

Alberta Engvall Siegel

- Born 2/24/1931 in Pasadena, California; deceased 11/3/2001
- B.A. (1951), M.A. (1954), and Ph.D. (1955) all in Psychology from Stanford University

Major Employment:

- Stanford University, Professor of Psychology in Psychiatry, 1969-
- Stanford University School of Medicine, Faculty Member in Department of Psychiatry and Behavioral Sciences, 1963-

Major Areas of Work:

- Aggression, social policy as it relates to child development

SRCD Affiliation:

- Editor of *Child Development* (1964-68)

**SRCD ORAL HISTORY INTERVIEW****Alberta Siegel**

Interviewed by Lloyd Borstelmann
At Stanford University
September 15, 1992

Borstelmann: Let's start by my asking you something about your background in terms of how it might be related to your going into child development.

Siegel: Well I grew up in Pasadena, in southern California. Pasadena, at that time, was a center of progressive education. It had been influenced by people at the University of Chicago. I can't say who. And one of the signs that they were into progressive education, is that they had what they called a six-four-four-plan. And that meant six years in elementary school, four years in junior high and then four years in junior college, so that, the community was supporting fourteen years of public education for the residents. And the significance for me was that, when I got into the eleventh grade I enrolled in a junior college, a four year junior college which was an old institution that had closed during World War II and then reopened in 1946, which was the year I went into the eleventh grade. And it was very exciting, not only because it was a new institution with a new staff and a new student body, but also because the GI's were flooding into the schools that year. And so our junior college had a mature student body and it had a very youthful faculty. Many of the people who taught at the junior college were themselves graduate students at UCLA or Claremont and were supporting themselves and their families by teaching. So that when I was a junior and senior in high school, I was able to take oh, a year and a half of psychology from high level psychologists. So that by the time I graduated from high school I really wanted to be a psychologist, because I had liked these classes so much. The important one was taught by a man named Ross, Robert Ross, who had previously taught at Stanford. And who gave a very fine course in the classical topics of psychology, perception, learning, sensation, and comparative psychology. It wasn't one of these watered down high school courses; it was a serious psychology course. It compared very favorably to the one I took at Stanford as a freshman, very favorably. So I knew I wanted to be a psychologist when I entered college. And before I came to college I had another experience that helped to point my direction, and that is I was sent to something in Sacramento that was called "California Girls State". And this was an assembly for a week of girls who came from throughout the state and organized a mock government, with a mock legislature and so forth. And I was elected the Governor of Girls State. So during my senior year, what would have been my senior year in high school, if we had such things, I was invited to come to Sacramento as the Governor of Girls State, to speak to a conference of people that was planning the 1950 White House Conference on children and youth. I didn't of course know anything about White House Conferences on children and youth. By the time I got old enough to go to one, why they had been discontinued. So this was my connection to the White House

Conferences. But there were four of us, the Governor of Boys State, and the Governor of Girls State and then, I think the head of the California Junior Red Cross, one male and one female. I think we were the speakers at some luncheon in some hotel. But it was very exciting; Earl Warren, the Governor, was there and spoke. The Governor came over and shook hands with us and so forth. And I suppose the people who were there were the leaders of voluntary agencies and social agencies and stuff. So anyway, I gave a very idealistic speech. And afterwards a handsome woman came up to speak to me with her husband. And said that she thought that was a very fine speech and she wondered if I might give her a copy of it. And she said that she was a child psychologist, and this was Lois Stolz, whom I'd never heard of, but became a great life long friend of mine, and so the way she would tell this story, I responded by saying, "Oh, well I plan to be a psychologist or I plan to be a child psychologist." And then she said that she taught at Stanford, and I said, "Oh, well I plan to go to Stanford." And I don't think I was quite that persnickety, but that's the way she tells the story. Anyway, so I told her that I hadn't written out the speech, I had just extemporaneously, but I'd be happy to write it out and send it to her, which I did. And she gave me her card. So I sent her this speech and we had a little correspondence. And then I settled down and got my application into Stanford, and was very promptly accepted, maybe you know, just like a week later. And neither of my parents had gone to college so they didn't know that there was anything unusual about that. And I had very good grades and all that. And then I started to realize that my friends who were applying to Stanford had not gotten their acceptances. And so I didn't say much about it. They hadn't heard and they went through the usual channels. But what I learned later was that Doctor Stolz had come back to Stanford and spoken to the chairman of the psychology, Doctor Ernest Hilgard and said that she had met this young women in Sacramento, and that I had expressed an interest in Stanford, and that she thought I should be encouraged. So he had then sent a note over to the admission's office and the head of admissions at that time was an historian named Rick Schneider, who has since become a great friend of mine. And so Rick just went ahead and admitted me when the application arrived.

Then when I got to Stanford, intending to be a psychologist, I took the regular courses. And eventually in my third year, I went through my undergraduate program in three years. I took a class from Dr. Stolz. Meanwhile my mother had been saying to me, at regular intervals, "Have you ever gone in to see that nice lady who came up to you in Sacramento?" And I said, "Oh, I've walked by her office a number of times, but she's always so busy and she seems to be so important, and I don't really have anything to say to her." So I enrolled in her class in adolescent development. And in order to get into that class I'd had to take a child development class from Frances Orr and of course general psychology and statistics and there were several prerequisites. So I was a senior before I took the class. And at the end of the first lecture as I was walking out, she stopped me and said, "Have I met you before?" And I blushed, and I said, "Oh, yes Dr. Stolz I met you once in Sacramento," and she said, "Well you're not Alberta Engvall are you?" And I said, "Well yes I am." And she said, "Well did you come to Stanford?", and I said "Oh, yes I've been here for three years." So she said, "Well you better come to my office right now." Then I had told her that I had walked by her office many times but, that she'd always seemed so busy. And she said, "Well I always wondered what happened to you." And she liked students very much, and spent a lot of time with them. And I hadn't really known any professor at Stanford who was that way, particularly, and so I had just kind of assumed that unless I had business with a professor. I wasn't meant to darken their door. So then we got acquainted with each other, and at that time. I thought I'd be an anthropologist; I had kind of changed my interest. And there was a field at that time, that was quite prominent called cultural personality that was a lot of fun to read, it had developmental aspect to it. But it was more interesting to an undergraduate than child psychology. And you read Ruth Benedict, and Margaret Meade and all these people. And they just seemed to have done such wonderful things. And national character was being talked about and I thought that was good too. So anyway, I went to speak to Dr. Keesing, who was the chairman of the Anthropology Department, and asked him if I could do graduate work in anthropology. And he discouraged me. He said that there were these few very well known women anthropologists, but that in general anthropology was a very difficult field for women. And I now think that, that was correct. I think he gave me good advice. I told that story to his son, who is also an anthropologist, and his son was just mortified, to hear that his father had deflected me from...

Borstelmann: He was such a chauvinist.

Siegel: Yes a sexist chauvinist. But at the time, I think it was right. Yes indeed.

My husband had a brother who was an anthropologist and we got to know a lot of anthropologists in subsequent years and they all have chronic diseases. You know. That they've picked up in the backcountry. And it's not a, it was not a good field for women. Anyway I think he gave me good advice. And I talked to Dr. Stolz about it

and she said she thought I should become a child psychologist. And that was a hospitable field for women. That she was then, I suppose, in her 60's, and she said all of her major professors in child psychology had been women, back in the 1920's, at Columbia. And so that's how I ended up in child psychology.

Borstelmann: And then she told you that you'd better come to Stanford and work with her, right?

Siegel: That's right, that's right. And so that's the way it worked out.

Borstelmann: Just like that. All right, well then let's talk a little bit about your graduate years and what was most important about that time to you. And how did you get into, what was your dissertation research. And how did you get into that, and so forth?

Siegel: OK. Stanford in 1951, when I entered graduate school, was a good psychology department. The whole university had kind of run down during the war, and I suppose during the Depression as well. And it was starting to build up again. And we had very fine graduate students and we had a very fine faculty. Dr. Stolz was teaching child psychology, Dr. Paul Farnsworth was teaching the history of psychology, Quinn McNamar was teaching statistics and research methods, Edith Dowley was running the nursery school, which was out at Stanford Village [now SRI International].

Dr. Hilgard was teaching systems and also learning. And I had taken psych I from him in 1948. He was graduate dean for a while and may have been out of the department a little during my years. Don Taylor was still at Stanford, he subsequently went to Yale. Doug Lawrence was teaching learning perception. And then we had some marvelous visitors Alex -- visited here for a while. Frances Orr was teaching child psychology, Maude James was still active at that time, and she taught mental testing and ran the clinic, Lee Winder, was active in clinical, Kenneth Little was at Stanford on the faculty in clinical. I'm sure I'm forgetting some people. But anyway it was a very fine group of people. Dr. Louis Terman was still around and still coming to his office, but he was retired, and so one would see him. One would see Melita Oden, but they weren't dealing with students as far as I know. And all of this was taking place in Cubberley Hall, which the psychology department shared with the school of education. And of course the school of education had its own group of psychologists, including Art Coladaucci, who later became the Dean, as well as some very good other people.

Borstelmann: Stanford was apparently unusual in that respect, in that the people in education tended to be specialists from various areas like psychology. Am I right?

Siegel: Yes. And I think that's been even truer in subsequent years. I thought you were going to say unusual in the close relations between psychology and education which I think was also true. Cubberley who was the great figure in education was a good friend of Terman. And I believe Terman had his appointment initially in education. Terman took his Ph.D. with G. Stanley Hall and then went down to Southern California for his health and was at the L.A. Normal School.

Borstelmann: L.A. Normal School.

Siegel: That's right. And then he got the call.

Borstelmann: A precursor of UCLA.

Siegel: Oh was it?

Borstelmann: Yes indeed.

Siegel: Oh, I didn't know that. Well then he got quote "The Call" to come to Stanford. And when he was, after he published his mental test in 1916, I'm now quoting from what I've read about him in recent years, he was asked to go into the psychology department. And he was asked to get active nationally in psychology. And he said, "Well I'm not a psychologist," and gradually moved into psychology in subsequent years. But he really thought of himself more as an educator.

Borstelmann: What about your peers, your fellow graduate students in those days? They were people who became of some imminence subsequently, that's Stanford's track record.

Siegel: Well we had some very good classmates. Of course the most important to me was Sidney Siegel, who had entered graduate school at the same time I did, but was fifteen years older than I was.

Borstelmann: So that's where you met.

Siegel: That's where we met. And he had been a high school teacher and he had been an engineer, and so statistics was easy for him. And statistics was difficult for the rest of us, most of us. And so everyday....

Borstelmann: Particularly from Quinn McNamar.

Siegel: Particularly from Quinn McNamar. So every day McNamar would teach the graduate students from eight to nine in the morning at Cubberley Hall. And then from nine to ten-thirty we would all have coffee with Sid Siegel and he would teach us what McNamar had just taught us. And then some of us would have review sessions after that.

Robert Sears came to Stanford while I was a graduate student. So then I started working with him as well as with Lois. And I was thinking about that today on the way over. And I was thinking how effortless that transition had seemed, between Lois Stolz running the child development program and then Bob coming, and how they seemed to have very good relations with each other. And the reason I keep saying seemed, as far as I know they did, and if they didn't, I certainly never heard about it. But Bob came in psychology and his wife Pauline S. Sears came in the School of Education. And she had a lot of very fine students.

Borstelmann: Well all right. Now about your research as a grad student, your dissertation, what was it and how did you get into that?

Siegel: You know Bob was very interested in aggression.

Borstelmann: Yes he was.

Siegel: And got me interested in aggression. And Lois was very interested in child development and social policy. She had been one of the founders of the SRCD, and like so many of the founders then, they saw child development as linking the sciences to social policy for children.

Borstelmann: So this is the roots of your own involvement in child development and social policy.

Siegel: So when I said I wanted to do a dissertation on the effects of television on children, well that sounded good to Lois, because it was an important social issue, and it sounded good to Bob because it was a study on aggression.

Borstelmann: These are early TV days, as far as general, general exposure to TV.

Siegel: That's right. And basically there was no literature. The literature was the old 1930's literature, on the effects of movies on children. And there was some literature on the effects of radio on children. But there was no literature on the effects of television. In fact...

Borstelmann: Well television was so young at that point.

Siegel: Exactly. That's exactly right. And our field was so small, that we didn't have people just running around looking for every possible topic that anybody could think of. But I was talking to Albert Bandura last night at a party, and he was saying that when he did his studies of television which were published in 1961, let's say he did them in '59 or '58, he did not have a television monitor. The child was brought into a room and shown a screen and then behind the screen there was a movie projector and the movie projector was projecting onto the screen. And the child was told this is television. But that was better than mine, I didn't even have that. I just showed children movies. And the title of my paper was, "The Effect of Violence" I forget.... Violence on Television," I think. It wasn't, I'd

have to look it up what the title was, but it wasn't television, it was movies that I was showing. And I mean, we didn't have it sorted out in our heads as to...

Borstelmann: Most intellectuals didn't even have sets at that time. You know.

Siegel: And this is always cited as, you know, the first study, or one of the first studies of the effects of television. And I did it at Penn State in 1954, '55. By then I had married Sid, and he had gone on the faculty at Penn State and I went back there as a Pre-Doc. Ray Carpenter, who was the head of the psychology department gave me a fellowship. He had a, oh what was it called, he had a media research center, I can't remember, instructional something, anyway he gave me a fellowship. And I used that year to do my dissertation. And I would take two children from the nursery school at Penn State and walk them over to the psychology clinic, which had given me a one-way vision room. The nursery school at Penn State, at that time, was not very research oriented. And they did not have one-way reviewing research facilities. So I would walk the children to the psychology clinic. And I had a friend there working with me named Ellen Tessman who was a graduate student. And the children would come in and sit down, and watch these films. Which were either aggressive content or neutral content. And then Ellen and I would observe their play from behind the one-way screen. And she was the key observer; because she didn't know which film they had seen.

Borstelmann: Condition, yeah.

Siegel: And I kind of backed her up because I did know. And then afterwards I would retrieve the children and take them back. And then the same children would come the following week and see the other film, in a counter-balance design. Bob Bernreuter was the head of the psychology clinic, and one day I came schlepping in with these two little children. And first I take off my coat, and my scarf and my fur hat and my fur boots, and then I took off the children's mufflers, and their boots and their jackets and all that. And Bob Bernreuter was standing there with a visitor and he said, "Now, if you want to see an example of real dedication, you just have to watch Mrs. Siegel. Every day she comes in here and she has these two little children and they are all bundled up," and so forth and so on. And he went through this whole account of what I was doing. I didn't even know that he knew who I was. But it was true, it was very inconvenient. And the other thing was, the film, the so called aggressive film, had to be approved by the chairman of the child development department, who ran the nursery school. And she wasn't going to approve any film that was very aggressive. Because she already knew that aggressive films weren't good for children, you see. And so it was what you might call, kind of a weak treatment, it was a Woody Woodpecker. And at the time I remember, --- and I went to New York, and we were trying to find really aggressive materials. And we were invited to the office of the censor, the New York State censor. And he showed us this stuff that he had clipped out of movies before they could be shown in New York. Just stuff that was just on the floor in the editing room. There was no way you would have ever shown that stuff to children. He was really taking some bad stuff out of films at that time. And of course he has since, his function has since, vanished at the state level. And I wouldn't have ever proposed to show some of that stuff to children, but I might have shown something a little more aggressive ... you know what children see in prime time television now is so much worse than what I was allowed to show them. In any event, that was the way I got my dissertation done. And it was done, so called, in abstentia. And the people at Stanford were very good about reading my manuscript and getting it back to me. And you have this idea that if a student leaves, they'll never finish their degree. And of course, I was determined to finish, and I did. I actually, I think I was twenty-three-years old, when I finished my Ph.D. I'm not sure, no twenty-four. I was born in 1931 and this was 1955. So anyway they were determined to get me done, and I was determined to get done. So I did this study which was basically inconclusive. And then I got a fellowship from the American Association of University Women, the following year. And did another study in which I did get some findings, about quote "the effects of television on children," only this time it was real.

Borstelmann: It was real?

Siegel: So my two studies on the effects of television on children are, one a study of films and two a study of real TV.

Borstelmann: But you got some results to the affect that?

Siegel: Well the particular study that I did that time had to do with the portrayal of occupations in the media. And I had the notion that if an occupation were something that was familiar to a child, his expectations of people in that occupation would be based on his own familiarity. But if an occupation was something that the child had never had any exposure to, and then you asked him, you know, what are people like that do that kind of work? What he would answer, would be what he had learned from the media. And so I wrote a script about a taxi driver because we didn't have taxi drivers in our little town [State College, PA]. And then these radio dramas were played in school for first or second graders. Gosh I haven't looked at this study for so long I'm not sure I can tell you, but I think I had an aggressive version and a neutral version. And then the children were asked about what are people like in different occupations. And one of the occupations in there was taxi drivers. And it turned out that the children who had heard the aggressive versions, on these radio shows, thought that taxi drivers were aggressive. And the people who had heard the other version didn't. That's my memory of that study, but anyway I published that one. And that's the total work that I ever did on television and social behavior.

Borstelmann: Well it was, well it relates to your roots here with Lois, of child development and social policy, and its implications for that.

Siegel: That's right. And then, of course, Al Bandura did the work that really opened up the field a couple of years later. His papers were published in 1961. And mine came out in, I don't know, '56 and '57 or something like that. So he did the real work.

Well you know we were talking about children doing darling things, when I was at Stanford I did my masters degree with Lois and she had a big study going, The fathers relations with new born children, in which she compared children whose fathers had not gone away to the war, and they were mostly men who were in defense research at Stanford, she compared those children with children whose father had been in the service. And she'd gotten interested in that topic because when she taught child psychology at Stanford in 1946, where these GI's were flooding into her class, and talking about, would then come to her office and talk about their problems with their children. But anyway I remember one child who had not been separated, but her father was in defense work. And I remember watching her through the one-way vision screen out at the old Stanford Village Nursery School, which was probably where your child was enrolled. And it was a doll play situation and this child arranged the child play so that there was the Daddy and the Mom and there were the two little girls, and then she started the play by saying Daddy has to go to Washington, and then she would go FrummmmmFrummmmm. And Daddy would fly off to Washington. And then Mommy and the two girls would cook and sew and do the laundry and do all this home stuff and the girls would go off to nursery school. And then at the very end of the play, when the person who was collecting the data said, you know, it's time to stop, then Daddy would come back from Washington, you know, FrummmmmFrummmmmFrummmmm. And I often wanted to tell, I know that family, and I've often wanted to tell them that story, because it's so darling.

Borstelmann: Oh yes. Right. So then you had the extra year to do research there and you did this second study, but then....

Siegel: Then I went into the faculty at Penn State. I was an Assistant Professor in the Department of Child Development. And I did that for a couple of years, mostly teaching graduate students. That had been predominantly an undergraduate program and I think the Dean and the Chairman of the department saw me as somebody who could help build up the graduate program. And then Sid was invited out to the center. So

Borstelmann: To the Center for the Study of the Behavioral Sciences at Stanford.

Siegel: Which had started up actually just as we were leaving, I think it opened in 1954, just as we left. And Bob Sears had been quite active in getting that started. He had helped facilitate the location of the center at Stanford. Ralph Tyler was getting the center going and inviting the people, but he didn't have a place to locate it. And Sears suggested that he locate here at Stanford. So Sid was a fellow at the center in '57-'58. And I came out and taught at Stanford, replacing Lois, who had retired that year. At that time Stanford had a rule that you retired at age sixty-five. So Lois retired, and she remained active for a number of years, teaching freshman seminars, and doing research. But she didn't do the primary teaching in child --- and they hadn't yet found her replacement, which was Eleanor Maccoby. And so for that intervening year, I did the teaching, I was the acting assistant professor. And I remember

one of the wives of one of the men up at the center, when she was introduced to me said. "Oh, I know about you, you're the lucky one, she said, you're the only wife at the center who has a job."

Borstelmann: Oh isn't that interesting.

Siegel: And the rest of us are all just stuck here for this year, was the implication, twiddling our thumbs. But you're the lucky one. Well I didn't think I was so lucky.

Borstelmann: Yeah you were busy working.

Siegel: I was overworked and underpaid. But I enjoyed teaching at Stanford. And Lois had a student named, Lynette Cofer. And Lynette, Lois kind of turned Lynette over to me and Lynette and I did a study together that year out at the Stanford Village Nursery School.

Borstelmann: Uh huh. I didn't know that connection.

Siegel: In which we followed up on some of the things I had found in my dissertation and had never had a chance to follow-up on. Mainly that there are differences between the first time you watch children's aggressive behavior and the second time you watch it, regardless of whether you've shown them one kind of film or another kind of a film. They're just sequence effects. Which I had counter-balanced for those in my dissertation but they were there. And I'd counter-balanced in the design, and therefore when they were there, I knew they were there. And so we did a study, in which we looked at that and we also looked at children's aggressive play in the presence of an adult and in the absence of an adult. And found that children behave more aggressively when an adult is present, to each other, more aggressively to each other, than when an adult is absent. And I had some explanation that had to do with; I think I used the word super-ego.

Borstelmann: Super-ego.

Siegel: It isn't a word I would use any more. But my idea was that if an adult is present, the adult is playing the super-ego function. Where if an adult is absent

Borstelmann: Control function, yeah.

Siegel: The children have to fall back on their own, understanding of what's allowable. So their more aggressive when permissive adults are present. So that was Lynette's first paper, and was one of my early papers. And then of course, Lunette went on to study the effects of television, and she really did have television by then. And she took her Ph.D. at Cornell, with Alfred Baldwin. And then went on the faculty at Penn State.

Borstelmann: So after your year out here [Stanford], you went back to Penn State?

Siegel: That's right- for another three years.

Borstelmann: Now when did Sidney Siegel die?

Siegel: When we came back to the center

Borstelmann: Another time?

Siegel: Another time. And he died after we had gotten here for that second visit, in November of 1961. He died at the center of a heart attack. So I just stayed on at Stanford. We had become friends with David Hamburg, who was also a fellow at the center at that time. And he had been asked, in 1957-58, while he was at the center, to become the chairman of the psychiatry department at Stanford. But he had an obligation to go to the National Institute of Mental Health, in the intramural program, which he honored. And he went to the NIMH and ran that intramural program for three years. Meanwhile Stanford horsed around and horsed around and then periodically they would come back to him and offer him the chairmanship again. And finally after he had been at the NIMH for three years, he had felt like he had honored that obligation, and had done some of the things that he wanted to do there. And I guess

Stanford wasn't going to take no for an answer. So in 1961 he came to Stanford as chairman of the psychiatry department. And actually the Hamburg's lived down the street from us on campus from Sidney. And so immediately after Sid died, he asked me if I would join his new department. And I had already realized that I probably would not want to go to a new place. I would probably either go back to Penn State or it would be nice to stay here. So I thought that sounded very nice. And I stayed on at the center for a total of two years because after Sid died. I took on the task of finishing some of the things that he was writing, that I wanted to get out. And his assistant came out from Penn State and worked with me. And we had friends from the National Science Foundation to make that happen. Everybody cooperated really beautifully. The Center gave me an extra year and the NSF put some money behind it and we got that worked out. And then I joined this [the Stanford Psychiatry] department.

Borstelmann: That's how that came about.

Siegel: And I've been here ever since.

Borstelmann: Now were there any psychologists in that department at that time.

Siegel: Yes. David had recruited Gig, Seymour, Levin, who is a comparative psychologist. And he has been here ever since. And I think Rudolph Moos had come about the same time I came, maybe a little earlier, and he's been here ever since, and of course. Karl Pribram was in the department at the time that David got here. And stayed on for many years, and he was actually a neurosurgeon by training, but he functioned as a psychologist. And then David recruited Bill Dement, who is an M.D., never took a residency, and has Ph.D. in physiology. But he really functions as a psychologist, in his sleep research. Then there were some clinical psychologists as well. But clinical psychology hasn't really thrived at Stanford over my life time.

Borstelmann: Well and of course, the psych department gave up the training program along in there someplace.

Siegel: That's right.

Borstelmann: So there was no connection there to bolster psychology here I would think, clinical psychology. But now what were the expectations of you, in coming to this department.

Siegel: David was trying to build an interdisciplinary institute of behavioral sciences. So I was expected to represent the field of Child Development in that. And we had an anthropologist at that time named Peggy Goldy, she was a woman who had got into anthropology. I talked to you about the fact that I would have liked to at one time. We had a geneticist, we had a social psychologist. I did social psychologist too some, but Rudolph Moos is kind of a social psychologist personality psychologist type. And then the field of psychiatry over the years went increasingly biological. David's interests went increasingly biological. Department Chairmen you know have a lot to do with what happens in a medical school department. David became interested in primatology. We had primatologists on our faculty, Jane Goodall.

Borstelmann: Yeah right. I remember that.

Siegel: And in recent years our department has become increasingly biological in its orientation, and then most recently increasingly clinical. Which pleases the rest of the medical school, that we've finally started doing some clinical, some serious clinical work in our department.

Borstelmann: Sure, sure. OK, well you've been here most of thirty years, haven't you [joined this faculty in 1963].

Siegel: That's twenty-nine years.

Borstelmann: Who's counting!

Siegel: I had a nice sabbatical down in Chapel Hill once. You remember you and I and Harriet used to have lunch together.

Borstelmann: Oh, yes, Yes indeed. OK let's take a look a little bit, at your time, your years here. What have been the important things to you in your time here? What has mattered to you, and what's been your contribution to the enterprise?

Siegel: Oh boy, that's a good question. You know, when you have been an undergraduate at an institution, you have a much more sentimental attitude towards the institution, than other people do. So I have probably done a lot more, oh let's call it university service, than I ever would have done at Penn State probably, or if I had gone to yet another institution. After Sid died, I started getting offers to go to other institutions. And probably if I had gone to any of those I never would have done all the university service that I've done for Stanford. We had a commission on education at Stanford, what was it called, "The Study of Education at Stanford". It was headed by a lawyer, named Herb Packer. And I was asked to chair the subcommittee on the education of women at Stanford. And there were ten subcommittees, and mine was the only one that actually did any research as far as I know. The others, well they collected university statistics, and in a way that's what I did to, but they tended to be kind of philosophical. But I had a group of students -- I gave a class called "Sex Differences in Education" or something like that. A bunch of students showed up, most of them thought it was going to be a class on sex. That was before the word gender, you know. Gender came later. But the ones that remained and worked with me, each of them did a term paper on some topic about sex differences in education at Stanford. And on the basis of that I wrote a report for the Packer Commission, on the education of women. And on the basis of that I was offered all kinds of jobs, to be president of this or that women's college. And I would get these telephone calls asking me if I wanted to be a candidate to be a president of this or that women's college. And I would say, "Well where did you get my name?" And these colleges had written to Stanford for our study of education, because we were ahead of the other schools by a few years. So anyway I was offered jobs as head of women's college. And discovered that, that wasn't what I wanted to do. I remember that I went to Wellesley; they were looking for a president. The president of Wellesley gives a sermon once a year. She stands up in the chapel and gives a sermon, and the title of the sermon, if I'm remembering correctly, is "God is Love". And it's been the same title every year. Now I may be misremembering. But anyway when I went to visit Wellesley, a woman student had the assignment of taking me around and she wanted me to see the chapel, which was one of these, oh, I remember it as Victorian, Edwardian monstrosity, whatever it was, I mean, we have one of these at Stanford too. Just you know, horrible period in American architecture. And I actually went up and stood in that pulpit and I thought to myself, you know I'm not going to do this, I don't think this is going to work. And I was very flattered that anybody would want me. But I thought I'm not, this isn't what I wanted to do. I heard later that the faculty had thought I was one of the more suitable candidates that they had met, because I was an academic. And the faculty is always worried that the administration is going to appoint a businesswomen. So they thought that, that I would be good. And of course the women colleges do want women as their presidents. But you see, they don't want spinsters because spinsters aren't a role model, for their women students. Students don't look forward to being a spinster in their lives. And they don't really want married women, because a married woman has a husband, and what do you do with them. And they don't really want divorced women, because that isn't quite in the good old Christian tradition at these colleges. So you're left with widows.

Borstelmann: You were very suitable.

Siegel: And the woman who was eventually appointed the president of Wellesley, at that time, she's no longer president, she had very fine qualifications. But she was written up in the New York Times and she had been widowed twice. And one of my friends clipped it and mailed it to me, and wrote on it, she out widowed you. Once I had an interview with one of the president of one of the other women's colleges, who wanted me to be his Provost. And he came out to meet me and invited me to have lunch at one of those fancy hotels in San Francisco up on the roof, of some fancy restaurant. He told me that his faculty wanted me to be their Provost. I don't think they called it Provost. I think they called it Dean of the Faculty. So I said, "Well I know that widows are very attractive to women's colleges." And he said, "What do you mean?" So I went through this thing that I just said to you, now this is all changed now, of course. But in those days that was the thinking and each college had to go through it on its own. They didn't know it hadn't been written down or anything else. So I got all through with that speech and he said, "Well Dr. Siegel, I didn't even know you were a widow. He said, "What were those four categories again?" And I thought, oh my-gosh, this man is the president of a women's college and he doesn't even know what these categories are.

Borstelmann: He hasn't got a clue.

Siegel: And of course what these colleges found out is, as incentive is that you're the queen bee in a relatively small enterprise. And you live in a beautiful house, which is staffed by, loyal folks who are going to do your bidding. So they're holding out a life style. And I remember this one man that I was just speaking about. That was the way he talked to me, had I ever seen the house that the Dean of the Faculty lived in. Well I must come to visit because it's such a charming house. And of course I've lived in an apartment all my life, since Sid died. I've never had a house and don't really expect to. But it was, Ruth Benedict taught me this expression. She said it's a Russian expression. "You're scratching me where I don't itch." So they're holding out this, you see, this sort of thing. I would live in this beautiful house, and I was thinking, well keep trying but you haven't hit it yet. You haven't pushed my buttons. That's what we would say now, you haven't pushed my buttons. I worked for a while as an Associate Dean of Undergraduate Studies. And that was another one of these sentimental things where I always felt that Stanford neglected undergraduate studies. And finally we had a president who took undergraduate studies seriously, and created a Dean of Undergraduate Studies, who was a very fine man, named Jim Gibbs, also an anthropologist. And Jim asked me to be his Associate Dean, so I took that job. And after ten months, it was either a question of getting me institutionalized, or getting out of that job. I disliked it so thoroughly. I really have never disliked a job so much, as I dislike that job.

Borstelmann: Because...

Siegel: And I've thought about it a lot. Because by then I had been the editor of Child Development, I had finished my term as editor of Child Development, and everybody would say to me, "Oh, you're such a marvelous administrator." And I did a very good job of editing Child Development, if I may say so. And everybody would say, "Oh, you're such a good administrator, you'd be so good at administration." Well editing a journal is a form of administration, where you sit with a lot of papers and you put them into a pile, and you mail them around the country, and you get recommendations. But administration with undergraduate students is a totally different thing than administration with your professional colleagues. And I couldn't stand it. And I think part of the problem is that I don't delegate well, which administrators have to do. And when I was editing Child Development, we were getting about 300 papers a year, and I put a lot of time in on that. And I read all those 300 papers.

Borstelmann: That's almost one a day.

Siegel: Yeah! And if I didn't agree with the referees, I would get another opinion. But I didn't publish anything that I didn't personally feel confident should be in the journal. Now you couldn't possibly edit the journal that way now.

Borstelmann: How did you get into doing that?

Siegel: By a telephone call from Al Baldwin. Al Baldwin was the president of the SRC and how did he know me? How did I know Al? I knew him. Well, one thing was I had been asked if I wanted to be the Chairman of the Department of Child Development at Cornell, when he was leaving that chairmanship. But I think the reason we got to know each other, may have been through Division Seven of the APA, Division on Developmental. I edited the newsletter for that division. That was my first little editorial thing. Now you want to go back to childhood. When I was ten years old I edited a newspaper.

Borstelmann: Well we really didn't need to go back that far.

Siegel: ...Which was called the messenger and which I printed on my own printing press. I had one of those little child printing presses. And I sold it all over our neighborhood. So I've always been a frustrated journalist. So I did, when I was at Penn State, I did the newsletter for Division Seven for several years. And like every newsletter editor, I improved it and enlarged it and you know.

Borstelmann: And all that stuff.

Siegel: All that stuff and so then that's why Al called me.

Borstelmann: Well you've been very active with university affairs, as you've indicated and so there were other important kinds of rolls that you took on, as well as this "Dean" job, which you opted out of then.

Siegel: Liked the people -- hated the work.

Borstelmann: But there were others here that you must have enjoyed doing or gave you some gratifications.

Siegel: We had this program in human biology, which was not a graduate major, and I taught in that program. I liked that very much; I thought it was a very exciting program to teach in.

What else did I do at Stanford? I chaired a lot of search committees. I put together the idea that we should have an Ombudsman at Stanford, at the time of the "troubles". And I have a little, sort of Quaker, in my background. So I'm always looking for ways to reconcile differences, and help people get along and mediate and stuff like that. And so I came across this idea of an Ombudsman. I was on a committee that was supposedly, I don't know, bringing together the student and the faculty and the administration. And I recommended an Ombudsman and I put that together and chaired search committees for that.

I chaired a lot of search committees for Stanford, after I was made professor. Stanford gave me courtesy appointments in other departments. They don't do that until you are a professor. Once I was a professor I had courtesy appointment in psychology and a courtesy appointment in education. Both of which have degree programs in child development, which of course we don't have in the med school. So I was working with the graduate students in those programs.

Borstelmann: Now you've also been very active in SRCD affairs.

Siegel: And I've been very active at the National Institute of Mental Health.

Borstelmann: Yeah. I know that.

Siegel: Not lately.

Borstelmann: And child development and social policy either one of the things in SRCD. What are -your thoughts about them, in terms of how that went, is going, or was. In other words I'm asking how you see us with respect to this matter. I guess.

Siegel: You know I understand the governing council of SRCD, and I was also on the publications committee, I guess those were both after I was editing *Child Development*. But one of the perennial problems with SRCD is that somebody wants to endorse something, or get behind something. And we had no mechanism for....

Borstelmann: Handling that.

Siegel: ...Handling that. So Bob Sears, when he was president, his idea, I think, was, I mean I don't want to put words into his mouth, and he's dead now and can't speak for himself, but I think his idea was, you should have somebody who's your social policy person, mostly to keep those people out of your hair, just somebody to fend them off. I don't think he really felt that SRCD should do anything about these issues. Bob was very academic. And I think his experience at Iowa, which, you know, was meant to serve the people of the State of Iowa; I think that left him with the belief that, that's not the proper function of child psychology. So he didn't really want SRCD to get into social policy. He wanted a mechanism to keep it away from the core of the organization, I believe. I think some successive presidents had pretty much the same view. There were others in the 60's and 70's that really had more of the ideas that the founders had, had about wanting to do something on behalf of the welfare of children, more than Doug Lawrence --- did. And I have been closer to that camp. The SRCD had a long range planning committee that Harold Stevenson set up when he was the past president, having served two terms, first as president elect for two years, then as president for two years. When he was the past president he wanted to do some long range planning for SRCD. And he got funds I think from the Grant Foundation, to do long range planning. And he set up a committee; Harriet Rheingold was on that committee, I was on it, Bert Brim, Ross Parke. Berry Brazelton, oh there was another man, who's name I can't bring to mind.

Borstelmann: That's a pretty good crew.

Siegel: A man at the University of Delaware who studied reading, I can't think of his name. But it was a good crew. And we met and we made a number of recommendations to the governing council. All of which were accepted. We recommended that SRCD have summer institutes. And we recommended that SRCD have study groups and funding for those was found for those from the foundation for child development, which Orville Bert Brim was heading, and he was sitting on our committee. And we had summer institutes, you know, at the various universities. And then these study groups

Borstelmann: On social policy?

Siegel: No I haven't gotten to social policy yet. But what we didn't do in our two years of meeting as a long range planning committee, we didn't do anything about social policy. And this was this niggling problem that was hanging out there in the periphery. And nobody in the organization wanted to deal with it.

Borstelmann: Touch it.

Siegel: Touch it. So I proposed at the end of that bi-annual, when Harold had, had such a successful run as chairman of the long range planning committee, and had gotten so many things, you know Harold is the most creative man in our society. And he has done more for SRCD probably, than any single individual. And he had gotten all this stuff done, and I said, "Now look, we haven't tackled social policy." And I believe it was in our charge, but whether it was or not we all wanted to do it. So I said, "Why don't we have a son of long range planning?"

Borstelmann: Or a daughter.

Siegel: And run for another two years, but just focus on social policy. Well that became a more streamlined version of the same group. Some people dropped away, it wasn't their interest. And we couldn't have this large of group anyway. But we had this mechanism set up for study groups. There was going to be a study group on habituation, and there was going to be a study group on language development or whatever the heck it was. So I said, "Let's have a study group on child development and social policy." And the members shall be. Berry Brazelton, Harriet Rheingold. Harold Stevenson, and myself, and I will chair it. So we used the mechanism that we had established within SRCD, to study social policy. And it was a way that this group, who all loved each other, could continue to meet and continue to work. We called ourselves the --- because we laughed so much when we were meeting, especially with Ross Parke but also Berry Brazelton. And Harriet would sit there and look disapproving. And Harold would sit there, you know, drumming his fingers and we were all just kidding each other and having such a good time laughing. Well eventually Harold got into the spirit of it and so did Harriet and she started laughing too. That's how Harriet and Berry Brazelton and Harold, who had been my friend at Stanford, we worked then for two years on social policy. And we proposed to the SRCD that there be a standing committee on child development and social policy. And we proposed that at the 1977 meeting of SRCD, which was held in New Orleans, and Berry Brazelton who figured out the way to do that, and he said have a social policy day. Have it a day in advance of the meeting. Have some good speakers, arouse interest. And then the governing council will see, that there is interest in the membership for this. So we did that and I think Ron Haskins helped us plan that social policy day, if I remember. I maybe collapsing some of this stuff, you know. But I think Ron helped us in New Orleans, certainly Berry did.

Borstelmann: He did his dissertation with Harriet.

Siegel: That's right. And Berry got a man from Denver, who worked on child abuse, Kempe, I don't know how you say his name, he got him to speak. And, oh I forget who all. But anyway we had a one day program in New Orleans. It attracted a lot of interest. So by the time we went to governing council with our proposal, they already knew that the membership was ready for this and wanted it. So we set up a committee on child development and social policy, of which James Gallagher then became the first chair. And he got some things going. And then, maybe I was the second chairman. I don't remember. And I think Harold was an early chairman.

Borstelmann: Edward Zigler got involved in this?

Siegel: Ed served on the committee. Ed is perhaps more of an activist than SRCD wanted. Ed would have liked to have seen, and Ed was on the governing council at one time or another, and Ed would have liked to have seen the

social policy effort move faster than it did. And he would have liked to have seen it have more of a Washington presence than it did. And I think that the reason that he was not put in "the" leadership role, he was always in "a" leadership role, but I think was that there were people in the society who didn't want us to be as activist as he wanted to be. Then at some point somebody went to Dorothy Eichorn, who had become the executive officer with the notion of the congressional science fellowships.

Borstelmann: Oh yes.

Siegel: Under AAAs. Now those were already underway. And the first one or two classes at the congressional science fellows had been chosen, and that program was funded by the Foundation for Child Development and also by the Grant Foundation. It was a large program. There was some dissatisfaction with the quality of the fellows. And it was felt. I guess on the part of the foundations, I wasn't a part of it at that time. It was felt that there might be a better selection procedure for the fellows if SRCD got involved. And so Dorothy and some agreed to that and assigned the task to the social policy committee. So the social policy committee did not make the decision up front that SRCD would have these fellows. But it inherited them from Dorothy and the leadership. And then that became a major function in the social policy committee, to run that program. And I think it was a very good program. And after I was chairman of the social policy committee, I then became chairman of the selection committee for the congressional science fellows, and that person had been Luis Laosa and he became the chairman of the other committee, and he and I just switched jobs. So Luis and I for four years ran that social policy effort.

Borstelmann: Is that program still in existence?

Siegel: It certainly has faded back. One of the things that were felt was that we needed a presence in Washington. And initially we had a person who worked one day a week out of her house. And then pretty soon its two days a week, and then it's another person and then pretty soon they need a little office to be close to downtown. And by that time we had the fellowship program and the foundations wanted us to have a physical location. So I did a lot of horsing around, trying to find a university location for that office. And the foundations actually had a more political notion than the SRCD did. They wanted the office to be down near the congress, which was where the fellows were working. And I wanted the office to be on a university campus, but there wasn't any obvious connection. The most obvious connection would have been Catholic University because they were mounting the Boy's Town Center's and had a social policy ----. But there was something about a university called "Catholic" University that didn't sound quite right. Plus apparently it's not located in a part of town that people wanted. The women that worked for SRCD, see all of our officers in Washington were always women, and they didn't want to be located in that part of town, and Georgetown some how didn't work, and American somehow didn't work. And there wasn't a strong program in child development at any of these institutions. So we ended up having the offices just in office buildings, and that became expensive. And pretty soon we not only had an officer, so called Washington Liaison Officer but then she had an associate. So we needed an office that housed two people, and believe me they were plenty busy. The funding was coming out of the foundations. We had marvelous applicants for those programs during the Carter era, when things were happening on behalf of children in Washington, and into the early years of the Regan era. But as the Regan era stretched out, Washington lost its attractiveness for the kind of person you wanted to have a year. And, you see, my thought about all of that was, that the function of the congressional science fellowships is to give the SRCD some know-how about the executive branch of government. We always have plenty of know-how about the National Institutes of Health, because we had all of our members studying on study sections. And you'd get a meeting of governing council it was like a meeting of the study sections. Everybody was a study section. But you'd start talking about the executive branch, nobody knew, how to move. I realize that the NIH is in the executive branch, but I mean in a more political way. Nobody knew how to move, nobody knew anybody, nobody knew, they didn't have any connection. So I thought that the congressional science fellowships could train up a cadre of SRCD members who knew. I'm sorry I'm going to have to stop, I was doing this backwards.

Borstelmann: You have somebody waiting for you?

Siegel: No I'm just worried about your time. Are you OK?

Borstelmann: Oh, no. I'm fine.

Siegel: I'm telling the story of my life. I can tell this....

Borstelmann: Of course you are, that's what I'm here for.

Siegel: But I was saying it backwards. We had people who knew the executive branch; because they knew the NIH and they knew how to get upstairs from the NIH to the surgeon general, and so forth, and the office of education. What we didn't have were people who knew the legislative branch. I was saying it backwards. And that's what the congressional science fellowships were going to offer us, from my view. I thought that's what they offered SRCD. As far as the foundations were concerned, they didn't care so much about SRCD, they wanted to do something for children in Washington. And as far as the fellows were concerned, that was their in interest too. They didn't tend to be SRCD stalwarts; they tended to be members of SRCD. But they weren't

Borstelmann: They were activists.

Siegel: They were more activists. And they wanted their Washington year. And I think it was a wonderful program, did a marvelous job. I think as the years went by, either we had gone through the pool, and there weren't any more of those people out there. Or Washington had become so unattractive during the Republican years, or what ever. I believe the program became less attractive in later years. And I believe the applicants were not as strong as they had been initially. Eventually the foundations felt like they had done it, and they wanted to move on to something else. They wanted SRCD to take it over. The foundations were paying for the Washington Liaison Office. SRCD was paying a pittance, but the Lion's share was being paid by the foundations and so there was a kind of a confluence of circumstances. By this time I had done it all, I had rotated through all of the offices and I was out of it. I was a little bit miffed that I wasn't consulted, in some of the decision making, but the decision was to close down the Washington Liaison Office. And the function that it had played was taken over, to a limited extent, by the American Psychological Society. And one of their...

Borstelmann: APS

Siegel: APS and one of their employees is part-time consulting employee at SRCD, and does the remnants of that function. But the Washington Liaison Office had really become the home of the congressional science fellows. And that's really what they were doing. They were administering that program. And once that program lost its foundation support, the officer lost much of its oomph. And it scaled back closer to what we had envisioned in the first place, which was a very part-time enterprise. I would not have placed it with the APS because I'm a big believer in interdisciplinary and I hate to see SRCD get too psychology oriented.

Borstelmann: That's right.

Siegel: But many, many people don't see it that way. And many people in the organization, and to them it's just obvious, that that's what you would do, you would go to the psychological association.

Borstelmann: Yes, Interesting.

Siegel: Well what I say we should do, is we should turn it off and go get a ...

Borstelmann: I wonder if there's other aspects of your career to date, that are more salient, or that particularly salient or memorable for you that we haven't touched upon so far?

Siegel: Well I did do more writing in the field of television and children. And the result of that writing is that I served on the Surgeon General's --- Advisory Panel on Television and Social Behavior. And that's how I got to know Eli Rubenstein, who you may have met.

Borstelmann: Oh, yes.

Siegel: Subsequently he was the staff director of our program. He and I have stayed friends ever since. And then 10 years later they did an update on that report, and I served on the committee that oversaw the update, and Eli also served on that committee. Eli and I served on a panel for the National Science Foundations on the effects of TV advertising. He and I kind of became a team, and really did a number of things together, and liked each other very

much. I never did more research on television. Not having done any in the first place. I never did any more. But any way, and I never had the capability, but it led to all kinds of things. And John White once said, some place, that I was the Godmother of television research in North America, and that's about right. I did a lot of things to make television research happen, without doing much of it myself I served on two or three study sections of the NIH. And those experiences are very valuable for keeping up in the field. I especially enjoyed the one that gave the research career awards. And we were able to travel around the country and identify promising young people and promising young psychiatrists and support them for a research career. At Stanford, what I have been doing in recent years, is an awful lot of grievances and sexual harassment situations, and sex discrimination situations. I don't join organizations that try to advance the cause of women and try to segregate men and women. I don't believe in that. And so when the administration looks around for a woman on the faculty to serve on some high level panel, who isn't going to just be, it's... know in advance what her views are going to be, where they find me. And I end up doing a lot of that. And I'm just now planning to get all of those files out of my house and get them over to this office, now that I have this office and all of these beautiful new file cabinets to fill. And who was that said to me, "Isn't that wonderful, because all of those files have such bad karma, and you'll get all of that bad karma out of your house."

Borstelmann: Indeed.

Siegel: But we had a situation here, where a professor of surgery became an indignant neurosurgeon.

Borstelmann: Indeed, that got a lot of publicity.

Siegel: And so there was a high level panel, four people; the Senior Associate Dean, the Deputy Chief of Staff, the Chief of Nursing, and then another women faculty of the Medical School. Right! All of these other people understand why they are on the committee, but the fourth member is yours truly. So I learned all about neurosurgery. And just now we're finishing a report in which I learned all about radiology. And in almost every one of these situations, I have come down on the side of the woman. So I get a little nervous when people say, "Well this committee is going to be just another white wash." Heck, you know my record is almost 100% that I recommend on behalf of the woman. But I don't sign too many petitions, and I don't sit around and participate in seminars about, "Can you have it all?" And all of this bogey stuff that's going on with respect to women. But I do, most of what I do at Stanford now is really on behalf...

Borstelmann: I think there's a difference between a militant and an activist.

Siegel: That's probably about it; I'm the activist, who's not the militant. But there is so much of that stuff going on now at the universities, at least in this university. And, when Lynette and I saw each other in July that was most of what she was talking to me about. Because she's getting into that kind of stuff at her university and she wanted to hear what happened at Stanford. So I gave her an ear full. It's all confidential, but you can talk about it. One of the things that I did for the SRCDC, was I got the history committee started. Alice Smuts got in touch with me through Harold. I forget what the original contact was, but I said to her, "You know, I know all of the old timers in child development, because they all live in California." She was writing the history of child development. And I said if you'd ever like to meet these people, why come out to California. So she couldn't resist that. And she came out and did oral history interviews with Mary Jones and I think she saw Nancy Bailey, and talked with Lois. Lois was kind of a professional oral history giver.

Borstelmann: Yes I know.

Siegel: And talked with some other people, Jean Walker McFarland. I believe she talked with her. So then she was telling to me about this problem, that people in the field of child development, their papers aren't properly taken care of, and don't get into archives. And so I said we ought to have a committee at SRCDC. I think it was that same meeting in New Orleans in 1977, that I brought Alice to the governing council, of which I was a member of that time, and she made her pitch and a lot of people said, "This has been the only interesting thing that has happened in this whole council meeting." You know, everything else is, journal budgets...

Borstelmann: Routine.

Siegel: Yeah, routine. And this was exciting; she was talking about what had happened to Arnold Gazelle's papers, and what had happened to the various folks. And so they set up this standard committee on history and they bought the chairman. Bob Sears. I believe by then he had published his...

Borstelmann: Yes in '75 it was published.

Siegel: Yes, so they knew he was....

Borstelmann: Re-visiting, our ancestors.

Siegel: That's right. And he did that because, Milton Seun wanted to publish a monograph about his interviews that he did, that were. And Bob gave Milton Sen a lot of help, as he was the editor of the monographs. He gave Sen a lot of help in getting those materials into shape that monograph. And that got him interested in history, and he went ahead and did that chapter for, volume four, or whatever, whatever he used. And then he became chairman of the history committee. And Alice I think was made a member of that committee. And they got money from Phil Sapir.

Borstelmann: I think so.

Siegel: To get that committee going. And so I was a very minor midwife, but got it going. So I never served on that committee, but I've always been tickled to see how it's developing. And now I have a. I finally have a graduate student who's interested in a historical topic that I've always wanted to investigate. And she's just done her dissertation on it, and that is the history of orphanages.

Borstelmann: Oh my, yes.

Siegel: I've always wondered why that literature on the effects of orphanages was so confused and muddled. And I suspected that it was because people didn't understand orphanages. And I now feel very strongly that, that was the case. So I have this student named Bernadine Barr who has been working on the history of orphanages.

Borstelmann: Good. My impression from all you've been saying is that you really have enjoyed teaching.

Siegel: Yes I have.

Borstelmann: And could you talk a little bit about that and having your own ways of teaching and what it is that has been rewarding to you about it?

Siegel: You know when Helen Koch was given the G. Stanley Hall award, I think it was Larry Kohlberg who said that, in presenting the award, he said that, a student would go to Miss Koch and say I'd like to study some topic, and Miss Koch would say, "Well I don't know anything about that topic, but if you would give me a list of references I will learn." So end up knowing that Miss Koch was a fine professor, but don't associate her with any one topic of research, except I suppose siblings. But you know, she launched Harriet, on Harriet's work with institutions, which came out of Harriet's clinical experience. And she launched Larry Kohlberg on his work on moral development and suffering. And I always admired that so much. I met Miss Koch once in later years, and she was kind of a starchy character. But I admired that idea that a student could come to a professor and the professor would encourage the student to work on what they were interested in. And the professor would try to learn. And I haven't done as well as Miss Koch did, but that's kind of been my...

Borstelmann: Your approach, right.

Siegel: I had a, I guess one of my pet students, was a man named Fred Volkmar, who was a medical student, and came to me on the recommendation of Joe Hunt. He had done his undergraduate work with Joe Hunt. And the trick with Fred was helping him fit in psychology study with being a medical student and then later a resident. So it took a lot of accommodating, but we managed it and Fred got a masters degree in psychology while he was in medical school. And he's just not gotten tenure on the faculty of the Yale Child Study Center, and as he told me at the time, "I didn't have to do any bio-chemistry to do it!"

Borstelmann: Incidentally didn't you spend a year at the Child Study Center?

Siegel: Yes. And that was in an effort with Fred to finish up some stuff that we had done together. By then he was working there, and it was effort to finish up some research, I don't know that we succeeded really in doing that very well, but we made a good try. I get very fond of my students, and I continue to be close to them and continue to work with them, in the cases when they're quite gifted and they become my friends. I would say that teaching in a medical school you have to have a lot of tolerance for somewhat superficial approach to your subject, because you don't have enough hours to do much more than a superficial approach. And you also have to be willing to teach students who aren't really very interested. Some are, and some say, "Oh this is the only class that I've had in medical school that makes any sense to me." But more commonly they say, "I don't understand why I'm supposed to be learning this." And see, you have to be able to work with that to teach in a medical school. Also the case in teaching residents, it is surprising how people who go into psychiatry and child psychiatry how little they know about human development.

Borstelmann: They know nothing, and how little they care. Yeah, right.

Siegel: They're commonly very technique oriented. They want to learn to be a good interviewer; they want to learn to...

Borstelmann: Be a good therapist.

Siegel: Be a good therapist, but don't bother me with the facts. Yeah, that's true. So I would not say that teaching in medical school, is the most rewarding career for a developmental psychologist. And I think one of the reasons that I have enjoyed working with the NIMH so much, and with the social policy committee, and with the SRCD, is I had to do things like that in order to be a developmental psychologist. Because there is nothing intrinsic to a medical school, that makes you be a developmental psychologist. Everything makes you be a clinical psychologist, which I'm not. But that's where all the press is, including my own faculty colleagues in the rest of the university who think I'm a clinical psychologist because I'm in the medical school. And who could blame them? So it takes some doing to retain an identity as a developmental psychologist.

Borstelmann: Well I would guess that your appointment is somewhat unusual in medical schools, that is to say, a non-clinical child psychologist.

Siegel: That's right in a tenure line position, it's unusual in another way, I was the first women ever to be promoted to the rank of tenure in the medical school at Stanford, to be promoted to the rank of professor. And there had been women psychiatrists and women clinical psychologists around here, but neither of those fields creates the academic credentials that enable you to be promoted to professor. And more commonly I think the developmental psychologists, who have been in medical schools, have been in strictly research assignments, I think Frances Graham, for example, was on the faculty...