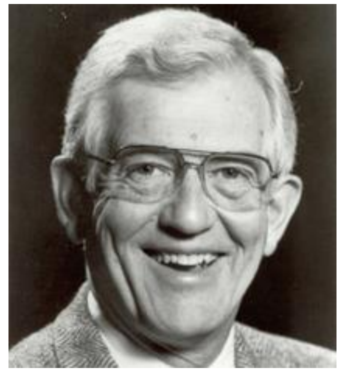
Willard W. Hartup



- Born 10/27/1927 in Fremont, OH
- Spouse – Rosemary Hartup
- B.A. (1950) and M.A. (1951), both from Ohio State University, Ed.D in Human Development (1955) Harvard University

Major Employment:

- Currently Professor Emeritus at University of Minnesota

SRCD Affiliation:

- President Elect (1991-1993) and President (1993-1995)
- Consulting Editor (1973-1977) and Editor (1983-1989) of Child Development

SRCD ORAL HISTORY INTERVIEW

Willard W. Hartup

Interviewed by W. Andrew Collins
At the Institute of Child Development, University of Minnesota
December 7, 1994

Collins: This is at the Institute of Child Development at the University of Minnesota, the date is December 7, 1994. In talking about your history in the field, let's first begin with your personal history. Tell us a little bit about your family background, and any childhood or adolescent experiences that may be of interest. Where were you born and grow up school in life? Questions like that are, I think, the basis for what we want to talk about here.

Hartup: I was born in Fremont, Ohio two years before the 1929 crash. I think in many ways I'm a child of the depression, although I did not grow up in a family that suffered immense stress during that period. My parents were secondary school teachers. We lived in a rural area in northwestern Ohio. And in areas of the country like that, school re-organization had taken place on a very extensive scale following World War I. Mainly it was the centralization of schools, or consolidation of schools as it was known. One room schools were abandoned in favor of township or multi-township schools. And my father was superintendent and agriculture teacher of Jackson Township High School. Which was a school, at the elementary school level, that consisted of four classrooms, six grades spread over four classrooms, and a secondary student body probably about 40 -50 students in six grades. So it was a relatively small place even though it occupied a modern building. We lived across the road from school, but my friends were primarily farmers. This was not in the village, this was really out in the country.

My parents grew up, my mother grew up, in the family of a Methodist minister. My father grew up in the family of a coal miner and farmer. The school atmosphere that came from my parent's interests, I think, was very much a part of our household. They never really argued in any way that I can remember all the time I lived at home, in favor of a career that stressed academics or education. Although teaching was always one of those things that was kind of "there", as something that a lot of people in our family did. My father actually wanted me to be an engineer. But I had actually no aptitude for anything mechanical, as you well know, from the struggles we've had putting the recorder together to start this interview. And I am not terribly interested in mathematics or any of those other disciplines that go into the making of an engineer. He gave that up. Actually' I don't know if he gave that up, because he was killed just before I went off to college. So I have no way of knowing whether he would approve of what I do now.

The word psychology probably never crossed my parents' lips, at least that I can remember. Certainly not as something that one did as a profession. But I think, nevertheless, having grown up in an educational household was formative in some respects. People might be interested in view of the fact that I have spent so long studying children's peer relations, just what my own were like, or at least my own perceptions of them some 60 years later. I was I think one of those reasonably popular kids, in both elementary school and in high school. Maybe even a little more than that, by the time I got to high school. Although I lacked certain skills that were very important. I'm not athletic, I am not very confrontational in any way. My social skills are conciliation, good performer academically. I had some interest in music and other kinds of things that brought me a reasonable amount of attention.

In both grade and high school I had one or two best friends. Usually they were the boys I competed with also. And I had a larger group, or social network, that surrounded those friendships, and those made up of kids of both sexes, both in elementary school and high school. So I don't think it could be argued that I became interested in peer relations from a compensatory point of view. Nor can it really be argued that I was so caught up in my relations with the peer culture that I've never got out of it.

Collins: Now when you finished high school, you went right into the military. Can you tell us a little bit about that transition and some of the ramifications?

Hartup: Well this was in 1945, actually the summer of 1945. Of all the times close in and around World War II, that was probably the time to become a member of the Armed Forces because one didn't have to be afraid that they were going to be shot and at the same time there was still enough of the military infra-structure left, that it could be used for ones socialization. And I look back at the two years that I spent in the army as incredibly important to me in that regard. It was the first opportunity I had to live away from home. I had worked, I've had a job ever since I was 14 years old. But I view the jobs one had to do, or the job one had as an enlisted soldier, as good for me. I learned a lot of things about what work is for a person somehow there. And the military provided a structure for independent learning and other kinds of things, that would be very hard to come by elsewhere. It didn't take me to very, very exotic places. I went to Panama, for example. But some of the friends I made turned out to be very important ones. Not because I had kept them as friend, I know no one now who I knew then. But I think one in particular, who was a young radical, radical I thought then. Terribly radical, because he had put in two years at the University of Chicago! And he insisted that, among other things, that I embark on a program of reading which included two main writers. One was Marx and the other was Freud. So I had a copy of The Communist Manifesto in the bottom of my foot locker for, I think, most of the year. I mean as far as I know, there were no rules against it, but it was there. Freud he didn't supply, but the USAFI collection and the day room which in Panama where I was had a copy of The New Introductory Lectures by Freud. And I really remember reading that as well as O'Neill's plays, which fit with Freud very well sometimes actually, when I was doing guard duty or other kinds of things, when I probably shouldn't have been reading. I was secretary to the sergeant major of the company that I was at. I was essentially part of the secretarial pool for the Commanding Officer of this post (Fort Sherman, Canal Zone). We didn't have very much to do then either and very often I would read instead of being at work. And I knew as soon as I'd encountered Freud that I wanted to be a psychologist. And I knew when I encountered the O'Neill that I had interest in language and in the creative use of language that I wanted to exploit.

So that when I came back to the university (I had been at Ohio State for two quarters as a reservist at the beginning of this period) I immediately went to the College of Education, and asked them if it was possible to be a double major. Well the Department of Psychology offered it's venues in the Arts College. So I started out in undergraduate school in the very same way that I have lived all my 30 years at Minnesota--administratively within the College of Education, and with a major, a teaching major, in English. But also with a major in the Arts College in psychology, which is, for people who may be reading this later, the place here at Minnesota where we have our undergraduate program. We're administratively in education but offer our major program in Arts. Well psychology was….

Collins: You went back to Ohio State, after you got out of the army?

Hartup: Yes, and that was sort of the opinion within my family, if you were interested in going to college, you did. We didn't have a lot of money, as I said my parents had been teachers so that they had jobs, or my father had a job. My mother couldn't work. If you were a married woman you absolutely could not teach in the school where I went to school. But we didn't suffer terribly during the depression. But attending a place like the University of Chicago or Harvard, is what people in some other world did. And people in our family went to the state university. Fortunately for me, Ohio State had, at that time, had a marvelous psychology department but psychology when I first encountered it in the lecture class of Delos Wickens, was not what I thought it would be. It was experimental psychology not psycho-dynamic theory as outlined in Freud. I stuck with it partly I think, because I was bullheaded and I made a commitment. And God dam it I would do it anyway. But I gradually encountered some of Ohio State's stellar people as an undergraduate. People like, George Kelly and Julian Rotter and from that, although I didn't know it at the time, I think I found my theory. And that was his kind of social learning theory.

So I was doing this and just taking a lot of courses. This was a time when the campus was flooded with veterans. People weren't paying much attention to special programs for undergraduates like honors programs or anything like that. I just took courses. Made great grades and that was it. But I ended up not knowing what the Hell to do. My work in English education, because it was English education, I threw in as much poetry writing as I could. But I decided pretty much by the time I took my degree (1950), that I would never be able to make a living as a creative writer, or an artist, much as I might have liked to have done that. I just was not enough of a risk taker to try that. So I thought maybe teaching of English was it. But I had a frightening experience early on in my teacher education program, where the kids just went wild. And I felt I never ever could set foot in a - that was a day care center - again. But it really took a ghastly experience trying to teach Dicken's Christmas Carol to 7th graders just about 45 years ago, to convince me that there was no way that I was ever going to become an English teacher. And that was in spite of the fact that the methods courses that I had in English education from a man called, Francis Howard Seeley, were among absolutely the best courses in English that I ever took in my entire life, bar none, literature, rhetoric, they were just wonderful books. And I think some of his lessons about the use of language and what we try to do with language I still try now.

Collins: So was it at Ohio State that you became interested in Child Psychology?

Hartup: Yes it was, but not until I became a graduate student. As I said a minute ago, I finished my undergraduate work not having any notion as to what I could do. So I thought, since I was in education, perhaps maybe suddenly, school psychology would be a way to go. Except that school psychology wasn't the coherent field that it is now. And I wasn't even sure that I wanted to do it anyway. John Horrocks was my major professor and he endorsed the idea of continuing my graduate work in that field. So I got admitted to the program in Educational Psychology (1950's) for a masters degree. But my interest weren't really focused so I wangled permission to take the four-quarter sequence in clinical psychology. And this was a very, very talented group was in that group and Leonard and Judy Worrell and well, Brendan Maher was the teaching assistant. So it was a very interesting group of people to work with. But I didn't know any of them really, I wasn't in that program. And Rotter taught the first course and that's where I encountered Dollard and Miller's textbook (Personality and Psychotherapy) that was just out. And we used that. And that's what I meant earlier by kind of supplying me with a theory.

But the second quarter was taught by Boyd McCandless and his subject matter was essentially mental retardation. He fairly recently had been at the Wayne County Training School with Sidney Bijou and also it was all mental retardation and that's what his course was. Which I wasn't very interested in but God did Boyd ever make you work. I mean, it was just every night in the library. I mean, we think now we have problems about supplying students with Xerox copies of the program. I think of the years of my life that I spent at the table in the library. Most of them hours between 5 p.m. and 1 a.m. And I think, wonder what we were trying to do.

At any rate, Boyd was a developmental psychologist as everyone knows, so late in that quarter, after the midterm, he called me in because I had made the top grade on the midterm, and wanted to know who I was. And so when he found out that I was kind of drifting towards a second degree at Ohio State, but drifting also in directions that were not at all clear, he said, you must get out of here. And asked me whether I'd ever thought about a career in child psychology or studying children, because he himself had his degree from Iowa and that was what his basic work was in. Well to make a long story short, by the time that quarter was over, I had the offer of an assistantship from Harvard working with Bob and Pat Sears, and Eleanor

Maccoby and Harry Levin on the Patterns of Child Rearing project which I was to take up the next fall. And Boyd continued on my masters committee and I very well remember at the time of the final oral, which took place that summer (1951), Boyd asking me to distinguish between the areas of child psychology and developmental psychology and if I could differentiate my interests according to that distinction. Well I goofed that question, I mean, no one ever told me the difference between the two. But essentially it was a fortuitous event -- a professor who took an interest in me, that opened up that whole field. Later on Boyd made a site visit to Harvard, it was for the Marshall Fields Foundation in Chicago. Charlie Spiker and some other people from Iowa came to visit Sears after he moved to the Laboratory of Human Development, the unit he was involved with at Harvard, to find out what was going on there. This must have been about two years later (1953). And he had remembered me from that time. There was nothing else we just said, hi, hello, how are you? But two years after that, when looking for a job, I applied to Iowa and he remembered me. Things were much more casual in those days. So they didn't even ask me to come to Iowa City. They instead sent Ruth Updegraff to interview me in the Sky Restaurant, somewhere in the Biltmore Hotel in Providence, where the floor show was known for women in diaphanous costumes. So we sat and watched the ladies with diaphanous costumes, (Rosemary was along) and I had my interview.

Collins: Rosemary is your wife?

Hartup: Yes. But that's something of a digression because the study of, my study of child psychology or developmental psychology essentially began when I got to Harvard.

Collins: And was your work at Harvard pretty much tied to the Patterns of Child Rearing project?

Hartup: Well it was, I think, tied almost exclusively to that project. I mean, I was shocked in some ways by what passed for the curriculum. I mean, it's still that way. And it's an alternative approach to graduate education that is a very viable one, which is to turn students loose to do whatever it is that they wanted to do. I mean, you didn't have to take any required courses. I was in the Laboratory of Human Development which was a unit of the School of Education. And stayed there partly because of my earlier education interest but partly because of where the laboratory was and partly because I discovered early childhood education before I'd been there for very long. So it was expected that because the Whitings were on the faculty, that one might take courses from them. Bob never offered anything but seminars and usually it was almost independent study seminars. I remember waiting every afternoon, two days a week, for him to wake up from his noon nap so that he and Chris Heinicke and I and a few people could have this seminar. But I got out on my own and took courses from Gordon Allport and Dick Solomon and who else, Bill Estes, who was in clinical program (that's not the Estes, there was another one). So I kind of did that because I thought, gee all of these people around here I don't want to let all that go to waste. But I'd never really worked with him in any sense. But as I look back on it my really whole education there consisted of working in the laboratory or in courses or on individual study basis, or what have you, with Bob and Pat Sears, John and Bea Whiting, Eleanor Maccoby and Harry Levin who was my thesis advisor.

Collins: Now did you do your thesis on there from the Patterns of Child Rearing Project?

Hartup: No I didn't. By that time, other people were mining that data set and it was pretty clear that not much was going to turn up by way of correlations between the child rearing variables from the interviews with mothers that were conducted, for example, and the child assessments which I had been assigned to work on. I mean, I did doll play on my hands and knees with five year olds for one entire year. We did three sessions on every child and there were 379 children in the original sample. And so we drove back and forth to Newton or went on the street car which took an awful long time, with our kits, with dolls and other things. Until I just thought I couldn't stand it anymore. But the thing is that I learned more from that experience than I ever could have learned from anything else. And largely from Pat Sears about assessment, observation of children and so on and so forth. Later on I spent another year coding the interviews from that study with Eleanor. And from her I learned some of social psychological techniques of that kind of measurement. And a lot of other things.

I never actually took any course with Harry. I just sort of fell in with him, because I had some vague notions and he encouraged them, doing an experimental analog study which were new things at that time.

Unbeknownst to me, people like Jack Gewirtz was doing it in Chicago and Gerry Patterson was beginning some things, although that was a little early I guess he hadn't started yet. But at any rate I conducted that study in which we were interested in consistent positive interaction between the kids in an experiment as compared with inconsistent feedback in relation to the child's dependency behavior directed at the experimenter herself (the experimenters were women). That study turned out to be novel enough that it interested a lot of people in the field. And I still regard, although no one reads it anymore (they read it for 20 years) as one of the better empirical contributions that I made.

So I drew something different from each of these people. I think from John Whiting, now you would think that OK, you'd get a good dose of "Becoming a Kwoma"-- that he had studied some ten years earlier. I did do that. But I took independent study and learning theory with John and I took a seminar with him and Dick Solomon and Sandy Stevens and a bunch of other people in motivation. So I had these people in very interesting kinds of roles. And our Saturday seminars were wonderful. I mean, we met together every Saturday morning to really discuss things related to the project. Sometimes things having to do with decisions that needed to be made. Others just interesting things, it might be an interesting visitor or somebody with a little manuscript, with a little data in it, maybe a few correlations of .15, .20, .30 and you sit there for three hours trying to make sense of what those were like. But it was a marvelous tension amongst that group of people. Tension I mean in the sense of different backgrounds. I mean, neither Eleanor or Harry had a background in developmental psychology. All of the six teams of investigators who were part of the Six Cultures project were in training there at Harvard during that time. So people like the Levines and the Monroes and the Romneys were all around housed with people like Chris Heinicke, me, and some of the other people who were psychologists. So it was a wonderful time but I got indoctrinated or educated I guess, in developmental psychology in very idiosyncratic ways. And I knew very little about many aspects of the field. And we can talk about that later.

Collins: Socialization doesn't end with graduate school.

Hartup: No it doesn't but that experience really set the direction of my research in terms of focus on inter-personal relationships and their role in individual development, a focus on the family. What it didn't do, was find any intimations of the importance of peer relations to child development.

Collins: Thank you I think we'll stop there and then we'll continue on a different side.

[Tape paused and resumed]

Collins: We're at the Institute of Child Development in Bill Hartup's office. This is Andrew Collins. He and I are talking about his career and his feelings about the field of child development. The date is December 7, 1994. So when you started in the field, what were your major research interests and where did they come from?

Hartup: Well, as I just said, I worked with the Sears, Maccoby and Levin group on the Patterns of Child Rearing project at Harvard. But I had broken away a little to do an experimental analog thesis, dealing with the condition of continuous nurturing. Sort of interrupted nurturing as related to the occurrence of dependent behavior in children. And I took that combination of interests in dependency and in relationships, which I had gotten from work with Searses and Eleanor and continued that. So that when I went to the University of Iowa, the first things I did, were to do a couple of studies which continued that work, only extending it a bit. The second thing that I published for example, was an experimental analog study focused on the same experimental manipulation -- that is continuous versus interrupted, warm exchange with an adult experimenter-- only as related to the occurrence of aggressive behavior on the part of the young child. Not the occurrence of help seeking and other forms of dependency, which had been the focus of my thesis. That study was a masters thesis by Yayoi Himeno and was, I think, my second publication. But I made a couple of false starts at Iowa also. I had gone there to work with Ruth Updegraff in the Iowa graduate program. It was devoted to the training of early childhood educators. Shirley Moore was one of the students in the program.

Collins: You know that he's an assistant professor?

Hartup: Yes. And so I did one very elaborate observational assessment study that focused on what we would now call non-social competence and how to measure it in preschool age children by observational methods. And I just went blind for two years watching kids. It was very good for me. I did publish the results of that study, but it fell like a big thud. And I did another thing which was an objective study of parent imitation, that is, set up kind of doll play things which were techniques that we had used at Harvard. Attempting to determine whether they could be used to provide stable individual difference measures of kids and the extent to which they were imitative of their parents -- their fathers and their mothers. So I was still interested in adult child relationships. Things that related to internalization, dependence, and so on.

Collins: So there was a real continuity from….

Hartup: So there was a real continuity and that lasted for about a year. And then two fortuitous events occurred that changed my whole life. And they occurred almost simultaneously. One involved Grant, my son, who was in nursery school with Janet Cantor the daughter of Gordon and Joan Cantor. And they carpooled together. And they began to describe their animosities towards one of the other kids, a boy, on the way to and from school. And then Joan and Rosemary would talk about this a lot. And the kids were particularly creative in how they would "do in" David if they got a chance. And so after awhile when those things were related to me time and time again, I thought, well, I'm going over there and see what's going on. Well it turns out that there was a strange kind of ambivalence that marked the relations of both of these kids with David, who was a little terror, and struck fear into the hearts of a lot of kids, including our own. But who also had a lot of confidence and so that there was a very, very positive attraction for Grant and for Janet with him. So that got me to thinking about the kinds of things that happen in children's relationships with one another. And what sorts of significance that they might have for the socialization, for the development of the individual kid, and so on.

Well at about the same time, and J really didn't have anything to do with this, a couple of graduate students talked with me one day, much as graduate students around here talk about creating a lab group around one set of questions that they were concerned about. Which is, just what is it that we know about children's relationships with other children and what they contribute to social development? And the first question to me was pretty much, what do we know about this area of activity. Well in short order we discovered that we didn't know a lot. That we knew quite a bit from work done in the 1930's and the 1940's that was descriptive by those who really know, that was exploratory. Well to make a long story short, we organized a seminar, we conducted a study, and that study was published. It was the first paper in my whole long series of studies on friends and their influence on development. It was a marble dropping study. We simply had the social reinforcement delivered in one condition by a friend and in another condition by a non-friend. Sorry, no in the first study it was by a popular kid and the second by a disliked kid, in the second study it was a friend. And that sort of set everything going. I pretty much abandoned the field of parent/child relationships and projective techniques. I didn't abandon though, all of what I had learned about observational assessment. And what can be learned by direct observation of children. So that, that kind of strategy has been one that I have used consistently through study after study after study.

Collins: I'm curious whether all that two years you spent observing children to learn something about competence, might have planted some seeds for this as well.

Hartup: Oh I think it did. And I think the role that I had at Iowa which was, had some commitments to the early education and training of students in that area, that led me to focus some attention on those relationships. And what I remember they had been looking for, a person to take the position at Iowa, the exemplar that they used in recruiting that year, was George Thompson. And Thompson's work had been, while he was a graduate student, had worked with Kurt Levin and others but had worked more in classrooms than they had. But Thompson's main area of interest was in kids' relationships with one another. He had also some interest in teacher/child relationships which I didn't share.

Collins: Do you think that the research history at Iowa made a difference in your getting into this area?

Hartup: No.

Collins: That really wasn't

Hartup: I think it (Levin's influence) didn't influence at Iowa, except as exemplified maybe in the work of Boyd McCandless. I mean, this was the era of Hull-Spence theory and that was very dominant there. We'll probably come back to that later.

Collins: Well was that a thread, that you just mentioned in that last statement, is the connection to Boyd McCandless, was he at Iowa when you were there?

Hartup: Yes right. Yeah and he, well do we want to talk about my going to Iowa now, or do it a little bit later?

Collins: Well again, let's do that a little bit later. We'll stay on the topic of your research. But let's just note that Boyd was an influence both at Ohio State and Iowa.

Hartup: Oh, Sure.

Collins: You sort of talked about how you moved into the peer relationship area. And I assume that you had characterized your contributions to the study of peers and their contributions to development as the, sort of the of your career.

Hartup: Well it's the one I spent the most time on, sure. I think, that I'm best known for, I think I'm credited with some things that aren't necessarily true. I mean, you'll hear people talk about, well he discovered this whole area of socialization and its significance. And that just isn't true. I mean, our ancestors, like Florence Good enough and Helen Dawe and a lot of others that you can name, were working on this 40 years before I was. But there hadn't been much done in this area during the post war years. And those were the years in which the mixture of psychoanalytic and social learning theories were coming to dominate developmental psychology. Clinical psychology was growing like crazy. The theories of personality and so on, were all that interested enormous numbers of psychologists. And from those theories come a strong emphasis on family relationships. As the major contributors to child socialization then there really isn't much. Freud or Erikson or anybody except maybe Harry Stack Sullivan that had much to do with peer relations. And so early on in a child's social life just simply wasn't worked on. There were other things on the research agenda. And so I came along at a time, not just when that was being exhausted, but when I think, view points were changing to allow for the possibility that different social systems feed into the process of socialization. And that we'd better know something about some of these other ones. Study some of these other non-family social.

Collins: How do you regard your work as having moved that along? What contributions particularly stand out? When you reflect on your career as having been influential in sort of shaping the direction of the studies of your life.

Hartup: Well I guess I would like to think that there are two. I think probably, it's almost a consensus among the colleagues that work in this field, that the synthesis that I wrote for the 1970 Mussen Handbook on peer relations and social organization, really brought this area back to life in a very strong way. It enunciated a number of different kinds of research that had been begun, but had been abandoned, that now were ready for work. And served as a kind of fertile place for people to get ideas to work on. It was that, more than a summing up of a resurgence of activity. Because not a lot had happened in the 1960's. It was more a harbinger than a summary of a period of sustained growth. But things were happening in the 1960's. I wasn't the only one that was beginning to address problems in peer play; Gerry Patterson was. I mean the psycho-experimental studies and that wonderful monograph Patterson, Littman and Bucker wrote was published in 1967. And so there were things like that, that were beginning to appear that demonstrated that there was something to be learned by putting some attention there.
Well. as you know, I provided the second Mussen chapter for the 1983 manual and I think both of those had an impact, at least I like to think they were, they were very widely cited. And I got a lot of gratification

and satisfaction out of producing them. But I also, I guess, think that there have been at least a handful, of empirical studies that have made a difference. So that some of my work, in actual empirical research has helped move the field along too -- studies like that group that we published in the late 1960's -- I'm thinking of the descriptive studies of Roz Charlesworth and the study relating friendliness to altruism with Brian Coates. I think that group of studies has, well and also the correlational work that related aggression and a number of other social behaviors to popularity and rejection. There have been thousands of studies of that sort since that time and that pattern of relationship there is the extent to which rejection is positively correlated with rejection but not with friendliness and that acceptance as related to friendliness but not with rejection has held up in every study that's been done in subsequent years. But then, oh at about each decade, there have come a few other studies that I think were influential. I think of the thesis which Wyndol Furman published with me and we published together on effects of socializing between shy children and younger peers. Made an impact, I think Oh this study from the 80's with Brett Laursen and Doran French on conflict.

Collins: Brett Laursen?

Hartup: Yeah right. So you know, scattered in a long through there I think that there's been respectable empirical work.

Collins: What about the work on aggression that is, still continues to be, widely cited?

Hartup: Well that's kind of transmuted into studies of conflict. Yes I've had a strong and abiding interest in aggression and conflict. Although I guess I don't regard my contributions to the study of aggression as highly as those of other people like Patterson or Ken Dodge or John Coie. I mean, I've been kind of on the edges of that field.

Collins: I understand. If you sort of had to sum up or identify a couple of pieces of writing that you've done, published or unpublished, that sort of sum up your position in child development, the best representation of your work in child development Can you do that?

Hartup: Well in addition to the ones I've mentioned, I guess maybe I would cite the two papers I wrote for the special editions of American Psychologist. One in 1979 about two social worlds, where I was saying similar things to what Ross Parke was saying at the same time about the necessity to study both family systems and peer groups separately but together. And then the paper that was published in, was it '89?

Collins: Yes.

Hartup: On relationships and their significance. I think since '89 I've published one or two other papers on relationships and their significance in development that may be a little bit better, but probably have not been as widely circulated.

Collins: I see. When you think back on things, do you think there were times when you were just fairly quite wrong-headed about the way the field should go or about the importance of studying particular things? Would you have done things differently?

Hartup: I mean I've already mentioned a couple of places where I went up blind alleys. But I've went up blind alleys later, too. I think one of the things that happened to my work on aggression is that eventually I just sort of ran out of ideas and I couldn't think of major things to do with that. I had done a major observational study with Jacques Lempers and Doug Sawin and several other people. And we just couldn't get the stuff out of that huge data set that I thought made any sense. Although we did publish one American Psychologist piece that was part of the data. And after that I just sort of gave it up for a while and I didn't go back to that area until in a very different form, I took up the study of conflict about ten years back. So that's happened, you know, from time to time. I think there have been a number of things about my work that I wish were different. But I've always rationalized it by saying, well one person can't do everything. I mean I think, for example, that my work is, for a developmental psychologist, is remarkably a developmental. You look at my empirical studies, one after the other, and look for age differences or longitudinal studies on that subject and you don't find it. And yet I talk a very strong development game in a lot of my other writing. I mean this may not be the place to get into why it's developmental, but it just is.

Collins: I think it would be useful if you could say a few things about that.

Hartup: It's also non-contextual and yet I have argued in lots of places the necessity for taking contextualist viewpoint about development. And yet the actual numbers of studies that you can find in which I have examined behavior across context is not very big. And certainly the study of broader social context, like social class differences or ethnic differences, cultural differences. Once again I've talked a lot about these in some of my writing, but never organized the studies to pursue them. The other thing that I think, to some extent, I'm less defensive about, in some ways, is the frequently made charge that my work is atheoretical. I don't follow a grand theory, and the problems I selected to study have not come from systematic pursuing of hypothesis that flow from a theoretical core. The way some people's work does. The selection of research problems for me has been almost an intuitive process. I just say wouldn't it be interesting if...and then you go on from there. Supplying the theoretical framework comes later sometimes. Although usually there's more by the way of theory implicit, or in the back of my head, there might be a beginning. So that's why I say I don't accept the charge that my work is "A" theoretical quite as often as it's made.

Collins: Yes well of course those triggers come up a lot of times in connection with research funding attempts. Maybe you could say a few things about your own experiences with research funding over the years. You sort of lived through a charmed era in which research funding was readily abundant.

Hartup: Yes it was readily abundant but 111 tell you that I probably took advantage of it less than any one *of* my colleagues, whose work has been published in the same places and in the same degree as I have. I actually calculated about three years ago, the average cost in terms of external funding of every one of the about 50 empirical studies that I had published by that time. And that average cost was extending from 1958, when I first published one, until I did this about 1990, was $5,600 per study. I don't think there is a person in the field who could match that, in terms of how cheap this was.

I don't have a very extensive background in individually held ROI research funding. When I was at Iowa, there wasn't much pressure. First *of* all, funds weren't as available as they became later. But, secondly, Iowa had built into it's budget money for research assistants and so on. So I did what I did there, which wasn't a huge amount anyway, on the backs of graduate students. And I pretty much did the same thing here for a few years.

And then in 1971 about ten of us pooled our resources and attained a Program Project Grant from NICHD. We had that for 15 years. Our allocation from that, well we didn't divide up the money, we shared numbers of resources; shop, computer resources and a number of other things. But my allocations from that for research assistants never exceeded more than about $30,000 a year. It wasn't until that grant ended that I ever applied for an RO 1 of my own. And you know something about that, I had trouble getting one. I was at a point where I was exploring different areas of research but also I wasn't used to really presenting my research in the tightly framed theoretically relevant way that the federal funding is requested. Or in the detailed sequence of experience that usually goes along with a successful ROI grant. I did get one which I held for three years but I wasn't able to get it renewed. So right now I'm just without research funds. I've been able to do a couple of studies in the last few years with very small grants from the Grant Foundation and other sources. So I'm continuing my pattern of doing my research on the cheap. Now maybe that's why I haven't produced more than I have, I don't know. But I just never have had a research empire.

Collins: What about your experiences from the other side. I know you've been involved in policy making and advisory functions in research funding apparatus. What about your experiences?

Hartup: Oh dear, this is so complicated and it has changed over the years. Basically I'm a fan of the peer review system that was used in most of the federal agencies that we relate to; NIH, NSF and so on. I don't believe there is a power elite that one can define fundamentally. There are ways to use the system and ways not to. But I think the alternative which would be decision making exclusively by the federal bureaucrats, would be a disaster. That system is difficult to monitor continuously. I mean, one is at the mercy of who is on the study section. And that's good sometimes, bad sometimes. But I don't know what other system we could devise that would work any better. I think in recent years as funds have become tighter, I have grave concerns about the targeting of so much *of* the research money for big projects. Including interocution projects for special problems of one kind or another. As opposed to ROI funding. I'm a very strong believer of ROI funding even though I've never had a lot of it, as the kinds of funding that moves our field more satisfactorily. We have some big problems in the future as government is supposed to shrink. That means that science funding will shrink. Just how agencies spend their money is going to be *of* grave concern to all of us.

Collins: What about your own experiences in study sections and council. I think we can do that pretty briefly?

Hartup: Well I was, before that I was a member of the Committee on Maternal and Child Health which at the time, this was the early 1970's, reviewed training grants program projects.

Collins: That was NICHD?

Hartup: NICHD. Then after that, a few years later, I was appointed to council. Other than that I've never been on other study sections. I felt I didn't want to go back and do that again. I'd done my duty for about ten years.

Collins: I think I first met you when you were on the Maternal Child Health Committee. You did a site visit to a program at Stanford that I was a student in. And I remember sitting in this student meeting and a site visitor came into the library in Jordon Hall.

Hartup: That's interesting.

Collins: Yes it was very interesting. Why don't we take a break, check on the tape and then take up on the next topic. OK. We're now ready to talk about personal institutional contributions. Why don't you just sort of give a brief summary of the institutions that you've worked in since leaving graduate school at Harvard?

Hartup: Well I've worked in only three and with the exception of the first, it has been my incredible good fortune in having worked at two of the most outstanding places in the field. And I would not have had the same career, had I not been at those places. My first job was at the Rhode Island College of Education for one year in 1954-55, where I taught six undergraduate courses, all new, in one year. I then moved to the Iowa Child Welfare Research Station, at the University of Iowa, where I stayed for 8 years. And where in that entire time I never taught an undergraduate course. I think that discontinuity is why I still have no great reputation as an undergraduate teacher. I was thrown in and drowned the first year and nobody bothered to hone any skills or give me an assignment in the next 8 years that would bring me back.

Collins: You say no undergraduate teaching?

Hartup: There was no undergraduate program. Toward the end of the time we did feel that the survival of the Station might be shored up if we used all those resources, in some way, to benefit undergraduates there. And we did offer some things as joint offerings and I did some lectures, but that's all.

Collins: That lasted till about 1963?

Hartup: 1963. And then I came to the Institute of Child Development here. And I think, I mean, the first undergraduate course I taught was the introduction course. And I've taught undergraduate courses ever since. So 8 years there and now 32 years here essentially describes my institutional career.

Collins: And in Minnesota you've been in several capacities.

Hartup: Yes, do you want me to talk a little bit about Iowa?

Collins: Well let's just get the capacities you've been in here and then we'll go back and talk about Iowa and then Minnesota.

Hartup: I came here as an associate professor and was promoted the following year. And from the second year I was here, I was associate director to Harold Stevenson's directorship. I stayed associate director until he left in 1971 and then the faculty asked me if I would become director. So I was director then for 11 years. Until you became director and that's pretty much it.

Collins: Since then you have been in a regular professorial role. And you were recently named Regents Professor which is the highest professorial honor given at Minnesota. Now let's go back to Iowa because a lot of changes took place at Iowa during the time you were there and the changes that are taking place at Minnesota. But let's concentrate on Iowa for a minute. Talk something about how Iowa changed during the years you were on the faculty there.

Hartup: Well when I went to Iowa, the director was Boyd McCandless and he had set about resuscitating the program after an interim period that had not been very happy following the Searses departure in 1949, '48. The Station had been run by committee during that time and had drifted. The quality of the students was down and so on. Boyd and a number of very young people, like Charles Spiker and Alfred Castenada and Charles Smock, were three of them, put together what became known as the first program in Experimental Child Psychology. Some people associate that specialty or concentration with Minnesota but actually Iowa was there first. And the combination of people who were there, which included some social people like, John McDavid and me, and the ones I've named. There were older folks around like Ralph Ojemann and his program in parent education. The Station attracted what were probably the best students in developmental psychology during that period. That is the decade of the 1950's. Students like Lewis Lipsitt and Shep White and Fran Horowitz and Hayne Reese, just to name a few of the more distinguished ones, were the students there. And I, the young professor, was on the Ph.D. committees of three of those. So it was really pre-eminent in the 1950's a place for graduate education. And the research that was coming out of it, although some of it was linked to Hall-Spence theory which was soon to lose its cachet, was still dominant in that period. And I think that the bias or the views which the faculty tried to exemplify in it's work. That is, their essentially positivistic and empirical purist views. We can learn most of what we need to know about developmental process, by using the experimental method by focusing on processes of learning and motivation. And doing this whether you're interested in social behavior or whether you're interested in cognitive functions. But it met with considerable degree of success. These people were productive and they were visible in their professional societies and what not. So Iowa was very much alive at that point. But it didn't last for more than about 10 years.

By 1960 Boyd became restless. He was a charismatic person who I think, not everybody on the faculty supported in the same way. And he decided he needed to take leave and went off to Pakistan for two years. And when he came back he decided to leave the university. And when that happened a couple of other things simultaneously happened. John McDavid left because, well it was clear he wasn't going to get tenure. I had tenure but I became convinced that my work was less valued than I thought it had been previously. Ruth Updegraff was about to retire. And so there was a kind of narrowing of program focus. And this was taking place at a time in the early 60's when the field, as a whole, was beginning to be interested, once again, in other viewpoints. Structural viewpoints of development, Piaget in particular, which were not resonating there at Iowa. When Boyd left, Charles became acting director of the institute and then director. And for those people who knew him, I think many found Charles pretty formidable inter-personally -- to relate to. He was, I think, a major contributor in the field. And I wouldn't want to take anything away from that. Except he was unable to transcend, shall I say, Hull-Spence theory. And I think it became clear to most people that, that kind of transcendence was needed. And I think maybe by just being the kind of formidable person that he was, tended to discourage the work of some of us who he didn't understand or completely appreciate. I think he came to understand and appreciate this later, but then it was too late. So by the early well by the late 1960's, the quality of students there, the research funding and so on, led the university to question whether or not this was a program they could support. Well, they had an annual budget of something like $400,000 of hard money. And outside viewers came in and said, yes this is a program that should be supported but it's got to have change. Somewhat like one of our local programs here, right now.

Collins: Yes.

Hartup: But that change didn't happen. So I don't know exactly when it was, but by 1975 or '76, the program was essentially dissolved. I came to Minnesota for a semester, we were on a semester system, in 1961. One of my colleagues at Iowa didn't like that very well, because that was during the time that Boyd was gone and thought that I should have stayed home. But Harold was up here by that time and Shirley Moore was here, and things were beginning to happen here. So I thought it would be a fun place to take a leave, we didn't have a sabbatical system. Well in two years this group up here was after me to make a permanent move, and by then I was ready. I think it was a case of not being comfortable there. Or at least not sensing the possibilities for growth. But more than that really exciting things were beginning to happen here. So it was just like, you know, being asked to be part of a whole new culture that was obviously going to go some place. And that was impossible to resist. Plus my family was a in a time when living in the city, like the Twin Cities, as opposed to living in Iowa City, where. We had been very comfortable as a family, seemed like a better thing to do.

Colllins: Would you have stayed at Iowa if things had looked more promising there, if they had been as vital as they were when Boyd left?

Hartup: Well I don't know. My subsequent history here suggests that I might have. That is, if Iowa had retained the same kind of vitality both among the faculty, students, and the kind of research that is being done, as Minnesota has I think, been quite successful in sustaining over a very long period of time, it's quite possible I could have stayed. Not necessarily with the same people. I don't think that was the issue. But I've always been a sucker for quality -- program quality -- and it has had so much to do with my own work. I mean, I think really the large part of what success I've had, has come from being around and collaborating with really excellent students and good people. I can well imagine, if I had been offered a position at the University of Mississippi, where I had once interviewed, that the field would have never heard from me again.

Collins: I know. Now Minnesota. Now what things were happening that sort of absorbed you right away?

Hartup: Well, I mean, I was very excited about the possibilities in working peer relations and child development. Some of my own work was, and I found students here who were interested and willing to explore those same kinds of things. The students here became increasingly good over the 1960's. I mean, there weren't very many students around in the early 1960's. But the few that were around were very good. I mean, Aletha Houston was here and Rachel Clifton was here, but they were all committed to other people in terms of their mentors. But these students began to turn up with increasing frequency. And so I began my own work. The other thing was the Institute itself. Because Harold's vision was almost impossible for me to resist in terms of kind of contagion. I mean, there was, we're going to do it, by God. You know, we're going to make a difference. And that atmosphere pervaded the place during the first ten years. The atmosphere of building, of constructing something for the field.

Collins: Was that more than a commitment to experimental child psychology?

Hartup: Yes. I think it was a commitment to clinical child psychology as much as anything. And by the end of that decade we had worked out a system of joint administration with the clinical program of the Department of Psychology. Unfortunately Psychology retained all the power over the curriculum, so it didn't work. And we gave it up by the middle of the 1970's. We felt what had been a parent/consultation service that Britt Ruebush directed, into a clinic. Our child clinical facility that was really very successful but as clinics began to die on campuses, ours began to die here. But there was strong commitment to work in psychopathology, and we hired a psychiatrist, Carl Malmquist, was here. It didn't work out very well, but we had one. We tried a different thing.

Collins: The atmosphere of innovation.

Hartup: That's right. And building up the critical mass that the faculty consisted of enough people with enough different interests, that it would then draw graduate students and research funds and so on. Because by the 1970's I don't think there was a place that could match us in terms of capacity to recruit people, the overall mix of faculty and so on.

Collins: How would you describe the evolution of the Institute during your time here?

Hartup: Well I think the 1960's was a period of building. I mean, Harold Stevenson -- Harold's place which while not really completely de novo was badly in need of shaking up. Shaking out some of the people and programs that were here. Revitalizing what was known as parent education. Finally eventually getting rid of it for a time. And then building as we just were discussing. The 1970's which was the period of my directorship, was, I think, a period of conservation as much as anything. Really the lines were full. And during that time we did create certain new programs like, the Center for Early Education and Development, Child Care Center, were things that I spent a good bit of time on but I didn't think the period that was one of building. And I don't regard myself as having contributed enormously, to the infusion of intellectual talent in the faculty. I think if you look closely, the only really superior talented person that I brought on to the faculty was Megan Gunnar. There were some notable failures during that time. And your tenure as Director was much more successful in the recruitment of new faculty and for the faculty revitalization. I don't know how you see it, but I see the 1980's as a revitalization, that is where the major accomplishments of the 1980's.

Collins: Well I think that's due to a lot of historical change in part. The 70's was a rare time when…. What courses have you taught? What's been your main avenue in making a contribution in the next generation of students, essentially?

Hartup: Well I have taught regularly throughout my time here at Minnesota. Always in the field of social development or personality development except for the first few years when I also taught the introductory course at the undergraduate level. But I haven't taught that course for years. The social development courses that I have taught have changed, of course, over time. I think that they have affected what's been happening in the field. Right now they have a strong emphasis on relationships and their developmental significance and I even teach a course in that area for undergraduates. I have been very committed to the training activity that takes place during student's apprenticeships and research. I have never published an empirical study, I don't think, that hasn't been with a student collaborator. I see those experiences as two way streets. I mean, they were as important to me, as I think they were to the students.

Collins: In my view, your extensive contribution is to training of young researchers. Is that your perception as well?

Hartup: Well it is and it isn't. I have not been principal mentor of an immense number of students. I think the number of my own doctoral students is about 24 or 25. Which is a relatively small number for people who have been around as long as I have. Among these students are some very good ones, people like; Andy Newcombe, Celia Brownell and Doran French and Brett Laurson, was a student that you were his principal mentor. I know I contributed a lot to that because he worked with me on research and we are still working together.

Collins: A lot of your effort, and I think it's been with people who have had other mentors as well. I think of Bill Graziano, for example, who was not even in this institute, but was a social psychologist.

Hartup: That's correct, and his present dual identity as a social psychologist on one hand and a developmental psychologist on the other and that I'm as much responsible for that later identity as anybody. I suppose that's true generally that there are, at least I'd like to think that's true, that there are lots of students that have come through here, that have learned something from me, in one way or another, that they have put to use later. I mean another one of your students is Tom Berndt who went on to work in my own area.

And I guess that was partly due to what he got from me. But he does it with his own unique, theoretical views and his own unique emphasis and, I think, he got those more from you.

Collins: I think Tom is a creative conniver.

Hartup: I think that's what the good students do and I think what we set them up to do here.

Collins: I think that's right. How do you see your contributions to the area that's commonly known as applied child development? Do you see yourself as having made an impact on things that are commonly called application?

Hartup: Well I think that is, well certainly practitioners in a lot of different fields have told me enough times over the years, that they have profited a lot from the kinds of things that I have done in research for me to know that I have had an impact there. But I haven't done very many things with the objective of quotes "doing applied research", or investing heavily in the kinds of dissemination that would be of benefit to practitioners. Because the kind of work that I do is not very complex conceptually. And it concerns issues that practitioners are concerned with. I mean, they are concerned whether children fight or whether children are rejected by their peers. That doesn't have to be explained or defined like highly complex theoretical concepts do. So just putting it out there and letting the consumers serve themselves from the cafeteria has been my approach.

Collins: I remember back in the early 70's you were involved with a couple of volumes from NAEYC translating research for people who were training to be early childhood educators.

Hartup: That's true. There was a project which was driven by the desire to make an applied contribution. That was at a time when I was still more heavily involved in early childhood education than I have been in the last 15 years or so. I mean, I was very invested in the training of early childhood educators and in the advancement of that field generally at a time when Head Start and other programs were going. Although I was never a Head Start trainer, or a Head Start innovator. What I was concerned with and what we tried to do in those volumes was, to put some research based materials between the covers of a couple of books that could be marketed to people that were training the Head Start people and who were training the early childhood educators and obviously our universities and I think it was quite successful. A lot it was base consumers that bought these books.

Collins: Let's change focus and talk about SRCD and your involvement in that organization. When did you first join the society?

Hartup: 1957 or it might have been '58, at least the first complete volume of the journal that I have on my shelf is 1958.

Collins: You were influenced to become a member by the people in Iowa?

Hartup: The Iowans were all SRCD'ers. They had just been to the 1955 meetings at the Falls Research Institute. They'd gone by van. But I didn't really have a sense of what the Society was like until the 1957 meetings were held on campus in Iowa City. There were 100 people there, who were at all sessions. All the sessions were a symposium that ran for about two and a half days. There were many people like Bob Haggarty and Dottie Eichorn and Bill Martin and some others. But I still didn't get a very good sense of what the Society was like until two years later when I went to the Bethesda meetings. By that time I had simpl y spent more years learning the field and I have a lot of learning of the field to do after I retire. And a part of that worked with the Bethesda people. I have been in that project and in things related to it in the field, but I didn't really get into that field. So I was much more savvy about things. Plus the fact that there were more people at those meetings and I just had a better sense of what SRCD was all about. The first organizational duty that I had for the society was as Program Chairman for the 1965 meetings, which were held here in Minneapolis once. I think there were about four or five of us on the Program Committee. And I remember meeting at a hotel room some place with Justin Aronfreed and Bob Haggerty and sort of putting out the things that had been submitted on the floor and dividing them into piles and got the whole job done in about two hours. Those meetings were held down at the Raddison and that was a wonderful place to have a meeting and the second hotel-based meeting that the Society had produced. It was a very good meeting. People had very good reactions to what we had done. And it kind of set the tone, I think, for the meetings that came after that.

Collins: Well, what about your involvement in the research activities of the society?

Hartup: Well I think they've been pretty continuous from about 1960 on. In the early 60's there was a, oh kind of a cabal in our part of the field, that dealt with childhood socialization and personality development. About the best way to account for processes of inter-nationalization. Larry Kohlburg was hammering his theory out and Jack Gewirtz was extrapolating all kinds of things from the Skinnerian base and AI Bandura was working off a different base. And I was part of that group. We had symposia at just meeting after meeting you know. Some of them were rather wild affairs. I mean, people disagreed and they didn't mind saying so. I'm not sure that every one of those symposia cast much light on things. But for the people that were participating it was a remarkable kind of experience. Because I really think this conference did help in working out their own ideas. I had to say that's probably as acute an intellectual time as I've ever had. That is a sort of intellectual excitement, when you can sort of peer through the fog of hangover, you know, how those symposia went! After I became involved in peer relations research I kind of drifted away from that group. And began trying to do some of my own I guess. So, most of my program activities have been around those kinds of things since.

Collins: What about your involvement in governance in the Society?

Hartup: Well I guess you'd call it governance. The first committee that I worked on was the Publications Committee. I was eventually it's chair. We had so many things to deal with.

Collins: What year was that?

Hartup: Early 70's. There had never been a Publications Committee before and several of the projects were in trouble. The Review of Child Developmental Research series, which had been so successful, volumes 1 and 2. Volumes 3 & 4 were just hung up on a variety of things. I ended up ghost editing volume 3 just so they'd get it done. And the Editorship of Child Development was not in very good hands. I mean, things just didn't... we had to raise so many questions that the editor finally resigned and that's never happened, I mean, before or since. So I don't know. I bring those things up now to illustrate that some of the decision making in those early years when publications were more in the hands of larger groups within the society then they had been historically. I mean, when Child Development, The Monographs, everything connected with publications. Bill Martin ran everything in the 1950's and more then society as a whole, took on a responsibility for things in a way that hadn't happened before. And some of those things were early things that were done.

Collins: Yeah, just sort of getting the kinks out

Hartup: Right, I became a member of Council in about 1976. I worked through the presidencies of Mary Ainsworth and John Flavell and Eleanor Maccoby. Those were really interesting years. I mean, it was a way of working on relationships with some of those people that I had not had a chance to before. And there was a tremendous sense of solidarity.

Collins: Those were major growth years in the Society.

Hartup: Major growth years for the Society. You know, all of the kind of excitement that goes along with growth and attaining a status in the scientific community that we had not had accrued. So it was fun as well as taking the work seriously.

Collins: And after that you turned to the editorship?

Hartup: Well I was asked to be Editor...actually, I was asked to be Editor once while I was still on Council. I guess I was asked to be Editor at the time Mavis was asked. We didn't really do formal search procedures then. So I don't know really whether she was hesitating or whether they asked me and then they asked her, it doesn't matter. But by 1983 when they asked again, I was ready to do the job. And I don't know, I think those years were pretty successful. It was a different kind of task than what Mavis had, in the sense that, submissions went through the ceiling while she was editor. And then it began to decline somewhat and declined steadily during my editorship. So we needed to preserve what we had, namely, an excellent journal with some submission decreases. But I think if you look back over the quality, the quality of what was published during the period between 1984 and 1990, that it's not appreciably any different than what it was before, even though research funding had begun to retract and some other things happened. We were probably the last of the editorial teams to take on the load that we did. Hundreds of manuscripts. Things have simply changed, as you know, so that now it's really not possible for people any more, to take on those kinds of loads, when they're responsible and accountable for so many other things in the universities. That's been one of the things, as president, I've been really interested in helping to work out, is a new way of managing the journal for the next century. That will reflect some efficiencies and other ways of doing business. That will still make it possible to produce a high quality journal, but to do it with less investment of the research capital we have in the field. Dh maybe invest in more people with less per person.

Collins: Right. You were elected president elect in 1991. What can you say about the context of your election.

Hartup: Well I mean, the context was defined, I think, mainly by the two previous presidencies. Both of which were accompanied by some turmoil. Sandra Scarf'S presidency had a certain amount of controversy connected with her. Largely because of the kind of science she does and what she stands for with respect to that science. And a lot of people don't agree with her and don't mind saying so. So trouble got stirred up that way. But I think that any Society can withstand that kind of trouble. At the same time issues were coming up accompanied by some restlessness and dissatisfaction and even in some quarters, outrage about the slow pace in which the Society was responding to issues of diversity. And the fact that the journal didn't contain much on the development of kids, other than white, middle class kids. The fact that the Society was moving so slowly to incorporate and to support minority scholars in its membership. The fact that we couldn't make up our minds whether to become an international society or not. These things came into focus in Bob Emde's presidency or he brought them to a head in away, by saying, look we've got to do something more than we have about these, and he appointed a task force on diversity. And while I thought at the time that maybe these issues could be worked out some other way, I think now that maybe this task force was a good idea. Even though it's never submitted a final report and we still don't have in writing, everything that was discussed during those times.

But I made a commitment, and this was not under any external pressure, to simply pick up what I thought was a most doable and pressing set of issues from that and see if we couldn't develop and consolidate methods within the Society's governance for dealing with some of those. So I think I would describe my presidency as an effort to consolidate the resources of the society around some of these issues. It think we've done some really pretty good things. I think we have, for one thing, brought about the current structure that places inter-committee and intra-council management in a place that it never had before. Which sounds very structural, and is, except that it deals with some of the diversity issues that Bob brought up. For example, we have a Committee on Racial and Ethnic Affairs that, until a year ago, made all of it 's recommendations, made all of it's noise, made all of it's rewards unilaterally, directly to Council. Or just went about it's own business. Other committees would hear about the things that had implications for them via the Council. In other words, things would go from one committee to Council and go back to another committee. I set up a simple mechanism whereby new committee chairs would simply hold a two-day meeting, once a biennium, to layout the agenda and to establish communication channels between one another. And it has just been very successful.

We now have a Committee on Committees and it's designed simply to be more systematic and I think, better at selecting particularly younger people, to become involved in governance. So that it's not just done sort of haphazardly as it has in the past. We are centralizing the administrative offices of the society. From a time where we have been in 3 or 4 different places, from Berkeley to Chicago to Ann Arbor, to wherever

the editorial offices might be, we will, by the end of my presidency, be very close to having everything centralized at the University of Michigan in Ann Arbor. We will have membership there, the programming we'll do there, the new journal operations will go through there and all the executive office stuff. So these are all sort of administrative things. But at the same time, I think the Council has more time because of this to actually talk about strategic opportunities in research and to give some attention to what role the society might play visa vis research funding agencies in Washington.
We've spent a lot of time this last year re-establishing for the Society, rather than just for individuals, like Barry or Sandra, but establishing for the Society, a visibility with the directors in NIMH, NICHD and NSF and some of the other places. Where there can be no alternative for some of the kinds of problems that we, as a field, face in the 1990's. It has just more and more become so we can't leave to our committee on research policy or to our Washington representative, all of the communication that needs to take place between us and our funding sources. The SRCD governance has to be continuously involved in that. Meaning, the executive committee, or what have you. So we've worked a lot at establishing those kinds of things.

Collins: Are there additional things you want to say about the most important changes that are taking place in the society since you first became a member of it?

Hartup: Well I think the extreme domination of the psychologists in the Society has been leveled a bit. Nobody really talked much about it. It just developed that psychologists sort of took over and people like Howard Meredith were fewer and fewer as they died or they just dropped out. It was only in the 1970's that the issue as to what the future of the society as an inter-disciplinary society began to be discussed seriously. And finally the steps were actually taken to sustain the society, at least partially, as an interdisciplinary organization. And that was done mainly under Mary Ainsworth's presidency. That's when the program. It wasn't until two presidents later, after Eleanor Maccoby, that this alternating psychologist, no-psychologist tradition in electing Presidents was established. That's really all the tradition within the...it's all well, that has produced, at least in governance, a visibility of other disciplines than psychology that just wasn't there before. And it's not effective in the journal and that bothers a lot of people. Our conference programs are more diverse. So that's one thing, I think, the interest in research policy and in child development, social policy issue is another big change. That happened about the same time in 1970. We never had Washington representatives and we never had workshops and committees. The fact that we have committees as extensively as we do, is a relatively recent development in SRCD governance. I think until the *1980's,* I don't think there were more than 1 or 2 committees. Now we have at least half a dozen. So I think SRCD is more participatory in the way it conducts its affairs. And as a business we, we're in pretty good shape. We have adequate financial reserves and people are willing to attend to our finances so that we don't do too bad on that score. At the same time we still count as an organization committed to assisting in science.

Collins: OK. Let's take a break.

OK we're back now to talk about sort of the big picture with respect to the field. Could you just comment on the history of the field during the years that you've been active in it? Especially, focusing on major continuities and dis-continuities and the events that have been related to these.

Hartup: Well the field has changed in a number of ways. The theoretical positions which guide research, I think, are very different now from the time when I entered the field. That was a time when" grand theories" were, sort of planning themselves out. Or at least across the first 10-15 years of the time I was in the field, that's what happened. The last 15 or 20 years has been the period, as I see it, of integration. That is theoretical integration. Where we're left with some of the notions that were the foundations of behaviorism, but are blended with biology and cognitive and lots of other things at the moment. Some of the problems and issues that developmental psychologists concern themselves with, have changed but a lot of them are the same. It seems like we cycle back every 15 or 20 years on some of the same issues. Probably the best example of that has to do with the nature/nurture question. It was not a question which many people were actively pursuing when I first entered the field, but became so again the middle of the 1970's, and continued to be so up until now. Parent/child relations have continued to be a strong area of concentrated study in the social part of the field. But nowadays that is counterbalanced with concern about and ways that other social systems are articulated with parent/child relations in determining the course of the child's development. So

! think, once again, of problem and issue, as well as, integration of theoretical views has been the story of the last 30 years. More ways in which the field has changed is I think, to strike an easier balance between what might be regarded as basic and applied researches, the substance of them. The pressure to do that really mounted during the 1980's. And I think the field was responding in a way that is quite different from the way that those tensions played themselves out in the 1950's and 60's and 70's. I think the area of application of work in developmental psychology that was most prominent in say the 50's and 60's was to psychology and another field -- clinical psychology. Nowadays, it's across a much broader spectrum. The final way in which the field has changed, of course, is that of the people in it. The cohort that I represent is represented by a very small number of people. If you actually look at the number of people between 60 and 70 who are in the field, let alone, who are still active, it's a relatively small number. It's not that it's zero, but it's not as many, compared to the number of people who are in the field who are in their 50's or 40's. And that number or the size of successive cohorts is decreasing again, but I think it's an important issue or observation to make. One could talk about historical trends in great detail and sort of think this is maybe not the place to do that. But at least at a macro-level those are the trends see.

Coillins: And what about your own sort of personal view? How has your view of the various issues changed? The importance of the various issues changed? Do you see some that you once saw very important, not very important any more?

Hartup: Well one of things that I think I see as maybe more important, or maybe it's just more feasible to study, is development from a multivariate or broad perspective. Thirty years ago one studied relatively narrow focused problems in parent/child relations for example. Or imitative influence or discrimination learning etc,etc. Now, at least in our part of the field, we've got the quantitative power, the statistical methodologies, and a number of other things to really take seriously what we've known all along, which is that development is an out growth of a wide range of processes which are interactive in extraordinarily complicated ways. And that includes environmental and relationship influences on development. And I think this is creating a hugely different field from what we had 30 years ago. When I was grappling with the personality chapter for the Annual Review of Psychology last winter, I think that's the overwhelming impression that I had, is that we're finally beginning to see some payoff of studies which are long term follow-up multivariate studies -- studies which embrace many different levels of analysis. This is a different sort of focus or functioning in children's social and personality development and very interesting results are being acquired.

Collins: What are your hopes and fears for the future?

Hartup: Well I think they're more demographic than anything else. That is, I think we've got very rich theoretical foundations to work from. I no longer lament the absence of any grand theory with which to articulate all of our data. We've got really good ideas out there to pursue. I think that we've established a standard for scientific activity that is pretty good. And I think it's the envy of some of the other behavioral sciences. So, we've accomplished something that is a good base from which to work for the future. My concerns are less those than who is going to be around to do all the work because of changing conditions - - mainly in academia -- but also a change in conditions with respect to funding this research. I really am concerned about the talent pool and its steady erosion because we haven't been replacing even the small number of older people who are retiring. We're scarcely doing that. And I'm not sure we're replacing them with people who are as talented as they were 20 years ago for example. Maybe the situation will right itself. I mean, social trends have a way of kind of coming around after time and if the need is sufficiently acute the people out there (and I don't mean professionals, I mean lay persons, decision makers, legislatures) will gradually understand that we've got to know more about behavioral development than we do. And will come around, once again, to funding the field more adequately and funding the people who work here. But I think we're in for a fairly long period of uncertainty about the future and living with students who have low morale and who have high anxiety about getting themselves into research positions. I think one of the major tasks that lies ahead in the next 10 years is to somehow, us senior people, is to somehow provide as best we can, support for our younger colleagues in getting themselves established in productive research groups. Foundations need to do this, the government needs to be concerned with this, and I think places like the Institute of Child Development need to be concerned. I'm terribly concerned about the fact that we have not appointed an assistant professor in this program in the last six years.

Collins: Many departments are in that same situation, I think. It is a way in which the human circumstances have trickled down.

Hartup: I think also, and I'll just make this a quick note, rather than give a disquisition about it. I really think that it's unfortunate for the future of studies in social and personality development, but I think also there's been a gender shift wherein fewer men are coming into the field now, than there were at any time that I can remember. And I can't document what my concerns may rest on, other than just the feeling that it's been a very rich field to be a part of over my life times simply because both men and women have been working in it.

Collins: Yes, right. In somewhat more equal numbers than in a lot of other fields. I agree with that. Well tell us something about your personal interest in family and interest that really extend beyond the walls of the university where you've been working. And especially the way in which those experiences have contributed to your scientific work.

Hartup: Well I mentioned the one family event that probably had more to do with my research interests than any other. I haven't been the kind of psychologist that has drawn endlessly from family anecdote or family experience to inform my research activity. Everybody wonders about that and I'm sure you've been asked lots of questions, you know, what is it like to be a child psychologist, and have 2 or 3 children, etc. etc. I don't know about you but I have worked out a funny kind of compartmentalization in my life over the years. I certainly have not been afraid to use what I've known, as a psychologist as well as I can. My behavior as a parent, or my behavior as a spouse. But I have refused to try to work out a code book for child rearing on the basis of what I know or do as a developmental psychologist. So I think there have been lots of ways in which my family life has informed my work. But it hasn't been a direct, straight, kind of connection. And I even find it difficult to describe it.

Collins: There is one part of your, sort of, personal pre-direction, that has affected your work and field enormously and that's your interest in international relations.

Hartup: Yes as it turns out I have extraordinary, intense interest in travel. Although I don't consider myself to be a kind of amateur anthropologist. I'm not as caught up in the study of culture or in cultural differences as many people are. I'm just a tourist. And my international work -- like a lot of other things that we've talked about today -- actually got started by a fortuitous event. Franz Monks from the University of Nijmegen had a study-leave and came through Minnesota in 1968 and turned out that he was soon to become the professor of developmental psychology at Nijmegen. And he was looking for ways to improve his programs and appointments. One of the things that he thought that ought to be done is to develop ties between that program and people in the United States where he thought that most of the good work in developmental psychology was being done. He was looking for people to come and spend short periods of time, longer periods of time and even was looking for U.S. trained people to put on permanent appointment there. In the space of two years he had recruited two of us: Brendan Rule from University of Alberta and myself to come for sabbatical leave - year long - to the University of Nijmegen and had recruited one U.S. trained psychologist Inge Ahammer, to join the faculty on a tenure line.

Well, that sabbatical invitation changed my life. It has resulted in an adjunct appointment with the University of Nijmegen which continues to this day. I have spent leaves of various lengths there, probably 5 or 6 times at that same university. I helped to promote three Ph.D. students during that time. And have developed a network of other relationships in Holland that is very close. I consider Holland the 2nd academic home and I probably will be going again next year. We're doing longitudinal studies now with our group of students, headed by one of my former students, Kees van hies hont And I take great pleasure and interest in working beside these people.

But one thing grew out of the sabbatical year that I spent there in 1970-71 that probably should be mentioned at least briefly and that is my contributions to the International Society for the Study of Behavioral Development. Because its first bi-annual meeting had been scheduled for the end of that time and the time I was there. And Franz invited me to be a member of the Organizing Committee to put things together (in a shorter period of time than we do now). But about 150 people came to that meeting. I infused it with some elements that were not entirely to Franz's total liking. It was a little more systematic. A little bit more open in terms of inviting contributions and so on than had been envisioned for the Society. And there were more Americans there than had been anticipated. But it wasn't so aversive that I wasn't shortly asked to become a member of the Executive Committee of the newly organized Society and, within 9 years, by 1979, I had become the president of the Society. That was an office then, which was not elected by the membership but was appointed by members of the Executive Committee. So it was not through democratic process or completely democratic process that I obtained that office. But I used that presidency also probably, to Americanize the society even further. I couldn't stand the fact that it's bank accounts were in seven different countries. I couldn't stand the fact that we didn't have an accurate membership list And a few other things, and by the time I quit 4 years later, we had organized accounts, an accurate membership list and had increased the membership by about 100%. So I left that kind of a stamp on that Society, too. I've remained an ardent participant in its affairs and just have written an account of its first 25 years. And it remains very high in my affections. And work for the Society has led to lots of other kinds of international assignments that I agreed to mention in here. Maybe one or two just to list them, of special significance to me: one was the six weeks of teaching I did in South China in 1986, another was a very productive leave that I spent at the University of Cambridge in 1984. Things of that kind have been very important to me.

Collins: Thank you very much, that brings us to the end.