another part that I think is very important where I will acknowledge my own ideological perspective and that is that I've always been an environmentalist, that I am against racism, that I am against the sort of excesses of innatism which have tried to be genetic reductionists and that sort of thing and in my current writing I am still fighting that battle because I feel that in a sense, it turns out that every generation has to refight the battle, the nature/nurture battle over and over again. As an anthropologist I'm keenly aware of the culturally varying environmental conditions and my experience in Africa and other places has only deepened my conviction that the conditions under which children are raised are very variable and therefore I am an environmentalist and I'm very opposed to the kind of innatist positions that would see humans as locked into a single universal framework. So that is political of course because it has lots of political connections. I try to argue it however on strictly scientific grounds, empirical grounds and as well as accepting a lot of the advances in genetics and biology and so on which I think we have to accept.

Runyan: Okay, is there anyway political and social events have influenced your teaching?

Levine: Yes, well I think of course one always has to realize that students are affected by these things and therefore you have to address them where they are. I feel that these days I'm caught in a very strange kind of bind because on the one hand when I'm addressing psychologists, I'm the cultural critic of psychology who is attacking a field that is generally very universalistic, tending toward a biological innatist orientation and very stuck in a kind of old-fashioned positivism so I have to talk out of that side of my mouth. On the other hand, as an anthropologist I'm fighting against the post modern Solipsism, radicalism and so on and it's really hard as a teacher to talk out of both sides of your mouth like that. Part of the problem is knowing who your addressing, but my problem is I have students from everywhere in my classroom, so I do talk out of both sides of my mouth that way.

Runyan: Partly just addressing the kind of perspectives the students bring in or colleagues and all the changes over time.

Levine: Yes, and I do try to emphasize, in fact we do have some very strong still effective traditions in this field, child development field as it has been developed over the years of trying to be balanced in our views about science in psychology and so on. In other words we don't have to look upon the political swing of the pendulum as simply something negative about the field but rather that the field adapts and changes and encompasses this debate and represents it over the years in a different way and that's the way it is, that's all.

Runyan: Could you reflect on your experiences with the research funding apparatus over the years and this can include your participation in shaping research funding policy or implementation or securing support for your own work in related manners?

Levine: Well, I certainly have a long history there. I guess I got my first NIMH grant in 1958 and my National Science Foundation support started when I was a graduate student. I had an NSF fellowship and so I guess between NIMH and NSF I've had a lot of support and have also participated in their peer review committees over many years. I was also very privileged to be supported by NIMH with a research scientist development award for five years and then a senior one for almost ten years so that I was supported for fourteen years. I didn't do the fifteenth year because I had to leave the University of Chicago to come here.

Runyan: Roughly what years would that have been?

Levine: Good point! It was the years 1962 to 1976, my salary was fully paid by NIMH even though I was teaching at the University of Chicago and I had a very reduced teaching load. It also supported some of my training in psychoanalysis. I wish I could say that I had an impact on the policy of grant giving but my work for NIMH and NSF was just on deciding who got grants and stuff like that and as for my own publications I doubt that they have that sort of impact concerning policy. I don't know what to say, I mean as an anthropologist I've always been you know like the anthropologist on the research panel. If it was on the NIMH side, actually we use to have two anthropologists on the NIMH research panel.

Runyan: Who was the other one?

Levine: Well, it was originally Morton Fried and then it was followed by Roy D'Andrade. At NSF I was on the anthropology panel so there in a sense I was the psychologist, that's the way it kind of worked. I don't think I did anything more than anybody else in this area, so I don't have anything much more to say about it.

Runyan: Is your time at SSRC related to funding decisions or policies?

Levine: Yes, but let me say something else, I will come to the SSRC thing later but when government research money started declining in the late 70s and particularly the early 80s I then started getting more deeply involved than I had been in getting money from the Ford Foundation and other private organizations, private foundations. I had a lot to do with private foundations where at times I thought I did have some impact on their policies, Ford Foundation particularly where I was involved in this woman's education program that went on for a while and I've had money from Rockefeller and the population council and the Spencer Foundation of course so I think that, I mean in the last fifteen years, most of my funding, not all of it, some has come from NSF but most has come from private foundations and I have done various kinds of consulting, you know for especially the Ford Foundation.

Runyan: Did you have a sense of how funding availability effected the course or direction of your work?

Levine: Yes it did unfortunately, not up to the 70s but after when things started getting tight I decided to get more involved in population and health issues in relation to maternal education. I saw that there was an opportunity to use some of the discoveries that were coming out of demographic research about maternal schooling as a powerful determinant of child survival and reduced fertility. I saw the possibility of getting funding from sources that were dedicated to health and population if I emphasized that and at the time I had been working in this African society which has the highest fertility rate in the world, as it turned out, I didn't know that until late in the game. So I did, I mean for the last fifteen years, well the last twelve years really, I have been working in this area very intensively in getting money that was from the health research foundations.

Runyan: Okay, could you please describe your connections with the major universities that you spent time at and if you could describe some of the changes?

Levine: Repeat your question.....

Runyan: This question was about major universities you've been associated with and a variety of sub questions about, you know what changes in the units happened while you were there, what objective were being pursued? What frustrations and achievements were encountered and lastly the role you think that the unit played in the history of child development research?

Levine: Well, that last question could take up a great deal of time because basically I've only been in two departments. My first two years, after I got my Ph.D I was at Northwestern University teaching in political science and anthropology, so I am going to leave those two years out. The most important thing about those two years in terms of my own career was simply that that's where I started working with Don Campbell. Then I taught for sixteen years at the University of Chicago, Committee on Human Development and now it has been eighteen years in the Human Development program at Harvard. Both the Committee on Human Development at Chicago and this program here at Harvard have very long histories and I'm not going to go into all the detail about them because it would take up too much time. At both Chicago Human Development and Harvard Human Development I was always playing the role of the anthropologist, quite frequently the only anthropologist but not always. Before I left Chicago I brought Rick Sweder into the Committee on Human Development and then I left and so he has maintained that tradition there and here at Harvard when I arrived Bea Whiting was a professor in this department. She had been my teacher years before, but then she retired four years after I got here, so I was by myself again. So in both places I was the anthropologist. At Chicago my NIMH award prevented me from being the chair of the department which I deeply felt gratified for. So I never, even though I did play a role in the governance of the department, I was never the chair of it and I didn't really determine its policies although I participated in the determination. I can't say that things changed all that much in the sixteen years I was there, or that I had a great impact on them because of my NIMH award I really kept myself aloof to some degree.

When I came to Harvard it was very different, I came as a regular professor. I was going to participate totally and I did and I was chair of the department for all together more than six years although it was in two different time periods. The first one was '77-'80 and the second one was '85-'89, something like that. It was close to seven years and the big changes that occurred were when I was chair the second time. The first time I had no impact on this department. The department was run, as many Harvard departments are, as kind of each professor completely on his own with a sort of a feudal system and it took some doing to get beyond that. But later when Pat Graham became dean and I became chair again there was the opportunity to appoint new people to tenured positions and then in the late 80s I was able to organize and push through the appointments of Kurt Fisher, Howard Gardner, Catherine Snow, Carol Gilligan as professors and that was enormously gratifying and it did make a big change in this department in every way. It wasn't simply the change in the way a department operates, its a much more unified department nowadays than it ever was before but it also changed in the way in what it sort of represents although it's an extremely diverse group of people and now we can't say that the Harvard Human Development Program represents a single position on child development. It certainly doesn't and perhaps it's even less that way now than it was in the past. On the other hand, it's less locked into various orthodoxies than it was at one time and I guess I have to say that specifically it was Larry Kohlberg who had his own orthodoxy as it developed and the other people were just working on their own kinds of things like Jerry Lesser on children's television. He wasn't an exponent of any particular theoretical position but I remember students coming, first year doctoral students, coming to Harvard in the late 70s and saying well I came to this department because Harvard represents the structural developmental position in child development and I don't think any body can say that now. I mean we represent some very strong positions, I mean Kurt Fisher and Carol Gilligan certainly represent things, their own views of child development, as I do but there isn't a sense of a kind of orthodoxy about it. It's much more sort of provisional. Everybody is working with theoretical positions against research findings, there's change that's going on all the time too. So I mean things have changed here, I guess everything's become very pluralistic but still more unified as a result of the procedural changes where we have dedicated ourselves to creating a more of a unified department than we ever had before. I mean in all fairness to Larry Kohlberg as well as the other people who are here, their model of a functioning department was one in which each professor established a kind of workshop. I mean there wasn't any attempt really at unity anyway and now we have a different feeling about it.

Runyan: Could you say something about your perceptions of the place that this unit or the Human Development program has in relation to the history of child development and research?

Levine: Yes, you mean just for the Harvard department?

Runyan: Or Chicago if you would like to do that too.

Levine: Well, it's very funny, here's where I think Larry Kohlberg had a great influence. You know when these departments were started both the one at Chicago at 1940 and then the one at Harvard in 1949, they were called Human Development and that was kind of funny because they were called Human Development and that was really to distinguish them from child development, that is they were dedicated in the abstract to the idea that development continued beyond childhood so they used the word human development instead of child development. But as Larry pointed out of course they weren't very dedicated to development that is to the concept of development to taking development seriously along the lines of Werner, Piaget kind of thing and so it was really Larry Kohlberg who both at Chicago and at Harvard, I mean he taught at Chicago for some years too, we were together there. At both places he represented the sort of more developmentalism of the place. It's important to remember that, well the Chicago department didn't have a very strong theoretical orientation when it was started by Havighurst, I guess Ralph Tyler was also involved in the beginning of it, these people were not theoretically oriented. They were practical kind of people, very sophisticated people but they were propounding a theoretical position and they thought it was terribly important to study the life span and child rearing and that sort of thing. The Harvard department started in exactly the opposite sort of circumstances. They hired Robert Sears who was at that time the leading Hullian behaviorist who was primarily concerned with children and child rearing and he had come out of the Yale Hullian tradition. He was at Iowa and he had brought to Iowa John Whiting as his assistant and when he came to Harvard he was allowed to bring John Whiting here and that's how cultural studies got into the Human Development curriculum here. Sears then, after four years left for Stanford and then Whiting took over the laboratory of Human Development so the cultural side became very important. But the point is that the Sears and Whiting group had a very strong theoretical position which was that of Hullian behaviorism and the translation of psychoanalysis into SR, to stimulus response theory so they were just the opposite, very strongly theoretically oriented. Chicago was always a kind of pluralistic place and the one thing that was much more developed of course at Chicago was gerontology. They really moved into the life span much more than we ever have here. We're still, here at Harvard primarily devoted to child development and the rest of the life span has not been heavily emphasized.

Runyan: In terms of institutional development, do you have a sense of what relationships, if any, there were between the child development program here and the evolution of the social relations department?

Levine: Well, I think it is very interesting that a number of us who are now professors in this department were trained in the Social Relations Department at Harvard and that includes Kurt Fisher and Howard Gardner and Carol Gilligan and certainly the demise of the Harvard Social Relations Department has created an opening for the kind of thing that we do here which is interdisciplinary social science focused on the life span. If the Harvard Social Relations Department were going now the way it did in say the 50s when I was a student there then it would be a very different thing. The distinctiveness of the program that we have here would not be as great. As a matter of fact of course in the 50s there was a laboratory of Human Development and I want you to know that many of the people in it were also in the Social Relations Department. The laboratory of Human Development at Harvard when Bob Sears and then John Whiting ran it was connected with the Social Relations Department, not institutionally but through the dual participation of both faculty and students. For example, initially most of the students who studied with Sears and Whiting took their degrees in the

Social Relations Department, all the early students were like myself got their Ph.Ds in social relations. The first person who got his Ph.D in the Education Department was George Goethals. The first EDD in Human Development but most of us were students in the Social Relations Department came over to work with Sears or Whiting and retained our affiliation with the other department. Also there were people who had dual appointments and Sears and Whiting themselves had dual appointments in social relations but the most important figure who was primarily in social relations but played a major role in the Harvard laboratory of Human Development in the Ed School was Eleanor Maccoby. When I was a student she was very important in the Human Development program in the Ed School but her appointment was in social relations, so the boundaries were very permeable really.

Runyan: In terms of the institutional division of labor as the son of the interdisciplinary approach to the life course shifted then from Arts and Sciences, Psychology Department now over to the Human Development program here?

Levine: Definitely we are the inheritors, we are the ones who are keeping the light alive so to speak of that tradition of interdisciplinary social science focused around psychology but not exclusively limited to psychology. One thing we have now also with Catherine Snow and Courtney Cazden is also the linguistic part emphasized.

Runyan: Could you say something about your experiences as a teacher of child development research and a trainer of research workers?

Levine: Well, there are a lot of things I could say about it but let me just say that my years at Chicago I didn't do a lot of teaching, I mean I taught one course a year, one quarter course a year and participated in some of the other courses. I didn't have a lot of students either who worked closely with me, I had some students but it was only really when I got to Harvard that I really started intensively dealing with students and I've had a lot of students here in the last eighteen years. The most important thing I have to say about that subject is that in recent years I've had the good fortune to get students from third world countries which Bea Whiting and I tried to get established here to train students in child development from third world countries and we had a lot of trouble. But in recent years I've had students from a wide variety of places, Mexico, Nepal, Malaysia, China, Japan and that has been a great thrill to me and I feel very strongly that that's the way part of our field has got to go. We can't just always be the outsiders doing the studies in their cultures, they also have to do these things and I think I've made some contribution to helping that happen in certain parts of the world. I should say that we had a long, here's a little bit of history that I think is interesting, in 1964 there was a conference sponsored by the US Department of Education at the University of Chicago. The conference was run by Bob Havighurst, Bob Hess and me and we brought in people from all over including the Whitings from Harvard and it was a conference on Cross National Studies of Childhood and Adolescence and it set an agenda for future research. I actually have the report of that conference in a stack of papers of here, 1964 that was. That conference then gave rise to a Carnegie Corporation of New York program to start child development research units in Africa which John Whiting and I took on and then we had Roy D'Andrada as a third collaborator in that project. It was suppose to be in various parts of Africa but because of political difficulties it ended up being just in east Africa in Kenya and Uganda and then because of political difficulties it ended up just in Kenya. The thing about it is that we didn't have a perception at that time of the difficulties of implanting child development research in a society that isn't ready for it and I would say that although the people who were trained, the Africans who were trained from east Africa went back to their countries and did wonderful things in the field of education that those fields were not ready for child development research, those countries were not ready for child development research in a way that some of the Asian and Latin American countries are. In other words, we learned that you have to have a country where there are institutions that will shelter the careers and protect the careers of people who have been trained in the United States. It's

not enough to have them trained, you know you take somebody from a totally undeveloped country, give them a Ph.D in Child Development from Harvard, they'll go back home and they'll become minister of agriculture the next minute or vice president of the country because they're just seen as one of the leading educated people. So you really need almost a surplus of educated people before people can think of having a life long career in child development. You know, child development research hasn't really gotten going in a very strong way outside of North America and western Europe, it's very thin on the ground even in a place like India where there are vast numbers of intellectuals, there are vast numbers of universities. Yet there are only two universities that have anything approaching a child development program in India and the number of people who devote themselves to research on child development in India, you could fit them all into this room, easily. Even Japan has been fairly late in getting started and I think I know all the Japanese child development people which is an indication in itself how small the group is. But I think things are moving along very well in Japan now.

Runyan: Did some of this lag come from slow developments in psychology and social sciences as a whole or something particular to child development?

Levine: Oh, I think it is particular to child development. I think there are several things, one is that anything like the professional study of children is a low status thing. There is this stigma associated with dealing with children in human societies at that kind of institutional level. I mean, somebody wants to get a Ph.D in something grand like physics or economics thinks that it's beneath them to deal with child development. Child development has never established the kind of status in the world of exported university models that attaches to, well economics is the best example in the human sciences, or medicine.

Runyan: How about other social sciences like psychology or anthropology or sociology, do they have much status or not?

Levine: No, they don't have much but they have more than child development. Psychology has been slow in spreading but then there's another problem that you run into apart from the status of the discipline, is the quality of the work and a lot of the work in psychology in third world countries is low grade because it's just completely low level replications of things that the third world scholar learned as a student in America. They don't take account of the setting and the challenge and opportunities of really understanding the setting in which they are doing the research in their own country so it gets to be very dreary work and that's very sad. What you have in Japan now are some people who are doing very sophisticated research that is distinctively Japanese and not just a repetition of American models in psychology. So that's part of the picture, child development is an after thought for most developing countries. The only way you get it going is by saying children are dying on a fantastic scale, or children aren't being educated properly and in fact all of that scary horror story stuff has helped enormously to build child development in this country and in Europe too. The cause of children is heavily motivated by tales of abuse and killing and horror and death and so on and not by positive orientations to a large extent.

Runyan: Could you say what courses you've taught and if you like anything about the tension between teaching and research in child development?

Levine: Well, I teach a basic course that's now called Child Rearing, Family and Culture. I have called it different things over the years including one time Cultural Psychology which possibly might be the first time that that was used was 1987 as a course title, so that's what I teach. Oh yes, it is a great challenge to teach this to a mixed group of people from psychology, anthropology and education backgrounds and it helps my research a great deal because it makes me formulate theoretical ideas in a

way that can be communicated so that I think a lot of my sort of theoretical conceptual frameworks recently have come out of my teaching.

Runyan: How about your experience in applied child development research or in applied work in general, and something about your role in putting theory into practice?

Levine: Yes, let me just go back, I'll try to include an earlier question you asked in this which is my relationship to the Social Science Research Council.

Runyan: Right.

Levine: I participated in many different social science, SSRC committees over the years and I was on the board of SSRC from 1978 to 1983 and the last three years I was chairman of the board so I did play quite a role there. Currently I am on the committee on Culture, Health and Human Development which I helped found and I was the original co-chairman of it with Arthur Kleinman. As applied research goes, I mean one of the things about the Social Science Research Council is that it tried to bring research into contact with policy deliberations and I've also had some contact with the National Academy of Sciences efforts in this regard. I should mention that I was on the National Academy of Sciences Committee on Child Care Policy in the late 80s. We put out a book called, Who Cares for America's Children which was about the whole issue of daycare and how it should be organized and so on and I guess I was the only anthropologist on that panel. So in these various ways, I mean I have been located at some of the interfaces between child development research and public policy and the other social sciences. In my own research, as I mentioned before in this demographic connection with research in different settings throughout the world on the effects of schooling on mothering as it effects health care and fertility. All of this has had a great impact on my research I would say because when you are coming to appreciate the problems that are in the real world that are being faced by policy makers you formulate what you are doing in terms of research in a different sort of way, in a way that can be understood by them, in a way that addresses itself in some ways to their issues. It's also given me a very strong sense of the difference between a research agenda that is driven by policy considerations and a research agenda that goes in directions that might be just scientific and I'm very keenly aware of that because, and I say that in my new book with Suzanne Dixon, Berry Brazelton and various other people, Child Care and Culture, Lessons from Africa. In that book I do argue in the first chapter that child development research has been heavily driven by agenda coming from the fields of education and psychiatry in the 20th century. In other words, that even if you look at the journals as they are now, a journal like Child Development, you have to understand that a great deal of what they are studying has somehow been legitimized as relevant to either the field of education in a very general way, like cognitive development and competence and all of those sorts of things or mental health psychiatry issues like emotional development and anxiety and all of those sorts of things so the fact is that in the background there are practical considerations that are pushing the field along and I argue that that orientation, I mean it's perfectly understandable that people formulate questions for child development researchers which come out of legitimate concerns about mental health and education. But I argue that when you take those questions out to other parts of the world you can get into trouble intellectually because those questions are framed in the context of our society, not their society and you get not only questions but answers that are, if not absolutely wrong are very partial and are full of blind spots to what's really going on in their society.

Runyan: In terms of these theory or policy roots of research where would you put your Human Conditions book?

Levine: Yes, that was a book that tries to look at the historical trends in the field of the conditions under which children develop, these trends particularly in the 19th or early 20th centuries changed so

radically the conditions of children in the western world and then started spreading out into other parts of the world that it's really a manifesto for arguing that we have to pay attention to these larger issues, the historical trends out of which our science of child development in fact comes. It just amazes me in a way that now one has done this earlier, I mean has tried to ground our whole field in, not just it's indigenous western context, but in a larger context of the changing fate of children in the world. I think that out of that kind of perspective you gain a lot of strength in seeing how the research we do is grounded in our own cultural preoccupations. So that was an important thinking through of some of those global issues and historical issues.

Runyan: There are some questions about your experiences with SRCD. First one, when did you join SRCD and what were your earliest contacts with the Society and with whom?

Levine: Yes, well I'm a strange case, my earliest contacts with the Society were with a lot of people in the field whom I knew either because they were at Chicago, like Larry Kohlberg who was very active in the 60s when I was not or people I met at NIMH or at various conferences and so on. I knew a lot of people in SRCD certainly from the 50s onwards however, I didn't join until 1987 and I'm not exactly sure why. I guess my life was organized so that I was able to meet lots of people in the field and I didn't go to the meetings.

Runyan: When was the first meeting you attended?

Levine: That's a good question...I can't remember but it was in the 80s. I just didn't go yet I was so active in all of these things, I mean I was even a member of the Social Science Research Council Committee on social and affective development in childhood so I knew lots of people and I just didn't bother going to the meetings. I was more active on the anthropology side in terms of meetings but in recent years I have gotten much more interested in SRCD and its issues. I didn't go to the last meeting I was in Sweden, it was in New Orleans and I just decided I didn't want to leave. I was working on this book on African child development and I didn't want to interrupt what I was doing but normally I do go to the SRCD meetings and I do participate. I've done a lot of reviewing for the monograph series in particular and I've been on the nominations committee for what it's worth. I like SRCD, I did note however that at the last meeting I was at which was Seattle, in the 3,500 people present there were only about five of us who are anthropologists. That, it seems to me is terrible, really deplorable.

Runyan: Have you been involved in SRCD governance at all?

Levine: Well, just as I said, I was on the Nominations Committee, I just went off this past year.

Runyan: What do you believe are some of the most important changes to occur in SRCD and its activities since your association with it?

Levine: I don't know if I can comment very much on that. I think, I don't know that it has changed a great deal frankly and I still think that something should be done about the journal. It was the nature of the journal, Child Development, which probably put me off from joining for so many years. It's sort of narrowly lab report type of publications just seems to me an archaic, obsolete way of dealing with an exciting field.

Runyan: What kind of changes would you like to see in the journal?

Levine: I think there should be papers of different length. They will publish cross-cultural studies if they are quantitative. Okay that's fair enough but they cut them down so you can't include the contextual data and that's ridiculous and they should permit, as by the way Developmental Psychology

does, they should permit longer articles that enable you to put in the contextual data, if it is cross-cultural. They should have at least occasional book reviews and not just piggy-back on the APA's contemporary psychology book reviews. I can't imagine why there shouldn't be book reviews in Child Development, of course there should. I mean it gives you a sense of the field and so on and there also should be an essay, I mean occasionally they do publish theoretical essays these days, I'm sure they pat themselves on the back because they do a little bit more of that than they use to, I think it's far too little theory in the thing. The journal represents the field as being made up of nothing but people doing very precise, oh either experimental or other kinds of quantitative research and with the idea component reduced to it's minimum and I think that isn't an accurate representation of the field and it certainly isn't desirable.

Runyan: Could you please comment on the history of the field during the years you participated on any major continuities or discontinuities and events related to these?

Levine: Yes, I have a story about that all right? I should say also, by the way before I go into that story that another piece of the story of the journal was once told me by Bob Sears and it's very ironic because Sears who was such an interesting guy himself told me that when he became the editor of child development he expunged all of the variable sorts of articles that they had had in the journal up to that point and set a new standard which was that every article had to be a report of empirical findings following a certain model. In other words, Sears created the monster that now is Child Development and he was very proud of it. He felt that he had created a scientific journal out of child development whereas before they had sort of like impressionistic reports by a pediatrician about somewhere. That was all eliminated, that was his view of the thing and I think it's really interesting this effort to turn the child development field into something that looks really scientific.

Runyan: Did he have any later second thoughts about that or did he stick with that view as long as you knew him?

When he said these things to me it was many years later so I doubt that he had any second thoughts. Sears, like the other Hullians you know, was an avid logical positivist and his view of what science was was patterned after the sort of experimental natural science models. Even though he was deeply interested in psychoanalysis and got training in psychoanalysis the last part of his life he was, I guess he never finally finished it, I mean he was doing a psychoanalytic biography of Mark Twain.

Runyan: He did articles on Mark Twain. Some of them are quantitative analysis of Mark Twain's correspondents.

Levine: I didn't know that but you know Bob was a very multifaceted sort of guy but still he had this strong, very strong positivistic thing which I had too so I'm not complaining! But I am complaining about the Child Development journal. Now my big history of the field is looked at, you have to understand, I look at the field from the perspective of the participation of anthropology and anthropologists in it so that has to be taken into account. Remember that Margaret Mead was involved in the child development field from 1926 onwards when Carmichael published his Manual of Child Psychology in 1936, Margaret Mead had a chapter in that edition called, Research on Primitive Children. When he put out the second edition in 1954 he asked Margaret Mead to redo it and what she did was she just redid the bibliography. It's very frank, I mean there is no concealment of it but she says only the bibliography is new. So in 1954 they reprinted her chapter from 1936 and then when Paul Mussen took up the third edition which was to come out in 1970, I was asked to do Margaret Mead's chapter and I found that well basically nothing had been done since 1936 you see. Here it is 1968, so I had to start from scratch because the field had totally changed since, well she must have written it in 1934 and we are talking about 1968 so I really had to start from scratch and that's partly why I wrote it. A

great deal of the article is just methodology, thinking through the methodology of research designs in the field.

Runyan: I'm not sure what you are say, there wasn't any research since then and you are saying the field had totally changed?

Levine: No, well the field of child development had changed so much and there had been a great deal of research even on the cross-cultural side which I have been involved in, some of it the Six Culture Study. It's just that I couldn't use Margaret Mead's, you know the point is the people who were doing other contributions to the third edition had a chapter in the second edition to look at which was at least usable to some degree, but the one that I had went back two editions so that I couldn't use it. It was like a different world, it was a historical document as far as I was concerned. But I mention Margaret Mead because she and Ruth Benedict made the case in the late 20s and early 30s which had an enormous impact on psychologists which was that children develop in other cultures in the world and there is something to be paid attention to there and that a cross-cultural study, that child development should encompass cross-cultural research and of course this was very actively taken up by the Yale group, the Hullians initially Bob Sears and John Whiting, so John Whiting became the first anthropologist who was part of a group of psychologists. So in other words, the ground work had been laid initially by Margaret Mead and Ruth Benedict and then by John Whiting as part of the Yale behavioristic group to have the cross-cultural study of child rearing be a full member of the child development field and it was the notion that child development is incomplete unless you include anthropology and anthropological studies of child rearing in other cultures, no question about that. The sense that you absolutely had to have the cross-cultural dimension was very much a part of the whole field of child development in the 1950s. Basically that was completely eclipsed in the 1960s, it was expunged and I think it's a very interesting thing. In other words, the big interpretation of history is not appropriate here, this was regression.

Runyan: How did that happen?

Levine: It happened because in the 1960s you had several developments, one was the rise of Piagetian models, which were universalistic. Second was the rise of compensatory education. The social program as a determinant of a lot of research going on domestically here in the United States, the war on poverty. So at the one level you have Piaget saying we can discover universals without going anywhere except Geneva and at another level you have the war on poverty saying we have to study children here at home and deal with the kids who aren't making it. Those were two developments but there was a third actually, very important development too which was the rise of experimental psychology. That child psychology should follow experimental models. The thing that you have to realize, when people say how could that happen, how could it be that suddenly that there use to be child psychologists who took other cultures into account and then there got to be ones who didn't. You have to realize that in psychology there wasn't a great deal of child development research. All the theoretical principles from child development research in psychology came from animal research. It was all research done in rat labs, the white rat who when Hull published his book, Principles of Behavior was based on, except for one chapter, entirely on research done on white rats and dogs. So the fact is that most of the people who were writing books even famous books about child development were basing their psychological principles on laboratory research done with white rats. They weren't doing research on children, there were just a few people doing research on children who were sort of theoretically motivated types in the field. So in the 60s people started really getting into doing research with children, with human children and these people had not been necessarily trained in the same mode of the unified Yale model that put together rat research with cross-cultural studies and so on. What's more, the most important thing is that the 60s was the period in which academic programs expanded by an exponential factor, especially in the field of psychology. That's when they use to say,

you know, if the rate of expansion of psychology continues, if it continues at the same rate then by the year two thousand there will be more psychologists than there are people in the world or something like that but the fact is that all programs including child development programs expanded enormously and because the exciting things were experimental child, which was new Piaget and other cognitive development stuff and compensatory education and other applied sorts of things related to poor children, that's what was being taught and so then suddenly you got a new and very large generation of child developmental people who didn't read Margaret Mead and Ruth Benedict and they didn't read John Whiting of the Six Culture Study and they didn't know about those things and they weren't taught that it made a difference to look at other cultures. So by the late 60s and early 70s, especially by the 70s you have a situation in which we have to start from scratch all over again. It's worse than Margaret Mead faced in the 20s with G. Stanley Hall when she had to use her Samoan data to attack G. Stanley Hall but at least Hall was like the first explorers of the field of adolescence. By the 60s people had a lot more confidence that they did know how to do psychology and they had a lot of knowledge and it didn't require cross-cultural perspectives so we had to fight from a very marginal position again and I feel that we are still there, we're still in a marginal position but I think that the new movement toward a cultural psychology has changed things somewhat so that things are moving. There's a definite trend toward a greater movement now toward something that I think is more cultural.

Runyan: Do you want to say a little bit about that movement, cultural psychology?

Levine: Sure, it's one of those things, I mean it's very important to realize there were always people doing cross-cultural work, some of it good and some of it not throughout the period when nobody was paying any attention to it. I mean the Whitings here at Harvard did this phenomenal job of keeping the field going at a time when no one was paying attention. It's really interesting that you might say the 50s was the period when anthropologists got disaffected from psychological anthropology and then the 60s was the period when psychologists turned away from the cross-cultural but meanwhile the Whitings just kept going and they kept training students and many of them very fine students throughout that time and doing really important pieces of research of which of course the Six Culture Study still is a real landmark. That was conducted in the 50s and published in the 60s so all of that has been going on and now how do we happen to have a cultural psychology movement. Well of course the political roots of that are now because of multi-culturalism and the need for psychologists to get in on the perception that America is a multicultural country, that cultural diversity has to be studied that even if we are talking about minority groups in our own society we have to admit that they have a culture, they have alternative ways, not all of which can be derogated as that of a discriminated or oppressed group and so on but rather that there maybe something legitimate in what they do. So psychologists are forced from their teaching even to sort of pay a little bit more attention to cultural variation. At the same time there have been, of course other figures who've come up like Michael Cole who fought for the development of a cultural psychology and has certainly worked with children and in educational settings and so on. I think an important thing about Cole is that he is an experimental psychologist, he's not an anthropologist like me and so he's an insider to the field in a way that I could never be and therefore carries a certain kind of weight and he's been arguing for this and he's worked with Sylvia Scribner and so on all the years long. Then you have, well people like Rick Shweder who have created certain publishing opportunities like the Cultural Psychology book that he edited, with Jim Stigler as the senior editor. Stigler, Shweder and Herdt, 1990 and based on conferences and that brings together the different streams that have contributed to this whole area and of course one of them that must be mentioned that Mike Cole was very connected with is the Vygotskian. I think the whole interest in Vygotsky in child development is another very important connection with cultural psychology because even though Vygotsky didn't make a big deal out of the word culture, his models are easily connected with a cultural perspective. So the two psychologists who did more than any other person to bring Vygotsky into developmental thinking in recent years have been Mike Cole and Jim Wertsch both of whom have been editors of the journal called Soviet Psychology and both of whom know the Russian

language and have spent time in Russia, it's very interesting. Of course Jerry Bruner knew about Vygotsky many years ago and even used it in his own work but in relation to Vygotsky, Bruner was so far ahead of his time that it still was another twenty years before the rest of the field caught up with him. I think, just while mentioning Bruner that his book, Acts of Meaning is a very important legitimizing force for the field of cultural psychology because for him to endorse the idea of cultural psychology makes a very important statement to psychologists since Bruner has been a very key figure in advances in psychology over the years and particularly of course in terms of the cognitive revolution in psychology.

Runyan: Do you want to say anything about how your views concerning various issues have changed over the years?

Levine: Yes, oh my views have changed enormously because I was training here at Harvard with John Whiting totally in the framework of the Yale model of putting together psychoanalysis with behaviorism and cross-cultural perspective so I was very much a behaviorist when I got my Ph.D. It was only when I was teaching at the University of Chicago that I started changing and there were two things that made me change. One was learning through Larry Kohlberg something about Piaget and reading Piaget and the other was getting training in psychoanalysis. When I got trained in psychoanalysis I realized that the efforts by behaviorists to operationalize psychoanalytic ideas were not very adequate that there was a great deal of psychoanalysis that couldn't be easily reduced to the kinds of operational concepts that could be studied the way behaviorists wanted to do them and that therefore we had to take a more phenomenological approach. For me the impact of psychoanalysis was heavily to look at conscious experience in phenomenology and where you might say culture and the unconscious come together. So I changed away from a behavioristic mode in my years at Chicago and got more interested in what people think and how they feel as oppose to just pure behavioristic and I also moved in a more relativistic direction and I used to be much more concerned and you can even see that in my 1973 book culture behavior and personality with always locating universals, knowing what was universal and seeing cultural variation within a set of holes that could be pinned down as universals but I've given up that to a large extent too.

Runyan: Given up what?

Levine: Well, the notion that you can identify the universals in advance of more research, you know of having done the research, I mean I have become more relativistic. Part of the reason I became more relativistic is that when I got to Harvard I found that a lot of the students were heavily influenced by Jerry Kagan who was so convinced of the universals of development and that he knew them that I started preaching against that. It moved me in a more relativistic direction. I think that one often does that, you have a position which is perhaps intermediate and then you get polarized by somebody who has taken an extreme position on one side or another and that certainly happened with me. I'm not a long term relativist but I think that a relativistic strategy is needed in the short run in order to get psychologists to take cultural variation seriously and a lot of what I have been doing has been trying to write things which make people take cultural variations seriously to see that it really makes a difference to developmental problems. It's not just frosting on the cake or window dressing or something at the periphery but something very important and I guess I wouldn't have thought that I would still be fighting this particular battle some years ago but I think the field has gone backwards so I'm fighting an old battle.

Runyan: Gone backwards toward universalism or gone backwards toward nature dominated field?

Levine: Both toward nativism, toward universalism and toward a notion that you don't have to take seriously cross-cultural data. I mean that's what I said the field had gone backwards, I felt between the early 60s and let's say the late 70s.

Runyan: I'm just wondering would you put the movement back towards a Robert Sears sort of more hard science view of psychology as part of what's happening and if so it seems to be so different because it's probably a movement away from logical positivism.

Levine: Well, the Hullian behaviorists made some devastating mistakes so therefore we can't go back to them. It isn't just the rigid logical positivist position; they made some basic mistakes in relation to the psychology of children which are embarrassing. There was a whole rash of mistakes. They didn't have a central place for language and cognition and it was because when you generate all the principles of behavior from rat experiments and you can't interview rats and you don't know what they are thinking, you can only speculate and you certainly can't talk to them or get them to talk to you. You can talk to them, you can't get them to talk to you!

Runyan: Thanks for clarifying that!

Levine: So if you are locked into the idea that all the principles of behavior were already given by rat experiments then you have to do what Miller and Dollard did in social learning and imitation, you have to generate all of the higher order thinking of humans and their language from principles that have come out of a nonverbal vertebrate and that's exactly what they did, so their model of language in Miller and Dollard is all based on labeling, you know on associative conditioning of words with things and stuff like that. I mean that simply after Chomsky, after an awareness of how children in fact, after Roger Brown and etc., have an awareness of how children do acquire language you can't look at that kind of stuff with anything but humor or a mellowed historical perspective. They didn't even have a place for cognition, I mean thinking had to be some complex set of SR connections that was built up into abstract higher order thinking but compounded of SR connections. I mean they always poo-pooed other psychologists, we were always taught that the gestaltists were pathetic, fortunately I had studied gestalt psychology at the University of Chicago before I came here and studied behaviorism so I did know the gestalt principles. I did take on the ideology that they were pathetic and unscientific and so on, you know one ape puts a box on top of another and so on. The point is that they try to develop a psychology without language and cognition as anything but epiphenomenal stuff and so, I mean we couldn't go back to that now, it's impossible because psychology has come a long, long way, that's an important part of it. When I say there was regression I don't mean that the field as a whole regressed, I mean that it regressed with respect to its willingness to pay attention to cross-cultural data just in that single respect. So here we are, I mean we are still arguing a lot of stuff that shouldn't have to be argued if every psychologist had, you know had either taken a course in anthropology or been exposed to certain kinds of things.

Runyan: I wonder if another strand of it is partly child development or also being partly child clinical oriented which may not support cross-cultural research which may be more theory driven.

Levine: Yes, and I think that the child clinical thing is running into a lot of problems there whether they are willing to recognize it or not. I mean they are very normative, they are legislating morality. They don't want to face up to the fact that it's not science, it's morality that is involved here to a very large extent but the multiculturalism is making them face up to that in a rather obnoxious way because minority groups say you can't make my child into something that comes from your middle class white background and so on, that sort of thing. But there is a grain of truth in that, the grain of truth being that if you insist on leaving out the cross-cultural perspective than you can make the mistake of thinking that you are discovering universals at home when in fact they are not universal at all but you

are just not paying any attention to them. John Bowlby had that problem and I tried to talk him out of it. Here's a really interesting thing, I mean Bowlby who was certainly one of the most scholarly people in the child development field as well as an advocate of emotional ideology. In his first book on Attachment for Volume I he quotes data from my work, from my published work to suit his own objective, I mean I felt that he twisted what I had published to make it sound as if that you don't really have pay too much attention because I'm in this other culture. In any event, after I did the longitudinal study that is in this new book I was on my way back from Africa, my wife and I and we went to see Bowlby in London and we tried to tell him, you know we saw a lot of things that are really very distinctive. He just couldn't hear it, he couldn't listen. He was so totally committed to the notion that he had discovered what kids need and I think that's where the child clinicians are in trouble because they have such a strong sense about what children need and they don't realize that there is a broad spectrum of things of things that children need and you can't tell what the basic need is by just looking at children in our own society. Anyway that's my little speech.

Runyan: What are your hopes and fears for the future of the field?

Levine: My hope is that the field will be reconstructed on a basis that takes other cultures into account and that it also spreads around the world so that there are groups of child development researchers in other countries and cultures who are very active and not simply accepting American models and so on. My fear, which has been partly realized, is that as some kind of cultural psychology of child development gains legitimation. It will result in a lot of second and third rate work and I think that has happened already and it's a very painful thing to talk about because it means then that you then have to say, well I'm a cross-cultural psychologist but I never read the journal of Cross-Cultural Psychology because the stuff that's published there is a lot of junk and so on. I mean, it doesn't seem to me that other fields have to deal with this problem, but that's my fear is that the mediocre will be the enemy of the good.

Runyan: It's happened somewhat in the field of psychohistory.

Levine: Okay, I take it back it's not unique! It's actually even worse, I guess in psychohistory.

Runyan: Last, could you tell us something about your personal interest and your family especially the ways in which they may have had a bearing on your scientific interests and contributions.

Levine: I have a very simple thing to say, that the most important thing that ever happened to me was becoming the father of children. My first child was born when I was almost 38-year-old by which time I had written endlessly about child rearing. Well, I had been publishing articles about child rearing for twelve years at the time that my daughter was born and in the course of the next few years then when my son was born and so on, I learned so much about child care from the parents' perspective. One of my articles that has been most influential to me and I guess to other people was an article on Parental Goals, a Cross-Cultural View which I wrote in the course of the year, 1973-74 and it was published in Teacher's College Record in 1974 and I wrote it just after my second child was born and that was my opening shot into the area of parenting studies which has become the focus of my research ever since and it was all completely based on the perspective that opened up to me by looking at child rearing from the parents' perspective in my own personal life as a father. Realizing that parents see things very differently from just looking at the parent/child relationship as an outsider. It was a reflection of my effort to intellectualize my position as a parent.

Runyan: In terms of cross-cultural perspective I wonder how you react to Nancy Scheper -Hughes, Death Without Mourning?

Levine: Death Without Weeping, well I think that the data are not all in on that controversy, Nancy has her own perspective and Marilyn Nations has her own. Marilyn Nations is somebody whose work comes from the same part of Brazil and doesn't agree with Nancy's view of these mothers and I think that I'm not somebody who posits a natural mother love complex that it just is hormonally produces so I'm certainly willing to see that under various kinds of extreme circumstances as described by Nancy for Northeastern Brazil that you can have a whole culture which has a different view of promoting child survival. But I think the data, I always go back to you know the comparative data that we have available, the comparative data do not show that even if that it turns out that the culture there is the way she describes it, the population is the way she describes it, that that is very widespread in the world. Most people are trying to promote the health and development of their children as they see it in that culture. They're not trying to decide which kid deserves to live and which kid should die. I don't want to say that it's an aberration, I don't know what it is yet but I'm not willing to accept the sort of broader implications of what she found there at this point.

Runyan: Are there any other topics that we haven't touched on you'd like to say something about?

Levine: Well, yea I just keep thinking about the history of the field and the move from a behavioristic to a more phenomenological perspective and more open ended kinds of research. I mean I still feel that there is a very strong behavioristic side to me but that as I see it the behaviorists of the 30s, 40s, 50s, and by behaviorists I of course mean the Hullian behaviorists, I don't mean Skinnarian, that those people were much too limited. I mean that now we take language as a major aspect of data, this is part of what we are interested in so therefore the boundaries between like a behavioristic perspective that wants to take acts as the primary focus is not as distinguishable from a phenomenological perspective that wants to take say language because speech acts are there to be studied and there's no difference between the speech act and language. So in other words just in terms of my own relationship to the field, I am still fairly behaviorist but I can include language and cognition and so on and still say well what we can study most easily is behavior but we can have much more complex and interesting models about what's going on behind the behavior and so on.

Runyan: When you say behaviors, do you mean in the sense looking at behavior rather than behavioral theory of causation?

Levine: That's right, a more methodological behaviorism rather than a theoretical one. But at the same time I do think that the behaviorists gave us some important things and I think that my training as a behaviorist makes me sensitive to those kinds of things. When I look at infant psychology, I mean infant psychology is an area where some kinds of associative conditioning and so on can operate with infants and do and some things are learned that way.

Runyan: Last question, did you ever get to meet Abram Kardiner who got you started in all of this stuff?

Levine: I never met him bizarrely enough, I have taken an interest in reading things about him and a lot of what I read is not very good, maybe it's just as well I didn't meet him. Okay, well thank you.

Runyan: Thank you.