

Alfred L. Baldwin

- Born 10/5/1914, died 6/18/1998
- Married to Clara Baldwin

Major Employment

- University of Rochester
- Cornell University
- University of Kansas
- Fels Research Institute

SRCD Affiliation

- President (1963-1965)



SRCD Oral History Interview

Alfred Baldwin

Interviewed by Arnold Sameroff
At the University of Michigan
1994

Sameroff: You have been selected to be interviewed for the Society for Research in Child Development historical survey. We're going to be fairly informal with this interview, so we'll begin with a general auto-biography involving your intellectual history, a discussion of your contributions and your academic career in child development, and then your experiences with SRCD in particular, where you served as President. Hopefully at the end we can have a general discussion about your feelings about the field during your lifetime in child development. So let me begin by asking you to talk a little bit about your growing up, your family background, and your early experiences that may be of interest.

Baldwin: I was born in Kansas. My father at the time I was born, taught Latin and Greek and was on his way to becoming a psychologist, I'm not quite sure how. But in any case, when I was about three he went to University of Chicago to get a master's degree in psychology. He stammered rather badly and developed some ways of treating young people, mostly, who stammered. And so he wrote a master's thesis on that, and this was in 1916, or so about. Then he came back to Kansas and taught psychology and in just a few years began to move into administration. The time that I can really remember him I was seven or eight. He was Dean of Men in Kansas.

Sameroff: What was the name of the university?

Baldwin: Friends University. He taught psychology. I know about this because when I was in third grade I was doing very badly in school. So he took me out of school and for one semester he gave me assignments and I sat in the back corner of his class in psychology, and instead of doing the assignments, listened to him talk about psychology. I repeated the third grade, but I spent the second semester studying the things he assigned and listening to him talk about psychology. What I mostly remember was things about illusions. I had really no idea what kind of psychology course he actually gave. Actually now I'm very curious, because he was very much oriented towards humans. He wasn't interested especially in perception. He was the Dean of Men and so it was a very human affair. I really don't quite understand how he moved into psychology, but I think it was somehow through his feeling about stammering and trying to develop a program that would help stammerers. He wasn't really in private practice or anything like that, but every now and then there would be a student who stammered a lot, and he'd come to the house, and so I'd know about it. And I stammered. I stopped stammering almost instantly when I left home and went to college. My father had improved a great

deal, but he still stammered, I think, probably most of the rest of his life, although not badly; it wasn't anything that was really crippling at all.

Sameroff: So you would give an environmental interpretation to your stammering?

Baldwin: I guess I have to, I have no idea. I remember in high school I was very uncomfortable. And I don't understand it, because it was -- I don't think it hampered me in terms of giving papers. It was much more in terms of everyday conversation that I just remember I would get into a terrible tangle.

Sameroff: So you went back to grade school, and --

Baldwin: So then I went back into third grade, repeated it. And from then on I got very good grades. So that was his strategy. I think he felt that I had been -- in fact I had been jumped, I think, from maybe into first grade too early and then maybe even to second. I think maybe I skipped a grade. And I think he felt I just had gotten too far ahead of myself, and so he kind of remedied that by keeping me out. And at least in terms of grades it worked, very well.

Sameroff: This was in a town in Kansas?

Baldwin: This was in Wichita.

Sameroff: So it was a fairly large city.

Baldwin: Yeah, it was a fairly large city.

Sameroff: And you finished your high school there as well?

Baldwin: Then I finished junior high there and this was about 1928 or so. And at that time, he got a leave and went to the University of Kansas to finish up his doctorate in psychology, and I was a tenth grader, so that I got to know the psychologists at the University of Kansas. And then it turns out that a few years later I went there. As an undergraduate I had members of the faculty that I had known several years before. How all this influenced my psychological development, I have no idea. The reason I went to The University of Kansas, rather than Friends University where I would ordinarily have gone because that's where my dad taught and it was in town, was that I got a four-year scholarship to the university. So that then I went to the University of Kansas with that kind of financial aid. And it happened that I knew all these people in psychology. And I guess it never really occurred to me to go into any other field except psychology. I had really decided I was going to go into psychology and psychiatry and get a double degree: a medical degree and a Ph.D. I don't know when in my undergraduate career that particular ambition developed. So that I did take -- I fulfilled all the pre-med requirements, so it must have been while I was an undergraduate that I had these intentions. So I fulfilled all the pre-med requirements. In fact as a senior, after I was a major in psychology, I took medical physiology in the medical school, which at the University of Kansas the first two years were on the large campus and then you moved to Kansas City Hospital for the remainder of the time. So that was what I was doing. And also during my undergraduate career I developed a very strong interest in mathematics, so that I had the equivalent of major in mathematics as well as a major in psychology. After I graduated I stayed at Kansas for a master's degree in psychology. And then I continued to stay at Kansas and I worked on a master's degree in mathematics, which I never actually got. But for a couple of years I had an assistantship in psychology and also an assistantship in mathematics. I don't think I've ever been any richer, financially, in my life. Two \$500 assistantships in 1933-4 was a lot of money.

Sameroff: Do you remember the people who were on the faculty at Kansas at that time?

Baldwin: Yes, I became very good friends with some of them. Raymond Wheeler was Chair. He was a kind of an offbeat, very offbeat, Gestalt psychologist, and that's what he taught. In terms of child

development, Beulah Morrison was my professor of child psychology, but I wasn't particularly interested in it. Theodore Perkins I think probably became my closest friend and mentor at the University of Kansas. Another very influential person was J. F. Brown, who had been at Berlin and had worked with Kurt Lewin, and then came back to Kansas and we did some research together. He introduced me to Kurt Lewin and to topological psychology and to the topological psychology meetings. They became very influential in my whole development. Curiously now, my master's thesis was trying to see if there were stable individual differences in the learning curves of rats learning to run a maze. I put a sample of rats through a number of different kinds of mazes, to see if the jumps and the dips in the learning curve would match so you could tell which rat was which by what kind of a learning curve he had. I guess I thought of them as the personality of rats. But in any case, it's very curious, because in so many ways it's like the longitudinal studies that I've been so much involved in.

Sameroff: Were you successful in finding individual differences?

Baldwin: I wrote a master's thesis that got passed. The thesis said that it was proved, but as I go back over it, I doubt it. But there may actually be something in that hypothesis; I mean it's not a ridiculous notion at all.

Sameroff: What was Perkins' specialty?

Baldwin: I guess sort of theoretical Gestalt psychology. He was very close to Ray Wheeler.

Sameroff: There was a whole Gestalt group at Kansas at that time.

Baldwin: Well kind of, yes. But at the same time it was an offbeat Gestalt group, so that while I guess Wertheimer and Kohler knew about it, Lewin is the only one that actually ever visited and gave a colloquium in the department. And J. F. Brown and Wheeler were not at all close. At that time Wheeler was trying to find cycles in history. He had a great big book of history, trying to identify the major cycles in history. I think that probably had something to do with my looking at the cycles of rat's learning. J. F. Brown, I think, was not impressed by this at all. He was interested in abnormal psychology and later he became very much involved in psychotherapy. But he was a field theorist. When I finally went to Harvard as a graduate student in 1938, I took with me some data that I had collected under Brown's encouragement. The theory was that the perceptual field had a universal law of gravitation, just like a physical field. And, therefore, that the Muller-Lyer illusion looked longer because the arrows at the end of it pulled the lines out and the arrows that went inward pulled the lines in. The filled space looked smaller than unfilled space because it contracted because of gravitation. What I was trying to show was that the after-image of filled space was actually shorter than the after-image of unfilled space. I took the data to Harvard and it became my first publication. The data came out right, but looking back on it I have many misgivings about the way the assistants recorded their after-images. These were the days of the depression, when laboratory assistants were freely hired with government funds. The people that served as subjects were these students and they had a lot of good reason why they should get the kind of data that their supervisor wanted, so that I don't really trust it.

Sameroff: It doesn't sound like much has changed over 60 years in terms of graduate students.

Baldwin: I think that's right, yeah.

Sameroff: Were any of the people you were involved with in the psych department; teachers of your father?

Baldwin: Yeah, some of them.

Sameroff: You sound like he had fairly applied interests.

Baldwin: Oh, yes, in fact, I guess something else I should say is that, when I was about an eleventh grader, I worked on his thesis. I did an awful lot of statistics of it. It had many correlations in it. He was a very good teacher, and so that he gave me the book and told me to figure out how to run a correlation coefficient.

Sameroff: This was before calculators, what --

Baldwin: Well it was the day of calculators that were just a little better than adding machines. If you wanted to multiply, you put in a number and then you held down the plus bar and counted the number of times that it added.

Sameroff: Ah, the good old days. Correlations were about as far as you could go with that methodology.

Baldwin: Factor analysis wasn't invented until I was in graduate school. It was close to being thought about. Anyway, so my father was trying to show that people's intelligence improved with college education. At that time the belief was that intelligence crested at 18 and never improved after that; he was really trying to disprove that and show that the college experience actually improved IQ. The thesis panned out and he got it published and got his degree.

Sameroff: What was your master's thesis in mathematics, or did you have to do a thesis?

Baldwin: Well I didn't ever do the thesis; I just took the courses really and never really finished up an M.A.

Sameroff: Was it mostly statistical kinds of issues?

Baldwin: I discovered when I got to the next step beyond differential equations, function theory, that I was not a real mathematician. There were kids in that class that just loved it and they just ate that stuff up. For me it was all really hard work. So I really stopped without a degree, but I learned quite a bit of math. I'm sure that one of the reasons I got into Harvard is because I had a background in mathematics.

Sameroff: So you decided to go on for a doctorate?

Baldwin: Yes.

Sameroff: And had you applied to a variety of schools, or had you focused on Harvard?

Baldwin: I applied to Chicago where Thurstone was, because I had read his book on factor analysis. I'm not quite clear exactly what it was that attracted me to Harvard. I guess just because it was a top-notch school. So I didn't apply very many places. I was accepted at Chicago, but I decided to go to Harvard. And I must say I'm not really clear right now why.

Sameroff: Did they have a fellowship?

Baldwin: Yes, I had an assistantship, and so that I was essentially financially independent. My folks hadn't had to support me actually since I was a freshman. I'd had this scholarship as an undergraduate and then I got graduate assistantships. So I went to Harvard in 1938. And there it was a very interesting group of first year students: Darwin Cartwright, Jerry Bruner, Mason Haine, John Harding and Jack French. We were all of us fairly conceited and rambunctious and condescended to let the faculty teach us.

Sameroff: Speaking of faculty, was it just one department at that time?

Baldwin: Yes, this is before the split [separating the department of social relations from the psychology department]. Boring was nearing retirement. He was my advisor my first year. I worked with S. Smith Stevens the first year; he was particularly attracted to the mathematics that I had. Gordon Allport, who later supervised my thesis, was chair. He gave a course the first year I was there in life history. My doctoral thesis developed out of that research. Charles Harsh later went to Nebraska. Beebe-Center was also there.

Sameroff: About how many people were in the department at that time?

Baldwin: Well the department in Emerion Hall had six or eight. The Harvard clinic was geographically separated. Henry Murray was on leave all the time I was a graduate student. The functioning head of the clinic was Robert White. He and I were very good friends.

Sameroff: So these people were running a clinical program and they were doing therapy with people?

Baldwin: Yes. They were also doing research using concepts Murray was working on, the needs and presses and all those things. There wasn't a clinical training program that led to a clinical degree until after I was long gone. But nevertheless there were students who were doing therapy.

Sameroff: Did Murray teach any courses, even though he was on leave?

Baldwin: No.

Sameroff: So he never taught a course while you were there.

Baldwin: No, I never had a course with him. I only met him when he was back, but I was very much interested in the research they were doing.

Sameroff: So you showed up with this group of about seven conceited graduate students?

Baldwin: Yes, that's right. Then, either my second or third year at Harvard, Kurt Lewin came as a visiting professor. While I was at Kansas I had already gotten to know many of the people in topological psychology through J. F. Brown. I had met Fritz and Grace Heider, Roger Barker and Herbert Engel.

Sameroff: The Heiders were where at that time?

Baldwin: He was at Smith and had just come from Germany within the last year or so. Anyway, I talked to Kurt Lewin even more while he was at Harvard and I began to go to the topological meetings that were held every year.

Sameroff: So there was a Society in Topological Psychology?

Baldwin: Yes.

Sameroff: Was that the official title?

Baldwin: Yes. It first started out a kind of informal meeting, and then later became a real, so that I continued to go to the meetings of topological psychology for six, eight, ten years after my doctorate.

Sameroff: Were they mostly social-psychological themes?

Baldwin: Yes, by that time, Kurt was at MIT and then later moved to Michigan and developed a social psychological research program. And so the topics were mostly in social psychology. But it always

focused on topology, because he was really convinced that this was a mathematics that you could actually apply to human behavior. So he really meant it when he called it topological psychology. One of the high points of the meetings was later when an honest-to-God mathematical topologist came to the meetings. He was working with Cartwright at Michigan at that time. And in the course of those meeting I met Leon Festinger and other people who went into the Institute for Social Research at MIT and then at Michigan.

Sameroff: Did all the Gestalt belong to the Society or were Wertheimer and Kohler and the perceptual people sort of in a different ball park?

Baldwin: Wertheimer never came to the topological meetings, and I'm not sure that he really thought that Lewin was a true Gestalt psychologist.

Sameroff: Oh that's interesting.

Baldwin: I'm not sure they didn't think so, but at least he didn't consider himself particularly concerned with Lewin's kind of field theory.

Sameroff: He had his own field theory.

Baldwin: Yes, that's right.

Sameroff: So you had come to Harvard, but you hadn't really thought of someone specific that you wanted to work with. I mean you ended up with Boring as an advisor. Was that kind of an accident?

Baldwin: Yes, they may have just parceled us graduate students out, or maybe Boring was the advisor of all the first year students. He helped me a great deal as an editor, because I was working on the article on the after-image of filled and unfilled space under his supervision. And boy did I learn a lot about writing articles from Boring. He was terrific as an editor.

Sameroff: Well he certainly left his mark on the field.

Baldwin: Yes indeed. His *History of Psychology* had already been done and he taught a course in history. But the most influential course I took was with Gordon Allport that first year on life history and the analysis of lives, concrete lives. One of the things we read were the letters of Jenny which became the data for my doctoral thesis.

Sameroff: Why don't you say something about the letters from Jenny.

Baldwin: She was the mother of a friend of Gordon Allport's and had a very fascinating but troubled and unhappy life. She wrote letters to this friend of her son's and he saved them.

Sameroff: She was writing personal letters to this younger man? Why did she pick this person?

Baldwin: I think just because she was complaining about her son, and there was somebody that knew him. And she felt that her son hadn't exactly deserted her. But nevertheless she was very unhappy and I guess this friend and his wife were sympathetic with her and so she got into this correspondence with them.

Sameroff: I see. Now the reason Allport had collected this correspondence was what?

Baldwin: Because he was interested in life history. And I'm not sure whether this is confidential, I think now that it's kind of accepted, he was the friend of the son, but at the time it was confidential. He told me that.

Sameroff: But he had a series of sets of these letters, of people's life histories?

Baldwin: No he just had this one that he happened to have collected. By now it's been analyzed by a number of people from the different points of view, and has been published as a set of letters. I think they are called *The Letters of Jenny*. But anyway, in the seminar I got the idea of dividing these letters into episodes and reading the characteristics of each episode, and doing a kind of a factor analysis of the things she talked about. The hypothesis was that the things she talked about in the same episode would be related in kind of a naive psychological hypothesis. And the things she talked about the most were things that were important to her, and somehow had some kind of psychological meaning. I went through and took each episode and rated it and used one of the earlier computers that worked from punched cards.

Sameroff: That was the IBM cards.

Baldwin: Yes. I put each episode on a card and ran them at night on the computer in the Dean's office. I found the frequency of every topic. And the number of episodes in which any pair both appeared. I got a kind of a correlation table that became my doctoral thesis. And then I tried to interpret it and then I got some other psychologists to write interpretations of Jenny's personality on the basis of reading the letters and tried to compare the two. And that became my thesis and my next publication. And actually this thesis led me into child psychology by a curious route. Lester Sontag, the Director of the Fels Research Institute at Antioch College, came to Harvard looking for a psychologist. He was the director of the Fels Institute longitudinal research. The staff at Fels collected data on a longitudinal sample and each child's data was recorded in a "black book." And so when Sontag heard about my thesis, he said, "Here's the man I ought to try to get in order to try to analyze these black books and do exactly this kind of research on all these individual kids." That's the way I got the job. I wasn't interested in developmental at Harvard. If anything I was interested in a field theory of perception. But this was 1941. So I got my degree six months before the war began.

Sameroff: So you were at Harvard essentially three years.

Baldwin: '38 to '41. Yes.

Sameroff: And so you really didn't look for a job. You just did your dissertation and the word got out somehow. Had you presented a paper?

Baldwin: No. Sontag was looking for a psychologist and happened to hear about my thesis. Jobs weren't easy to get, so that I grabbed this one when it came along. People then didn't send their vita all over the country. Jobs were easy enough to get that you weren't really desperate, but they were hard enough to get that you didn't turn one down either.

Sameroff: Well before we leave Harvard, how would you summarize your education at Harvard? Did you feel that you got a good education or was it, graduate students who were smart enough did what they were going to do anyway? Was there some imprint?

Baldwin: Oh, it was a very important experience. And I am still impressed by the people there, the faculty. I feel I got a good education. It was partly because I was in a group of bright students, and we prepared for the prelims together. We argued a great deal with each other and had a lot of very different opinions. And argued about politics, you know, 1938-39, Germany, Nazism, so we had lots of things to discuss besides psychology and lots of differences of opinion.

Sameroff: What would have been the big issues in psychology that you felt as a graduate student? What were the kinds of things people were fighting about in those days?

Baldwin: I guess we felt that the faculty were interested in minutiae and not really interested in important questions. We were wrong, of course. Cartwright had come from Gestalt psychology, and Kohler was on its staff. I think we felt these people were piddling around with unimportant problems and we wanted to really get in there and try to get some important ones. And we were a bit contemptuous of the faculty; we must have been terribly obnoxious as graduate students. But they loved us. Some of them said afterwards they just stopped worrying about us: "Here's a group who were going to educate themselves, we didn't have to worry about them."

Sameroff: Was this different than the year that had come in ahead of you?

Baldwin: Oh, I think so. I think the faculty felt that, as soon as we hit the place, that "here's a group that are going to be a pain in the neck, but they are going to educate themselves and they're going to do a good job, and they're going to become good psychologists." And practically all of them have.

Sameroff: I was recently at a reunion of Yale graduate alumni and people from different decades talked about their graduate education at Yale. So Neal Miller represented the '30s or something of that nature. He said the faculty at Yale was inspirational. Half because they were so great -- I mean to be near them -- and the other half because they were so bad that a graduate student said, "If that guy got to be a professor at Yale, then it shouldn't be a problem for me." But so, you're saying some of the issues were that these faculty were essentially dealing with trivial issues, but what were the content issues that were going on at the time? Was learning theory coming in?

Baldwin: This was the time of Clark Hull at Yale. My contemporaries at Harvard came from the Gestalt professors at Swarthmore and I from Kansas. We were all critical and skeptical of the behaviorism at Yale. We did not think that conditioning could account for all of human behavior, which wasn't what they actually thought, it's what we pictured they thought at the time. So we didn't appreciate it at all, I didn't at least, the positive contribution that Clark Hull made and the strong emphasis on trying to build up a theoretical psychology that actually predicted. Later when I came to know Bob Sears well I changed my opinion of that group a great deal.

Sameroff: So this was kind of a Harvard-Yale thing at the time, or were there learning theories represented at Harvard as well?

Baldwin: No there weren't at Harvard. And I don't think it was Harvard versus Yale, I think it was behavior theory versus a cognitive theory.

Sameroff: But it was Harvard versus Yale.

Baldwin: Well except that the only people at Harvard were this bunch of graduate students, not the Harvard faculty.

Sameroff: The faculty were doing something completely different?

Baldwin: That's right. Allport, Boring, Stevens, Beebe-Center; these people weren't fighting behavior theory.

Sameroff: They were still doing psychophysics?

Baldwin: Well they were doing all the things they were doing. So that, actually, Stevens was working on typology of physique. Beebe-Center was working on the neurological connections that led hairs to stiffen up. So that he was doing things that would make a hair on his arm rise up. And of course Murray was certainly more cognitive in his theories, but he wasn't criticizing Clark Hull particularly.

Sameroff: Were they calling it mentalistic rather than cognitive in those days? Was that the way the learning people talked about them?

Baldwin: Skinner of course was very behavioristic, but he wasn't at Harvard at all when I was there. But I think Clark Hull wasn't a Skinnerian.

Sameroff: Do you think the impending war or, I don't know if it was thought of as impending at that time, but certainly the conflicts in Europe lent themselves to the seriousness of the graduate students, thinking that they were going to have to think about important things? Or was that independent of what was going on in the world, compared to the faculty that you thought were being kind of trivial at the time?

Baldwin: In 1938 and '39 the students did not want for the U.S. to get involved in the war. I remember I was almost alone in approving Britain's declaring war and I was not militaristic, but I think none of us seriously expected to be involved. I think I was the most militant of them all. And I'm not really militant I mean, I think I was convinced that Hitler wasn't going to get stopped unless people really did something. But there was a great deal of mistrust of American militarism. In June 1941 I got a job at the Fels Research Institute.

Sameroff: Why don't you say a little bit about the history of the Fels Foundation and the research center.

Baldwin: It was founded in 1929 by Samuel S. Fels, the manufacturer of Fels-Naptha Soap. And he was very interested in the development of individuals. I think I met him once, but I never actually had a chance to talk to him or to understand his motivation. At that time there was the California longitudinal study with McFarland.

Sameroff: The Oakland study hadn't started yet.

Baldwin: It hadn't started.

Sameroff: The Berkeley study was underway.

Baldwin: And there was a study at Cleveland and the Yale studies were underway. People had essentially, had learned a lot about physical growth by just this persistent daily recording of growth processes. And so physical anthropology was a very important field. And these growth studies had made important contributions in studying the development of intelligence. That was the reason the Fels study was started. They got people at birth and expected to follow them until they were mature adults or older even. It was one of a small group of important studies. Later longitudinal studies became very unpopular because of the lack of experimental and control group and because they were so costly in terms of the amount analyzable data you could get out of the labor that went into the collection of it. I don't think now anybody would consider starting a big longitudinal study like Fels, or the Berkeley growth study. They'd be much more focused and try to get a particular group in a particular kind of circumstance. But at that time this sort of systemic collecting of behavioral information and physical anthropological information was really very fruitful. They learned an awful lot about how teeth developed and how the muscles developed in this kind of broad way.

Sameroff: So they'd measure body size, weight, muscle strength, and then there some behavioral things, like IQ testing.

Baldwin: That's right, and behavioral records. At Fels, the kids of one age all came to nursery school for a month every fall. We kept detailed behavioral records of their nursery school behavior. Every child came in every six months, on their birthday and half-birthday. During the visit, my job was to interview the mother about the child's development. We also had home visitors who went out and observed the mother in the home. They would rate the mothers on 30 behavioral scales. It was a very detailed and complicated set of data.

Sameroff: When did the study start?

Baldwin: It started in 1929; I came in '41, so that it had been going twelve years.

Sameroff: They were taking kids continuously? Or was there a cohort that they chose and followed up?

Baldwin: No they took new subjects anytime. We tried to recruit any sibling of a Fels child when he was born and also we accepted new families if they volunteered. I think it was about a dozen new cases every year. We followed the old ones but added twelve new ones every year.

Sameroff: So it'd be roughly 144 or so.

Baldwin: By 1941 that was about the size of it.

Sameroff: Why Yellow Springs, Ohio?

Baldwin: Well, because, there probably were some other reasons, but it was a very stable population. So that we had very few people that moved out. We had a good many rural families. We had intellectual faculty families from Antioch College, so it did provide really quite a nice range of socio-economic status, except we didn't have any families that we now call "urban underclass." But we did have poor people, usually the rural poor.

Sameroff: Were they affiliated with Antioch at all, or --

Baldwin: The Institute was part of Antioch, and there were a number of Antioch faculty families in the study, but the rest of the sample had no relation to Antioch College. It became part of Antioch when it was set up in Yellow Springs. This provided, you know, an intellectual outlet for Institute members. I was an assistant professor of Antioch all the time I was there. The staff all had Antioch appointments, and I occasionally taught courses at Antioch. But it wasn't picked because it somehow fit the Antioch philosophy, which was also unique in the way of an undergraduate college. I really don't know exactly how Mr. Fels and Antioch got together.

Sameroff: But was this usual for graduate students at that time, to go off to research centers, or did most people go into more academic settings?

Baldwin: Most new Ph.D.s went to academic settings. By going into a research program I was able to publish quickly. Plenty of research data were already collected. I did not feel the pressure to publish or perish. But I did not choose Fels for that reason. Afterward, I realized that I had avoided the publish-or-perish pressure.

Sameroff: How long did you end up staying there?

Baldwin: Well I stayed there till '49.

Sameroff: So about eight years?

Baldwin: But two or three years in the middle of that I was down in Florida doing research on radar bombing.

Sameroff: Let's hang on a minute. We're moving to Fels, you've left Harvard behind, and now you're beginning your career as a child developmentalist, or were you thinking that way? Or were you just seeing yourself as a psychologist interested in life stories? When did you start thinking of yourself as a child person, or was that again further down the line?

Baldwin: Oh I must have started thinking of myself as a developmental psychologist from the very beginning. I'd been there about six months, struggling to work my way, trying to figure out how to apply my doctoral thesis to these black books. Actually, then I did do a very similar kind of research on day-by-day longitudinal records of nursery school behavior using the same procedure that I had with Jenny's letters. And that got published. The home visiting program at Fels had already been started. Horace Champney was running it. He had devised all of the rating scales, and the procedure for collecting the data, and the rating of it. And so there were three or four years of data already sitting there. After I'd been at Fels for about six months, Champney left. Sontag gave me that data and shifted my job from the analysis of black books to the analysis of home visits. I inherited this tremendously rich batch of child development data. Out of the analysis of that came the monograph "Patterns of Parent Behavior." In the next year or so I actually did research based on my thesis, but I never did any research on the black books; that was the reason I had been hired.

Sameroff: So you had published a number of *SRCD Monographs*? No, this was before SRCD.

Baldwin: "Patterns of Parent Behavior" was in *Psychology Monographs*. And I guess the next one was also. It was more of methodological study, "The appraisal of Parent Behavior." And there were a number of little papers.

Sameroff: What would be the general conclusion?

Baldwin: Well in the "Patterns of Parent Behavior," I analyzed the intercorrelations of the 30 parent behavior scales. I used a procedure called "Syndrome analysis." It was not factor analysis, but resembled it in some way. We found three or four patterns of parent behavior and began to do a little bit of the analysis of what the children were like who were raised in these various patterns. But it was essentially to classify parent behavior into a small number of patterns.

Sameroff: Were there other people who were considering patterns of parent behavior?

Baldwin: Oh, yes, but not in 1945 when "Patterns" was published.

Sameroff: So there was a group. So you began to interact now with these --

Baldwin: No we didn't interact face to face in the '50s. I mean, everybody was publishing and I did interact, but I was pretty much doing this on my own. And I didn't entirely agree with the various patterns that other people were proposing.

Sameroff: But in terms of the field, everyone was part of the American Psychological Association -- would that be the main professional organization? And these were APA journals at the time?

Baldwin: Yes, that's right. SRCD existed since the '30s, but it was a multidisciplinary group focused on the description of child development. Tom Richards, who was the head of the psychology department at Fels when I joined it, was secretary of SRCD. *Child Development* was a journal, and *Child Development Monographs* was a journal. I don't think the *Abstracts* had been started yet, but I am not sure. As secretary, Richards was editor of all of the child development journals. After I had been at Fels a year or two Richards left and I became head of the psychology department. He had introduced me to SRCD. SRCD was much less concentrated on psychologists then. Physical anthropology and medicine and pediatrics were all represented, and at that time SRCD was genuinely a society interested in the development of the whole child and all aspects of child development.

Sameroff: Now this was kind of reflected in the organization of Fels.

Baldwin: Sure, I guess I hadn't thought of it, but yes.

Sameroff: Or were these other research centers -- Iowa, did they have that kind of multi-disciplinary representation?

Baldwin: Well California and Cleveland did, Iowa didn't. It was much more focused on behavioral development. And of course Iowa had Lewin there for a while. SRCD was certainly full of, not just behavioral research, but pediatric and anthropological studies.

Sameroff: When did you go to your first meeting?

Baldwin: It must have been '41 or '42, probably the first year I was at Fels. It was in Chicago. And it was much more like a medical society. I remember that one of the baby food companies gave a breakfast for the whole society. It was a small group, a few hundred members.

Sameroff: So here you are now at Fels and you're beginning your child development career. And you published your monographs, and following this longitudinal data set, and was that what you continued to do at Fels?

Baldwin: That's right. Using these home visit data, I looked at the differences in childrearing of first children and second children. I looked at the effect of the mother's pregnancy on her behavior toward an older child. I wrote a series of small articles that tried to exploit this data.

Sameroff: What would you say would be the biggest contribution to the Fels years, in terms of your own work? Or the things you would see as --

Baldwin: Oh I think it was the patterns.

Sameroff: At that time, in addition to identifying the patterns, were they showing relations to outcomes for children?

Baldwin: To some extent, yes. We soon discovered that the Fels sample was heavily weighted with intellectually academic parents because so many of them were connected with Antioch. We did have a group that weren't, but we had a small portion of the real range of patterns of parent behavior. Particularly, we didn't have any really poor urban families and almost no black families.

Sameroff: Well you weren't alone because the rest of psychology didn't either.

Baldwin: No we weren't all alone, but I began to realize what a very limited sample we actually had. The bias was not as obvious in physical growth or tooth development or psychophysiological development.

Sameroff: Did that have any influence on your leaving Fels?

Baldwin: No, I left Fels because I was invited back to Kansas where a new group of Gestalt psychologists had collected. Roger Barker, Herb Wright, Fritz Heider, people I had known in the Topological Society. Wheeler was no longer chairman. Roger Barker had been invited to chair the department of psychology and he started out to recruit a group of topological field theory psychologists.

Sameroff: You had this great longitudinal sample and yet you were willing to leave it. Did they have appropriate inducements? Or you just felt like going home again, or that's too personal?

Baldwin: No, it was that I liked these people; I had known them and I was excited about working with them. And I still was interested in field theory. The research I planned at Kansas wasn't particularly developmental research.

Sameroff: You were thinking that this is a change in the work you were going to be doing? Going back to field theory?

Baldwin: I guess so, but it wasn't all that deliberate either. There were personal reasons. I did like these people and I felt kind of like I was moving into a place that was going to be exciting and to make its name.

Sameroff: So in 1949 you pick up and move to Kansas?

Baldwin: Yes.

Sameroff: We jumped over the war years. You did a three year stint in war research in the middle of your career?

Baldwin: Yes that's right. From '42 on I was draftable, so that going into some kind of military research was an alternative to being drafted in the army, and so I was hired for the National Defense Research Council research project located in Florida. Radar was not quite top secret, but it was highly confidential at that time. This group of people was doing research on radar bombing, mostly on how to train bombers to do radar bombing. I joined that very interesting group. Donald Lindsey was head of the group. There were some half dozen other psychologists from various universities around the country. We were assigned to various jobs that needed to be done. I worked at an airbase in Florida writing a workbook on radar bombing used in training bombardiers. Another time I did the statistical analysis for a comparative study of two different bombsites. We were all friendly and everything. The biggest single research I did was to analyze data on a bombsite. The Air Force wanted to know how accurate the bombsite could be. They would measure the distance from the spot the bomb hit to the target. It was my job to analyze this data.

Sameroff: Was this completely a sideline?

Baldwin: I think that it was almost completely a sideline. In the comparison of the bombsites I had to learn some statistics about circular error that was in two dimensions rather than one.

Sameroff: Ok, let's move on to more child development years. Ok, so now we make the transition to Kansas.

Baldwin: I came back to Fels and I was there for another 2 or 3 years, and then I went to Kansas. Fels acquired a chemistry program and a real physiological program. The psycho-physiological program was blossoming.

Sameroff: This was before Stanley Garns?

Baldwin: Yes. Stanley Garns hadn't yet come to Fels at the time I left.

Sameroff: So that longitudinal study went on. And now you're moving to Kansas. How long were you in Kansas?

Baldwin: Four years. So that it was a combination of a group of field theorists, Gestaltists, topological psychologists, but at the same time it had a clinical training program that was closely affiliated with Menninger's in Topeka. We had clinical graduate students who were essentially being trained by Karl Menninger. Most of them were not very interested in topological psychology. They went into clinical psychology. Of course, they were very much indoctrinated with psychoanalytic theory. About that same time the Murphys moved to Menninger and Bob Holt was there. They were not particularly psychoanalytic. It was a very interesting mix when the Menninger psychologists and the Kansas psychology faculty met together. It was complicated in terms of administration. I had a number of clinical theses that I directed.

Sameroff: What kind of position was yours?

Baldwin: At that time there was a clinical program and a non-clinical program at Kansas. So I was in the non-clinical program. And this included all kinds of people.

Sameroff: So what kind of research did you do at Kansas?

Baldwin: Actually, I did not do much research because I became chair of the department after a year. And I really had a lot of students that I was supervising and I was writing a child psychology text. I got to know Fritz Heider there, and became very fond of him and very much influenced by him. I really didn't get much research done.

Sameroff: Beginning psychology?

Baldwin: Yes. And I taught statistics. And then, also, I became involved in an NIMH study section. So at Kansas, and later at Cornell, I spent quite a lot of time reviewing proposals for study sections and doing site visits.

Sameroff: How old were you when you went to Kansas?

Baldwin: I was 35.

Sameroff: So you weren't ready for your mid-life crisis yet? You were just getting into administration, and what happened four years later?

Baldwin: And then I was invited to Cornell, to become head of the department of human relations.

Sameroff: Were they building a program?

Baldwin: I guess being the head of the department was attractive.

Sameroff: Now you were chairman at Kansas at the time?

Baldwin: I was, yes. But I felt very much as if Barker was really the head. I wasn't at all jealous of him, but I became chair because he was really too busy to do the administration. He was working on the small town labeled Midwest and all the kids in that. And he went to England on a sabbatical. He had a real research career to attend to and chairing departmental meetings were just not very exciting to him.

Sameroff: What was your rank at the time?

Baldwin: I became a full professor.

Sameroff: So that was at --

Baldwin: 35.

Sameroff: 35. That's a good developmental period you had at Fels. And even though you didn't see yourself as a child psychologist, obviously people thought of you that way, because now Cornell is saying we want you to head up our child and family program.

Baldwin: Yes. So it must be true.

Sameroff: You were going to head up the new program.

Baldwin: It wasn't a new program; actually, it was an old program. But Cornell was a curious place administratively, particularly in psychology. The psychology department had been Titchener's baby for years so that child psychology had gone into home economics and educational psychology had gone into education. Kurt Lewin, when he came to this country, went to Cornell, but not in psychology. He was in child development and family relations. The department of child development and family relations was in the school of home economics and was very much a child development program, heavily psychological, but also with a strong emphasis in sociology. It ran a nursery school and it was certainly heavily oriented towards teaching. It ran an extension program; we had consultants in the extension program going out and visiting all the counties in the state about their child development programs. Other departments were food and nutrition, home economics education, housing and design, clothing. It was a very interesting and different kind of setting for a psychologist. Bob Dalton, the previous chair, had set up the child development program a number of years before I went there. He stayed in the department but he wanted to get out of the chairmanship.

Sameroff: It was like a post-war department? Around 1946 or so?

Baldwin: Yes. His resigning the chairmanship but wanting to stay as a member of the department led to the vacancy that was offered to me. There were other psychologists: Urie Bronfenbrenner, Mary Ford, and others. We interacted with the psychology faculty a good deal. About the time I came Bill Lambert came to psychology, and he and I became very good friends and worked together. We helped plan a big cross-cultural study with John Whiting and Bea Whiting, who were at Harvard, and Bob Sears, who was at Harvard before he went to Stanford. I was also invited to join one of the Social Science Research Council committees, where I got to know Bob Sears quite well.

Sameroff: Had the Social Science Research Council been around for a while or was that new at the time?

Baldwin: No, it wasn't very new.

Sameroff: And so they were getting interested in some child development programs?

Baldwin: Yes, partly it was child development and partly it was because I wasn't necessarily seen as a developmental psychologist, I think more of a general psychologist. Anyway, I got connected with an idealistic program of 100 parallel studies of 100 cultures. Bob Sears and John Whiting were two members of the SSRC committee. John and Bea Whiting got a grant to do a five-culture study. Bill Lambert and other people at Cornell were in charge of the study in India. I was not part of that team but I helped plan the study. In some ways the study of Midwest by Barker and Wright at Kansas was comparable, so I was able to help plan the India study.

At Cornell, my own research was on pride and shame. I had a grant for it and hired Harry Levin to work on it. I was also working on a text that I had started at Kansas, *Behavior and Development in Childhood*. So I was busy.

Then I was also doing research on some ideas of Fritz Heider's about "naïve psychology" as a foundation for scientific psychology. I was trying to make it more systematic but still keep it naïve psychology. And I still think that that's a very promising line of development for child psychology. So that out of that came my research on kindness and how kindness is perceived and what's perceived as kind and what isn't.

Sameroff: Those kinds of variables, I'm trying to think of what category to stick them in. I mean some of them, when you talk about shame and things, people would put in the emotion category.

Baldwin: Personality traits, I guess.

Sameroff: Personality. Personality, of course. Something we don't talk much about these days.

Baldwin: Personality. So the first study was with college students. And essentially I'd say, "If somebody decided to benefit you instead of harm you, how kind is it? If somebody decides to benefit you and harm himself, when he could have benefited himself and harmed you, how kind is that?" I predicted, as anybody would, that it would be seen as kinder if a person sacrificed his own welfare to benefit somebody else, than to just benefit the other person. It's very simple minded, but of course it worked out. And then we looked back on kids and found that they were very sophisticated too. So that three- or four-year-olds would agree perfectly well, that to harm yourself and benefit another person was kinder than to benefit the other person and benefit yourself also. That study was published in the late '50s.

Sameroff: Did you have a grant to do that?

Baldwin: No I just did those studies with graduate students. The first grant I got was on pride and shame. It was the first NIH grant I ever had.

Sameroff: So was Harry Levin like a post-doc then or was he more?

Baldwin: He was a research associate and then fairly soon began to teach. Pretty soon he became a regular faculty member of the child development department. Later he shifted to the psychology department, and later became Dean of the college.

Sameroff: So at Cornell now, you're an administrator, you're working with many students, and you've got this interest in personality. Would you say that your concern with children was to see how their behavior related to the adult behavior? Was there some point where you were doing something developmental, in the sense that there was something different about kids, or hypothesizing something different? Or were these mostly models of adult personality?

Baldwin: At that time I kept being so surprised how smart kids were, how sophisticated they were about concepts like kindness. But we would find some complicated situations in which there would be real development. Most of the time when you translated these kindness situations or shame situations into childlike situations, they responded very much like adults. Parents were often surprised at how sophisticated their children were. They had no idea how their children became so sophisticated. And I still think somebody ought to exploit it more now. And then I was quite active in SRCD then.

When I was at Cornell I was a member and later chairman of a study section. I did many site visits and got to know a lot informally about the status of research in personality. Those were the golden days in terms of getting grants, in how freely the money in a grant could be used, and for study sections, how freely a member could site visit.

Sameroff: I should say there's a lot more money now but it seems to go a lot less far.

Baldwin: Yes, absolutely. And at Cornell we got to use our overhead for research.

Sameroff: So how long did you remain at Cornell during this period?

Baldwin: I was there from '53 to '64.

Sameroff: And how did that period end?

Baldwin: I met Clara; we got married. Cornell had a rule about nepotism. So it was time to move on. We went to NYU.

Sameroff: And what kind of position was that?

Baldwin: I was a psychology professor and she was in the school of education.

Sameroff: Was there a developmental program or a child program at NYU?

Baldwin: I was professor of developmental psychology, but there was no strong child program. People were studying developmental psychology, but it wasn't like the child development department at Cornell.

Sameroff: Was there a group or just people in the department?

Baldwin: The whole department was just individual people. There wasn't a very strong departmental feeling. I made good friends with some individuals, but I never had any feeling of a strong department. There was a very strong social psychology program. Moe Stein, George Kleinart, and Bob Holt were there.

Sameroff: Doing clinical research?

Baldwin: Yes.

Sameroff: Is that before Holt went to Menninger or after he left Menninger?

Baldwin: After he left Menninger. He was at Menninger when I was at Kansas. Actually I first got to know him at Harvard. I think he was a graduate student at Harvard in 1940 or '41.

Sameroff: So what kind of work were you doing when you were at NYU?

Baldwin: We began the first longitudinal study of our own. We were interested really in how parents interacted with children and tried to do actual observations of it and were very much interested in the methodology of it. We compared a group of Washington Square parents interacting with their children to a group of Harlem parents.

Sameroff: People in Washington Square were mostly university affiliated?

Baldwin: NYU is in Washington Square, but the people who lived near weren't all university people.

Sameroff: Certainly middle class.

Baldwin: Oh yes, middle class. And then we had a Harlem sample that was a part of Frank Palmer's Harlem sample that he was studying. We were very much impressed by what good parents Harlem people were. This was the first time, I think, that I had ever gotten to know real lower-class families. And so that in many ways ever since, I mean as I've thought about this interview, it seems to me that one of the things that has happened ever since then, is that I keep finding people that you don't suspect are going to be, they are somehow not going to raise children very well, and they turn out to be doing a great job of it. And just a great deal of wisdom.

Sameroff: So you were doing nursery school observations.

Baldwin: These were nursery school age children, but they weren't observations in nursery school.

Sameroff: Oh, they were coming to the lab?

Baldwin: They were coming to the lab and then we'd give them some toys and let the mother and child play. We were very struck by how many of the Harlem mothers were focused on getting that child to read and to succeed in school. The Washington Square mothers were relaxed about academic skills.

They were more likely to interact with the child at the level of fantasy, like building a bridge across the Atlantic Ocean and such. Then at the same time, we did this same sort of free play at the hospital with disturbed kids and their mothers.

Sameroff: These were psychiatrically disturbed children?

Baldwin: Yes.

Sameroff: Was this at Payne Whitney?

Baldwin: Yes.

Sameroff: But you used the same tasks and were videotaping?

Baldwin: Yes we videotaped them. One of the methodological things that we were trying to do at the time was to narrate interpersonal interactions in a standard way, but not an artificial way. We wanted the narrator to have the freedom of ordinary English, but to use enough restrictions on the grammar that we could actually do a computer analysis and count the number of requests by the number of times the mother gave the child information or criticized the child. We tried to have categorization of these words, that each implied that somehow the mother wanted the child to do something, or express positive affect, or she ordered him. So we tried to really use ordinary English as a language for observing and recording, but be able to computerize the narration. English grammar is so complicated that we had to also make it somewhat standardized.

Sameroff: So this was kind of a pioneering use of computers, in a sense, because most people were doing time sampling of individual behaviors.

Baldwin: It was trying to use English, trying to naturalize the observation so that you could use the observer's skills as an observer to tell you more about what the child was trying to do, then you could get with a time sampling in a pre-categorized system.

Sameroff: And this was so you would be able to have a computer do the analysis.

Baldwin: When you get into time sampling then right away the size of the data set becomes so horrendous that you need computers. We would type into the computer an English language narration and use a program to reduce it to a set of counts of categories.

Sameroff: How long were you in New York?

Baldwin: Four years: '64-'68.

Sameroff: Then you went back to Cornell?

Baldwin: Yes, as the Director for the Center of Research and Education.

Sameroff: Had they gotten rid of their nepotism rules?

Baldwin: No, but we were in different departments, so it didn't count as nepotism. I was in psychology and Clara's position was in child development. And also at that time, Harry Levin had persuaded the university to relax on nepotism. Jackie and James Gibson were both professors of psychology.

Sameroff: So that was breaking the nepotism barrier?

Baldwin: Yes, that's right.

Sameroff: So there was hope at Cornell.

Baldwin: Once I got there, I ran into trouble; we had some funding for the center, but we didn't get other funding. There was not much support from the School of Education, and it wasn't quite clear what its role was. The president, who conceived the Center, left. I'm not sure how it would have worked out had he stayed. Anyway, after just a couple of years, I moved into psychology as a professor.

Sameroff: What kind of new research program in psychology were you doing then?

Baldwin: Norman Garmezy was a visiting professor at Cornell. And at the same time he was working with the department of psychiatry at Rochester with John Romano. They were setting up a research program on the effects of the hospitalization of a parent on the pattern of family relationships and on the psychological development of the child.

Sameroff: So this was going to be a project looking at the offspring of patients and seeing what kind of effects of being in these kinds of families had on the children.

Baldwin: I guess today we'd call them the protective factors in families with a psychotic mother or father. I remember one mother who was very psychotic, but the child's crib was absolutely neat, orderly, and clean. The way she treated the child was not very different from any other mother. The fact that she had hallucinations and was nutty the rest of the time didn't really affect the way she was a parent. We had tapes of the patient and the spouse interacting with the four-year-old child. If you showed the tape, nobody in the psychiatry department could identify who was the patient and who was the spouse. So that it just furthers this notion that there are lots of things about raising children that people who have various kinds of disadvantages, even when they are quite sick, don't necessarily show it in their child-rearing. Some of them do show it in their child rearing. This problem has intrigued us for the last ten years.

Sameroff: Was this your primary involvement in the University of Rochester Child and Family Study, during this period at Cornell?

Baldwin: Yes, that's right. We were also observing families at Cornell, in an observation room there. And we were also going down to the Paine-Whitney Clinic in New York and observing mother-child dyads there.

Sameroff: Using the same methodologies?

Baldwin: Yes, pretty much.

Sameroff: And what came out of that period?

Baldwin: Mostly just wisdom, I think. There were some publications out of the University of Rochester Child and Family Study that we reported in papers, or at society meetings.

While I was at Cornell, I wrote the theories book.

Sameroff: *Theories of Child Development*. That was a good use of time.

Baldwin: And then that was later revised. I guess we were at Rochester when we revised it. Well I remember the theories book was a very popular book. It sold quite well.

Sameroff: Did you get a new car or anything out of that one?

Baldwin: Nope. Put some kids through college on it.

Sameroff: The beginning of the involvement with the Rochester group eventually ended up with your actually moving to Rochester.

Baldwin: Yes, that's right.

Sameroff: So what was going on in Cornell in child development during that period you were there? That seemed to have a fairly large group of people working in the area.

Baldwin: Yes but now, really, Jackie Gibson was a developmental psychologist and was very active, but I wasn't involved in that at all. And there was research going on in the child development department, but I wasn't involved in that at all particularly. Bill Lambert was working on the cross-cultural study and I was somewhat involved in that.

Sameroff: You really never talked about that, what did that study involve? That was the one with Sears?

Baldwin: Yes, that was with Sears and Whiting.

Sameroff: That began during your first period at Cornell?

Baldwin: Yes.

Sameroff: That was through the SSRC group?

Baldwin: It was a comparative study of child rearing in six cultures. The Whittings actually supervised the data collection and family child interactions in six cultures. Bill Lambert and Leigh Minturn became involved in one of the six cultures, a town in Northern India.

Sameroff: This was the six cultures study. And what was your involvement in that?

Baldwin: Mostly I helped plan it. They were observing mothers and children interacting in a naturalistic situation, so they were doing exactly the kind of thing that we were doing. We put all of our heads together planning the study, but I was never an active researcher in that study.

Sameroff: So this was really one of the first controlled anthropological studies where people went into different cultures and used the same methodologies instead of naturalistic description. Cornell didn't qualify as a culture in this sense? The American one was something called Orchard Town, or something?

Baldwin: Yes, I think it was a place in Maine. They didn't want to have a university town so that, I mean, Harvard didn't qualify either.

Sameroff: They wanted the American town to be roughly comparable to the samples in other societies.

Baldwin: The observation and sampling was a much more complicated problem than ours where we were just observing an individual family, really just observing a mother and a child. They were trying to observe community situations, so it was a much more difficult problem.

Sameroff: Basically you were contributing your wisdom to the design.

Baldwin: Yes. The eventual publication of the six cultures study was, again, a very interesting perspective on child development.

Sameroff: Now, you're going to move to Rochester, the final episode in your academic odyssey, where you are going to engage in a new area of research, the longitudinal study of mentally ill mothers. Could you tell us something about that line of research?

Baldwin: Yes, we were continuing this analysis of the behavior of mothers with their kids, using this simplified language to describe it. And so our part of the research in the department of psychiatry was to see each of the families, the patient and the spouse and the child. I guess I'd better go back and describe the study. The samples were groups of four-year-olds, seven-year-olds, and ten-year-olds who had a parent who had been hospitalized. It was a longitudinal study repeated three years later when the four-year-olds were seven, and the sevens were ten, and the tens were thirteen. We observed the patient and the spouse (if there was a spouse) and the child in a free play situation, which then we videotaped and described using this simplified English language, and then we proceeded to analyze it in terms of the frequency of different kinds of interactions and different kinds of requests. And thus make an analysis of family interaction. That was our part of the data collection. We began to do this before we ever moved to Rochester academically. We commuted from Ithaca, on occasion, and we'd run data. We continued to do this after we had actually moved to Rochester and where I was in the psychology department. The data led to several articles in books and in journals. But the data had never really been completely analyzed.

Sameroff: What were the conclusions that came out of that set of studies?

Baldwin: In general I think that it was that, at least our conclusion was that, the patient who had been hospitalized did not interact very differently with the child in the play situation than anybody else. They didn't stick out like a sore thumb as being crazy. And, in fact, in some cases they were so similar that we actually showed tapes to psychiatrists and asked them to pick out which, whether the mother or the father, had been the patient. And they were completely unable to identify the one, even though these had been seriously ill people. In one case it was very striking that the father's interaction with the child did not look particularly abnormal, but on the side he was reporting to the mother how he had answered some of the questions in the interviews in the other parts of the study, and his answers were obviously psychotic. We knew that he obviously was the patient, but you couldn't tell it by the way the family interaction moved. It was as if his interaction with the child was an isolated bit of behavior, which is very curious.

Sameroff: Did you see any difference between the four-, seven-, and ten-year-old children interacting with their parents? As they got older were they more sensitive to the parent's mental illness?

Baldwin: I don't remember seeing any evidence that they were sensitive to the parent's mental illness at all. In this situation, which was of course not stressful, these families amazingly just acted like any kind of family that you might see. I mean, there were emotional scenes and kids occasionally had tantrums, but it happens in all families in observations.

Sameroff: Was it your participation in this research project that motivated your move to Rochester, or were there other issues?

Baldwin: Oh, it was partly that. It was partly that we could get a pair of jobs, my wife and I, that made the university attractive to us. There was an ongoing developmental psychology program with a number of people in it: Arnold Sameroff, Michael Chandler, and David Elkind. At Cornell, I had felt somewhat isolated from the kind of developmental research that I'd done before.

Sameroff: You just wanted to get out of the Ithaca snow belt, is that it?

Baldwin: Into the Rochester one.

Sameroff: Into the Rochester snow belt. So at Rochester, you completed your psychiatric work and then --

Baldwin: And then, just about the time I retired, the Rochester Longitudinal Study, a different study entirely, got the money to study their sample at age thirteen. This study was headed by Sameroff. It was a group of families that had been recruited around 1970, when the parents had been interviewed and the child had been tested in early childhood and then again at four, and they were now going to be seen at age thirteen. Clara and I took responsibility for that round of observations and again at eighteen. And we are now in the midst of analyzing the eighteen year data and the longitudinal aspects of the study in collaboration with Sameroff and Melvin Zax.

Sameroff: So essentially, Rochester is where you ended, or are ending, your professional career. No more moves anticipated?

Baldwin: No. I think not.

Sameroff: Well, in reflecting back over your research history, what do you think were the significant continuities in your work? And conversely, what were the major shifts that you considered to be significant? You've already talked about a number of these, but just looking over your whole life, how would you see these?

Baldwin: Well, the continuity is clearly longitudinal studies. I never particularly intended to make a career out of longitudinal research. The cognitive studies were what I felt was what I had to contribute, but the cognitive studies never became a major research program the way the longitudinal studies have.

Sameroff: And do you think about any times when you shifted the way you thought about things?

Baldwin: Well, certainly when I moved to Kansas, then I quite thought that I was really shifting into a kind of cognitive social research program, which might be developmental. Then at Cornell, since I was in a child development department, I continued to do the same kind of research, but always with the notion of doing it with adults and then seeing how children develop the adult point of view. When we went to NYU we really got back into longitudinal studies and developed an observational technique that we've used frequently since then. The Rochester Longitudinal Studies have not been directly observing mother-child interaction. We've been interviewing the mothers about their mother-child interaction.

Sameroff: Given this history of research, would you reflect for a moment and say what you feel are the strongest parts of your research program and your theoretical contributions, the impact of your work, and its current status. Feel free to be as egocentric as you want. History will certainly round out what you have to say.

Baldwin: I personally feel that the work based on Heider's ideas about naïve psychology is potentially quite important. For a time, any cognitive theory faced much skepticism just because it was not purely behavioral. Then when cognition became a more respectable concept, the cognitive psychologists were careful that the concepts were operationally defined. One paper I wrote was never accepted because the editors said it was "just common sense." The terms were operationally defined, but it seemed too much like naïve psychological thinking. I personally feel that psychology would be better accepted by society if it sounded more like "common sense."

The longitudinal research has consistently found that parents who are "disadvantaged" by race, or poverty, or mental illness do not raise their children in some clearly "abnormal" way. When observed, they look very much like all the rest of us parents. The "abnormal" outcomes of the children still is not very predictable from looking at descriptions of parent behavior, although we have made much progress since 1940 in understanding the outcomes of child rearing.

Sameroff: Well, those certainly are important implications, and your research certainly took on a quality that had implications for applied research and applied programs, especially the work with what we call high-risk families now or multi-problem families. Which papers would you say represent your best thinking about child development?

Baldwin: Well, the empirical work in the two *SRCD Monographs*: the analysis of family free play and the role of the patient in family interaction.

Sameroff: That's the one that you and Clara and Bob Cole collaborated on?

Baldwin: Yes, and also the first monograph on "Patterns of Parent Behavior." We actually were the editors of the monograph in which there were other articles that we, the three of us, wrote three chapters of it.

I think in terms of general contribution to the field, that the theories book was very influential. It didn't influence in the sense that being a theory, but I think it clarifies. Students who read it and had it as a text found that it clarified theoretical relationships among various theorists. So that I expect that I'm more likely to be remembered for that book than for any specific empirical research that I did. Well, there had been conflicting theories and there had been attempts, in the field of personality, to write books about the contrast between scores of different theories. But I tried to not do too many, and I tried to really get inside the theorists. And I think that because of my contacts that I really felt I was inside Lewin. I knew really what he was trying to do. With the contacts I'd had with so many people, like Sears and so on, I think that the analysis there, of behavior theory, captured what they were trying to do. I think that the chapter on psychoanalytic theory was probably the weakest. I don't think I still really understood what Freud was trying to do. And particularly what his theory of child development was about. I'm not sure he did. So that, in many ways, the text has been the most rewarding intellectual job I've done.

Sameroff: Well, let's move on out to SRCD. I know you've had a role to play in that. You'd mentioned that your first meeting was in the early '40s. At that time your main interest was in the topology group. Now you were beginning to participate more in the child development group.

Baldwin: When I was at Fels I went to the meetings. The first time, I was really just kind of getting acquainted with the people. Then, after I began to report work on parenting behavior and reported to SRCD and to Division Seven of the APA, I think I became known then as a worker in the field of parent-child relationships. And then gradually, as I did site visits as a member of a study section, my acquaintance broadened and frequently it was among child development people. I became better known within the Society and eventually was elected president of it.

Sameroff: What year was that?

Baldwin: Yeah, it was somewhere in the early 1960s. It met in New York City when I was at NYU. The society wasn't very big, it had become more and more a society for developmental psychologists, rather than for developmental anthropologists, or pediatricians, or developmental biochemistry. It was trying very hard to broaden out and not to become just a psychology society. That was something that we were very concerned about.

Sameroff: This was while you were president-elect?

Baldwin: Yes, that's right. I was elected president-elect and I was about to become president this meeting. So I was just inaugurating my tenure as president. And Bill Martin, who had been secretary and treasurer and editor of all the Child Development journals, resigned at that meeting. And so, my task, along with the incoming board, was to find some kind of a replacement, or to make new arrangements about handling the editorial jobs, administrative jobs, of the organization. I think it was

then that SRCD first developed its contact with the University of Chicago. But the society at that time kind of became much less a personal responsibility of a single person. Responsibilities were spread out much more. And it became much more of a professional organization like APA and other professional societies. Before then it was more of a “mom and pop” operation, with one executive director handling all the business activities and the journal.

Sameroff: So then, in your presidency, you had mentioned that there were concerns that the organization, which had originally been multi-disciplinary -- pediatrics, a lot of medical disciplines, anthropology, physical anthropology -- was becoming very much psychologized.

Baldwin: That’s right. And I think this organization has been worried about that ever since. It tried as best it could to invite other people in. But, it has attracted so many developmental psychologists that now all the others make up a small minority of members.

Sameroff: Do you see that as a continuing problem in SRCD, that weakens the organization? Or is it something that we shouldn’t be worried about?

Baldwin: I think it’s too bad that we don’t have a more interdisciplinary group, because many of the most influential people in society’s treatment of children have been pediatricians like Julie Richmond. I think the advantage of it being broadly interdisciplinary is obvious. But, at the same time, I don’t see any realistic way to accomplish this, because we certainly don’t want to exclude developmental psychologists. And I think the Society has really, even though its membership has been concentrated, has always been trying very hard to keep integrating with other developmental fields beside psychology.

Sameroff: So that they did have a broad range of interest?

Baldwin: That’s right, and I think it’s been a stronger society, because it tried to.

Sameroff: Did you have any other roles within SRCD? Any other offices you held?

Baldwin: No, I don’t think so.

Sameroff: I know recently you got some award.

Baldwin: That was from Division Seven.

Sameroff: Oh, that was APA. But I don’t think you mentioned that. We didn’t really get through awards, but I think it was a major recognition of your contribution to the field from the developmental psychology division of APA, giving you its Scientific Recognition Award.

Well, moving away from SRCD to the field of developmental psychology in general, what do you think were the major continuities and discontinuities in the field during the period that you were active as a professional?

Baldwin: I think certainly the thing that people talked about most was the shift in what was popular research. Lots of people talk about the marble-dropping-experiment period. Psychoanalysis was influential in the ‘40s. Then in the ‘50s and ‘60s, because Hull and other behaviorists were dominant, then gradually other kinds of research took over.

I don’t really feel as if developmental psychology has been faddish, except in a few cases. I mean, the good research that gets done attracts other research like it, so it’s not particularly bad to be faddish. It’s taking advantage of things that work. But fads do, to some extent, produce a big quantity of output in fairly small fields for a while. But by and large, I feel as if SRCD has been a discipline with breadth and with concern for very important things. And has dealt with and thought about important

social issues and health controversies, so that I'm very proud of having been connected with SRCD as long as I have.

Sameroff: Have your views concerning the importance of various issues changed over the years?

Baldwin: Oh, sure. I think that disadvantaged kids, and particularly understanding how that leads to crime, has become just a terribly important issue. And I certainly didn't feel that 40 years ago. I think a lot of people in the field recognized it as very important earlier than the general newspapers. It has also become a sort of crisis. I think research on disadvantaged people, patients, poor people, or whatever, has become more and more sensitive. And the fact that there are great numbers of disadvantaged people who function very well is something that I think we need to keep remembering. It's terribly important. And it's so easy to deal with generalities and to think that only advantaged people do well.

Sameroff: It sounds like some of your hope for the future is to keep this lesson in mind.

Baldwin: Yes indeed, I certainly do.

Sameroff: Do you have any fears for the future?

Baldwin: Oh, my. Oh, my. Yes I do. I fear for society, American society in particular. I can see us becoming a third-rate country, but I have a fair amount of confidence that we won't. I think we've been threatened this way before, in different sorts of ways. We lived through McCarthy and we live through a lot of bad things. But this is certainly, I think, a real threat, and one that hopefully we are recognizing as important enough to deal with, and is something that SRCD and developmental psychologists can do something about. This is really up our alley.

Sameroff: Improving a lot of children through child development?

Baldwin: And particularly understanding how it leads to crime. Because it doesn't -- I mean, I don't think that poverty has always led to crime. I'm sure it's been correlated with it. But so that I think we need some insight as to what's going on, as well as some obvious kinds of things we could do to better it. And we should certainly try to better the life of disadvantaged people.

Sameroff: Well, I think that pretty much covers your professional history and your professional participation. I don't know if you feel free to tell us if there is something about your personal history with your family. Especially in the way these experiences may have had a bearing on your scientific contributions. I don't know if there were, we did talk about your parental contributions, certainly your father, anyway.

Baldwin: Of course, Clara Baldwin (my wife) being a developmental psychologist has obviously been very important. Much of the research we have done was because we both found it challenging. I would not necessarily have done exactly the same thing had I been all by myself, nor probably would she have if she'd been all by herself. So clearly that's been very important in our development.

Sameroff: Well certainly there's some difference of opinion about having a husband and wife being in the same field and actually working on the same project, and whether or not it helps relationships or hurts relationships. Have you found this to be something that can work? Do you have any perspectives on that?

Baldwin: With me, it's helped relationships. I don't know whether she would say the same thing. We've been relatively free from feeling competitive about this. Her role in our research was the things she can do beautifully, and my role has been the things I can do particularly well. And so that it's been better research because we have both been involved. And so it happens that our roles fit pretty well together. Not necessarily has to be.

Sameroff: Well thank you very much for your time.

Baldwin: Yes, indeed. You are very welcome.

Sameroff: And SRCD thanks you.