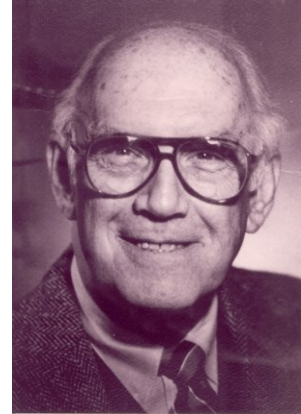


Irving E. Sigel

- Born in 1922; died February 26, 2006
- Spouse: Dr. Roberta Sigel
- Ph.D. in Human Development (1951) University of Chicago, M.A. in Human Development (1948) Clark University, B.A. in Psychology (1943) University of Chicago

Major Employment

- Distinguished Research Psychologist (Emeritus), Educational Testing Service: 1989-2006
- Distinguished Research Psychologist, Educational Testing Service: 1976-1989
- Chairman of Research, Merrill-Palmer Institute: 1959-1969
- Adjunct Professor, Wayne State University: 1959-1969



Major Areas of Work

- Cognitive development

SRCD Affiliation

- *Monographs of the SRCD* Editorial Board (1977-1983)

SRCD ORAL HISTORY INTERVIEW

Irving Sigel

Interviewed by Frances Horowitz
April 21, 1995

Horowitz: So tell me about your family background and your early intellectual history.

Sigel: My family background—I came from a New England city, Worcester, Mass., a small very ghetto-like existence. I mean, the Jewish community was very insulated and isolated and close. There was a lot of, for some reason, intellectual interests in all kinds of things. I really don't know quite why because ever since high school I was very active, not only in school things, but in social causes like the peace movement and interfaith movement and all of this stuff. Not having any money, like many other Worcester boys, we went to Clark University because that was right there.

Horowitz: But Clark was a private university.

Sigel: It was private, but one of the objectives that Jonas G. Clark had was to provide opportunities for poor Worcester boys. So we had very low tuition. We had scholarships. Two hundred dollars was the tuition and if you did well in high school you got one hundred dollars off, so the tuition was a hundred dollars a year. And we lived at home, and I walked to school half the time, and I worked my way through college. I planned a major in either history or go into the rabbinate, one of the two because I had a mentor who was just a fantastic rabbi. He was conservative, but modern; someone who was basically a philosopher. We spent an awful lot of time discussing issues of religion and ethics and always questioning and getting into these things, and it was not the dogmatism as much as it was an exploration. I thought, gee, that's the life of the rabbi, until I talked to my father. He said there's a lot of other things that happen in that kind of existence.

Anyway, when I went to Clark I was going to major in history or premed. I was going to be premed, but I could never get through organic chemistry. Biology was a favorite subject, but chemistry was a dead bore and so I thought, well, I'll major in history and become a history professor or something. I had my first course in psychology with Raymond B. Cattell and suddenly I discovered psychology. It was fantastic. What we dealt with then was McDougall's psychology and the issues of instincts and nature/nurture, all of these very exciting issues. It started to open up a new world. I'd never even heard of the field, psychology. We read all these sort of classic papers and it became fascinating. So I thought, it's a decent major and I could use this as my science requirement and get through school; all that's going to be very good. Of course, the next semester was sort of general psychology taught by a guy named Robert Brown who was the traditional kind. We used Boring's as the textbook, which was boring and it was a very narrow kind of thing. We studied everything discretely, you know, learning, vision, et cetera, all the way down. Somehow that got kind of tedious, but I said it can't be all like that because all the other stuff I had was so exciting. Then there was a chance to do some individual projects. I had been a subject in an experiment on learning a maze with electrical shock in a dark room and I never learned. I am a footnote in an article by Jack Bernard noting two subjects failed this thing. All my friends passed and I said am I so stupid. I was able to do a summer project, as a junior, on the relationship between IQ and maze learning. Although I used a finger maze instead, it was blindfold, and the correlation with my first attempt at statistics was .19. I left completely vindicated that in my scientific test there was not a relationship between the two. Then the next set of courses came up with people like Charlotte Buhler and Carl Buhler and they became very important figures in the way we saw the breadth of psychology and the many, many options. For a guy like me, I needed options. That was my primary objective way back when because I saw what the depression did. We didn't struggle but we were sort of a low middle class financial group; among Jews there's no such thing as a lower class. We were not truly lower class; it's just economics. In terms of our sense of our Jewishness we were not that at all because we had rabbis in our background and that stuff. So with all of these courses in psychology it became really very interesting. Then I did an honors thesis in studying interviewers and I developed a questionnaire; this was all in the honors thesis. And I got some rather interesting stuff because I was able to see the way interviewers differed and the way they interviewed depending on who the subjects were. This whole experience was a very real status one because I was able to have a little office. Clark was a school of 300 students. So by the time I graduated I'd had something like over 40 hours of psychology, written two theses so to speak. Rosensweig was the one I had the most trouble with because he claimed I was not conceptual enough to be a psychologist which was also another kind of issue.

Horowitz: Was Heinz Werner there?

Sigel: No, Heinz Werner came after the war. This was all between the time of '39 and '43. I'm not young anymore, but, anyway, they were very vibrant years and then I got into the military. There is no reason to go into all the details there, but eventually I got into what they called a personnel consultant which was equivalent to a psychologist. There was no category for psychologists in the regular Army; there was such in the Air Corps where a lot of these psychology majors went in, but I went in as a noncombatant. I was originally going to be a conscientious objector because I was very anti-war and all that stuff and felt the war was no solution to things and I was very active in the peace movement. But then once we began to see war was the only way to deal with the Nazis, and you cannot deal with these people through passivity, so I said, well, let me go in and operate in the medical corps. That didn't work out because I got sick. So I had to find a job and I ended up as a psychologist in the rehabilitation center for American prisoners with no less a mentor than Nelson Goodman, the famous symbolic logician, and he was in charge. He hired me, so to speak, as a psychological examiner. It was really crazy and I stayed in that role for the rest of the time because we were declared essential.

That was a very profound experience because here I was doing Wechsler-Bellevue IQ tests. I've probably given more Wechsler's than anybody; four a day for a couple of years to all kinds of men who came through this—Americans. I never knew there were illiterate Americans. They came from the hills of Pennsylvania, they were illiterate. And then the hillbillies that I met—these guys didn't know who they were or why they were and suddenly I thought, what is my IQ test telling me except these people just don't have any experience? How can a man harness mules and shoot a rifle and do all this stuff if he is dumb? And at the same time we had to teach these illiterate men, so we had a school to rehabilitate them and we taught them in three months from zero literacy to fourth grade in arithmetic and English, to read and write and to compute, plus a lot of other training things. This whole thing opened my eyes to what I learned in school in psychology; this doesn't make sense here, there is nothing sort of related. I've had all of these courses in exceptional children and IQ and vocational stuff and here I was; I didn't have to travel, I had that world right in front of me.

So then I decided that when I got out that I wanted to go back to graduate school, but where? I developed a bias against a narrow course of study that decontextualized human behavior; I wanted to see it in context. I had all kinds of conflicts about science. I mean, the thing is how do you scientifically study something that is so complicated and that is so influenced by so many sources? So I applied to a number of schools and I was accepted, except for one. I ended up at the University of Chicago. Meanwhile, after I got out of the Army, I became a probation officer for six months and I worked up here in Westchester county in the children's court, and there again I saw all of these problem and psychological reports which were useless. You know, this kid has this kind of an IQ and so on. Okay, but where do we go with that? I was asked to stay on because they thought I did a good job, and I said this is just patchwork. I can't do this, just going down and fighting with teachers. I wanted to get into a more basic psychological kind of activity. I went to Chicago and as I was sitting there to enroll I noticed that they had a human development program which I didn't know about—I knew they had a psych department. When I looked at the courses in anthropology, psychology, biology, all of these things—this was great. But the place was in chaos because they had never expected this influx of veterans. I mean, we came there in such huge numbers no one was prepared for us.

Horowitz: Now did they give you a fellowship; how did you pay for this?

Sigel: The GI bill was there, which was the greatest thing in the world, and that gave us 50 dollars a month, tuition free, and a certain stipend for books so there were no out-of-pocket expenses. But by this time I got into the courses and they were just fantastic. I was very impatient because I spent three years in that Army. I wanted to just get through there as fast as I could, which was a serious mistake. But I came in more prepared than 90 percent of my fellow graduate students because they couldn't believe I had all these courses: social psych, abnormal psychology, personalities, statistics, all that stuff.

Horowitz: You had taken those courses—

Sigel: At Clark I had over 40 hours as an undergraduate. Then all the Army experience. I knew all about testing. I met the great David Wechsler and had an argument with him because I was in a real world where these things were very important. So anyway I took this program and Helen Koch became my great mentor. I took a course with her. We had to read the *Manual of Child Psychology*, the 53rd version in total—an old book—and write papers. We had three courses, three papers, and tons of reading. To survive there, you could only party the first week of the quarter, then you disappeared for three months, then you emerged at Christmas time. I never worked so hard in my life. I wrote a paper, a review of Goldstein's *The Organism* which was kind of a holistic integrated view of the human. If you know everything there is to know about the relationship of parts about a person you know what you have to do. So I wrote this paper and Helen Koch was very impressed. However, on the exam she asked us to describe Myrtle McGraw's chapter on physical growth. That was a disaster. I thought, oh my God, I

flunked this course. Well, she paid no attention to that stuff. It was relatively superficial in certain places. There were a lot of theoretical concerns but there wasn't any kind of commitment to a theoretical point of view. You had to sort of construct your own and that meant that I read a lot of psychological theory, e.g. Spence, and I couldn't relate that to being human. And then I read a lot about the Gestalt stuff and fell in love with Lewin who became my big mentor. But I was an isolate because all the other people there were into personality theory and developing personality theory and heavily into Rogerian stuff. And they became really schools of thought: whether you were you a Rogerian, are you pro or against non-directive therapy? That was one of my big problems; I had a problem with Carl Rogers. When I mentioned something about some tape we had heard he was not very accepting of my criticism. He replied when I asked a question, "Well that and a nickel will get you a cup of coffee." As it was, well okay, I couldn't really deal with this. But then while there, after my first year, I was able to take my prelims and they were not a problem. So I just began to take courses that I wanted to. The courses that I had the most trouble with were in statistics because we had either Thurstone, who I thought I didn't want to deal with—his reputation for being anti-Semitic and so forth—and there was Holzinger, who I took a course in factor analysis from, and then there was Stevenson, who taught analysis of variance for the first time. You realize, at this time I read Fisher. Stevenson was the worst teacher in the world. He would write and then erase, he'd write and then erase, so a lot of this I just ended up learning on my own except with some help.

I also took a job because we just wanted a little bit more money. We didn't have any children at the time. Roberta was teaching at Indiana University which had an extension division in East Chicago and they wanted a psychologist. So I taught one course in elementary psych and I did academic counseling, and that's when I tried out a lot of Roger's stuff which was for the birds. I mean, one of these students would just fall asleep while we were saying nothing and we were going nowhere. I decided clinical is not for me. The thing is there are so many reasons why people are acting this way. So then I decided I was really interested in what was then called genetic psychology. It had nothing to do with genetics, but I had a course with Ward Halsted and I got very interested in the connection between the brain and behavior and personality. The details here again are unimportant historically except that he wouldn't let me use the test, the Halsted battery, that he invented. He wouldn't allow me to use it for my dissertation in spite of the fact that Helen Koch offered him the money to build it, which at that time was 500 dollars. He wouldn't let me do it.

Horowitz: Why?

Sigel: He felt, I think, that this instrument should be restricted to people working in the brain in his field and he was very jealous of it. I could never get a real rational reason, neither could Helen Koch. Ralph Reitan was his major student. Ralph is a contemporary who focused on neurological diagnosis, now the famous Reitan. He's done all that stuff now and made a big career for himself. But I was very convinced that there was a lot in the relationship between brain and behavior. In fact, now if I were to start all over again I'd go into neuroscience. That's where I think a lot of excitement is. However, I couldn't do that so I wanted to develop some way—I don't know how I got interested—oh, from reading Goldstein and Scheerer. They had this wonderful monograph. I got really interested in children because I was again looking for how things happen, where do they come from? So Helen Koch, who I picked as my advisor, was a tough lady and there are many anecdotes about her. She was one of these people who, if you're a student of hers, she wanted your bibliographies and she read them all so that there were no slip-ups when you reviewed the literature. She would tell you yes or no, and this [cognition] was all new to her.

Ben Bloom was another guy who never thought much about cognition. I don't know how I got into this except that it seemed to me that the intellect was kind of an important issue, but not IQ because I saw in the army that the IQ really was limited. So then I proceeded to try to do a

dissertation developing it from Goldstein's tests. I wrote him and I asked to use them. He said, "It's impossible. You can't do dissertations with this, you can't quantify it," et cetera, et cetera. So I said, "Well, if I can't do those things I'll do my own things." So I reviewed all the literature there was on children's development of abstract thinking—this is what we called it then. Words like *concept* and so on were very rarely used. The whole language was so different when I read all that stuff back to 1900. And what also made me really frustrated was the fact that none of my colleagues, none of my fellow students, were reading any of this stuff. They didn't even know what I was talking about. So as Halsted, who was on my committee and then got off it, said, I'll warn you about one thing: if you are going to do this kind of research prepare to be alone. I had absolutely no fellow students with whom I could share anything that I was doing. They were all studying personality and Rorschach's and all kinds of stuff and I wanted to study cognitive development. I didn't know the word then because that was a bad word and it was a bad word at SRCD.

I remember going to my first SRCD meeting in 1953 at Antioch and there were 125 people there and I said this is the place to be. But I'll tell you about the one in Iowa City where I got into a big argument with Spence and with some of your fellow students on this issue. So I was talking about abstract thinking and that made no sense to anyone. I did convince Bloom to do an individual studies course; he and I read for a year. We read all the problem solving literature, Dunker and Gestalt people, and all this at this time was new. I mean, it was not anything that anyone was interested in except IQ and heredity and environment. So then it got to be that I finally figured out a dissertation, which was to study children's sorting behavior but using it in different symbolic levels where they sort objects, pictures, words because it was still an obsession after all these years. I studied 60 children in three age groups and gave them all five tests, did all of this stuff of going to schools and the whole bit.

Horowitz: Did you have any help?

Sigel: No, nobody helping me. I got into big arguments with Bloom about the statistical analysis. He said, "You've got to make this an analysis of variance design," and I said, "No, it doesn't fit the rules because these are all discrete," and I went through all that stuff and he says, "No, you have to." So I went and got some expert advice and he says, "Irv, you're right." I went back to Bloom and he said, "No, you're wrong." I said, "If you are going to keep me from getting a degree over statistics I'll do it your way, but I had spent a whole summer doing it my way and I got exactly the same results." I felt much better about it because I really didn't make certain kinds of assumptions. But in those days everybody was using analysis of variance; it was new and, if you read the literature then, there were a number of people saying you can't do this. But it didn't matter, because psychologists continued to use those statistics.

So anyway, after working with Koch, who would rewrite everything I did, I got my dissertation done with these kids. Again, I had no one to share my excitement with. I was very impressed because I came up with some of the dominance of meaning construct. I published that paper and I showed the developmental trends, although they were cross-sectional between kids six and eleven. I can still remember it like it was yesterday. I did get it published in *Child Development* in '53. It really was '54, but those were the days when *Child Development* had problems with continuity. I gave that as a paper at Penn State and there were five people in the audience at 8:00 in the morning; I'll never forget it. The only person that spoke to me substantively about it was Lorraine Nadelman because she was doing something very similar. So I felt I was really doing something that nobody cared about and I thought it was so interesting and significant in terms of what I was talking about.

So then I got my first job at Smith College and went there and that was another experience because I had replaced Jackie Gibson who left to go to Cornell with her husband because they couldn't get along with the Israels—Elsa and Harold Israel—who were basically anti-Semitic.

Although his [Harold's] name was Israel, he was a Presbyterian and was always viewed as being Jewish. So there was a lot of tension there. And after two years I had some wonderful opportunities. I introduced a course in teaching the Wechsler and I wanted to incorporate observation and evaluations, the students would observe the behavior in tests; however that was a no. No, because I had met at 8:00 in the morning; you don't meet at 8:00. I couldn't teach child psychology; that was only for women and Jean Carl Cohen taught that. She came from Iowa and she taught child psychology and we had big arguments about Piaget in a child psychology course or not. That was a kind of lovely place to live and we were very happy there and our first child was born there, but there was no future with those people so I had my first offer.

I went down to Yale for a possible job. Seymour Sarason, who was at Clark when I was there, was another important person in all this. He was a graduate student and he used to have big arguments Charlotte Buhler. I was so impressed that he could argue with her. So anyway, I went to Yale and I was going to be in the Institute for Human Relations or something and they canceled that job. Then I got an offer from Michigan State and we went there and that looked like it was going to be a good place, but they had a strict nepotism law. By this time Roberta had a Ph.D. and there was no future for her; she could only be a research assistant. One day Boyd McCandless came through and I had dinner with him, and he was with his usual bottle, and he said, "Irv, there's a great job at Merrill Palmer." "Doing what?" "Well, they have a big grant to do some research." Meanwhile, I've had two publications at Michigan State and some interesting projects looking at kids and families because, by this time in looking at origins, I decided that the origins really are not the infant alone but the infant in his or her family. So I got into that with some nice pilot studies. And then Boyd came along and told me about this big project on parent/child relationships on influence techniques, which was written by Fritz Redl and Dave Wineman, to look at the relationship between parental control of children and what are the core antisepic things that parents do in terms of child management. So I went and had an interview there at Merrill Palmer. It was small and I guess I liked the kind of size where you had a lot of access to the points of power and so forth. And also, Roberta could have a part-time job in Detroit at Wayne State. So anyway, we moved there and that became a very exciting intellectual thing. I was in charge of the project so I hired people. One was Marty Hoffman, one was Abbey Dreyer, and another was Irv Torgoff, and there were some comments about all these Jews and so what.

Anyway, we went to work and we really worked. I was there for 17 years. In the course of this a lot of things in my own mind happened. Aside from publishing and presenting these materials, we had all kinds of contacts with Diana Baumrind. We had some very important interactions with Kathryn Wolf at Yale, who I think was one of the most acute observers of infants. Have you ever heard of her? I spent a week with her and she was like the smoking Buddha: she was always smoking. Anyway, we began to use her procedures and it was in the course of all of this detailed work where I did a lot of observations and I felt I really got a sense of what children are like in an active social environment. I never did very much laboratory research with children, but that changed. Anyway, as I proceeded in this whole experience—I'm not going to go through all of it—there were some very profound changes in my own head because opportunities came. One was Head Start. We were asked to be an evaluation project, and an important study here was where we did a Head Start assessment, replicating my categorization stuff with these kids, and found that they couldn't do certain things. These very poor kids didn't categorize pictures the same way they did objects. Well, with that discrepancy, why? I mean, they knew the names. And all of this stuff became really interesting. So I proceeded to set up a study which we did under Head Start auspices and found that after a year these kids did better. I began to wonder what was it in Head Start because there was nothing very directly geared at that time. Anyway, we worked through that and that began to show me that maybe the way to change things was to intervene, and then we began to set up some intervention projects. Dave Bearison became one of my students. He came from Penn State. Merrill Palmer had students come from undergraduate colleges and we

did some intervention studies and were able to discover that we could make changes. We then worked with schools and got teachers involved as experimenters, because my theory was that if the teacher did the experiment then she could carry that on when she gets back into business. Well, we had the usual struggles. The schools wouldn't accept our findings. We found that we really had intervention programs that worked, but we were told, well, we all do that anyway.

Anyhow, I carried on a lot of work in that area and set up these infancy conferences, of which you know about, for about ten years and this was the spinning off from Pauline Knapp's notion. We thought there should be a research conference around infancy and we did that for many years and, again, with the naiveté. Instead of doing it now, like the Minnesota Symposium books, we just added them to the *Merrill Palmer Quarterly*, of which I was asked to be the editor. And I said, "No, I don't want to spend my time with that," and gave it to Marty Hoffman and he really did a beautiful job for many years and now Carolyn Shantz is doing it. Then I also hired Carolyn Shantz and Hi Rodman when I became chairman of the research area and we had a very strong group, including John Watson. These were all people that were there; it was a very stimulating and exciting place. So we just carried on with that. Meanwhile, I began to just evolve in issues around cognitive style and that's when we had another conference with Jerry Kagan and Dick Bell at Merrill Palmer around parent/child relationships. I became increasingly intrigued with cognition. That conference is where Jerry Kagan's paper on the identification of early one came, but they refused to have the proceedings published.

Horowitz: Who refused?

Sigel: The whole group. We had three volumes of fascinating stuff and I wanted to edit them and publish them and they just said, no, we shouldn't publish them.

Horowitz: Why?

Sigel: They felt there were too many things in there they didn't want publicized and it was not worth the work. It was hard to sort of push that when they were all in such opposition to publishing. But that's one of the issues you get in the field of trying to do something that your colleagues are reluctant to have presented. There were three volumes, I carried them around; they were burnt by mistake. But there were very intriguing issues around a number of the basic issues that we're still dealing with. They were very valuable archival data.

Horowitz: I don't understand what the opposition was based on.

Sigel: The opposition was irrational. I believe there were a lot of turf issues for one. You see, if we published this—I wanted them published as the conversations they were. Well then, who's going to edit it, what would we let in it? Some things I don't want said. So we had among the group—I don't remember who they were—a lot of resistance to having this as an open dialog. Initially I said this is going to be like a work group and I didn't, foolishly—I was just quite young then—didn't think of saying these are going to be published and we are going to publish papers. This is one of the first of these kinds of conferences in developmental psychology. I mean, years ago. So they were never published.

So anyway moving along from Merrill Palmer where this whole cognitive style became of interest. That's when Jerry Kagan and I did a couple things together and then we went our separate ways because his view and mine differed, but, I mean, it was just a difference of interests. I did a number of papers on cognitive style of children and that became of some interest. Then I began to realize that cognitive style is always part of a whole affective kind of thing and by this time Merrill Palmer was changing. The director retired and I had an offer to go to Buffalo. Roberta and me were always heavily involved with Wayne State so we were really pretty well settled there, but Merrill Palmer was going nowhere in a basket and I was asked to go to Buffalo to head up the developmental area and I did. I went with a grant from

the Office of Economic Opportunity—this was in 1969—to set up a preschool to test the hypothesis I had developed which is the thing I’m still involved in. So I guess whatever that means is this whole distancing model of activating cognitive functioning in young children. This was part of a study we did, without my knowing it, in testing the Piagetian idea where we found we could enhance kids’ cognitive functioning ala conservation if this system were used. Now this was a very interesting issue. Harry Beilin said at the time that what we did was really not a good experiment because we didn’t control for each task. In other words, we didn’t have one group for just reversibility, another group with something else. My argument then was exactly one of the problems I have with experiments. In life children don’t compartmentalize. We don’t know in reality what the interaction is among these various engagements in activities because no kid learns just classification as a single attribute or just attribution, but they’re blended and we did it sequentially. And when we did it sequentially we found what was said could not be possible. That, and these are kids with IQs of 150; these kids became conservers. But what we also learned then was that these kids would know a lot, but not know a lot. They would be able to use fancy words, like, “These are mammals.” “But what’s a mammal?” “I don’t know.” That is, they had no real conceptual base. What they had was superficial, which was why IQ tests again became a problem for me because you’re given the right score but that does not indicate the children have any understanding. So the whole issue of understanding and functioning became very central kinds of things.

So when we went to Buffalo I set up this preschool program for two-year-olds and got the worst kids that we could recruit on the assumption that this is where the beginning of language and activity begins to emerge. This was a very interesting experience because it was severely criticized by the black community for taking only poor black kids and by the white liberals for making these groups segregated, and that is a whole history in itself. So anyway, we went through and set up this preschool program after much deliberation. And it was an extremely exciting experience because we were able to work with these twenty children. We were supposed to have it for one year and I said no, we can’t throw them out into the street. So we kept them for three years, and the changes in those children were remarkable. We spent a lot of time both on cognitive functioning and a sense of identity; who am I, a lot of this stuff. We developed a whole curriculum and these kids went on into public school. By that time I left Buffalo and went to ETS (the Educational Testing Service). The person who followed them up, unfortunately, died of breast cancer a few years later. But we found that these kids in second grade were still doing very well compared to what we found with the control children. However, I was turned down in having this published because I didn’t have the right controls. My sample was not a random sample. The end result was—which infuriated me because we had the sample. What do you mean a random sample? And secondly we had controls. How can I get controls here? I’m not going to take a bunch of kids like this from the street so we had kids in other programs.

And one of the biggest learnings I had here was, in one of my favorite papers, was when we were doing the evaluation. It was with a number of sub tests in the Binet—I mention this because I think it was important. What we then did was I told the teachers what we were finding and they said that’s not true, these kids know, they can count, they know prepositions and so forth. Okay, let’s do something, I said let’s sit around the table and we’ll play a game. Okay, Johnny take two crackers, give one to somebody else and one to somebody else. Do you have any left? No. How many do you need? Three. These are four year olds. Well we ran a number of little tests like that and then we did prepositions. We said okay, go over there and on top of the book case, on the second shelf, is a telephone. Under the telephone is something I want you to bring to me and he did it. We did this again and again. These kids couldn’t solve the problem, give me three blocks or do a number of things we had with those things. I got very excited about these findings. I wrote it up and I sent it to the *American Psychologist*. I got back a very nasty handwritten note, “This is so obvious that we all know this.” So I said, okay this is very novel, and so I sent it off to *Human Development* and Klaus Reigel published it. I’ve had dozens and dozens of requests for reprints. So by this time I’m really thinking,

what do you do in this business to try to break out of some of these constraints? So anyway, I just ended up doing book chapters, which are not the way to go. That was what I learned a long time ago and I still know it, but it's too late. Meanwhile we had been giving papers at SRCD every time, I mean always doing that and organizing symposia. Then I had a chance to go to ETS and they offered to set up a preschool program. It was an experimental program and here's where I see the issue of applied versus social.

Horowitz: How long were you at Buffalo?

Sigel: Four years. Roberta could get a job at Rutgers and I could get a job at ETS and we're moving east, which was a very important kind of thing. I was offered opportunities to be chairperson and so on and I decided when I saw the kinds of things—a lot of strife and disagreements on issues that were not the way to have a compatible group. So I went to ETS and there was Michael Lewis and Walt Emmerich and Sam Messick and Hy Witkin. In those days, the early days, it was an exciting opportunity. In spite of my feeling about ETS's policy on testing and race and so forth and some of their big heroes like Thorndike and Carl Brigham, who were in their day racists, and whatever else in question, I thought this would be a very interesting opportunity. They offered me a very nice situation working out of this preschool at the regular research building. I could have one of my research assistants from Buffalo come who was into the language bit, so we were able to create a group. And in the second year after that I started getting funding so I had funding all the time. Then the whole issue of applied and basic research came very much into play because my argument is what you do in a laboratory is important; there is no argument about that. However, whether or not this is a generalization that transcends context in environments has to be tested. So what we did is—in effect over my career I did exactly that—the first studies we did on kids' abstraction and cognitive style. All of those were experimental in the sense of not manipulated—they were just experiments. However, there were some manipulations in some of the intervention studies, but even they were controlled. I mean you would have three or four conditions and so forth. Now, will that work if I did it in the preschool? Well, at Buffalo it did work. Some of those things did work but that was with a very special population of kids. So then we just took the same model and developed it at ETS and it worked. That was the way that kind of thing got going.

And meanwhile, my interest in applied research started, by just chance, with the *Journal of Applied Developmental Psychology*. That's because I got very concerned about the lack of interest of people in our field in the real world. There was a lot of policy interest, but when I began to see what they talked about they were really kind of abstract intellectuals. Getting into the nitty-gritty of something is where you begin to see it. It's not in the university laboratory school, but it is in the community situation. Although at ETS we tried to make this more extensive in getting a diverse population, we couldn't do it. It just became too difficult for many people to come in for just one morning a week. The work we did at Merrill Palmer did involve a lot of working class families—Polish Americans—so the whole orientation to a diversity in variation became something of real interest. I thought the journal might serve that role.

Horowitz: What do you think is your major personal contribution to research?

Sigel: Personal contribution?

Horowitz: What are your personal research contributions?

Sigel: Well, I like to think that the stuff we've done in the last ten years around parent belief systems, opening up areas like attention to application, parenting, a lot of the stuff on parenting and parent-child relationships and focusing on families. Focusing on the whole idea of parent beliefs and looking at a much broader picture of where the developing child emerges.

Horowitz: Where would you say theory fits into all this?

Sigel: Well, theory is really quite important in that it's a mini theory that I've developed which is kind of eclectic. It's really bringing together Piaget, Werner, and me in a sense that I had a model which is a theoretical model which is based on some assumptions and is still developing. I think now it is expanding to include a lot of stuff on discourse and language and a sociocultural kind of orientation so it's really a true, I think in my mind, a really true interdisciplinary model in contrast to one that is narrowly defined as a learning model. It's a cognitive affective developmental model and it is a theoretical model in that sense. It's derivative, in the sense that it's derived from these other people rather than being an original self-generated theory.

Horowitz: You've done a lot of editing. Do you want to talk about the contribution to the field that comes from editing?

Sigel: Oh, I think that's very important in that an editor is in a very powerful position to the degree to which the editor enables certain things to emerge. Now when people write papers and the standards are so rigid that even interesting things are excluded I think there is a disservice to the field. But I think one of the dangers in our field for the future is the conflict between the "rigors" of science without investigating what science is for us. Now we use the physical model of science and everything has to fit that. We talk about controls, for example; someone submitted a paper to our journal and the reviewer says this person used a voluntary sample therefore it's biased. My question is when, in psychology, when do we not have a voluntary sample, every sample is voluntary? So the thing is this is not like you are controlling things in other areas. So I think the editor's job is important. Plus, I feel what I've done is creating edited books like the *Parent Belief System*. The series I've done on applied developmental all offer some contribution that allows people to bring things together, something that is not easy in journals. And what I object to is the disrespect for edited volumes. No editor is going to edit a volume that he thinks is sloppy or terrible. And I think that this kind of status competition, arrogance to me, is self-defeating.

One of the big concerns I have in our field is the development of the diversity, of the intolerance of different perspectives. If we are going to continue that we are in trouble. Now I don't know how to change that or if it is changing or if our students are changing, but I think this is what those of our generation have to really foster. If you look back and you can see, for example, I gave a paper on the role of fathers and mothers and one of Spence's students came up to me and thought this was nonsense; I mean, fathers don't really matter, what are you doing? Focus on the mothers. And what do you think they used to do at the Iowa Child Welfare Station? They had only mothers. Sears did all of his stuff with Eleanor Maccoby at Harvard with mothers and the fathers were only referred to as the mothers referred to them. There's a woman called Ruth Tasch who gave an additional paper with me then and she was also criticized for that. I thought, my God, what are they teaching these people here? I'm a father; I know that I must count for something.

Horowitz: Tell me about your experiences with SRCD.

Sigel: Well, they are mixed. I was involved early on where there was a lot of the crisis around Bill Martin and those issues. I thought in those days I'd never become part of the circle. I don't know why. I was nominated once for council and I didn't win. Now part of it was my own fault. Maybe I'm just a troublemaker because there were a lot of questions I had about methodologies and these issues, which were never really accepted even in giving symposia. You see, I was in the program constantly from the time we were there. I think I missed two SRCD meetings. But to me there was a conservatism in the orientation of the journal and, with certain exceptions, even the monographs. Some of them were more variable in permitting

things to happen. You and I had that to do about one of the monographs. I couldn't stand your position. There was an important statement that had to be made and it's been getting a lot of publicity. But the thing is these are not popular issues and I think this is one of the concerns I have. If the excitement that SRCD generates, if it also has a lot of good intellectual excitement in these programs where there's a lot of open discussion, such as in the early days when there were really intense discussions of differences of opinion and people didn't walk away from them and not talk to each other.

Horowitz: Oh, that's not so. I mean, I remember Boyd McCandless talking about how Florence Goodenough and some other people did not speak to each other over the IQ debates.

Sigel: That's true, maybe I'm romanticizing it, but there were—well, there was also a lot of dispute around Bill Martin and the journal where people didn't talk to each other, where people wouldn't talk to each other and political things happened.

Horowitz: I came to SRCD kind of after that whole issue.

Sigel: That was not pleasant; that was a very sad time because Bill worked awfully hard for that journal. What everyone might have thought and the way that he was sort of unceremoniously removed, it was very partisan because he was blamed for things. And I think it's too bad that it created a kind of divisiveness that wasn't necessary. Now I'm sure that's gone, but SRCD does have a kind of inner circle of, I think, a certain group that tends to self-perpetuate itself. I think maybe now it is changing because there are names I see there that look different, but it's time. The other thing I think is the issue that you mentioned on basic research and taking positions on things. What if we had tried to do during the Selma times is to say we offer ourselves as having some expertise for social good. We had knowledge to make it available for dealing with social issues and that created a big problem. I mean, Harriet Rheingold said this is not the position of scientific societies. Our job is not to engage in social behavior. That's nonsense. I remember my friend Irv Torgoff said this is what the psychologists did during the Nazi time. They just said that's not our job; so look what happened. We should offer this but we shouldn't advocate. We should talk about the effects of policy, but that means that you also need a much better data base. So therefore, research that deals with these kinds of evaluations is very respectable and should be in our journals. The idea that there is a division between research and application and so on is nonsense. I'm afraid as I read some of the reviews I get from some of our reviewers, I worry about the way they think of this. And people say, you know what they say for application, just like you did in the grant, this has relevance for teachers and for clinicians. I write down this has what relevance, how can they use this, is it usable, should it be tested? Say more than one paragraph. If it is difficult for people to do that who send their stuff into a journal of applied developmental, and these are now a lot of the young people, something isn't happening in the way these people are trained. Now, what am I to say? I'm not in the training position. I'm out there sitting in an ivory tower. But when you look at it historically and from what I see as a journal editor this becomes a very, very real issue and I think an ongoing one.

Horowitz: Does that sum up your hopes and fears for future of the field?

Sigel: One is, I think, if we could only look at this as an intellectual exercise in issues and not politicize it in terms of the good, the bad, but for what it does, what it can contribute, I think it's good. I think the good sign is that for the first time, the volume, one of the full volumes of the Mussen handbook is going to be on *Child Psychology and Practice*. Now I think that's a revolutionary kind of thing. And the difficulty we're having among the editors of that volume—Ann Renninger and I are editing it—is to get people to be able to think and address the practitioner. It is incredibly difficult. This doesn't seem to be something in the mind set and I think, as social scientists who live off the public, we owe it something and that something is

what our knowledge can do for the people who can use it. So it's got to be in a usable form. The guy that I am really very impressed with is political scientist Charles Linbloom. He's at Yale—or was at Yale, he's retired. He talks about this as a very real question of how social science can be utilized and it's not a simple issue but it's one we must address. Unless we do we are going to lose all kinds of face and all kinds of support and who is going to pay attention. We talk to ourselves. That's what I'm concerned about.

Horowitz: What about your personal interests, your family, how they effected your work?

Sigel: Well, I think I'll just tell you one very little anecdote. Years ago when Hoffman, Dreyer, Torgoff, and I were studying children I was the only parent. And what they have told me since is that if they only knew then what they know now about parenting it would have been a very different project. I think it is a constant reminder of the reality of the individual case versus the general rules.

One other anecdote and I'll stop on this. When Roberta was pregnant we were at Smith College. She thought, well, she could get a job with one of the professors correcting papers or whatever. I said oh yea, because here's what the sleep schedule is of infants, so you are going to have a large block of time in the afternoon. Well, our first born slept exactly one hour a day, thirty minutes in the morning, thirty minutes in the afternoon, okay. So there went Gesell and sleep schedules out the window, and this to me is the big issue in terms of how we study. If we could look from the general rules, the law so to speak, in the individual case and get some kind of reconciliation of how we think about it. So our methodology has to be multiple, one narrative type thing in individual type cases as they relate to the general principles.

Horowitz: I think it's interesting now that we should have this conversation in terms of the themes that have run through our two recollections. One of the themes that is so impressive to me is the role mentors have played in our lives. It's interesting; we cross paths over Boyd McCandless who really did not leave an imperial body of work behind him, I think much to his great disappointment, but look what he's left behind him in terms of the people that he influenced and their lives.

Sigel: If it weren't for his giving a colloquium at Michigan State I never would have gotten to Merrill Palmer. We would have stayed there, but we had to go because of Roberta, but it was McCandless who identified the possibility. Helen Koch had a tremendous impact on the way I think and teach; she's a real Socratic teacher. I think mentors are really important and I can see that in the students I've had. Boyd McCandless, by the way, said to me, "You made a mistake going to ETS, you should have never left the university."

Horowitz: Well, he was very partial to the universities and I think he really believed, partly because he was such a superb teacher, that he really believed that when you were in a university and you could teach that you sort of multiplied your influence.

Sigel: He was right and I was a good teacher. I mean, I did very well with my students, had some innovative courses and I miss that. That was a tradeoff and it was moving always toward where I could have less distraction, although I find myself distracting myself by getting involved in a lot of other things. It's difficult just to teach and do research if you have any kind of social emphasis. But it's interesting that actually we were sort of generationally different in some ways because some of the issues that were no problem for you were real issues in the early times, like methodology and statistics and the notion of developmental psychology. There was no developmental psychology when I joined APA; it was called child psychology and it changed later with John Anderson and Dale Harris. The course I taught at Michigan State was genetic psychology.

Horowitz: But you know, methodology was an issue, but because I was at Iowa with such a very narrow point of view it really didn't come up as an issue.

Sigel: That's right.

Horowitz: I mean it was an issue maybe all around me, but at Iowa it wasn't and then in Oregon I was by myself. But when I went to Kansas that was at the point at which, shortly after that, Hull-Spence Theory died as a theory. And you had Piaget coming up in developmental and it was really the cognitive versus Skinnerians. And I was very comfortable with the Skinnerians and I was very comfortable with the cognitive so I personally never got into the methodology issue. Though the reason I was able to recruit Don Baer and Todd Risley and Jim Sherman and Barbara Etzel and Mont Wolf to Kansas from Washington was because there was such a big fight at the University of Washington over single subject design. And they couldn't get their students through and students were being used as kind of pawns in this argument. So I said come to Kansas, nobody's going to fight with you and we can live together. And so, in a sense, Kansas and what developed there benefited from this miserable fight at the University of Washington because they all wanted to get out of there and kind of create a department by themselves.

Sigel: This is one thing that I didn't mention in terms of Piaget's influence. I was introduced to Piaget at Chicago by Lloyd Warner and then I read everything at that time that Piaget wrote. In fact, Wayne Dennis asked me to write a book like John Flavell did, and I said I don't know French; I can't do it. But I did have a review of *Play Dreams and Imitation* in *Psych Bulletin* in 1950-something. It was such an arrogant review; I mean, "if he did this methodologically", what possessed me to say that? But Piaget has been a very real influence. I really didn't spend much time in this about my own conceptual development which has gone sort of dramatically from the Goldstein sort of holistic view to the interdependence—you see Chicago human development has been very interdisciplinary so I had all of these courses and to this day I find that that perspective is still not an easy one to communicate. This is a problem I've had with a lot of things in Division 7 and in SRCD, that a true interdisciplinary view requires a different kind of thinking in terms of the true interdependence.

Horowitz: You know it's interesting because SRCD, the Society for Research in Child Development, was founded as an interdisciplinary society and yet year after year, except for the early days, it has been dominated by psychologists. But now I think that there is a chance that that is going to change. I mean, partly because SRCD has gone to this alternation of picking somebody to be the president who is not a psychologist, every other election, but also because I think the sophistication of the fields that have to work together have to be on a par. You know, it is not possible for child development to work with molecular biologists because molecular biologists are not only working at a different level, but because they are working at a much higher level of the sophistication of their field and the extensive knowledge base of the field. So I think, as in some other fields, as we become more sophisticated—which is why you can now have child developmentalists who can really work with neuroscientists. Twenty years ago that would have been impossible; child development wasn't really even ready to work with people in that area.

Sigel: But I think there had to be something in the mindset that it's possible. You see, I think we began to develop a lot of, well, they don't have much to say but we really didn't begin to examine this. For example, in a course in psychology when I was an undergraduate the first chapter was the brain and all that; yet you never connected it to anything. It got to the point where you began to believe that there is not a connection. So you ask the question how can you learn without a brain? Well we don't know that, but that was what Ward Halsted really taught me. He showed me how this brain works; now that was a long time ago. Larry Frank, who was one of the old timers, he used to come to Merrill Palmer and he would talk about this interdisciplinary thing and the frustration that he had with getting people to think like that but

he was always in sort of cloud nine. But it's very difficult to get people to look at a kid and be able to see how can you look at this child in an integrated way, even within a family. So we have to sort of keep on. We're struggling with that, but our methodology is not sufficiently sophisticated for us to respectfully get that thing to happen.

Horowitz: But it is also because the mode of theory in our field is to throw out all the data associated with the old theory that you are now rejecting and like start anew. Hull-Spence Theory died but the laws, the basic behavior relationships that came out of behaviorism were also kind of dismissed. I mean, my major argument—that's the reason I tried to do that integrated theoretical analysis—my major argument was that the cognitivists and the Piagetians and so on, they wiped the slate clean in terms of the laws of learning like they didn't exist, like reinforcement wasn't a variable in the system. It seems to me that has retarded the field.

Sigel: Well, you see it's because we don't have a historical building up as part of the rules of the game. The rules of the game are forget history. I mean, you don't remember Hull, you don't remember Bijou. Those people are dead. So where do you go? You go with whoever the contemporary is, and this to me is the true danger of our field. For example, I'm trying to put together a book on perspective on representational thinking. Now if you can imagine that there are many people who talk about this in varying degrees. Some talk about it in terms of emotion, affect, self, others something else. Here we use the same words, yet we don't spend time clarifying what the meaning is. And then the other thing I've looked at is the relationship between language and thought. We say it is there, but where can you find data that will tell you what the effect is of parent language on children's thinking? The language people are over here; the psychologists who deal with language and thought go from some problem of middle class kids, solve certain kinds of language problems, then go to lower class kids, and the language environment of lower class kids is by definition better, therefore—that's not true. There's tremendous variation in middle class homes and we have it. I'm looking at those kinds of discourse patterns and I don't find them. I've talked to Katherine Nelson; I've talked to Catherine Snow. I look at the work of Lois Bloom. They tell me there isn't anything and yet we continue in our business to talk about the significance of language to thought. Ellin Scholnick is one of the few people who've done a study of the if-then or the conditionals, and the learning that you are talking about is that those learning principles are there. I'm making the argument which is also that what happens, what a child gets is not the belief system but it's worked through the verbal interactions of what the parent says. So this is, in a way, a reinforcement model. So if parents use questions, open-ended questions, we talk about that as linguistic learning. I don't have to use words like habituation or even reinforcement, but identifying that it's the direct learning, it's not making the inference, and our data show this. The beliefs of a parent, the conduit, are imparted through what the parent says but there is a lot of automatic learning, there's a lot of—in a way that you get these response patterns which are non-reflective; these are all there so it's how to get them all together and that's the tough part. But to me that's the excitement.

Horowitz: But you know there is very little integrative theoretical work being done.

Sigel: Oh, I know that.

Horowitz: If my interests have gone anywhere in this field it has been to how to do more theoretically integrated work. To take from the various theories that have a data base behind them. I agree with you in a sense, it's like people look at the color spectrum and they say I'm going to study green and the fact that it shades over in one side and shades over into the other and is involved in a context, and then someone else comes along and says no, no, green is not the right focus; let's study blue. And yet everything you've learned about from the first analysis you don't take with you and say how does that inform the next area.

Sigel: You see, that's what's happening. Piaget is out, the Vigotsky is in. That's nonsense. Freud is out. I don't know who's in in his place, but what I mean the outs—if you look at the history of psychology there are many places where interesting things have been done that don't get built in. Now to me the real puzzle, rather, is why some things drop away. I mean, why is Vigotsky suddenly the answer to everything and so now we're going to have Vigotsky and anti-Piaget. It's killing the father. I'm finishing a paper trying to bring together Piaget, Werner, Vigotsky, and me. It's really arrogant, but on the other hand these are the sources from which valuable things come about. I mean, actually, the stage notions of Piaget have something to say if you think of them in terms of developmentally not tied to chronological ages of the child. If you take development from Werner's perspective where it is a process of individuation, reconstruction, integration, this goes on forever. I mean why does development suddenly stop when you are 21?

Horowitz: I think it comes back to the lifespan.

Sigel: It is a lifespan.

Horowitz: You know lifespan theory is nowhere.

Sigel: Because I think that the thing is it gets imbedded in itself. I mean, it becomes the Baltes way of thinking or whatever; it's too narrow in its perspective. It takes a lot more to think truly lifespan the way you are talking. There is where you get into interdisciplinary stuff because now you are concerned with social events, the social scene and how the social scene changes. I mean, we are living in a different world than we did in 1950.

Horowitz: We are living in a world of enormous personal violence all around us.

Sigel: And complexity. Okay, I think we are concluding.

[End of interview]