Jacob L. Gewirtz

- B.A. (1945) CUNY Brooklyn College, M.A. in Child Psychology (1946) University of Iowa, Ph.D. in Developmental and Experimental Psychology (1948) University of Iowa

Major Employment

- Professor of Psychology, Florida International University: 1981-present
- Adjunct Professor of Psychology, University of Maryland Baltimore County: 1972-1979
- Chief, Section on Early Learning and Development, National Institute of Mental Health: 1965-1975
- Supervisory Research Psychologist, Laboratory of Psychology, National Institute of Mental Health: 1956-1975
- Assistant Professor of Psychology, University of Chicago: 1950-1956

Major Areas of Work

- Social learning, social relations/attachment and dependency, observational and imitative learning

SRCD Affiliation

- Program Committee member (1958), Chair of Nominations and Elections Committee (1968-1969), Governing Council member (1977-1979), *Child Development* Editorial Board (1957-1962)

SRCD ORAL HISTORY INTERVIEW

Jacob Gewirtz

Interviewed by Mary Levitt
June 29, 1998

Levitt: This is an interview with Jacob Gewirtz, Florida International University, Department of Psychology, June 29th, 1998. The interviewer is Mary Levitt.

Gewirtz: In response to question one on general intellectual history in the interview schedule, my parents were immigrants. My father immigrated to this country in the early 1900s, my mother in about 1922 or 23. They came from Eastern Europe. I was an only child. My first language was Yiddish. I learned English on the streets and in school and my parents improved their English because of my learning, which is why I think multi-cultural education is stupid. Education in a native language, which comes out usually to be Spanish, is not constructive for children, many of whom turn out to have accented English their entire lives because of that training. I went to a Yeshiva when I was at elementary school age and I studied Talmud and all sorts of other good things. When I got to the high school period, a friend of mine from school was going to go to a testing session to get into a special high school, which was called Townsend Harris, and he asked me to go along, to keep him company. I didn't know much about it, it was part of City College, now the City University of New York, and was under the board of higher education rather than the board of education. So I went with him, took the test, and it ended up that I passed and got admitted and he failed and was not admitted. There were three tests. One in language, one in math and one in -- language, math and English I guess. It was sort of like the GRE, except it had a foreign language. That was a school that was a three year high school, but I came in late, so I was there for three and a half years. Many of the students there were twelve years old. When I came there I was fourteen, I think, or fifteen. It had the advantage that about half of the faculty were PhDs in substantive areas, not in education. From there I had the choice of City College or Brooklyn College or Queen's College, but Queen's College didn't exist in that form. I chose

Brooklyn College, because this [the high school] was an all-male school and I decided to go to a school that had females as well. So I ended up going to Brooklyn College, where I majored in anthropology, in chemistry, and in psychology, I had three majors in other words. I ended up emphasizing anthropology and psychology. When I went into the psychology part of the studies I studied with such people as Heinz Werner, Abraham Maslow, Herman Witkin, and Solomon Asch. It was usually a pretty lively place, because I remember once having Wolfgang Kohler come to give a colloquium. I used to attend colloquia when I went to Brooklyn College under Abe Maslow's influence. I attended lectures by people like Karen Horney and Wolfgang Kohler at the New School. So I was more or less interested increasingly in psychology.

Levitt: There is a very high percentage of psychologists that came out of Brooklyn, isn't there?

Gewirtz: Yeah. The thing about Brooklyn is that you had a high percentage of everybody who came from Brooklyn. It was less specialized than my high school. My high school turned out, by now, about 30 or 40 novelists. Brooklyn was much more selective than that. I was a leftist when I was in college and I could talk to people who were Stalinists or Trotskyites or socialists. I once went somewhere with Norman Thomas, who was a socialist candidate for president who I found very interesting. So I was sort of intellectually apt, but my orientation in college was more or less whatever the potpourri would be that would be based on a gestalt emphasis on perception, sort of a holistic emphasis on personality, a major emphasis on therapy, particularly psychoanalytic therapy. We had faculty who were very charismatic like Abe Maslow; he was very charismatic. Once in a class Abe Maslow said jokingly, "You know what they do at Iowa, they condition eyelids" and the class broke into uproarious laughter, because that had nothing to do with psychology as they understood it. Subsequently when I had been a graduate student at Iowa, perhaps after I had gotten my doctorate at Iowa, I wrote Abe Maslow for a reprint and he sent it to me. He had already gone to Brandeis by then, and was chairman of that department. He remembered me, and remembered that I went to Iowa, and added a few words saying, "What would an Iowa guy want with shit like this?" and signed it Abe.

Levitt: Were you becoming dissatisfied with the psychoanalytic model while you were still at Brooklyn or was it after you went to Iowa? What made you go to Iowa?

Gewirtz: Oh, I was interested in psychoanalysis all along. I still remain interested in psychoanalysis. Maslow was more of a dynamic psychologist, whatever that means, but it tends to mean he brings in psychoanalysis when it is convenient and deals with other personality considerations when they're convenient. I was at Brooklyn College when I heard about Carl Rogers' work, for example, and many of the students were impressed by it, because it was actually a second system. It was not like psychoanalysis, you didn't have to be a physician or to go into a personal psychoanalysis, an endless personal psychoanalysis (I think Woody Allen has been in it for twenty years), but it offered them a notion of how you can do therapy without a huge expense of time. I never thought that one of my first teaching assignments when I got to the University of Chicago would be that I was assigned to teach a course jointly with Carl Rogers. That was sort of a bizarre experience which perhaps we will come back to.

Levitt: Yes, I would like to hear more about that.

Gewirtz: Carl Rogers was very upset by the experience.

Levitt: So what led you to Iowa?

Gewirtz: Well, I had applied to a few places. I went to Columbia to see Ralph Linton, who was then the chairman of the department; he was an ethnographer of reputation. I had also applied to Yale in anthropology and in psychology. I was rejected by the psychologists. One factor that I wondered about was that I had gone there and I had talked to Irvin Child and to Mark May. I had hitchhiked there and on my way back from New Haven, I was hitching and Mark May passed me as I was hitching. I never knew how much that played a role. Subsequently, since I was close to John Whiting who I had met at Iowa, and done some ethnographic work under his sponsorships, I got to know Irvin Child pretty well. I told Irvin Child the experience and Irvin Child mentioned that perhaps it would have been a good idea for them to take a chance on me when I had applied, but I had been accepted by the anthropology department, which at that time was one of the top departments, one of the top two or three or four maybe.

Levitt: Were those common interests at the time to be bringing those two fields together?

Gewirtz: Yes, Kardiner had written some books in the early 40s which played a role. Not just the Traumatic Neuroses of War, but he was emphasizing dynamic psychiatry and anthropology. And of course people like Margaret Mead and Ruth Benedict and people who had worked with Boas at Columbia had emphasized personality, and you had an awful lot of people who described personality as their field when they were anthropologists. John Whiting and Murdock had established the Human Relations Area Files at Yale. They were actually the data that came out of examining all the ethnographic treatises and trying to get measures for a variety of antecedent experience factors and a variety of personality factors, and they were using each culture as an individual. Certainly in Murdock's work, Murdock wrote a book I think on social structure that was published about 1948. Whiting with Child, who was his brother-in-law, had been continuing work on personality, not so much structure, but personality, when he was at Iowa and subsequently, so their important book appeared in 1953. So these were examples of what some ethnographers thought you could do. As an anthropologist as an undergraduate, I read Whiting's doctoral work on becoming a Kwoma, which was sort of, for us, not very scientific, but it represented in an essay form what socialization in that particular (I think it was New Guinea) society was. There were a lot of people who saw very little difference between anthropology and psychology, with regard to their interest in personality. Some of this emphasis came out of the Yale Institute of Human Relations where you have anthropologists, psychologists, psychiatrists, and sociologists working on a number of themes. On the frustration and aggression theme of 1939, [for example], you had Dollard, who was more sociologically oriented, who would write with Miller in 1950 and the like. So you had an openness to the notion that fields did not limit the subject matter that you were interested in.

Levitt: We seem to be coming back to that again today, do you think?

Gewirtz: I don't know because it's hard to know. The fields have distributed themselves differently than they use to be distributed. I don't think you have as much crossing now, because even within psychology, we have a differential emphasis on attachments depending on whether you're a social psychologist and you're interested in adult attachments, or a developmental psychologist and you're interested in mother infant attachments; or some people deal with attachments in the format of marriage. I don't know; there is a redistribution. I'm not quite sure how it is going to land. It's obvious that emphases have come, for example in developmental psychology, whereas out of pediatrics you got the emphasis on parental attachment to infants. I speculated about this in something I wrote in 1959 on determinacy. I speculated on what parents could do, what children could do, what circumstances could be provided to get parents attached to their children. But with Ainsworth diverting the Bowlby track (I consider it a diversion, which Bowlby sort of reconciled, in turn, in his volumes in the late 60s and afterwards), you had the emphasis on attachment, and certainly Ainsworth's work was on infant attachment to adults. The pediatricians, like Klaus and Kennell, came in in 1970 and were emphasizing the bonding or the attachment of the parent to the infant, but at the same time they were emphasizing the attachment of the child to the parent, to the point where some parents thought that if they did not bond with their child in the first few days of life, the opportunity to bond with the child would be lost forever. So you have forces coming together, both within and without psychology, today on attachment which represent a very different distribution of some of the issues, compared to what we had when I started out.

Levitt: Where did your interest in attachment originate? Was it while you were still in school, or later?

Gewirtz: Yes, when I was a graduate student I was interested in social learning and all of its form. In fact, partly I went to Iowa because Robert Sears had done some work on relating psychoanalytic concepts to the generic concepts of experimental psychology, in particular the learning concepts. He did that in an SSRC monograph he published in 1942 and, around that time, he had a chapter in Jane McV. Hunt's two volume work on personality that appeared in 1944. So I was aware of Sears and that was one of my interests in going to Iowa. Another one of my interests was in Kurt Lewin, who had been at Iowa when he came in from Germany. He spent a short period at Cornell, then he went to Iowa and he stayed at Iowa until about 1945. In fact, when I came he had already left. So I started out being interested in Lewin, but I didn't know he was leaving.

Levitt: What year did you start?

Gewirtz: 1945.

Levitt: So he had just left.

Gewirtz: He left just before I came, so then it was natural for me to work with Sears, because Sears was the director of the Iowa Child Welfare Research Station at that time and he was interested in a lot of issues that I was interested in. Some came out of psychoanalysis. I had noted some of Sears work when he was Hull's assistant, when Hull published a book on hypnosis in 1933, so I had known that name. I went to Iowa in part because I had a friend of mine from Brooklyn College, a geologist who was going to Iowa, and when I had to make a decision, the thought was just as sensible to go with this friend to Iowa. This was a male friend and we actually started out together in Iowa, he in geology and I in child development.

Levitt: That's a long way from Brooklyn.

Gewirtz: Yes, it was and it was also very inexpensive at that time. It was somewhat anti-Semantic as well. There were all sorts of euphemisms about the New York manners and New York habits, I suppose, which could be said to have represented the lack of New Yorkers and the lack of Jewish people at Iowa. But we did have examples that came with a fella named McGeogh, a world renowned expert in human learning, who was the chairman of the department until about 1942 or 43. He had written some definitive texts at that point. Some of the students who ultimately ended up working with Kenneth Spence, who then replaced McGeogh as chairman of the department, had been students of McGeogh. For example, Ben Underwood, probably also Farber, a number of people like that then became Spence's students in turn. McGeogh had some policies about Jewish students which were negative, and several of the students that came during that period had to go into clinical psychology because it was not thought appropriate for them to go into academic psychology. So it was not very different from what happened to Dave Shakow at Harvard. He was pushed into the clinical direction (and of course he became a very distinguished clinical psychologist) because the notion was that if you were Jewish you could not get an academic job, even though some of the very distinguished academic psychologists had been Jewish. Joseph Jastrow comes to mind, Hugo Munstaburgh, who was a visiting professor at Harvard during WWI, and there were a few others like that; but mostly Jews were not recommended for academic life at that time. When Kenneth Spence took over psychology before I came, he changed things around completely, so some of his students who got their degrees under him were Jewish at Iowa, the Kendlers, George Wischner -- I could think of other names if I sat down to think of them. So there were these strange vibes there when I came, but only at the level of administrative personnel, secretaries, people like that. I had been orphaned when I was in my teens. My mother died right around the time I was in high school, just starting high school and then my father died around the time when I was going to graduate school. I was a singleton, so, although I had many relatives in the New York area, I didn't have quite the pull to remain in New York that many others might have. Well, I was at Iowa then and paradoxically Sears suggested that I do my master's work with Beth Wellman. Beth Wellman was a remarkably lovely woman, a very able woman, who, with George Stoddard, represented the "environment can affect intelligence" school, which was a minority school in American psychology. The alternative group was the group lead by Louis Terman and had, I think, Terman's student Florence

Goodenough in it, a heavy weight who was at Minnesota, and Quinn McNemar, who functioned as a hatchet man for that group because he was very critical of the statistics of the Iowa group. Stoddard was director of the Iowa Child Welfare Station and he then became dean of the graduate school. When I came to Iowa, I think he had just left to be president of the University of Illinois, and Wellman represented that group. She had come to Iowa some years earlier. In fact, she was there when Bert Baldwin was director of the Institute, but she worked with Stoddard and had continued to do work during the period when I was there. You also had a number of other people like Harold Skeels and Catherine Bantam Bridges who wrote the essay on the origin of emotions in Child Development in 1932. She wasn't much of a researcher, but they were there. These people were all arch environmentalists, but it was Wellman who was the key figure, and I worked with her. I chose to work with word fluency for my master's to see how it related to other measures of intelligence.

Levitt: This was at the Child Welfare Station.

Gewirtz: Yeah, it was essentially a separate department, but on balance, about half of my courses were in psychology and half in the Child Welfare Station. But the Child Welfare Station had psychologists in it and it functioned as a psychology department. It had been founded at the same time the Berkeley Institute was founded, I think, along with the Minnesota Institute and the teams funded by the Rockefeller foundation. So I did my master's with Beth Wellman and I recall Beth once introducing me to Florence Goodenough. It was remarkably unfriendly I thought, but that was before I understood that she represented the notion that IQ would be constant for life and if you go back to the journals, usually the Murchison journals, you would find really savaging types of articles about Beth Wellman. For my doctoral work, I worked with Bob Sears on issues of succorance. It was fashionable at that time to take Murray-like concepts and to work with them in observational types of studies. I worked, I think, in my doctoral work on succorance, aggression, nurturance and concepts like that, but my entire interest had been in social learning and how a variety of the behaviors of social relevance come into being. To maintain that interest, I did work in experimental learning, and I took courses on primate learning. I actually did some studies, which were never published, with experimental designs which were devised with primates. So I was doing a number of things with learning. Of course my major mentor in learning was Kenneth Spence and, at the same time, I did much work with Gustav Bergmann on the philosophical basis of psychology, issues of logical positivism and the like. Indeed when I first went to Chicago, one of my classes was in the same classroom that Rudolf Carnap was in after mine was over, so I would often sit in on Carnap's class before Carnap left Chicago for UCLA. Chicago had a logic that you had to retire at the age of 65; I believe Carnap left for that reason. I know that Thurstone left Chicago for that reason to go to North Carolina, where they were much more flexible and could set up an actual department for him. So this has been a roundabout way of talking about general intellectual history.

Levitt: And some of your early mentors.

Gewirtz: Yes, I mentioned a number of them. I didn't become psychoanalyzed until I was at Chicago.

Levitt: So you still maintain some interest in psychoanalysis?

Gewirtz: Yes, I taught a graduate course in Chicago on the theories of personality with a substantial section on psychoanalysis, with a heavy emphasis on Freudian psychoanalysis and a secondary emphasis on those who came later. I emphasized that psychoanalysis was not biological in any way, that it was a social theory, such as other theories are social theories. Other molar theories, I'm using Brunswick's term, were not biological theories even though they used terms which might be used as synonymous with biological terms. The department had actually proposed me at one point to the Chicago Institute of Psychoanalysis, so I would be, while a young faculty member at Chicago, trained by the Institute. One of my interviewers was Thomas Szasz and, for some stupid reason, he and I fought a lot during that interview, so, even though years later I discovered that he had libertarian interests just as I have, and that he was a no-nonsense type fellow, the interview I had with him was really very bad.

Levitt: So that short-circuited your career in psychoanalysis.

Gewirtz: Right, in not going into psychoanalysis as a sideline. See this was to be, since I was not a physician, this was to be training psychologists who would be interested in psychoanalysis to have further training of academic students in psychoanalysis. Students who would forego the medical school experience, but perhaps I'm ahead of my story. So I was interested in learning always; at least once I went to Iowa, I was interested in learning. Before that I was interested in, I used the usual metaphors that those who did not use learning concepts used -- they were either perceptual metaphors or cognitive metaphors -- and had only a secondary interest at learning. My interest in learning was developed at Iowa and, since I had always been interested in personality, the mix, perhaps based on what Sears had done earlier, was to look at personality development as outcomes of learning processes, with secondary questions of what is learned and what is not learned. That's always been an interest and it remains an interest to this day in some of my research.

Levitt: Your current views on learning are not those of Hull and Spence.

Gewirtz: No they are not. I think there were some dead ends in the Hullian system or limitations of the Hullian system and so, even while a student at Iowa, I became a learning operationalist. Although I had understood instrumental learning and I had understood Pavlovian learning at Iowa, I had not understood any of these from the view say of Skinner, because those readings were not in the reading lists at Iowa. I had read some things separately from Skinner, and I got interested in Skinner when I moved to Chicago. I saw that my operational interests were expressed very well in the Skinnerian system. We did not have terms that were replete with surplus meaning or terms you could read a lot into. There were some interesting attempts made within the Hullian system, for example the secondary goal response and the like, but I didn't find them useful particularly. I found the Skinner terms useful subsequently, but I'll get into that later. I found the behaviorist approach that I used to be a very happy approach, in the sense that I was always concerned about the looseness of terms and the fact that terms could be given meanings that the database did not intend for them to have, and those who gave them those meanings tended to use a looseness of approach basis. It was convenient for many essayists to use these terms, essayists like Maslow, and while I learned my lessons from Maslow well, I could find no basis in the research I had studied to support Maslow's hierarchy of needs.

Levitt: Was your interest in operationism stemming from your association with Bergmann primarily, do you think?

Gewirtz: Well, yes, but I had many influences. Even before I took courses with Bergmann, I had been exposed to Bridgman's, "The Logic of Modern Physics" which was written in 1927, while Skinner was still a student. I found that orientation a happy one, since I could not explain, say Maslow's system as an example, without just assuming we had assertions of the system. There was no research basis for a lot of things that were done or said by Maslow and by others who operated in a Gestaltist logic. To continue general intellectual history, I see there are questions under institutions that perhaps it would be better to deal with under the general intellectual history. When I was at Iowa, one reason that got increasingly important to me for being at Iowa was that Iowa and Columbia, including Teachers College, had given the greatest number of doctoral degrees in psychology, in all areas of psychology, at that time. And it was a place which had rich traditions, which I came to appreciate. I also found it sort of very convenient to be there because, when I was starting to do my doctoral work, around the time when I finished my master's, it became clear that if I would have finished my doctoral work in 1947, I'd have a good shot at getting into the department at Stanford at that time. That was before Stanford became very strong, but it was still good. And if I had finished my doctoral work in 1949 or so, I'd have a shot at Berkeley, apparently a very good shot in both instances. So I felt very comfortable in feeling that at Iowa I had a chance to go to some of the better institutions. It ended up, at the time I was finishing my work there, that there were three offers I received almost immediately. One was from the University of Illinois, one was from Indiana University, and the third was from the University of Chicago. The Indiana and Illinois offers were at the assistant professor level. The Chicago offer was at the instructor level. Because I had in college been very involved in the work of Robert Maynard Hutchins, I was affected by Hutchins and the University of Chicago Roundtable, and because I knew that some of the departments at Chicago, like anthropology and sociology, at the time were world class and I subsequently learned that their department of economics was to become, if it wasn't already, world class, I decided to take my first job at Chicago. Interestingly Jacob Kantor, who was the head of the department at Indiana and who had brought Skinner to Indiana in 1945 (Skinner left Indiana to go to Harvard about three or three and a half years later), became a close friend of mine. Today in behavior analysis there are Skinnerians and Kantorians. The Kantorians essentially look down on the Skinnerians. They are much more sophisticated philosophically. Kantor wrote books; I think his last book might have appeared in the 1960s when he was already living with his daughter in Chicago after retirement; he was in his 80s. Kantor was, first of all, a PhD in philosophy from Chicago in 1917. That was the same year in which Tolman received his degree at Harvard, under Munsterberg oddly enough and not generally well known. Most of Kantor's books were very sophisticated philosophically, but also he was an early emphasizer of the operational approach and, it should be recalled also, that he wrote one of the major early textbooks in psychology from a behavioral point of view. This was in 1924, the same year in which Floyd Allport wrote his social psychology.

Levitt: What was the difference between Kantor and Skinner?

Gewirtz: Well, Kantor was more systematically oriented, in the larger sense of including philosophic issues. He went into greater detail on issues about the nature of physiology. One of his subsequent writings, I think perhaps in 1959, was on the principles of physiological psychology. He was not physiological in our sense, but it was an attempt to conceptualize physiological issues in ways which were continuous with the way black box input/output relations were conceptualized. I remember Kantor very well, because whenever I saw him at a meeting, since he knew I was to go to Chicago and not to Indiana, he'd ask how things were at Chicago, which he had a fondness for, and in that process he turned out to be a bit of a mentor. I was surprised, pleasantly surprised, to see that in the behavioral analysis field today, Kantor's writings play a very major role.

Levitt: That's interesting, because I think people outside the field associate it almost exclusively with Skinner.

Gewirtz: Yes, yes and the Kantorians include some of the Mexicans and some of the Spaniards who emphasize Kantor's logic. For example Emilio Ribes of the University of Guadalajara is effectively a Kantorian. In the United States, Linda Hayes is a Kantorian, there is a Kantor subgroup in the Association for Behavior Analysis and the like. And indeed in a recent paper, I had a sentence in there about protopostulates, which is a Kantor concept. Proto before the postulate you talk about which are sort of the unwritten postulates. I was asked by the editor to give a reference for that statement, the statement of protopostulates, so I e-mailed Linda Hayes (my books are all packed since I haven't yet fully unpacked). She e-mailed me back a reference that I could use for protopostulate. So I remember Kantor in those terms. Now when I was a student at Iowa, you know Kantor had written for years, but Kantor did not appear in any of the courses, nor did Skinner appear in the courses. We knew that there was "The Behavior of Organisms" but some of us waited till the end of our stay at Iowa to read it. This is not necessarily to Kenneth Spence's credit, posthumous credit; he felt that that was irrelevant to the field of learning. But for the things I was interested in, Skinner's work was very relevant. In any event, I took my post at Chicago, where my first experience was teaching a course with Carl Rogers, and where I found that people coming from difference sources, from Ohio State or from Harvard, had a quite different orientation about psychology then we had in Iowa.

Levitt: You said earlier that it was an interesting experience teaching with Rogers.

Gewirtz: Yes, it was because a lot of what he verbalized as his philosophy was not the way he behaved. Really, if one said something that Carl didn't approve of, he let you know he didn't approve of it, usually by manner, by the expression on his face, whether he turned away, or the tenor of his voice. In retrospect, I wondered whether he didn't communicate something to his clients, since he was very judgmental, which I consider to be a very appropriate thing to be. Consider the fact that very few people are judgmental now, or it is thought by the “new age” people that to be judgmental is sort of a lower characteristic of human behavior. I consider it to be very important. In any event, I respected him for having his strong opinions.

Levitt: So did you find yourself clashing with him in terms of your teaching styles.

Gewirtz: Well, he was not trained to have a theoretical position, in the sense that theory had gotten to be known in the late 40s and into the 50s. Subsequently it has gotten to be known even differently, but theory has in the eyes of many scientists much of a role to play. It organizes databases, it generates statements about the databases that are parsimonious, that reduce the number of terms that are needed to deal with the databases. It generates novel hypotheses; it provides a basis for convincing others that your functional relations are important. Theory plays many roles, and of course we’ve had many approaches to theory subsequently, but in my period, since I had been trained by the positivists, I had a notion that theory should not be treated as an informal essay-like system. Carl, who came out of a tradition part social work (I think Jessie Taft was one of his precursors), and with a practical orientation, didn’t learn about theory, but came to find himself an important theorist. I don’t think he ever came to grips with that; I don’t think he ever felt that he had to prove his theory. He used metaphors for the theories. He had ideal selves, real selves, and the like, but where he was not oriented formally to theory, and I was, and not only that, but I was also oriented toward a strong theory at the time, he had problems with me, as I did with him. Not to mention that he was senior to me in many ways, in age. He had many students coming from all over the world to work with him. He was known as an author, a father of a school, an author of many books. I was just basically a pipsqueak who had gotten a degree and come from some university out in the boondocks and conceivably there were lots of bases for problems in that. These were some of the bases. I learned when I was a chair that you shouldn’t try to bring oil and water together. It doesn’t help the oil and it doesn’t help the water. But he did have contempt for behavioral theories, which was a problem he had, and my concern was that he did not have much precision in what he said and he was sort of defensive about explicating everything that the theory really had in it.

Levitt: So what were you working on at that time?

Gewirtz: Oh, I started with some different types of problems. I started working on succorance or on dependency. In that period at Chicago, I wrote a good deal about dependency, because that was the concept that carried much of the water. There was no concept of attachment. Attachment didn’t exist essentially until Bowlby brought it in. If you looked at Sears’ monograph of 1942, the term attachment was used as people use terms in street language, but it was not meant to have conceptual importance. Of course, in Freud, object relations doesn’t include the word attachment and Bowlby, in part, was reacting to Freud and he was reacting to the theories there. For example, when Bowlby called Freud’s theory the cupboard theory of love, Freud was seen as having all the important things in the life of relationships depending on somebody feeding you and Bowlby thought less about that; of course, he was oriented toward Melanie Klein on the one hand, and he had some ethological background on the other, so a lot of what he wrote was in reaction to Freud and in reaction also to the learning theory of the period. Quite independent of Bowlby, Harry Harlow was reacting to the learning theory of the period, because Harlow had such things as the study in 1950 where Harlow made the point that sometimes people will do things so they could handle the objects they were working with. He concluded that there was no theory and no motivation or drive in the sense that Hull had motivation and drive and, if you will, in the sense that Freud’s theory had motivation and drive. So Harlow resented the term reinforcement, as Hull used it. He thought it was limiting. He thought it didn’t cover things and he might have thought, I don’t know if he did, that you can always call a consequence a reinforcer arbitrarily and not have a problem, but he had a problem because he couldn’t see drive reduction. And of course he came out in 1949 or so with the notion of learning sets, which also didn’t have a motivational thing, but starting in 1950, he came out with the idea that the consequence of handling was like a reinforcer; it supported the behavior. And of course then there was his 1957 or 58 material where he dealt with attachment; I forgot what he called it at the time.

Levitt: He might have called it love.

Gewirtz: He was rejecting the terms that the learning theorists were using. In a parallel way, he was doing what Bowlby was doing, rejecting the notion of reinforcement, as it was implied in Freud, and apparently dealing with important interpersonal behaviors and showing that they may not require it. Of course Harlow's own studies were flawed from my point of view, the studies he published on the nature of love. In the "Determinants of Infant Behavior," in the commentary on Earl's paper, I raised some questions about, some reservations about, what he had done.

Levitt: What were those?

Gewirtz: I don't recall all of them, but one of them was I expect that a clinging species is going to find it easier to cling to terry cloth, over wire cloth, than would a non-clinging species, and I wouldn't make more of it than that it's a species specific behavior. The evolutionary history of the species would have led to that. I wouldn't make any other point about that clinging thing and the fact that they were found to cling more or better to the terry cloth than to the wire cloth. I would have thought that was literally a non-fact, in the sense that that is what the species does, why make more of it.

Levitt: The feeding issue was confounded with the nature of the species.

Gewirtz: I thought the feeding issue was problematic in other ways as well. I don't remember whether these animals, when they were taken from their mothers, were suckled or not at any point, but I thought that you had a confounded discrimination experiment, having to do with several modalities involved in terry cloth, in stimulus modalities involved in the wire cloth, that sort of thing, and no control for aspects that would be differential in the two preparations. But obviously one has to be open to the young variety of species finding all sorts of things to do. They will do whatever maintains what they do; the issue gets to be what maintains the behavior once you get away from the species specific characteristics.

Levitt: You wrote an earlier paper on the distinction between attachment and dependency; was that one of the first forays into the attachment period?

Gewirtz: No, I got into attachment in the 50s because I was writing about dependency. I presented an analysis once that appeared in Child Development on dependency and how you would look at it as a potential outcome of learning, but in the 50s as well I got involved in ethology. I got involved in an odd way; I had been reading some of Bowlby's papers, but I was acquainted with Bill Verplanck. I had known Bill for a number of years, but by the time he was at the University of Maryland I would meet with him and I would learn from him, because he had taken a sabbatical at Cambridge with Robert Hinde -- a one year sabbatical in which he learned all about ethology and issues of that sort. So I became involved in ethology a little bit and I saw some of the relationships between ethology and psychology. I realized among other things that most of the learning researchers had paid no attention to the species. They didn't even try to be ecologically valid in what they did. They often set things up in a certain way, whether they were rats or pigeons or any other species, and I started realizing that the species specific issues were important. This was partly due to Verplanck and partly due to, I suppose, in some way, somewhere along the line, Bowlby made similar points. I had started on Verplanck's behalf to read some of Thorpe's work, Thorpe was Hinde's teacher. Hinde had to be thought of as a Tinbergenian ethologist, in contrast to a Lorenzian ethologist. This was at NIH, but I had, when I was at Chicago, a relationship with Eckhard Hess. It wasn't the most friendly relationship, but Eckhard was a gentleman and a thoughtful man and we would occasionally do things together. For example, Eckhard would invite me to a seminar which had only five or so people in it when Konrad Lorenz visited Chicago. So I started very early. I would have discussions with Eckhard, so I got, in the 50s, to see some ethological implications, apart from Verplanck. I had a lot of dealing with Verplanck, once I had left Chicago and had gone to NIH, because that was next to the University of Maryland. So I had different sources of interest in animal behavior, I suppose quite apart from having read things by Schnelrla and others. When I read a paper for the first Determinants of Behavior, I had a lot of ethological comments in there. I even tried to account for imprinting on the basis of reinforcement that exists in the "following situation" because we knew far less about imprinting in the 50s than we did in the late 60s early 70s, with the work of Hess and Petrovich and the work of Gilbert Gottlieb. In any event, in the first Determinants of Behavior, I brought a lot of ethology in and it was there that I speculated about a number of things that appeared in 1960 or 1961; it was published in that year. I had speculated there about attachment of parent to child and about the inter-attachments. I don't remember whether it was there, but I started to write about how to turn a child into a pet. If the child lives in an institution, teaching the child to be a pet often meant that you made the child competitive for the limited resources that were provided by caregivers. So all of this involved some thoughts about attachment and this preceded by far the book of 1972, which was essentially generated by a symposium I had arranged, I think in 1969, at an SRCD, which involved among others Bob Sears, Bob Cairns, Mary Ainsworth, Leon Yarrow, myself and I'm not sure who else. It was the book that came out of that symposium in which I try to make a distinction between dependence and attachment, because I felt that Bob Sears had given up the concept of dependence. So my share of that book was to make the case that you can retain a concept of dependence, even though you had a concept of attachment which related a child's behavior to the behavior of the parent. So I got into attachment well before that point, but I got into it under the heading of dependence in the 50s. Then, in the first Determinants of Behavior book, I got into it in an attempt to explain from a learning point of view how you could get attachment, at the same time taking into account species specific behaviors and the various unlearned conditions and the like. And actually, in that same book, I also advanced the notion using some of Spitz' concerns. I also advanced the notion about depression in infants, because I had remembered Spitz' publications of 1945 in Psychoanalytic Study of the Child. I've always had a problem with those, not just because of the odd way in which he reported the material (for example, not indicating even the country in which the institution was in which he worked), but also because, even though he was a physician, he took no account of the fact that you have gastroenteritis epidemics in institutions where you have many children and that these can be deadly. He did, in his report, point out how some of the children didn't thrive and other's died, but he didn't really give reports about the everyday interaction between mothers and infants. So in that 1961 paper, I also started an analysis, which I had started in earlier years in papers I had given to APA and to SRCD, about how a variety of breakups, between say parents and children, or traumata of other sorts, could lead to the absence of behavior on the part of the children, which is remarkably similar to what we call depression today. It wasn't fashionable to deal with childhood schizophrenia or childhood depression until about 20 years ago. Before that it was only adults who could show these behaviors.

Levitt: I recall that you talked about deprivation and privation as separate constructs.

Gewirtz: Yes, I tried to make that distinction. I am trying to think of where it was. I remember actually presenting papers in the late 50s on that, but I know that I started publishing about it first in the Determinants of Human Behavior, Volume 1 (that volume of course didn't have a volume number because it was the first of the series and it wasn't meant to be a series). Then I started at some point bringing that into discussions. At one point, in 1967 or so, I published a couple of chapters in a book on new approaches to child care, which was set up by a bunch of child psychiatrists, in which I tried to deal with the nature of stimulation first, in one chapter, and in another I tried to deal with some of the anomalous outcomes of removing stimulation. Then in the chapter I wrote in the Goslin Handbook of Socialization Theory and Research, which was published by Rand McNally in 1969, I brought some of these thoughts into a place that could be read by a number of child development people. I pursued this off and on from that time, in both in the developmental literature, social developmental literature, on the one hand, and in the behavioral literature in the other. I remember having a chapter in a book published in 1977, edited by Catania and Brigham, in which, among other things, I brought that in and that was the first bringing it into the behavioral literature.

Levitt: So you had a foot in developmental psychology and a foot in behavior analysis.

Gewirtz: Well, at first it was all developmental, but I got into the behavioral literature starting with that chapter in the 70s. I had some status in the behavioral field, because I did know people like

Skinner and Fred Keller. I knew them from the early 50s. I knew Kantor (except I didn't even associate this with the behavior analysis field exactly then), so I had some validity. I also had a lot to do with Charlie Ferster and Og Lindsley in the early 1950s, when I was still at Chicago. I would arrange for many of these people to talk at Chicago and I would visit; I know I visited the Harvard laboratories and I was attentive to some of their primatological research. I knew Murray Sidman, I visited his home in Maryland. So I knew a lot of the people who were the fathers of the recent behavior analytic strain. Then I got to be sort of thought to be important because a PhD student I had, Don Baer, turned out to be a great, a master, in behavior analysis. He wrote brilliant articles, did brilliant research and is known by behavior people as being great. When he received an award by the behavior analysists about ten years ago, he mentioned that I was his mentor, so I did have back alley entry into behavior analysis. I didn't publish in their journals; I published only in the developmental journals or clinical journals that had a developmental following.

Levitt: You've had a number of students that have gone on to distinguished careers.

Gewirtz: Yes, and who seduced me into having an appearance which I didn't intend, like in the moral development field which I never thought I would be involved in. In fact, I was originally on Larry Kohlberg's committee, or he asked me to be on his committee, but when I left Chicago that fell away.

Levitt: I think you once told me Harriett Rheingold was a student of yours.

Gewirtz: Yes, I was on Harriet's committee on her doctoral work. She wasn't my doctoral student, she worked with Helen Cook, but I was on Harriet's committee. Helen, who was an old school developmental psychologist and an excellent one, was sort of out of place in Chicago because they once had a home economics department and then they said, "We don't want a home economics department, it's like having a school of dentistry. We don't want these applied things" (they didn't consider law to be applied in the same way. They had a medical school which was meant more to do research than to be clinical). So Helen, who was a PhD from Chicago at an earlier period, came into the psychology department although she still ran the nursery schools. She took a liking to me and I would get on her students' committees often. In fact I'm trying to think, I don't know if I was on Irv Siegel's committee, because Irv was a doctoral student of Helen Cook's, but I remember having something to do with Irv, at least we met then, and there were people like that who came out of human development. You see, I was the head of psychology at Chicago, and there were few psychologists who were also human development, but human development also had people in it who were psychologists. Havighurst was not a psychologist. I think Neugarten may have been, I think she was. Bill Handy was a psychologist, but really came out of anthropology, and Bob Hess came out of education. Helen was in human development, but also psychology, and she was a very sound person; but any way, Helen Cook was Larry's dissertation advisor and also Harriet's.

Levitt: One of the questions asks about continuities in your work and what you see as having been retained over the years. What early ideas have you rejected or moved away from.

Gewirtz: You know I don't know. Clearly I shifted theoretical models underlying learning. I shifted before most others I knew did, and it was not out of ideological zeal; it was out of the fact that I found that the behavioral models, particularly that stemming from Skinner, were much easier to work with on the issues I was interested in, namely how social behavior was acquired and maintained; and of course there were derivative issues, like which are learned and which are nativistically given, and I thought the model would work better there too; and I thought the model was one that gave you methods that came be used. To this day, the conditioning procedures are very powerful in giving you ways of working with nonverbal individuals to find out how they rank order given stimuli, to find out how stimuli work. In fact, right now, in that same spirit which is no different than the spirit I had 40 or 50 years ago, we're starting to move into touch, which is an area that hasn't been worked on very much. For example Martha Pelaez is starting to work on touch with me. We are starting to do things that people who have worked on touch haven't done (including some of my own colleagues), where you say "well we messaged" or you say "well we poked" but there is no statement of the amplitude of the poke or the amplitude of the massage. I've been involved in the publication of two papers in the Journal of Applied Developmental Psychology in which I thought the papers were not publishable because there was no effective description of the touch stimulus. There was a preliminary description, "we ran our hands across the child's limbs and we had two complex stimuli. One had auditory, one had visual components alone, the third had visual and tactile components" but we never specified what the tactile components are. We said, a hand went there and we told how the hand went, but we couldn't indicate the amount of pressure. We couldn't indicate any contour in that and I was really always worried about its publication. I finally said to the senior author, okay send it in and see if it will be accepted. To my surprise the journal accepted the two papers, in different years. But right now what we are working on is trying to calibrate the amount of touch. I mean it is easy to calibrate the frequency of sound, it is easy to calibrate light in different ways. You can specify the source of the light, you can even do it in physical terms, but we have not done any calibration with tactile stimulation and that's one of the things we have a student working on now. There is nothing commercially available, and this is something I don't even talk about to a lot about my colleagues who are world renowned in this area, because none of the work of our colleagues involves anything about different degrees of the stimulus.

Levitt: So is it the precision of defining the stimulus and consequences that you find is a strength of the behavioral model.

Gewirtz: Oh, it is a strength, very much a strength. In that sense, the behavioral model is an extension of experimental psychology with its strengths, just as the behavioral model's charting of changes in behavior is a straight derivative of what's done in the laboratory. There is nothing different, and the power that comes from being able to see from moment to moment where the response is that you're implementing a treatment about, that power is awesome. Of course in applied work you can always see we are not having an effect without having to invest in many subjects over much time; one finds out very quickly that your operations are not having an effect on the behavioral outcome you are looking at. So you know immediately that you either drop the study or you change your operations, or you may look for a response that will more easily reflect the outcome you are interested in, but you know that very quickly.

Levitt: People sometimes make a distinction between theoretical behaviorism and methodological behaviorism, do you think that is a useful distinction?

Gewirtz: You can make such a distinction of course, but behavior analysis, the technology of behavior analysis, is what we are talking about here because it is a very powerful technology, very powerful indeed. It allows you to work particularly with nonverbal creatures. Now most systems can work with nonverbal creatures, but when you work with nonverbal humans who at some point have verbal skills or develop verbal skill or acquire verbal skills, many theorists choose to say that the verbal skills are the precondition for certain other achievements. If you can show that those other achievements can be achieved without the verbal skills, you have some very important information. So one of the advantages of being able to work with preverbal subjects is that there are many processes which theorists have thought are dependent on verbal skills and if you can show that these processes can occur in organisms without verbal skills, who aren't animals, you can make some points about it. Indeed I think of my discussion in a series of papers in The Behavior Analyst, about Tom Bower's suggestion that the subject had to discriminate something before something could follow. I thought we sort of dealt with an issue like that in that discussion, where, from a behavioral point of view, Tom's way of dealing with it was gratuitous. If the subject didn't make the discrimination, you wouldn't have had the behavior denoting learning and it becomes gratuitous to say that the discrimination was necessary. Obviously there was a discrimination, which you could take for granted; that was one of the issues that was involved there. Another issue may have had to do with verbalization, but I don't recall it right now. I know that issue comes up in the behavioral and in other literatures. For example there was some issue that came up in the behavioral literature, starting in the early 1980s, as to a process called "equivalence class formation" where you have what these people call "emergent relations" occurring. I don't like the term "emergent" because it has a Piagetian implication: that is, if you have

A associated with B and B is associated with C, will the AC relationship be formed, and they would call that an emergent relationship. So the methodology, the behavioral technology, asks you to deal with a number of questions which ordinarily would not be dealt with. I have a student who hopefully will be doing a master's thesis in which we deal with a comparable issue. That is, one of the things about development that leads to some problems, Is that you don't ordinarily know you have a color blind child until that child can talk, and you don't usually know what a child's visual acuity is until the child can use an eye chart, and that forces you almost to take the easy way out and to correct for images falling before or after the retina with optical solutions. So we have this person who is starting this thesis in which she is going to work with three-month-olds who don't talk, and she is going test their acuity using a system of verticals, commercially available by the way, so that you can deal with the issue of numerosity. With a series of pairs of vertical stimuli, you can make, say, the most numerous or the least numerous stimulus the discriminant stimulus, so that the behavior made in its presence will lead to a reinforcer. You could find out just what the individual's visual acuity is, is it a 20/20 child, is it a 20/200 child, or what not, and then you can have a before and after assessment. Then with all the [visually impaired] children, one tries to give visual exercises, like go from 11 to 5 on the clock face. So go clockwise with your eyes, go counter clockwise, go across; there are a number of exercises. These could be implemented via behavioral technology with these young infants and one can attempt to improve the visual acuity of the infant without using an optical lens. I don't know why it's never been done, but that is a thesis that's proposed now by one of my students. It's quite a novel thing.

Levitt: That has important implications.

Gewirtz: It would, I would think, but it will be interesting in itself just to be able to do it and I don't think other technologies exist to allow it to be done. It's simpler to work with surgery of the cornea or to put spectacles on than it is to try to use eye exercises. I would like to see that to become an alternative for improving, at an earlier point, visual acuity, and later points, if there is ever a slippage. At later points, of course, verbal instructions would carry the day if you have a cooperative subject and usually you do. But it's the earlier points and this came out of my just adding to lectures from time to time the business about color blindness not being noticed, and often being confused with stupidity by teachers, when we have simple procedures to detect that. With a kid you can do it easily by just putting a red ball on the green grass but there are a lot of different patterns of color blindness which we don't recognize, we just think of red green. So we still have theories of color blindness going back to Goethe's time. Goethe is known for a theory of color blindness (once we said in Chicago, "Go eathy"). So the technology can be useful. Let me just make the point I was making about equivalence classes, that the theory that some have used for the ability to do it or not to do it has turned on whether somebody has a certain sufficient amount of verbal ability.

Levitt: I wonder if you want to comment at this point about the kind of turning away from learning models of mainstream developmental psychology.

Gewirtz: I can of course. That's bothered me a lot, but you know a model is a model. If you can use a model which may not be traditional or may be different and it helps you to do your research, who's to say you shouldn't use that model. There are various ways of recognizing that what some of us call learning occurs without using a learning model and many people effectively do that. They have what they often call cognitive models; of course a learning model would be a cognitive model by definition because it's the acquisition of knowledge or the acquisition of knowing, and that's what cognition is supposed to be, I suppose. In any event, in the past 40 years, certainly 35 years if not 40 years, we have had a change in the balance in conventional approaches to development between learning and non-learning approaches. In fact, I may have simplified it because some of the more traditional theories, like Werner's or Piaget's, bring in environment, but they don't bring in the notion of learning in any way. My fantasy has been that if learning theories had been in existence, or were there for Heinz Werner or Piaget or many other's to play point-counterpoint against their theories, the theories would have been different and so conceivably learning theories would have been different. A number of the questions involving learning have been put to people like Piaget, but they are often questions that Piaget wouldn't answer and he didn't have to answer them. The fact that you can have conservation shown after training the conservation does not necessarily shoot down any aspect of Piaget's theory for Piaget, and he wasn't obligated, except to mention in passing that he understands that some people like to speed up some processes. He did speak of the American disease, which he used to refer to how Americans try to get things to happen much faster, and of course Piaget was not a person who made this mistake. His followers in education particularly made the mistake of referring to Piaget's use of age in a nominal sense as requiring that one find age norms for all of the procedures that Piaget worked with. I consider that the evil brought on by the educational establishment almost without exception. One can speculate about what would have happened, where would psychology be today, where would various theories be, if theorists spent more time with one another talking about their theories, and I suspect they would be very different. I think if Tolman had talked to others who had different orientations, his theory would have been different. The same for people like Hull and Spence. In the early 40s, '40/'41, there was some controversy between Spence and Tolman. Spence was doing something in '40 or '41 that was published in the Journal of Experimental Psychology, in which the issued turned on what the animals (these were rats) knew, and this was only really a continuation of the controversy that was started by one of Tolman's students about "latent learning," in which the issue came in, did the animal really learn something. Did you have to use reinforcement to get the animal to learn? The issue came about when Spence was doing something with his people; they were publishing something (in fact Festinger may have been in on that) in which they made the point that when you provided something in the goal box, the animal showed that he had learned, but if you didn't put anything there, the animal didn't show what he had learned. This was originally an interpretation of what Tolman had done and it got to the point where you had Tolman saying reinforcement wasn't important for learning in the experiment you are talking about, and Spence said but naturally it was. So you really had the two theories differing on what they chose to call an operation. I found that that bothered me a lot because it meant that the same facts were treated differently in the different theories, but the theorists themselves thought that they could confront the other theory and actually get it to change. I think that many psychologists have felt that over the years, particularly those who train conservation quickly or show that something had come up at an earlier age to show Piaget that learning is important. But it wasn't important, it didn't have to be important for Piaget; his theory didn't considered the issue of the rate at which you did various things and naturally even many learning theorists don't consider the rate of which you attain criteria, behavioral criteria; their issue is do you attain behavior criteria.

And so there were some issues which bothered me about the different theoretical orientations. I am trying to say several things at once. One, I am filling out what I had left out earlier, namely that there were different theories that some people, particularly my own teachers, had the notion that, even though theories could be different, and even though they are set up to do different things (although they usually weren't put in that sophisticated way), they were confronting each other, even though they weren't necessarily trying to do the same things. It got to the point where I could see that the same operation could be called different things and the different theorists could feel that they were vindicated by an experiment. I felt that was an awkwardness which gave me some concern about what is reinforcement, which helped me to move to a more operational system. But that's a separate point from what I left out earlier, from the point that you were asking about developmental psychology treating learning as sort of an irrelevancy almost. In fact I felt it was [treated as] an irrelevancy, I saw the SRCD as treating it as an irrelevancy, as you are familiar with. You are familiar with my concern that they actually don't feel that there would be anything lost if learning were not used as a category in their biennial program. They imply learning, but they don't have it; they would rather account for some of the same results without using the operations that are typical of, for example, the current learning approach. I find that difficult to accept, although I understand that two theories which have different origins and do different things, and may actually at the perimeters touch, need not attempt to explain the same phenomenon. If they do try to explain the same phenomenon, they should be allowed to; there's no other way to do it. The theories are operating as theories, they are doing what we expect of them, and what we shouldn't expect is that we will end up with one theory being the only theory that's left after all theories have collided.

Levitt: Last man standing!

Gewirtz: And I feel that there haven't been too many illustrations of that, but I feel one illustration of that in recent years in developmental psychology has been the Ainsworthian approach to reinforcement. Ultimately she didn't publish it as that, ultimately she published that as the Bell and Ainsworth Paper of '72. When it was in an original draft in '70 or '71, the subtitle had to do with reinforcement and how reinforcement wasn't necessary for learning, or that it could lead to results which were the opposite of what the learning theories expect from them. What she tried to do in that paper, which was changed dramatically, but still not sufficiently, before it was published, was to have a paradigm clash, even though she didn't understand learning as learning theories used it. You can't get anywhere with a paradigm clash, because you don't know the other theory as well as you know your own and usually you tend to have a lower threshold for accepting something from the other theory or defining something from the other theory as something in your system. I haven't seen that too often. That one was perpetuated by Stone and Church in their textbook based on some Johns Hopkins publications. In the Gewirtz and Boyd critique of that, which appeared in 1977, there is an attempt to say there are a number of principles within the behavioral system which would account for the results presented by Bell and Ainsworth, results they concluded shot down the notion of reinforcement. I don't know of many other attempts to have paradigm clashes. Naturally I don't consider paradigm clashes to be very useful. They can be useful in the sense that you can rev up the troops and get them to think that they are doing something that's holy or has sanctity, as against what the opposition is doing, and may get a lot more research out of it, I don't know. But it isn't useful in the sense that inevitably you have people talking past each other, especially since they are not expert in each other's theories. There is no theory that will last; we don't even know what our theories will look like in ten years, much less at the time of judgment day. I don't consider it constructive to have theories one against another. I do think, at any point in time, one theory could be more advanced than another and maybe be a better fit for data than another.

Levitt: You've written rather widely in that area, looking at, for example, the age variable and how it's been treated in developmental psychology and have written a number of other, I think, influential papers. What do you consider to be your most significant paper or most significant contribution?

Gewirtz: Well, what I have been doing in recent years is to apply behavioral logic to traditional developmental issues, with the corollary that some of these issues seem to have left out considerations which have to be brought into the picture when you bring in learning considerations. Some of these issues in addition may be treated as nativistically given issues of patterns by individuals who simply have a very low threshold for theorizing about that, since the research has not been done to ask whether or not we should be comfortable thinking of some things as nativistic or some things as empiricist, that is given by experience. I think what the work I've been doing in the past bunch of years has been oriented toward is to show how some learning paradigms can account for some of the phenomena that are either traditional or newly brought to the area of developmental psychology. By newly brought, I mean instances like attachment. I consider that a new issue, but the more traditional issues would be something like fear, fear of strangers, fear of the dark, and things of that sort. So what I've been doing and what a lot of my students have been doing is trying to generate questions about traditional issues anew using the technology, what I consider an advanced technology, of behavior analysis. In the process, we may not be able to do any more than to say that we can show that some of the phenomena at issue could be accounted for by, say, a discrimination learning paradigm, but we cannot say definitively that the phenomena in nature that have been talked about in the developmental literature come about because of a discrimination learning paradigm -- the experience implied in a discrimination learning paradigm. But at least we open the question by saying some of the phenomena can be shown to come into repertories much earlier than they have in the literature and they can come under the control of certain antecedent and consequent stimuli; in some cases, they can come into control of those events and that may be a paradigm for how future work might look at the phenomena, but we are not giving definitive results. We are not saying definitively, in most cases, what has happened; we are just making nativistic interpretations less plausible. It's not clear when one could even rule those interpretations out completely.

Levitt: Showing that something can be learned is an important accomplishment in its own right.

Gewirtz: In other words, I'm interested in the content of the learning, for many purposes, because it tells us about the organism. For example, a tactile stimulus will do something differently when it's a consequential stimulus or an antecedent stimulus than will, say, auditory or visual events or olfactory events. That tells us something about the human in a developmental context and that can be useful, but there are a number of issues that have been framed in the history of developmental psychology which can be dealt with at the same time. Some of those issues have been framed as nativism and in the nativism verses empiricism frame. Others of those issues have not been dealt with that way, but they do try to fill in some of the experiential phenomena that are often dealt with by using age or some other category as an independent variable -- a sense which is almost unscientific in its quality, which leads to discussion being in terms of essay rather than in terms of formal data. So more or less that's one of the things we have been doing and indeed it has a dual function. Firstly we are developmental psychologists and far back in the list is that we want to train behavioral psychologists to learn how development can be analyzed in behavioral terms so we have multiple audiences for all of this stuff.

Levitt: Very interesting line of work.

Gewirtz: Do you think it's worth my continuing in it or should I go and make some money? I haven't made enough money yet.

Levitt: Well, speaking of money, some of the questions have to do with research funding and participating in that process both as a researcher and with your experience at NIH.

Gewirtz: Yes, when I was at Chicago in the late 40s and early 50s, there was the beginning of having competitive funding. I did use to apply for competitive funding and I'd be routinely turned down in those days, partly because a number of individuals who would be on these committees, like Harry Harlow is a friend I can speak about clearly, did not understand how group statistics functioned, much less how you could ask questions outside the framework of group statistics. I, for example, have been a student in statistics my entire career and I'm quite sophisticated about group designs and group statistics, but I also understand single subject designs and the like. In that early period I was almost routinely turned down, as were a variety of other people in a behavioral frame, for not using or proposing to use group statistics heavily. This became one of the biases that I regret very much; that is, that behavioral psychologists felt it necessary to form or to found journals of their own. I was against that when it happened in the late 50s and in the 60s, because I felt that the discipline that would come to the general field by having people who were conditioning oriented would be lost or mostly lost, as would the discipline on the behavioral people be lost by the fact that they never had to mix it up in the same arena, the arena of reviewing papers for publication, the arena of sending in papers for publication and having them reviewed, and so forth. From my point of view, that's come to pass. In many ways, I'm dedicating my declining years to attempting to get content fields like developmental psychology, infant psychology, and the like to consider learning as I know it, as, at the same time, I'm bringing the developmental content into the conditioning field. I am the head of a special interest group in the Association for Behavior Analysis that deals with development. Now to get back to the earlier point about funding, many of the people like Harry Harlow would say explicitly that they wouldn't publish certain papers because they only had three subjects; never mind that the papers recorded the data for these subjects over say a year, 24 hours a day, and the results were amazingly uniform. He didn't feel that such studies were different from essays, and what's more he didn't have a basis for evaluating those studies. So that was the problem for much of the early pattern of research support and I failed to get research support when I was at the University of Chicago. Happily or maybe not happily for my career, I was invited to join the National Institute of Mental Health as a researcher in 1956 and it was apparently because of my behavior on schizophrenia theses and factor analysis theses at Chicago. Harriett Rheingold was there at the time, I think, so that was a factor. She wanted me to come and there I didn't need to do grant applications, because the internal budget of NIH supported my work. I did have to write reports of my work or proposals in advance. Thus it wasn't necessary for me to look for support from grant agencies, with one exception; that is my ex-wife, who was in a similar area, who was in the late 50s at the University of Maryland as a member of the faculty, started working with me on some projects for which we started to collect data on in Israel, when I was a visiting professor at the Hebrew University in the 1959 to 1962 period. We did what we could do to take advantage of what was uniquely Israeli for the interest of developmental psychology. For example, we would compare the different environmental groups including child rearing on the kibbutzim with one another, to relate different stimulation patterns children were provided with two subsequent outcome behaviors in their first year of life. When we returned from Israel (I was on a leave of absence from NIH) we found that, because of her own career pattern and the fact that I was tied in with a lot of other things at NIH, although I could devote some time to the work, we were encouraged to apply for an external research grant from NIH, which we did and which we got together. She was the principal investigator and I was sort of the consultant and unsalaried co-investigator on those projects; we did get about 5 years of support from the NIH on those grants, which were actually carried out first in the Walter Reed Army Institute of Research and secondly in the IBR, the Institute for Behavioral Research which was associated with Walter Reed.

Levitt: What kinds of changes did you see at NIH while you were there?

Gewirtz: Well, there were, in the laboratory of psychology where I was, attempts to do projects which were sort of problematic, aside from the work done with "under the skin" factors, which could be done straightforwardly and had a number of outstanding people; that program went very well; it was neurophysiological psychological research. A number of the students who worked with Hebb were in that project initially. And then we had the social projects; the ones in developmental were done by us. There were different sorts of projects. Nancy Bayley did work with her intelligence interests; she expanded that interest. Shaffer did work on trying to organize child-rearing behavior. Dick Bell was in that same business. Harriet Rheingold, also. We were doing individual projects (there were no large group projects), individual projects which involved good work on the part of a variety of different researchers, but it was still small potatoes against, you could say, what could be done. I think the project that was meant to be the most valuable was when you had psychoanalytic tapes collected, with the notion that this would allow research on the processes of psychoanalysis, and almost nothing came of that, so we had a lot of pretensions. We tried to do a variety of projects and we were differentially successful in doing the projects. I found quickly that I had to go to Children's Hospital where I did work with Barbara Etzel at one time. I went to homes for unmarried mothers. Harriet Rheingold and I used to work in Catholic infant orphanages, and I continued doing that work over the years, because I had some postdoc pediatricians working with me. As long as the Catholic orphanages remained, we could do a lot of things. We got involved with Seventh Day Adventist things, we got involved with issues having to do with births, the consequences of different types of birth, not much of which is published, but a lot of things of that sort. I ultimately left NIH around 1977, because I felt that if I didn't leave I would never make it to a university again, so I left even before I had another job, although I had been at the University of Jerusalem for two and a half or so years and I had been visiting for a summer at the University of Hawaii. At the end, I just left, and then looked around. I was hired for one year at Temple University in 1977 and '78 where, interestingly enough, they tried to get me a permanent appointment, but there was a change in deans and that didn't work out at that time. At that point, I explored for jobs in different places, I even gave a talk at the University of Quebec of Montreal, where if they would have hired me I would have taught in French. But ultimately there was a job offer at Stony Brook. In fact, the person who was in charge of the developmental program was leaving, so they asked me to go there and be in charge of their program. I remained there for two or three years, until I went to FIU on a leave of absence, but I remained at FIU. I went to FIU because, in the late 70s, I wound come to consult at the University of Miami Medical School in their developmental program. At a Christmas party that the director of the Mailman Center was having, I met a colleague that I had known for a number of years, who I was friendly with, named Gordon Finley (I had met him when he was at the University of Toronto, when I gave a talk for a symposium that helped open a building at the University, in about 1968 or 9). He asked if I was interested in moving to FIU; he said there was a job opening, so I said I could look at it and ultimately I ended up at FIU, simply because I like the climate and the colleagues were nice and so forth. When I got to FIU, I had an administrative responsibility at first and I got out of the habit of applying for grants, but I am still of a mind that I may apply for outside support at this point. I am going to receive a grant, a small grant, which I'll be able to use to support some equipment, building and the like that's going to help some of the graduate students who are working with me. I'm due to get that any day now, but after that I'll probably start applying for grants, simply because it's a lot easier when you get them. It's just that it takes too much time to write up the proposal.

One point to make in passing, that I may have left out earlier, is that I have been, since I went to graduate school, a behaviorist. I've used behavioral technology and I've spoken differently than I used to speak, and all my friends used to speak, in New York, which had a holistic dynamic gestalt orientation. It [this holistic orientation] didn't show up much in the Midwest which was, during the time of my training and beginning of my job career, more behavioral than was the East less perceptual, less dynamic and the like. But when I came into contact on committees in Washington or elsewhere with people out of pediatrics, out of perceptual dynamic psychology and the like, at the beginning I'd be treated as sort of the foolish person who was there on a technicality. Then people would find, and had very much difficulty in accepting, that I was very bright, but I just had a different orientation toward science than they had. I required that certain things that they could say to their colleagues be explicated when they were talking to me about them; they found that odd, that I was interested in many of their problems, but talked like somebody who wasn't, because people who used the metaphors, the paradigms that I used shouldn't be interested in some of the issues that they were interested in. So I often found this to be the case; some of these people were psychoanalytically oriented, like John Benjamin of Denver, or Sybil Escalona, who then was of New York City, or even Spitz. Spitz and I would correspond in a semi-friendly way and, actually, I did some work which attempted to replicate some of his work on smiling. So that was one feature that I had left out; that actually I often had a great deal of fun; the more fun, the more people who had a heuristic which was different from mine.

Levitt: Do you think the field suffers from intolerance of alternate viewpoints?

Gewirtz: Yes, I think it suffers from intolerance, from insularity, and the like, that you get to think in such a narrow way like the joke I once heard of the Irish lady who spoke of the Church of England minister, or priest actually, and she said, "Imagine them calling him father, him that's married and has children." So you have a mindset; in that case the mindset was that if you were a priest and called father, you couldn't have children. Of course the Church of England minister was an Anglican priest, which meant that he could marry and the like. So there was almost the attitude that you could not deal with developmental issues if you were behaviorally oriented and you couldn't have a dialog with somebody who was developmental if you were behavior oriented. Of course there was earlier, at one point, but now in the Society for Research in Child Development you do not have a panel for learning. The people who do learning research have to send their studies in to people who don't do learning research and many of them think that people who do learning research under the aegis of developmental psychology are either knaves or fools. They're literally hostile to behavioral approaches, which doesn't help the field, because their students are being trained to think of people who use conditioning models as being irrelevant to developmental psychology, and not to have to be dealt with at the annual meeting, and the like. I find this inexplicable and I find it harmful to the field, because things do change. Pendulums do swing back and we don't want people who have one position or another, or follow one paradigm, to exclude those who follow other paradigms because [they think] those paradigms are wrong. In science you do not have wrong paradigms. You have bad paradigms meaning unsuccessful paradigms, paradigms that do not support research effectively, that do not generate novel questions, and you have paradigms that do the opposite, who ideally we all want to follow or invent and follow. But we do not have paradigms that can be treated as paradigms were in the 30 Years War or the 100 Years War in western Europe, [thought to be] wrong simply because they were wrong. They were different.

Levitt: This gets into one of the later issues about your hopes and fears for the field, I wonder if you want to expand on that a little more.

Gewirtz: Well, some of the fears I had are changing; for example, the fear that many studies that are talked about as proper research studies are, in fact, not proper research studies, that they would be taken as such. For example, they can have fancy research designs, but they may use as their independent variables gross categories which conjecturally represent one independent variable or another. But properly, they are the independent variables and they are inadequate as independent variables. Whether they're ethnicity or gender or race or age, these are not variables which are recognized by the process theories of psychology. Happily we don't do as much research of that sort as we once did. When it occurs, the reports of such research must be recognized as essays rather than as proper scientific researches in general. Now if an individual isn't oriented towards a process theory, then many of these variables can have uses; for example, we do have uses for demographic variables; there is a field of demographics. In psychology, we often deal with norms underlying tasks or performances and we can find utility in, say, age points, for example, when processes spin out differently in different places, due to the variables having different features, or the range of the variables is different, or the amplitudes of the variables is different, which we could not necessarily, working with a limited homogenous group, discover cheaply. So there are purposes for some of these demographic variables, but if one is to study processes under various psychological theories, then these variables will get in the way. One should put in the antecedents thought to be relevant and study them in relation to the outcomes thought to be related to them directly.

Levitt: The problem comes when people infer processes from these global variables.

Gewirtz: That's right; they infer one process when many can be inferred indeed from the same such categorical variables. You might have many incompatible processes concurrently inferred by researchers, which means there's just a waste of energy and the like. At this point, I have always been friendly to the various theories, because theories are like different ethnic groups; they give you notions about things you haven't thought about; they indicate operations you might not have thought about; they indicate connections you might not have thought about. I find that theories can be of use to improve one's own theory. They can't be compared to other theories, but they can be used as catalysts of a sort. I've always been friendly to theories like Piaget's; in fact, I was associated with Piaget's getting his first honorary degree in the United States.

Levitt: Really.

Gewirtz: This was at Chicago in 1950 and I wrote the squib I guess, or the citation that was read for Piaget. Piaget's English was probably better than my French at the time, but they were both poor, so I didn't gain too much from him at the time. I just knew that up to that point his work wasn't appreciated in the United States, which could mean that it wasn't any good, or it could mean that we weren't any good. In such situations, it's always tempting to think that the work wasn't good and not that you aren't good. But finally, of course, Piaget's work was, in translation, brought to the United States and emphasized by people like Al Baldwin at Cornell and others. There was an early text in 1963 by Flavell, and Piaget got to be appreciated increasingly to the point where it actually represented the home team, when some of the previous home teams would treat it as the visiting teams, as is the case to this day. I think although there are now neo-Piagetians and there are now post-Piagetians, still Piaget, for many, is closer to the way they think of developmental psychology than is the work of, say, behavior researchers.

Levitt: You've taught many students over the years, would you comment a bit on your role as a teacher?

Gewirtz: Well, I blundered into being somebody who could identify good students early in their careers. It doesn't work anymore, but when I started teaching at Chicago, you could identify in undergraduate courses people who would turn out to be exceptional graduate students. It's hard to do that at FIU, where I am now. When there are a few good students, one gets confused between whether or not they have a good command of English reading and speaking. There are lots of bright students who don't speak good English at FIU, but you don't have the vessel for dealing with them and assessing them. It manifests itself in ways like having people whose first language is not English, for example, whose language is Spanish or one of the variants of Spanish, who when they take the GRE's do poorly, but when they get their degrees they turn out to be exceptional -- people you would be proud to send for jobs to the top universities of the land. So there are problems now in identifying good students early, but the problems aren't insurmountable. It used to be that you could identify the students by them asking serious questions right away. I remember people like Kohlberg, Walter Emmerich, and the Cairns, who you could identify very early as top students.

Levitt: That's Al …

Gewirtz: Al and Rose Cairns, Walter Emmerich, Lawrence Kohlberg, Donald Baer -- I identified him as an undergraduate as an exceptional person, exceptional student. It's getting harder to do that; now we have to do that by taking chances on students in the graduate program who may not meet the tests that many university systems use, like the Graduate Record Exam, which I find a dismal criterion, because, over the years, we can't do much about the students who took the exam and were rejected from admission; we don't know what they are doing. By the same token, we rarely pay much attention to the students we accept who really, really don't do as well as other students.

Levitt: As their scores might predict.

<u>December 1st (The interview continues)</u>

Levitt: We left off talking about experiences as a teacher of child development research and/or trainer of research workers. Comment on the tension between teaching and research in the field of child development?

Gewirtz: Hum, how did that start Mary?

Levitt: Describe your experiences as a teacher of child development research and/or trainer of research workers, what courses have you taught, please comment on the tension between teaching and research in the field of child development.

Gewirtz: Okay, I always thought that it would be best to be somewhere in a research institution and do anything I wanted to without having to teach or without having to worry about deadlines for students, for theses, and all of that. I found, to my surprise, that when I had the opportunity to do research without students, I missed the students greatly. I had depended on them, for many of the occasions, for me to do serious thinking. I had missed the fact that there were no students to react to my thoughts. I missed the fact that I could not react to student thoughts. Admittedly I didn't want to read everybody's theses and then make comments and then suggest different ways of putting things. But I realized that a lot of my work before I went to the National Institute for Health was based on having the availability of very good students, from whom I learnt very much, who I taught very much too, but who kept me on my toes without my realization that I depended on them so much. This realization dawned on me when I was at the National Institute for Mental Health and I had the choice of myriad types of projects under a variety of circumstances. I found that I had missed the students and often student-like figures, who were assistants, who might have been students at one time, did not do the same things as students did. They did not raise questions with the type of concern that a student would do, a student who was apprenticing to be in the field. They looked at the job as sort of an 8-5 type job. So I realized that, which is why after having been at the National Institute of Health for a number of years, I decided, given my age at the time (which was my early 50s), that if I was ever to return to a university, which apparently I would need to do to have access to such students, I would have to do so before much more time had elapsed. The opportunity arose when there was a retrenchment at the National Institute of Health in the late 1970s, 1977 to be exact, which I used as an occasion to resign, even before I had an alternative post at a university. This comment answers the issue of students. Now some of the students I had were simply extraordinary I thought, either in scholarship, like Rose Cairns, in utter brilliance and novelty like Larry Kohlberg and Don Baer, Walter Emmerich in a solidity and a concern for traditional projects, and quite a number of others who don't all come to mind immediately. So that was a surprise to me.

Levitt: That's interesting because I think we all have that feeling sometimes of just wanting to get away from the teaching and do the research.

Gewirtz: I can say this to you, but I don't know if I should say this here. I've even had occasions when I have had students at FIU, for example, where I am now, who I didn't particularly like and who caused me tremendous problems, I have one person in mind who is very, very emotional, but the existence of that particular student provided the occasion for me to do many things I would not ordinarily have done, particularly when I came to FIU, and essentially some of my research skills and research interests atrophied. I found that I could become a scientist again and actually do things; of course I had to be reactive to the student's need. I could even start work that I had begun at the National Institute of Health earlier, where I would do the unheard of, and that is to bring mothers and babies into the lab repeatedly for many occasions; up to today I think I had 60, I don't remember that mother but I think it was 60. And there we were essentially exploring doing the sorts of research with humans that before were done primarily with animals, namely where you had complete control, 24 hour control, over the animals. They were always in the animal room where they lived and they were available to be put into boxes, study boxes and the like. I could find that I could approximate that, albeit a half-hour a day, with humans. So there are a number of novel things that I was prompted to do because I had students that I might not have done otherwise and I might not have done elsewhere. I didn't do this at Stony Brook because it was logistically difficult to do things like this there. I found that I could do it here. So students are very useful and very interesting, even though they are sometimes pains in the asses.

Levitt: Okay next question, describe your experiences in so called applied child development research, in applied work. Please comment on your role in putting theory into practice.

Gewirtz: Well, because I am a conditioner, I have always been prone to using conditioning theory to deal with personality development, and actually the development of problem behaviors and the reversal of problem behaviors. They were always of interest and particularly when I came to FIU, Florida International University, where we could actually develop a program in behavior analysis. It became possible not only to train, not only to support experimentation research by students using conditioning concepts, but, since we in the state of Florida have a certification in behavior analysis which the state supports, we actually have been training some of our students both in developmental psychology and in applied behavior analysis concurrently. Some of the students actually did problems that were problems in developmental psychology but the flip side was always that they were problems in applied behavior analysis. So to the extent that behavior analysis came together with developmental psychology. I found that I was doing applied developmental psychology in the form of applied behavior analysis for the last 10 or 12 years. I have been very excited about this. Not only did it give us a way of giving students support when they were graduate students, but we could also give them support that they could not get in academic positions which they were trained for. Also a number of problems that were traditional developmental problems could be studied via conditioning procedures in the guise of applied behavior analysis, in the guise of applied developmental problems. For example the question of fear of the dark, the question of fear of strangers were not perfectly handled as developmental questions, but they at least allowed us to start gaining leverage on answers to developmental questions in the fear realm and in other realms as well.

Levitt: The next set of questions have to do with your experiences with SRCD. When did you join SRCD, what were your earlier contacts with the society, and with whom?

Gewirtz: I joined SRCD, I believe, in 1947, when I was still a graduate student; I submitted two papers to a meeting and they were turned down, I think by Tom Richards, who was the chairman of the committee then. I've been a member continuously, which makes it 50 some years now. I've always liked SRCD. I was a member of the program committee for a few meetings. I was a member of the Board of Directors for a period, or whatever the Board is called. Lately I find that the SRCD has become alienated from me, to some extent, by the fact that the paradigm I work with, namely a conditioning/learning paradigm, is one that is not in the ascendancy in the SRCD. Many of the panel arrangements have been such that it's hard for papers that are conditioning oriented to find places on the program. It's not only a problem for my own research, but also my research with my students, and it makes it difficult to give the student early on experience doing research and presenting the research to a group of peers. That would be optimal to be able to do. I have found that some of, for example, the current program leadership in SRCD is not at all encouraging for those who do research such as mine, which I have many regrets about. There are some panels which are very easy. I mean some panels you get on, just by sending something in you will get it accepted. The standards are relatively low; they don't have much demand. But other panels have competition with research done under the auspices of heuristics other than conditioning heuristics and where the reviewers often have a very strong negative opinion of conditioning research. So it's not a permanent condition, but it's a condition I've been sort of concerned about. This year being 1998 is a year when I approached the program committee through the chair, Dr. Marian Sigman, and I was told things by her which did not turn out to be correct. There is no concern at all about the issues I raised about this area of research, where child development touches on learning as a basis for some of the developments we call child development. It was not of much concern to the chair of the program committee that the casual decisions that were being made would limit this area in perpetuity within the SRCD.

Levitt: One of the questions is, what do you believe are the most important changes to occur in SRCD and its activities during your association with it? Does that fit under that heading and are there other kinds of changes that have occurred that you think are important?

Gewirtz: The SRCD is made of individuals from many disciplines; the modal discipline is psychology. Other disciplines are psychiatry, child psychiatry, anthropology, sociology, pediatrics, social work and the like, and the representatives from these various disciplines are not evenly distributed. By that I mean, for example, that since the modal group is psychology, you have a very large number of psychologists in the organization. But since the organization attempts to be interdisciplinary, it contrives to have members of the various disciplines be president in successive biennia, according to some principal, we should expect to have members of any one discipline be solely represented. I have no objection to that, but it is the case that work from the different disciplines is treated differently, I believe by the panels, and I'm not sure how one gets around it as long as you're committed to an interdisciplinary organization. But I'm a little uneasy about that; I feel that while there are advantages to having interdisciplinary groups, I think some of the groups which have considerably lower standards for research have been over represented in some ways, and I have sometimes felt that the meetings I attend were not as good as I had hoped they would be, but I'm not sure how to get around that. I don't know of any other changes that we have had. Oh there is one change that I am disturbed about perhaps and that is I feel that many individuals feel that policy matters are more important than research matters for SRCD. I think that dilutes what I consider to be the major research focus of the organization and makes many of the SRCD's actions an extension of some of the political parties; we have policy decisions which are more philosophically aligned with the political parties than they are with issues having to do with generic child development, and the policy issues that should be associated with generic child development. I suspect my position is likely to be an unpopular one, because many of the people who have these positions are highly politicized; I understand they are using the SRCD and other organizations as their political tools, but I suspect that a majority of members of SRCD might feel the way they do and not the way I do.

Levitt: What would be the mechanism for bringing research and policy together; you know, what would be a better mechanism for that to take place?

Gewirtz: There's no simple mechanism for bringing them together. By nature, policy is very intolerant of the passage of time. It will also magnify or amplify small research steps in large directions. For example, issues having to do with whether a child should be in day care or not, and the reverse of that, whether a child should be raised by a parent at home, who would then give up the occasion, after birthing the child, to have a career which might be useful to that parent after the child has grown, and useful to the field in a sense that the parent would not be wasted to the field. I think a number of issues of that sort have become politicized, so that actually certain researchers are sort of correlated with the politicization; there are notions of attachment in day care as a bad thing, and that's one sort of politicization, but you also have other sources of politicization, where people have been using the SRCD to have the government support day care without considering issues, philosophic issues, of the size of government, of the taxes that pay for these things, the size of bureaucracy, the further bureaucratization of child care and the like. This disturbs me, because I think that no good could come of it. I do believe that society as a whole should be dealing with some of these issues, but as they are being dealt with in the SRCD, at least as far as I can see, they're being dealt with in a very one-sided way by the partisans. I feel very uncomfortable about it. I know that there has been an attempt to have more applied implications of research. I get some announcements or publications to that effect, but I do not feel [that it's good] to have the research organization be converted into a partly research and a partly or mostly "policy making decision about the rearing of child in society" organization. I think the balance is not a comfortable one as I see it today. It's almost as if it would be better to have a research organization that's separate from a policy organization about children and how children are raised in society, how society deals with children, than to have the same organization try to do both sorts of things.

Levitt: There is one other question on SRCD and that is, do you remember the first meeting that you attended?

Gewirtz: I don't know if I remember the first meeting, I'll think of it. I remember the first meeting I attended, which was an AAAS meeting, when one very distinguished psychologist came in at 9 a.m. and was soused; that was my first meeting as a psychologist and I was shocked.

Levitt: Were you a student?

Gewirtz: Yeah, I was taken to a meeting by the faculty of the Iowa Child Welfare Station in my first semester there. We all drove in state cars from Iowa City to St. Louis where the meeting was held and the entire faculty and a lot of the students went down in these few cars. It was a fascinating experience for me. I met a number of people who were very distinguished. My major professor was Robert Sears and he would introduce me to some of these people; that was before I use to meet them on my own. There was always an excitement when you read people's work and then you met them. I found that that was very much important to me. I find, to this day, that I try to take as many of my students as are willing to come to any meeting that we usually go to and I would introduce them to most anybody that I know. By now I know almost everybody and I feel this is something I am obligated to do. I think students still find it pretty exciting.

Levitt: The field, please comment on the history of the field during the years that you have participated, major continuities and discontinuities and events related to these, and have your views concerning the importance of various issues changed over the years, and if so how.

Gewirtz: About the history of the field during the years that I participated in it, the field of child development, I notice some things are different, some things are the same, their continuities and discontinuities. First of all, it's one of the few fields in psychology that very early had some very outstanding women in it, which other fields didn't have to the extent that developmental psychology had, such as Nancy Bayley, Mary Cover Jones, Marie Honsik, Florence Goodenough, M. Merrill. Beth Wellman was very influential in my life. You had very many giant women and that was one thing I noticed about it. Now you don't notice it as much, because there are women all over psychology and child development and other areas and the like. The other thing that I have noticed are some discontinuities, in the sense that some topics that were once important and then became unimportant became important again. An example is the notion of intelligence, which currently is an area that a few researchers have put into the ascendancy, people like Gardner and this other guy with a "G" who deals with emotional intelligence. And then you have, who else has been dealing with intelligence, this guy Sternberg. A lot of that work I find sort of unimportant, but the other side of it is they've raised issues about intelligence which have been interesting. Some of them are issues that have been raised earlier. Issues about intelligence, the relationship between intelligence and emotion, for example. David Rappaport, for example, was concerned about that some 55 years ago. The issue today isn't as research oriented as it was then. Apart from that, it's also less emotional, in the sense that, when I got into developmental psychology, you had distinguished people like Terman and Florence Goodenough, and Quinn McNemar, who was operating as Terman's hatchet man, who were very hard on people like Beth Wellman and George Stoddard at Iowa and actually labeled them charlatans and whatnot and published things that we do not see in journals today. In fact we shouldn't have seen them in journals then, but we did. That we happily do not have today, but that would be one observation. I think I would call it a discontinuity, but I've always felt that the issue of continuity and discontinuity was a pseudo issue. If you make the units of change large enough you get discontinuity, if they are small enough you get continuity. I've always felt there was an artifact governing the issue of emergentism as the opposite of continuity. In other words, this is how I would act with regard to Piaget's emergentism. Other concerns that I've had are, well you've always had issues in the ascendancy and issues in the descendancy, as it were. When I got into the field, learning was an important area. Now learning is a less important area, that is adaptive learning, but you have other types of conceptual areas that have evolved as alternatives to the ways in which conditioning theories have handled the impact of environment on behavior. It's not that these issues have disappeared; it's that they are handled differently, and there are some controversial issues which were treated, from my point of view, erroneously in terms of the tradition underlying those concepts. Now [there are] other conditions that have changed in a way. We now at least do lip service to having an applied developmental psychology, which we really didn't have in the old days. We had one, but we never used the term applied. We were dealing with the effect of IQ on behavior, but it was not thought that it was an applied issues, it was rather a basic issue on the constancy of the IQ, whether or not environmental factors entered or not, and so forth.

Levitt: The early years where more applied anyway, right, the Iowa group and all?

Gewirtz: They were but they weren't; they were strangely pure, but they were also applied. I appreciated the fact that the work at the Davenport Orphanage was done in the late 30s, where there was an enriched group and an unenriched group. If you asked any of the Iowans who did that work, they didn't think it was applied. They thought it was basic research on whether environmental factors can change IQ; their thesis was that they could, and their thesis was justified in terms of the subsequent history of the concepts. No longer do people say the IQ is fixed for life, as Louis Terman did, and no longer were people called charlatans if they found that there were experiential conditions that were associated with changes in IQ scores, as was the case in the mid and early 40s. What other changes were there, I'm not too sure. There's a sense in which we have developmental psychology covering everything. I'm not sure that's good or bad; it serves some purpose, thinking that the group I belong to could cover everything with aplomb, and there's no harm in thinking of it as that. We have license to do that because everything can be developmental, but I'm a little uneasy about the fact that traditional developmental psychology has been diluted. Yet, who is to say that traditional developmental psychology, what I learned developmental psychology is to be, has to be perpetuated.

One of the issues that comes to my mind is whether developmental psychology can maintain its core if it becomes too diffuse. I've seen whole programs kind of disappear, as people became more involved in their own individual areas of discipline and started calling themselves experimental psychologists instead of developmentalists, or social psychologists instead of developmentalists.

I share that concern and in particular I share it with you, my interviewer, who is a member of the same group I'm a member of at our university; that is, there is a sense in which the definition has become diffuse, more diffuse than it became when we had life span or aging added to the notion of developmental. I think there were some novel ideas there, but basically everybody was interested in some notion of sequential changes there, whatever the realm of these changes. I feel, as perhaps do you, that I regret some of my earlier behaviors in the group in my own university, where I did not maintain a discipline in my own choices that would ensure that the core of the developmental group would at least be developmental. That's a concern I have now. It worries me and I'm particularly worried when I think of, say, leaders for the group, say, sequential chairs of the group. I know that there are some individuals who do not think of developmental psychology as I think a core group of developmentalists think and, unless we raise that concern to the fore, we will have problems that are just personnel problems based on the fact that some individuals do not resonate to the core concepts of developmental.

Levitt: This may lead into the next question, which I think you've addressed somewhat already before, but, just once again, what are your hopes and fears for the future of the field.

Gewirtz: Well, I've already mentioned, as you say, that the field has become politicized in the sense that some researchers are so concerned about some issues in the political world, call them practical issues or policy issues, that they are willing, without thinking, to subvert some of the core issues of a scientific organization, to have the organization itself deal with those issues or have it register it's opinions on those issues. This has concerned me particularly because a number of scientists in different fields have lent their names to areas of research which are not their own, I think of Carl Sagan as an example of that, as having their own scientific credentials behind the issues when they have no scientific credentials at all in the areas concerned. This disturbed me about Sagan, because he was very big in that; this becomes a particular concern of mine in an arena where some people have lent their scientific credentials to the politicization of some research having to do with the Vietnam War and its aftermath, and where the vice-president of the United States now is a believer who functions in the arena of junk science, where he pushes scientific principles which meet his political goals and does not deal with these issues as a scientist, but appears to be presenting these issues as a scientist. In other words, when he holds organizational meetings, they don't invite people from the other side to those meetings. There's an imminent possibility that he might become president of the United States and he is essentially a Luddite who's acting anti-Ludditean. So that disturbs me and it becomes very important for me that scientific organizations not be co-opted to deal with positions which are not really in their close expertise or that they deal with positions in their expertise on political rather than scientific grounds. I think there's an eminent danger of this happening. This started, I think, in the 70s, when many of the children of the 60s found that they were scientists in organizations and they found that they had increasing numbers and could actually use the organizations for political ends. Some of my own friends were doing this and this continues to this day.

Levitt: I seem to recall that there was almost a conscious effort to do that in the 70s.

Gewirtz: Yes, yes; this happened at a meeting, I believe of the SRCD in St. Louis about 25 years ago, where the issue of ERA, the women's Equal Rights Amendment came up, and there were many members of the organization, I have Florence Graham in mind, who were pushing for not having meetings in places like Miami, because in Florida there was no equal rights amendment. I was uneasy about that because, you know, there are various other issues about Florida; for example, there were members of the organization from Florida; the state of Florida would lose business, and many people would lose jobs, conceivably, because the meeting was not held there. But there was the conscious use of the SRCD to further a political goal. A lot of energy at the SRCD was expended on this issue, which I was uneasy about at the time. At the time I thought the Equal Rights Amendment was unnecessary, because I thought the Constitution called for that, did not call for women being different from men, just as I am against special hate laws today, because I think our laws cover murder and such things pretty adequately. There's a danger in societies like ours, that we have so many laws that people lose respect for laws, because very few of them are recognized and play roles. But that was something that was not as difficult for me, because it was sort of a one-time politicization, where people were trying to have the SRCD support ERA and they were trying to do it the only way they know. What concerns me more is the continuous use of a scientific organization to further a political agenda and this, I think, is increasingly the case. We've had cases in recent years, and I mention Carl Sagan as an example, but I can mention other scientists who have claimed by virtue of their being for certain political goals, based on research not in their areas. They were encouraging people who read about this to think that their own scientific expertise was what lead them to conclude that certain ends were worthwhile, not that they were doing so for their own value system. I think there's a danger that, if it gets to be known to the public as well as we know this, it will further remove credibility from science and science will lose its opportunities in their fields of expertise to have an impact on policy in society.

Levitt: Your hope then for the field is a kind of return to science, focus on science?

Gewirtz: Yes, we have many political organizations. We have many value organizations. God knows that it's easy to politicize scientific organizations, but I'm hoping that we can go back to an earlier situation where scientific organizations did not attempt to deal with policy matters and perhaps might break off in a scientific and a policy organization, so that the policy organization does its thing and the scientific organization does its thing. I'm hoping to get to an earlier simpler time like that.

Levitt: One last set of questions and its personal notes and I think we did talk about some of this earlier on, but just to add to it. Please tell us something about your personal interests and your family, especially the ways in which they may have had a bearing on your scientific interests and your contributions, or on your applied contributions.

Gewirtz: That's a hard question to answer. I chose my scientific pattern before I had a family. My children, actually, while they went to the university their mother went to and the university I started teaching at, had the notion that academic life was a very unprincipled life. They had the notion that faculty people at universities were all pushing their own personal value agendas, in whatever field they were teaching, and that many universities were actually emphasizing the teaching of values which were based on the faculty at those universities at the time. Faculty at most universities were usually socialist, leftist, sometimes outright Stalinist in the correct era, and my children felt that the faculty at many of these universities had a degree of hubris, based on the fact that they were given life salaries with tenure. They had easy jobs, they never had to deal with the creation of wealth, and they spent much of their time teaching values which were contrary to the values underlying our society and, in addition, were teaching them in the guise of not teaching them much of the time. In addition, they were teaching contempt for many of the majority values of the society. My children therefore concluded that they would not go to graduate school, because the end of going to graduate school was to be a person they considered to be hypocritical, in that you do not create wealth, but you try to teach people to have contempt for the creation of wealth This was hypocritical in that these people were also belittling the wealth creation faculties of the society. So my children refused to go in the footsteps of their parents and turned out to be very different than I ever expected on that basis. From what I have seen subsequently, if I had the maturity of thought that they had, I might not have gone into academic life. I went into academic life, in part, because my parents, who were immigrants, sort of had some notion that there could be nothing higher than somebody being a professor, and I did go into this as a Jewish person. I started studying at a time when there was a distinction in people of my background getting opportunities at universities. World War II was the dividing line. Prior to WWII, individuals who tried to get jobs in graduate universities had a very hard time if they were Jewish. At Harvard in the 20s and 30s people like, for example, David Shakow were sent to be clinicians, to do applied work because it was felt that, as Jews, they could not be in universities. At my own university, graduate university, the University of Iowa, when I came there in 1945, I had no story, I knew people like George Wishner and the Kendlers who were actually prevented from going into experimental psychology, and I knew of a few other people who were discouraged from remaining at Iowa. After McGoegh's death, Kenneth Spence assumed the chair in the early 40s and he didn't have a prejudice bone in his body. Some of his early and very good students were Jewish, like the Kendlers, and so many other students like Farber and others, who were Jewish; he didn't make the distinction. So WWII was the turning point. It was with the ASTP [Army Specialized Training Program] and the subsequent Congress subsidizing students to attend university, post the war, that many individuals went to university and to graduate school and started taking jobs in universities. So WWII was the turning point and many people like myself could go to universities and take jobs. It happens in my case I was a fan of Hutchins at Chicago, in the great books, and when I had an opportunity to go take a job the year I finished, I could go to Indiana, to Illinois or to Chicago, and I chose Chicago. The year before, I might have gotten a job at Stanford and the year after I might have gotten a job at Berkeley. My parents were dead by then, but they could understand something like this, and I guess that was a fact in my choosing graduate teaching and research. Also I had an undergraduate education with some giants, so that helped me to think of research. But having done a lot of research and having seen how there are fads and fancies in research problems, how some research issues come back again, again and again and are not resolved, and how many of the scholars who deal with these issues are not scholars, like in the issue of observational learning, I'm not sure I wouldn't have gone into business. I used to despise business, because I was a Stalinist sympathizer, and a Trotskyite sympathizer at an earlier point, but then I realized the power of the wealth building features of society and the fact that it's very interesting you get quick reinforcement. Dollar bills are very rapid reinforcers relative to say planning a project, getting support for it, doing it, getting it to come out, writing it out, sending it into a journal, then revising it according to some whims of some reviewers. You might find there are five to seven years between the project's origination and the project being published; and then you might not get into the right journal, you might not have the right indicators that a person can read and understand what you're about, and you might not get feedback. Meanwhile, [in business] if you had a good idea you can get feedback very quickly, so I might not have gone into this field if I had the foresight that my children have. On the other hand, if my children hadn't that foresight, they would have had comfortable lives, and they would have been able to take advantage of the bully pulpit that university teaching gives you, and they'd probably be married by now!

Does this finish, do you think?

Levitt: I think we are done unless there's something you'd like to add.

Gewirtz: Well, I'd like to add that I suspect that this is going to be lost in some archive someday. I'm not sure. It never would have occurred to me to do this, in the sense that first I didn't know what would happen to it, I still don't, and I don't know what the organization wants from it, excepting that there were some elderly members of the organization, including Bob Sears, who thought this would be a good thing. I'm not sure about sending an organization papers like this or other papers really. It's a scientific organization; it isn't an organization that does science on its history and if you will by extension it's current.

Levitt: I know as someone who's very interested in history also, I think it's rewarding to have it.

Gewirtz: Yeah, but as history? Somebody can do good research on it someday, but we aren't even using the same language, the various people who deal with this. I'm very suspicious. I think there are fields of investigation, contemporaneous investigation, underlying science that I would be very interested in, underlying how institutions work that I would be interested in, but I'm not sure that something of this sort gives you the material you need -- especially since many of these people who will do the work, if they do someday, will have their own bias systems. A lot of them may think that people who have done conditioning work are the systematic equivalent of rapers of children. There are people who feel that way, or have similar feelings, that these are unfeeling people, that they have a bad heuristic, that there must be something evil about them, that they probably, what these people would consider, belong to the wrong political party. We do know that historians these days do color their history very much. Historians do color their history and many things are not mentioned, I mean even going back the last 20 years, there are notions that Reagan was an actor and he brought on a lot of deficits and what not; in fact school textbooks have that, and they don't deal with the ratios of the deficit to the gross national product or anything like that. So there are many people in the press among the historians who distort history by their own values. And the argument can be made that everybody, whether it's a scientist or a historian, can distort what they are writing about in terms of their own values; in a sense they all do it. But I'm not very sanguine that what will turn out of all this will be something that's better than most history, which I don't think is so good. Sorry about that.

Levitt: Well, thank you very much for the interview.