

Frances Degen-Horowitz

- Born 5/5/1932
- B.A. in Philosophy (1954) Antioch College, M.Ed. in Elementary Education (1954) Goucher College, Ph.D. in Developmental Psychology (1959) The University of Iowa



Major Employment:

- University of Kansas - Chair of the Department of Human Development and Family Life (1968-1978); Vice Chancellor for Research, Graduate Studies and Public Service/Dean of the Graduate School (1978-91)
- Center for Research, Inc. - President (1978-91)
- The Graduate School and University Center of The City University of New York (CUNY) - President (1991-2005)

Major Areas of Work:

- Infant behavior and development, children's learning, theories of child development, intervention and compensatory education

SRCD Affiliation:

- Secretary (1969-73), Governing Council Member (1973-79), President (1997-99)
- Editor, Monographs of the SRCD (1976-82)

SRCD ORAL HISTORY INTERVIEW (Original Interview)

Frances Degen Horowitz
City University of New York (CUNY) Graduate Center

Interviewed by Irving Sigel
At the Graduate School University Center, CUNY
April 21, 1995

Horowitz: This is Frances Degen Horowitz and Irving Sigel interviewing each other for an SRCD history archive. This is Friday April 21st in New York City at the Graduate School University Center, the City University of New York. We decided that Irv will interview me first then I will interview him so that there's a kind of discreteness in this record.

Sigel: Okay, the first thing Fran is how did you get into this field in the first place? What brought you here?

Horowitz: Well, what brought me here was a long series of events. I grew up in the Bronx in an entirely Jewish context. When I was in the sixth grade my parents with some affluence moved out to Long Beach, Long Island. My father's blouse manufacturing business flourished during the Second World War I ultimately went away to a boarding school in Connecticut for two years and then I went to college- a year at Western College for Women in Oxford, Ohio, and then transferred to Antioch College in Yellow Springs, Ohio, from which I graduated. Antioch College in Ohio was this wonderfully unique liberal arts institution with a work/study program. This is in the early 1950's and I was a philosophy

major. My advisor, George Geiger, told all the philosophy majors that he felt philosophy had become a stagnant field and none of us was advised to go to graduate school in philosophy. I really had not given much thought to what I was going to do after I graduated except, it turned out, that I was going to get married. My husband, Floyd Horowitz, was at that time a graduate student in the Iowa Writers Workshop. We talked about going to Iowa but then I applied for a Ford Foundation Fellowship for a special program that took liberal arts students and made them into elementary school teachers. It was really the forerunner of the Master of Arts in teaching program.

Sigel: While at Antioch did you have any experiences in psychology?

Horowitz: I never took a psychology course but I was, for a period of time during one of my work sessions, a guide for visitors through the Fels Institute. I knew a little bit about the Fels Institute. I never took a psychology course because I was afraid it was too hard. I was deathly scared of statistics and math and anything that had to do with psychology conjured up statistics and math.

Sigel: What about your boarding school experience, there was nothing there that sparked a psychology interest?

Horowitz: The boarding school was a progressive co-educational school called Cherry Lawn School in Darien Connecticut, run by a very interesting woman named Christina Stahl. She enriched this boarding school by bringing all kinds of people who lived in the area to give talks and seminars. Karen Horney came regularly. I never connected her with any career aspirations. In fact when I went to college my goal was to become a journalist. I took Geiger's introductory course in philosophy and was hooked, so I decided to become a philosophy major. As I was graduating, I saw a notice about the Ford Program and I applied. Literally, the day I got married, on June 23, 1953, I received the letter saying I had been admitted into this Master's Program at Goucher College in Baltimore. So on that day, my wedding day, Floyd and I decided we would spend the next year in Baltimore. That's how unformed our post-wedding plans were. We went to Baltimore and I attended Goucher for a year and I studied for my Master's in elementary education. During that year lived in the former slave quarters on the Carrol of Carrolton Estate in Furthermillio, Maryland, which were then owned by a Baltimore theatre company that did summer stock theatre there and in the winter ran a theatre in Baltimore. Floyd was writing and worked at this repertory theater in Baltimore. He decided that he would go back and finish his MFA in creative writing but also that he really wanted to do a Ph.D. in English. So at the end of that year, with now a dog and two cats, we moved to Iowa City. I got a job teaching the fifth grade in the Iowa City public schools. This was a very interesting experience because it was kind of my first foray into a totally non-Jewish environment. There was only, I think, one other Jewish teacher in the whole of the Iowa City public school system. The reason I say this is because I wanted to take off for the Jewish holidays and there was no provision for this. I also wanted to do all kinds of things I had learned about teaching children in my Master's program and there was no provision for doing any of them. I go on about this because it has a direct impact of how I ended up in psychology. I kept making all these requests: Could I do a reading test to see what the various reading levels were in the fifth grade? No. They didn't give reading tests in fifth grade. Could I take the children on a field trip? No. They didn't do these kinds of things. Ultimately, the school psychologist arranged for the reading test but most of my other requests came to naught. I did, however, come to the notice of the administrators of the Iowa City public schools. Then in my first year, I took some summer school courses at the University. I thought I wanted to learn more about children so I took a course with Boyd McCandless in Child Development and I was very interested in history so I took a course in history and philosophy of psychology with Gustav Bergmann. I enjoyed the courses very much. There's a lovely story that goes with these courses. We had little in the way of extra money. I was able to take the summer courses, because Esther Crane, who had been one of my teachers at Goucher, sent me \$75.00 so I could enroll in the summer session. Then I went back to teach the second year and then during the second year I kind of decided that I

really wanted to go back to graduate school. So I applied to the School of Education at the University of Iowa because I figured I'd go on and get a Ph.D. in Education. Here I am a teacher and I'm going on in education. But it turns out, that spring I was also President-elect of the Iowa City Teacher's Association.

Sigel: Seem to be getting into presidencies without any trouble!

Horowitz: When I applied to the School of Education for the doctoral program, I later learned that the Dean of the College of Education at Iowa was a close friend of the superintendent of the Iowa City Public Schools. The Iowa City Public School's superintendent, Bufford Garner, told the Dean of the College of Education that I was a trouble maker and they should not admit me and so I was rejected. Boyd McCandless, I do not know how, found out about it. Maybe I told him I wasn't admitted. I don't remember, but he said to me that spring, why don't you come and be a graduate student in the Iowa Child Welfare Research Station? That is how I came into the field of developmental psychology.

Sigel: By being a trouble maker in education!

Horowitz: Yes, and Eleanor McCandless, Boyd's widow, heard me tell this story at the Boyd McCandless Lecture, after Boyd died. I was invited to Emory University to give the lecture in his honor and memory. I told this story which by then I had even begun to doubt myself. Was it really true? Eleanor told me it was. She knew that whole story, which is as he had told me. So I went to the Iowa Child Welfare Research Station at the University of Iowa as a graduate student in Developmental Psychology, never having had a course in Psychology. It was like letting a child into a candy store. It was an amazingly intellectually exciting experience. One teacher was better than the next, Kenneth Spence, Charlie Spiker, Boyd McCandless, Gustav Bergman, Alfred Casteneda. They were an exciting group of people who loved what they were doing. In retrospect, of course, the program offered a very restricted perspective of psychology. However, the intensity was so consuming and the training it provided was exceptional.

Sigel: The commitment.

Horowitz: Yes, commitment and intensity. I've decided, after these many years, that when people talk about narrow and broad and so on, it doesn't make any difference. What makes a difference is that you get hooked and that you get committed and that you get the basic skills of how to do research which was the whole focus of that. How to define problems and how to learn more.

Sigel: Now did you connect at that time anything in your educational experience as a teacher with what you were learning in these seminars?

Horowitz: No, in many ways it was like a separate world except that it was focused upon children. The Iowa Child Welfare Research Station was several floors up from the Iowa Psychology Department in what was then East Hall. (It's now Spence Hall) Spence Theory reigned. It was not concerned with children per se. It was concerned with general laws of behavior. Children were thought to be a convenient way to get at these general laws because, theoretically at least, at that time children were thought to be much more naive than adults. The whole tabula rosa was dominant. If you studied children you would see basic processes. Only at the very end of my tenure there did the name of Piaget ever creep in.

Sigel: So you were able to keep these two things separate, I mean, what you knew of children wasn't what you were studying at the Welfare Station.

Horowitz: Well, you know, I don't know what I knew of children. Two years of teaching elementary school doesn't teach you that much. I was very interested in individual differences which had been an interest of mine in the classroom. But the problem I chose to study didn't really focus on that, though ultimately it came back to that.

Sigel: What made you so interested in assessing these kids when you were there, like getting tests and all that kinds of stuff which is really kind of incipient psychological activity? You were doing that as a teacher for some reasons that you felt would be useful to you.

Horowitz: Oh, I forgot another part of the story. Somehow, while I was teaching I got connected with the Iowa Child Welfare Research Station and Ralph Ojemann. Ojemann had a program that was funded by the Grant Foundation called Preventive Psychiatry. The notion being that if you could teach children to understand that behavior had causes it would provide preventive mental health that would arm children with a more rational understanding of their own and others' behaviors. I was one of the teachers in that program. I don't remember how I got involved with it. We were given materials and exercises to use with our class and we were part of Ojemann's research. Ojemann was always attempting to get writers of children's literature to write that kind of material for the program. When I went into the Station as a graduate student, my initial support was as his research assistant. Ultimately, I got interested in other kinds of things and ended up working with Boyd McCandless.

Sigel: That's really fascinating.

Horowitz: The Iowa Child Welfare Research Station, I have to tell you, was one of the most exciting places that I had ever been, the only rival is this place, the CUNY Graduate Center, in terms of the electricity. But it was also enormously pressured. There was a lot of, not so much competition, but it was intellectually a very challenging place. The fact that I came with no background, it was like everything was new. I wasn't jaded. I hadn't heard any of this before and I conquered my fear of statistics. I mean, it was just wonderful

Sigel: Also, there was a lot of coherence and point of view in that place which helped. So you weren't involved in a lot of intellectual divisiveness the way other universities might have been.

Horowitz: I think that's true. Everyone had come there for the same reason.

Sigel: Right but in the faculty you wouldn't get a conflict when you were in Boyd McCandless' class and go into Kenneth Spence, you are not going to get that kind of ...

Horowitz: Well, there was a little bit of conflict, because Boyd was so much more eclectic and catholic in his taste.

Sigel: It didn't create the kind of intellectual conflict-- that you have divided loyalties I mean.

Horowitz: No, Ralph Ojeman was probably the odd person out in that setting.

Sigel: He was the applied person.

Horowitz: Well, then you had people like Irene Harms and Ruth Updegraff and a wonderful teacher called Howard Meredith, who did Physical Growth and Development. Howard Meredith was one of the most wonderful teachers I've ever had. Everyone who took a class from him realized he was also a very severe stutterer. After a week in his class, however, you never heard the stutter. That's how wonderful and mesmerizing a teacher he was. He had a real sense of integrity about him. That was

true of a lot of the people on the faculty there; they were so committed to what they were doing. It had its downsides. I don't want to paint a rosy picture because that kind of commitment says, you know, "What do you care about your family and all of those kinds of things."

Sigel: Well, who were some of the people there that were really very seminal that you think had a sort of real impact on you?

Horowitz: Well, Kenneth Spence who was the most brilliant lecturer I've ever heard and I say this having come from Antioch where the teaching there was also wonderful. George Geiger, who was my philosophy teacher. Louis Filler, the historian. These were stellar minds but Kenneth Spence had a mind as clear as a bell. Going to hear his lectures was like going to a concert. It was not so much the content as the clarity of the logic of his thinking. If I learned anything at Iowa it was how to think about things. Not so much the content but the way in which you approach and dissect and analyze a problem and then put it all back together. Boyd probably, in terms of substance, made the most impression on me because he had such kind of catholic appreciation of the breadth of Child Development. He was really the most child oriented person of all of these individuals. Charlie Spiker also had a clarity of mind that was stunning.

Sigel: Did you have anyone you work with particularly?

Horowitz: Finally I was Boyd's student and I did my dissertation with Boyd.

Sigel: And what was your dissertation?

Horowitz: My dissertation was the incentive value of social stimuli among preschool children.

Sigel: And what brought you to that problem?

Horowitz: I think, I took a class with Boyd and he identified some kind of issues and we formed the study. In fact, that study was really one of the very, very first studies of peer relationships. Bill Hartup in his early work used to cite my dissertation (which I published as an article in *Child Development*) as one of the seminal studies on peer relationships.

Sigel: So Boyd was one of your primary research mentors?

Horowitz: Yes.

Sigel: But you are also saying that people like Spence and Spiker also were. What about Gustav Bergmann?

Horowitz: Gustav Bergmann was a model, again for the breadth of his knowledge and for the way in which he built wit into his lectures. I'm telling you it was like going to small recitals and concerts when I went to classes.

Sigel: Now did your philosophy background relate at all to what you were learning there?

Horowitz: Yes, it's definitely related to the history of ideas which is what Gustav was the most interested in. I brought all of that philosophy background into it.

Sigel: So you were able to integrate a lot of that Antioch experience into your graduate training? So that it was really more than just the behaviorism of Iowa that you had there, you had the richness of what you came with it.

Horowitz: We also had to do some minor work. I did some work in sociology. But the great part of Iowa was the methodology. You really couldn't come out of Iowa without a deep understanding methodology, which is applicable to any topic. It gave you a sense of power that you had the tools to go out and do research.

Sigel: Now as far as your fellow graduate students, there were a lot of them weren't there?

Horowitz: Oh, yes. Shep White, Dave Palermo, Lou Lipsitt and I shared an office. Hayne Reese, those were the main people who went on and established strong careers in the field.

Sigel: These are your contemporaries?

Horowitz: These are my contemporaries.

Sigel: And your relationship with them, did that have any influence on your own intellectual development?

Horowitz: Well, I think a lot of them helped me. Shep and some were much more focused upon Spence-Hull theory. They were more theoretically oriented than I was. I liked Spence-Hull theory, but, at that time, I was not that interested in theory. I was really more interested in individual differences and interactions of children. I'd say that Shep and Hayne were probably the two most intellectually high powered people from the theoretical point of view.

Sigel: Now this is all sort of within the university but in terms of what else was going on at that time, was there anything in terms of a social theme? You talk about the Jewish issue, were you involved at all in that whole issue of being Jewish in Iowa?

Horowitz: Well, being Jewish in the Iowa Public Schools was an experience. My husband, who was pursuing his doctoral degree, became the associate director of the Hillel Foundation. So we were very involved. I taught Sunday school at the synagogue. We were very close to the Jewish community in Iowa City.

Sigel: So the situation that happened with schools was not replicated any place?

Horowitz: No, in the university it was never an issue.

Sigel: What kind of career direction did you see yourself as following?

Horowitz: Well, you know Irving that's very interesting because I didn't. I mean I was finishing up my degree and we decided to start our family and I went through the commencement two weeks after my oldest son was born and we were heading to Oregon where my husband had gotten a job at Southern Oregon College in Ashland, Oregon, in the English Department. I remember this wonderful conversation with Boyd McCandless the spring before. Floyd was looking for a job then. I told Boyd I would go where Floyd was going and I would try to get something too. Boyd said to me, "you know you really should have some clinical skills just in case." So I took a course in the Rorschach and in the Thematic Apperception Test with him so that I would have some testing skills and what he was trying to say...

Sigel: Options.

Horowitz: That's right. When we went to Oregon I then decided to try to do some teaching but the interesting thing is I also applied and got a research grant from Sigma Xi. I will always be very grateful to Sigma Xi for that.

Sigel: To do?

Horowitz: To do a follow-up study on my dissertation. When we went to Oregon I didn't have a real position (because they had a nepotism rule) so I stayed in the research track. That little grant that I received ultimately ended up in a publication and kind of kept me on the research track. I never fell off the research track as many people do who don't get positions in universities.

Sigel: So you intended to stay, but you had no real focus or career objectives at that time?

Horowitz: Well, I wanted to teach and I wanted to do research and it became clear that that wasn't going to be possible in Oregon so we decided we would need to look for other opportunities. What happened was our closest friends from Iowa City were Elene and Jerry Siegel. Jerry was in speech pathology and they were the couple that lived in the Hillel House- this is the Jewish connection again. They went to North Dakota and then they went to Kansas to the Parsons Training School, which was a school for retarded children in Parsons Kansas. A man named Dick Schiefelbusch, was the head of the Bureau of Child Research at the University of Kansas. He had gotten one of the early grants in mental retardation, this is in 1960. They were looking for some people to come for the summer and do research. So Jerry called me and said, "You guys want to come to Parsons, Kansas? We could give Frances a research fellowship to do research here for the summer. At the end of our first year in Oregon, Floyd and I and Jason went to Parsons, Kansas. I had this research fellowship there and then we went back to Oregon. Now Dick Schiefelbusch who had kind of spotted me went to the Dean of Liberal Arts at Kansas and told the Dean about me and said he'd like to get me to come to Kansas and that my husband was in English. So the next May we got a phone call asking Floyd on the phone whether he would like a position in the English Department at Kansas. I called Dick Schiefelbusch and said, if Floyd and I come to Kansas would there be anything for me to do there and Dick said, we'll find something, because they also had a nepotism ruling and I couldn't be on the faculty. On that basis we packed up, we left Oregon (with now two sons and a dog) and we went to Lawrence, Kansas, and sure enough Dick gave me a desk in the Bureau of Child Research and a hundred dollars a month that paid the baby-sitter. Floyd had the position in the English Department and I had my desk at the Bureau of Child Research. I applied for a post-doc, one of the early NIH post-docs and I got it. I stayed doing research and then I did some teaching and then Kansas, within three years after we arrived did away with the nepotism ruling. At that point, the Dean called me in, George Waggoner, a really wonderful man, and he said, "We have a Department of Home Economics and we've made an arrangement with Kansas State University. They have a School of Home Economics, we will not continue in the Home Economics Business." He asked me: If you would become acting chair of this department, what would you do with it? They had a nursery school, and so I told him about Iowa and the nursery school and how this served as a research program and so on. He asked me to be acting chair. This is 1964-I am five years out of my Ph.D., and that department evolved into what became the Department of Human Development and Family Life of which I was chair ultimately for ten years. That's when I got an appointment on the faculty and went into a tenure track position. I didn't map this out at all. I mean, I knew I wanted to do research, I knew I wanted to teach and then all kinds of things happened around that, fortuitously at the University of Kansas-because of Dick Schiefelbusch who wanted an academic department to be a partner with his Bureau of Child Research and because George Waggoner had a need to evolve a department of Home Economics into something else.

Sigel: How did you handle the dual career business, because that was an early time?

Horowitz: It was an early time but Floyd Horowitz is an unusual human being and we just worked it out. When we moved to Kansas we had our second child already, so we had our two boys. Lawrence, Kansas, is a very easy place to live. We lived within two miles of the campus. We had a student always live with us, free room and board in exchange for baby-sitting and chores and then we used my salary to hire people to be housekeepers essentially. Floyd worked a lot at home so the boys weren't with somebody else alone all day long and with us just totally gone. I could come back and forth easily. Our household has been more than 50/50 and it's always been on Floyd's side. I am very privileged and spoiled in terms of all kinds of domestic aspects.

Sigel: So that was never really a thing you had to work with. So that went along very nicely. Now in terms of your research, what were your primary interests at that time and can you sort of historically develop that?

Horowitz: When I went to Kansas I was still interested in social stimuli in children. I did one more study in that area but then I had had some ideas in Iowa. I was interested in simultaneous and successive discrimination learning and it came out of Charlie Spiker's class. I think the thing about the Iowa training, that I realized afterward, was that I was very interested in developmental issues, but Iowa was more "child psychology" than "developmental psychology."

Sigel: What made you interested?

Horowitz: Individual differences have always interested me and how they change over time but I had not articulated that yet. The NIH post-doc that I received was to do a developmental study of simultaneous and successive discrimination learning and to look on a cross-sectional basis how those changed over the course of development. It was a very elaborate study. While I was doing that study I got very interested in the individual differences I was observing. I began to ask myself: Where do these individual differences come from? I felt the need to look at younger and younger children, and that's what ultimately led me to infants—the desire to see those individual differences right from the beginning. In 1966, whenever the Eleanor Ames paper was published—somewhere around there— I decided I wanted to develop an infant research laboratory. Remember the very earliest Merrill Palmer Conference (when was that?). I had just begun at that time to try to set up a laboratory to study infant behavior and development.

Sigel: So that led you into the whole infant lab. Now what do you see as some of the real contributions you've made, that you feel good about?

Horowitz: Well, I feel good about some of the methodological contributions I made in the study of habituation and the development of the infant control technique. I feel very proud of the work that I did on the neonatal behavior assessment scale. Around 1963, '64 I was chairman of the department at this time and I had recruited a number of faculty: Don Bear, Barbara Etzel and ultimately John Wright. We received a big Early Childhood Laboratory grant that involved Dan Freedman from Chicago and some people from the Boston area— Barry Brazelton, Ed Tronick, and Arnie Sameroff who was then at Rochester. I had already started my infant lab and we were mainly doing work on visual and auditory perception. The very first study I published from this lab was on complexity. (John Colombo at the recent SRCO meeting went back and picked up those data.) I had a big fight about those data because what I was able to show was that the visual response to complexity across the ages from three to fourteen weeks of age was a function of prior exposure to that complexity. If we showed complex stimuli to babies at 14 weeks of age who'd already seen once a week for six weeks you got a very different response than if we showed them at 14 weeks and it was the first time they saw them.

Whoever did the review, I've never known, got very defensive about my findings and it was forced into a brief note in *Developmental Psych*. Brief notes did not carry abstracts and it didn't get picked up, but John Colombo went back and picked it up. We produced interesting, wonderful data there that now are very relevant to the whole field on infant memory but it was way before its time. That whole process of trying to get that published taught me a lot about publications. In any case, I met Dan Freedman and Berry Brazelton at one of the Early Childhood Laboratory grant meetings. Berry was just beginning the work that ultimately led to the Brazelton Scale, the Neonatal Assessment Scale. Ultimately we did all the work on the reliability of the items for that scale. I was interested in the Scale as a way to look at newborn babies and see the individual differences at birth. Not only am I proud of that, the thing I am most proud of about the Brazelton Scale is that I with Arnie Sameroff and some others prevented that scale from being developed in a way that would yield a single score. I was very concerned not to be a part of an effort, a testing effort, of newborn baby that would put one score on a baby. I've always had a lot of reservations about IQ scores as single identifiers. This was before all the current IQ controversy. But even in the 1960's I did not want to be associated with anything that was going to put a number on a baby at birth. So the Scale is a statistical nightmare but it's purposely a statistical nightmare. That's what a lot of people don't know when they criticize that Scale that a number of us, myself particularly, did not want to have anybody be able to produce one score on a baby at birth.

Sigel: So you have profiles...

Horowitz: You have profiles, you get clusters and you can do all kinds of things with it. The second thing about the Brazelton Scale that I feel proud of is that I prevented it from becoming a standardized test. I've always felt that it was a research tool and not a clinical tool. Now this is where Berry and I parted company and I don't think Berry has ever forgiven me for it because I felt the claims he wanted to make for that test were unjustified and that it went against the whole concept of what development is about. I feel there is no reason that you should be able to predict anything about developmental outcome from a measure at birth unless you have a highly damaged organism because if development is a dynamic process then everything that happens in between is going to determine that developmental outcome. To be interested in a scale at birth that would make a prediction to 17 or 18 years of age is, I believe, theoretically misguided. Berry and I had lots and lots of arguments about this and I said publicly then and I continue to say publicly that the Brazelton Scale predicts nothing. The fact that it predicts nothing does not make it useless. It tells you a lot about newborn behavior. I always required all my students to learn to give that Scale and to get a lot of experience with it because it was the best way to sensitize them to individual differences in newborn behavior as well as to see the range of capacities in newborn infants. It is a wonderful training tool for pediatricians, for psychologists, for anyone interested in early individual differences in children but it is not a predictive tool. Theoretically it can't be a predictive tool and trying to sell it as something that makes predictions about children I think is almost unethical.

Sigel: Now were there any continuities in your work from all of these ideas? In other words, did you proceed following through on what you are describing now with the infant research program?

Horowitz: Well, we continued to do work on visual attention I also continued to do academic administration. I was a department chair for ten years. Then I became an associate dean of arts and sciences for three years. Then I became a vice-chancellor for Research Graduate Studies and Public Services, and Dean of the Graduate School. During all this time, I was blessed with having wonderful students who worked with me and ultimately post-doctoral students: people like John Colombo and Charles Nelson, Kathleen McClusky-Fauset, Marian O'Brien, Patty Self, and Rex Culp all worked in my laboratory. I was always well funded, either with grants I got or being parts of group efforts with the Bureau of Child Research. I was always able to hire somebody to kind of run my lab on a day to day

basis. I usually hired someone who was a nurse. I was always concerned about the ethical issues related to our laboratory work and what students learned, about how they dealt with parents, how they treated infants, the kinds of things they said and did in the lab as well as in a hospital setting.

Sigel: Where did you find your time to do research and to write?

Horowitz: Well, you know you make time for the things you want to do.

Sigel: But the thing is, how much attention could you give to your lab in terms of a hands on?

Horowitz: Well, when I was Chairman the lab was just down the hall I could give a lot of attention and the colleagues who were in the Department of Human Development and Family Life, Don Bear and Jim Sherman, Mont Wolf, Barbara Etzel, Todd Risley, they were just wonderful colleagues, John Wright and they cared as much about my doing research as they cared about doing their research. The fact that I wanted to keep my research going was part of the kind of mutual respect we all had for each other. So when I was Chair, keeping relative hands on in the lab was possible. When I became vice-chancellor that became much more difficult and then I had to rely primarily on post-docs.

Sigel: So you made a decision of going the administrative route rather than staying within the traditional academic research place?

Horowitz: I guess it was not so much a deliberate decision as opportunities.

Sigel: But you took them.

Horowitz: I took them, yes.

Sigel: Now there must have been a reason that you felt that was a direction you wanted to go rather to continue in, as so many of your colleagues have, as still being professors and doing research.

Horowitz: I don't mean this as sounding immodest, but I was good at it and I loved being at the University of Kansas. There were many times that I knew that my skills as an administrator made a difference and it was hard for me to say no, I don't want to do that and just go back and do my own work and particularly at a period when there were all the civil rights upheaval and all the difficulties that were happening on campuses. I knew that I could play a role that would be constructive for the university. I always have loyalties to institutions. I mean I care a lot about the institution in which I live. I've never kind of felt it's an opportunistic home for me. I felt it was my intellectual home and I had a responsibility to keep that home. I had opportunities to work with some wonderful people at the University of Kansas and so, as you said, I took them.

Sigel: Now that means that you saw yourself as having perhaps more significant influence in fact both on psychology and other issues as an administrative role than as a laboratory researcher.

Horowitz: No, because I don't think I had any impact on the field because I was an administrator. I kept those two roles quite separate.

Sigel: You built up a program at Kansas.

Horowitz: Yes.

Sigel: So wasn't that the contribution to the field?

Horowitz: Yes, it was.

Sigel: So in that way you facilitated the development of a developmental program.

Horowitz: Yes, and of colleagues and I get tremendous pleasure out of that.

Sigel: Now that wouldn't have happened if you'd stayed in your little niche.

Horowitz: True. But it's partly because it has been so reinforcing to me. At Kansas the administrative positions I was in were always ones that were extremely instrumental in making good things happen for other people. I get a great deal of pleasure out of that.

Sigel: But also those were the people in the profession that you are involved with so you were helping things happen for them.

Horowitz: That's true for psychology, but it's generic because when I was vice-chancellor for research in graduate studies. I was doing a lot for all the fields on the campus.

Sigel: So you see yourself in a sense as a catalytic agent for the intellectual development of whatever the institution. Now, do you see then in terms of this shift in your career from essentially an academic researcher to this administrative role?

Horowitz: Well, once I'd become President of this institution (The Graduate Center of the City University of New York) where we are now, then I've had to kind of reassess. I should say, over the last ten, twelve, fifteen years a growing part of my interest in developmental psychology has been theoretical. It came partly out of my Iowa background which was so strongly theoretical but in Iowa it was not the kind of theoretical that ever grabbed me as a developmental theory. I think I took a lot of growth from my colleagues at Kansas, many of whom are Skinnerians and the fact that I taught the theories of development course which I loved to teach. I really built the course into an exploration of developmental perspectives that was very important for my own intellectual development. Going back to Boyd's influence, Boyd made sure we knew about Freud and about all the various ways of looking at development. I began to feel the need to try to integrate these kind of different strands of theory into a more comprehensive approach to development. In 1983-84 I had the opportunity to spend the year at the Center for Advanced Studies in the Behavioral Science at Stanford. I really went there to write up my course lectures that I had been teaching at Kansas as a book. But I discovered I couldn't do that, because what I did in the course in terms of lectures didn't make a book.

Instead I took six months to just read; it was idyllic. When I turned to writing, I found I couldn't write the book that I had intended to write. What I did write became *Exploring Developmental Theories*.

Sigel: Right, so then in a sense what this says from the kind of question here, there was this big shift from the more empirical, hands on, to the theoretical.

Horowitz: Well, it wasn't such a big shift, it was a gradual shift but it was a gradual shift also leavened by the involvement in things like Head Start, applied issues, all of the social controversies, the nature/nurture issue that I had always been interested in and that increasingly captured my attention. I want now to go back to Iowa. The Iowa Child Welfare Research Station doesn't exist anymore. I think one of the reasons it doesn't exist is that it would never evolve. It was very rigidly focused. When I was there that rigid focus was very helpful in educating me at that time. However, as times changed it lost its attraction for students, many of whom were caught up in the issues that roiled in the '60s and

'70s. I remember a story of how when the Head Start Research and Evaluation Centers were developed that there was a lot of resistance at the Iowa Child Welfare Research Station to even becoming involved in those issues. There was a rejection of the applied aspects of our field which I feel are important and for enriching the interchange with the more theoretical aspects. I think one of the reasons Iowa died (and I think it's a great loss to this country that that place doesn't exist anymore) is that there was not either the leadership or the flexibility to take the tremendously good existing theoretical and experimental strengths and find ways to evolve those strengths to address the issues that were coming into our field and that I think we ultimately had to address.

Sigel: Where does that broader interest come from? That just doesn't come from thin air.

Horowitz: My broader interest? Well, I think it comes from always being involved in community. I was always deeply involved in the Jewish community. In Lawrence, Kansas, I was very heavily involved in that community, in the broader community, and in the problems of the community. I find it difficult to live a totally compartmentalized life. You can't be in the community and see poverty and fail to think about that and what it does to children and what the kinds of issues are that we ought to be addressing and the public policy issues that come out of that.

Sigel: But that didn't seem to be very critical during your graduate training as it did afterwards.

Horowitz: Right, my graduate training was very unconnected to everyday problems except for Boyd. Boyd was the one who connected it. I mean when I write about the nature/nurture issue and I go back to the 1920s and the 1930s it's Boyd's voice that is talking. That's where I got that from.

Sigel: So now the idea is, they ask about the strengths and weaknesses of your research and theoretical contributions, don't you?

Horowitz: I can be my own best critic. The weakness is what you've pointed out, is that I didn't go put myself in my office and just do that. I sometimes think if I hadn't done any administration, what else would I have done in research. I'm sure I would have done more but would I have done better? I don't know but I was never totally only a full professor doing only my research and my teaching. I've never done that and had I done that all these years I would probably be at a different place.

Sigel: But that's a career issue not a conceptual issue, isn't it? I mean, it was a choice to find out whether you go one road or the other.

Horowitz: But it is a conceptual issue because if you do spend all your time doing teaching and research you probably are going to evolve intellectually to a different place than when you don't do that, so it is a conceptual issue.

Sigel: But the different place is different but not necessarily better or worse. I mean, you are putting it in terms of a weakness, the thing is, it's different.

Horowitz: It's different.

Sigel: So it's really noncomparable isn't it?

Horowitz: No, because I come back to what I was planning to do when I went to college. I love to write and I would have written much more. I learn when I write and I conceptualize. I think I would have theoretically gone in a much deeper direction than I have gone had I kept to a teaching/research path only.

Sigel: I guess that's always the thing, one never knows! Let's see, they talk about influence on shaping public policy around funding and things of that nature. What were your involvements in some of those activities?

Horowitz: Well, that was very extensive and very much through APA and SRCD. Particularly the Division of Developmental Psychology in APA. I'm very professionally socialized, I join organizations, I go to meetings, I do all those kinds of things and again I like them. I like the people contact of it and the feeling of being engaged. So early on I was active in both the Society for Research in Child Development and in the American Psychological Association, especially with respect to the issues of funding, national policy, Head Start Research and Evaluation. In fact, I just finished writing a chapter that reviews those early days of Head Start Research and all the issues that we went through. You can't touch any of that stuff without getting into public policy issues. I have always had an inherent interest in public policy. I think it makes a lot of difference what people believe about the capabilities of children and how that translates into education and public policies.

Sigel: Now was this one way of getting support for your activities as well or was it just part of a general social professional contribution?

Horowitz: Yes, it was a way of getting support. By caring about the issues related to Head Start and being involved in the early Head Start Research that's how we became, at Kansas, one of the thirteen Head Start Research and Evaluation Centers.

Sigel: Now institutionally you worked at Oregon and Kansas, that's it plus here?

Horowitz: Well, Oregon was just two years, thirty years at the University of Kansas and when you stay one place that long, you and the institution take on a kind of symbiotic relationship that for me was very positive.

Sigel: But you were very active in administration there so that you became part of the way the wheels were working and part of that.

Horowitz: Yes, and I have to say that Dick Schiefelbusch, who was the head of the Bureau of Child Research, and I worked as a pair. I could not have done many of the things that I did there if it had not been for Dick. He had a very large vision about what the Bureau of Child Research should be doing as a research institute at the University of Kansas. And he believed very deeply in interdisciplinary relationships. So you had the psychologists in Human Development and you had the speech pathologists and you had other people in other areas who really came together around problems. As a result, we could go after the Mental Retardation Research center grants and the Early Childhood Laboratory grants. They were all based in collaborations. It was a number of years until I got my first RO1NIH grant. I didn't start out with an individual research grant, I started out in the collaborative context and that in many ways enriched many of the things that I did.

Sigel: Did that have any effect in terms of developing your own views of the phenomenon of interest?

Horowitz: Yes, and it goes back to that summer that I spent in Parsons, Kansas, on this fledgling mental retardation research effort. I remember that I began to think about learning and development and retardation. That was during the period when the whole notion of arousal theory that Berlyne wrote about was being discussed. I remember writing a paper for a mental retardation journal on arousal theory and retardation and how the deficit may be not so much in that learning was not

possible but that the time it took to learn was going to be much longer. For many individuals who have low learning rates teachers and society give up. And the result is increased and cumulative retardation. That concept has informed a lot of what I have done. You can see it in my own theoretical work now. It came out of that opportunity to work in a mental retardation research project.

Sigel: Now, what were the exact dates that you were at Kansas?

Horowitz: I finished my Ph.D. in 1959 and that's when we went to Oregon and we stayed in Oregon for two years and then in 1961 I went to Kansas and I stayed in Kansas until 1991. During all that time I had one sabbatical year in Israel which is where I did some neonatal work and I had one year at the Center for the Advanced Study in the Behavioral Sciences. I took two sabbaticals in that whole period. The rest of the time we were just in Kansas. I did one summer at the Institute for Child Development in Minnesota. I was invited repeatedly to do things at Minnesota, but basically I was at Kansas.

Sigel: So the other thing that they want to know is your experience in so called applied developmental work. You did a lot of work with preschool and social reinforcements with kids and a lot of learning studies. Did you see that as part of your applied experiences?

Horowitz: Well, I have never made this applied/basic distinction so specifically. I mean, I've written so many grants in which I justify my interest in basic processes in terms of ultimate application. I respect people who just do basic research that has absolutely no obvious applications for anything because that's how science ultimately moves. I've always been much more at the intersect of basic and applied research. I did work in basic visual behavior and visual responsiveness because I am interested in individual differences. I believe individual differences will allow us at a very early age to identify children who need certain kinds of help. The Head Start research was obviously, in that sense, applied.

Sigel: Some of the programs that you were working with in Kansas, however, seem to have the direct relationship to potential curriculum development in the preschool, or teacher behaviors in preschool.

Horowitz: That was really more my colleagues. I didn't do any work in that.

Sigel: Oh, was that Barbara Etzel...

Horowitz: Barbara Etzel and Don Baer. The department of Human Development and Family Life that I founded and chaired for ten years was primarily made up of people who were in behavior modification. John Wright and I kind of held up the traditional developmental end of that. It was a very unusual place because there were no arguments. I mean, there were no paradigm arguments; there was respect across the aisles as it were. We didn't use students to fight battles with. But my behavioral colleagues taught me a great deal about methodology and about ways in which to look at problems that some people called applied problems but you could also look at them in relationship to basic research. For example, all the schedules, learning schedules, reinforcement schedules, and so on which they were doing in institutions for retarded children in trying to get children to learn to talk and do self-help kinds of things I was in that atmosphere though I myself never did that kind of work.

Sigel: So now we come to your experiences with SRCD. When did you join?

Horowitz: I joined SRCD as I graduated from Iowa. Everyone was sort of expected to join SRCD and I believe that in 1959, if I am not mistaken, SRCD was in Iowa City. I do not remember going to the meetings but I do remember Lew Lipsitt looking for Judy Rosenblith. It was the time when the

perinatal collaborative project began. That's what took Lew to Brown University. I remember this because Lew and I shared an office and I was taking the messages. I don't remember going to those meeting though maybe I did but in 1961 the meeting was at Penn State and I flew from Ashland, Oregon, to Penn State through Chicago. I was seven months pregnant and I remember Frieda Rabowsky looking at me as I arrived and she said, "You look as big as a house." We had flown on this horrible plane that couldn't land. It was a memorable trip. I attended those meetings and then I think I have missed only one SRCD meeting ever since. SRCD was my professional milieu. I also joined the American Psychological Association and Division 7. I also tended to go to all the APA meetings. SRCD luckily met only every other year. So I went every year to APA and I went every other year to SRCD.

Sigel: And your participation?

Horowitz: I always presented a paper. I always sent in a paper, it was always accepted and then began to ask to be on symposia. I began to propose symposia and then I don't know how but ultimately I was asked to run for secretary and so I served as secretary for SRCD for six years. Now Boyd McCandless gave me a big boost because when Boyd became the first editor of *Developmental Psychology* he asked me to serve on the editorial board. Once you begin to do that your name becomes known and you have all kinds of contacts. Whether that's how I got more involved in SRCD I don't remember. But when I was asked to run for secretary and I was elected, that put me six years working on and with the Governing Council. Then, at the end of that term, I was asked to run for Governing Council and I was elected. So I was another six years on the Council. The result was that I was organizationally involved with SRCD for twelve years running and that was a lot of contact with SRCD. Then I was also asked to write for and then edit SRCD publications. SRCD was doing those reviews of *Child Development Research*. Betty Caldwell asked me to do a chapter on Head Start and on early intervention. Then I was asked to edit the next volume in the series. Then I was asked to be editor of *Monographs*. So there was just a sheen of being called upon by SRCD and I love to respond so I did it.

Sigel: You must have done a decent job for them to call you back! What are some of the most important changes to occur at SRCD during your association with it?

Horowitz: I think it is the broadening of the view of research and I think it's a much livelier organization now in which the real issues of the world are part of SRCD in a way maybe they were not in the early years when I was first involved in SRCD.

Sigel: So you think they are becoming what, more socially aware or more socially involved?

Horowitz: Well, I think the issue of social policy and public policy as they affect children and families has become much more a part of SRCD.

Sigel: What conflicts do you see there between different movement, I mean, in the direction of social policy versus research?

Horowitz: Well, I think when a scientific organization, when a research organization gets involved with social and public policy, you always have the natural conflict between being a voice on behalf of something as opposed to being a voice that informs public and social policy but does not advocate. I think the latter is very important, that an organization like SRCD should be a source of voices that inform public and social policy as opposed to taking stands for one policy over another. Now it is very hard not to take stands especially now when you've got, and this is my own view, you've got crazy proposals about reducing school lunch and all kinds of support for children that we know will hurt children. For this reason I'm very pleased that SRCD has now decided to begin to issue, you may not know this, but to issue some research brief papers that will inform public policy. SRCD can say: Here's

what we know and here it's relevance to the policy you are considering as opposed to saying this is what we think you ought to do. I think you need to keep an organization as vital as SRCD is, with all the kinds of emotions in this organization, to stay on the side of making sure that the voices are there rather than taking political stands. The latter will destroy a scientific organization.

Sigel: So then is this a position that the Council agrees with or the power structure of SRCD?

Horowitz: Well, you know I don't know. When we were about to move here from Kansas which is now almost four years ago, I was asked to run for President of SRCD. I declined because I thought taking a new job and being President-Elect of SRCD would not work well together. When I was asked again to run, several years later, I agreed and was elected and will take office. So it's maybe kind of six years since I've been really very deeply involved in SRCD serving as Editor of *Monographs* and on the Council. I don't have my feel for things right now. A lot has happened in these last six years. So I don't know the answer to that.

Sigel: Now at this stage of your career as you look back, what would you say about the history of developmental psychology in terms of have your issues changed, have you seen how the field changed? What would be your kind of perspective from Olympus?

Horowitz: Well, I'd say we know a lot more. We really know a huge amount more about children and their development and we have a sense of the enormous complexity of the phenomenon that we are dealing with. As we know more and more we also know how little we know. So I feel like when I started there was just a little bit of stuff at the pot. Now the pot's about half full of just a lot of good information. I think a lot of the work that's going on currently is really very exciting work. It's kind of hard to keep up with all of it, obviously, but that's why I think SRCD is such a wonderful meeting. I just walk through some of the poster sessions. Of course, I gravitate toward the infancy ones which is where I still have a lot of interest but on the way I found myself being captured by things I knew nothing about because the topics are so interesting.

Sigel: Like what?

Horowitz: Well, violence issues, adolescent development, some stuff in cognitive development. There is much more attention to middle childhood now. When I taught I used to begin the course by putting a developmental time line on the board, evenly and then I put another time line in terms of where we knew something. I could show that we knew a lot about infants and we knew something about preschoolers and then we knew very little about the elementary school kids and then there was a kind of blip at adolescence-old stuff-and then after that very little. All that is changing now with the introduction of a life span developmental perspective of really seeing things in a continuum. I remember I used to say to people who would complain that their preschool child was very stubborn and I'd say a stubborn child makes a persistent adult and that you have to have this life span perspective to really understand early development. That if you just get focused on the phenomenon in the first three years or four or five or six years of life, I mean ultimately we are interested in productive and happy and satisfied adults and if you don't understand what sort of some of the goals of development are and see that in this life span perspective then our whole take on early childhood can be wrong. I think that's the most exciting part about the field and the life span perspective is not yet as embedded into the field as ultimately I think it has to be.

Sigel: So what are your hopes or fears for the future?

Horowitz: Of the field?

Sigel: Yes.

Horowitz: My fears for the future which I've just written about in this chapter is that we are going to do great damage to the ethos of this country by not being more vigilant about how we make our statements about what's important. I think behavior genetics is one of the most potentially destructive forces in our field in terms of what it's going to do to this country and to democracy. That's my great fear that well intentioned people have no idea of how the way in which they frame their questions and the way in which they talk about their results can be used to political no good purpose and that we have a social responsibility in that area. I think that we have to make sure that we train students to understand the social responsibility of what it is they are doing even if it is the most basic research because ultimately as you develop as a professional most people spill out of the narrow area that they are in. Some people don't and that's not a criticism, because sometimes your greatest advances come because people stay very narrow and very focused. But a lot of people don't, and our society has so many needs that we are called upon as developmentalists, as people who care about children, to opine and to contribute. If we don't understand that every time we open our mouths, that there is social responsibility attached to that, especially right now when there is a mean spirit that's taken hold of this country, you can inadvertently contribute in ways you did not intend.

Sigel: What do you mean responsibility or consequences? Are you talking about the consequences of how we say things?

Horowitz: The consequences, yes, but the social responsibility is inherent in making sure that what we say is not misused.

Sigel: So then that puts us into a different role than the traditional basic scientist that just lets the chips fall where they may, these are the facts.

Horowitz: Yes. I have never been able to be someone who says, these are the facts, let the chips fall where they may, because facts are socially constructed. Facts do not hang in the air all by themselves, in a vacuum like monads. The critical aspect is to be an informed voice that gives context to facts and that always notes the complexities involved in human development.

Sigel: And the hope is, that you see a lot of potentially good in what we are doing?

Horowitz: Oh, the hope is the vitality of this field. What we have to do is get more people in this field who can bring different perspectives into the field. I think there are different reasons for caring about diversity in our country. For example, in some areas, like chemistry and physics, diversity is important because we need the talent from all the diverse communities. In a field like child development, we need people who have different perspective and frame questions from different points of view. It's like the people who worked in gender. Until gender questions got introduced, whole areas were thought of in very different ways. We do not have enough diversity in our field. You go to SRCD and you look around and it's not as colorful a place as it ought to be.

Sigel: And the last of this is personal notes. They talk about personal interests, families especially the ways in which they may have had a bearing on your scientific interest on contributions.

Horowitz: Well, I'd say there are three. One is individual differences in terms of my two sons, who could not be more different than one another. The second is Jewish, of being very much a part of the Jewish community for all my years and looking at the world from a perspective that's different than the majority view. Whenever anybody says African American or Black, I substitute the word Jew and I say to myself, how would I feel if the sentence was about me instead of somebody else. And that's really

informed a lot of my own way of looking at things. Then the third is family, the pleasures that come from family.

Sigel: Well I think we've got about everything.