Joan H. Cantor

- Born May 8, 1931
- B.A. in Psychology (1951) Syracuse University, M.A. (1953) and Ph.D. (1954) both in Experimental Psychology from the University of Iowa

Major Employment

- Professor Emeritus, Psychology, University of Iowa: 1991-present
- Associate Professor then Professor, Psychology, University of Iowa: 1973-1991

Major Areas of Work

- Experimental psychology

SRCD Affiliation

- Member

SRCD ORAL HISTORY INTERVIEW

Joan Cantor

Interviewed by Willard Hartup
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Hartup: Okay, Joan, maybe we can just start by having you talk a little bit about your educational background and the Institutions that you were at.

Cantor: Okay, I was an undergraduate psychology major at Syracuse University in the late 1940s. Graduated from Syracuse, I believe in 1948. I got quite a bit of laboratory experience there with one of my professors who was teaching experimental psychology at the time. He recommended that I go either to Minnesota or to Iowa for my graduate training in psychology. My husband and I were going to school at the same time, both of us in psychology. He happened to be in child psychology at that time and I was in general/experimental psychology. We both applied to Minnesota and to the University of Iowa and my recollection is that he received an assistantship offer from the University of Iowa but not from Minnesota and I did not receive an assistantship offer from either place. I subsequently found out the reason was because I was a married woman and so they didn't think married woman were good risks, and in a certain way married women were not good risks because in general in those days women began to have a family, tended to drop, out so actuarially they were correct in that married women were not a good risk for a graduate department. At any rate we decided to come to Iowa because of its outstanding reputation in experimental psychology and, of course, my husband had applied to the Institute of Child Behavior and Development, which at that time was known as the Child Welfare Research Station. And he was accepted in that department and had an assistantship offer and I was accepted in the department of psychology in experimental psych and, as it turned out, after I got here they did have some assistantship money so we both were on assistantships from the beginning. I was in the psych department and he was in the Station.

In those days graduate training took three years. It was extremely competitive and there was a very detailed curriculum and you took a great number of courses that were designated and it was all laid out very carefully and there was a lot of competition in the first year. For instance, we all took Kenneth Spence's learning course our first year. I should talk about what I took because I don't remember the exact curriculum in the Child Welfare Research Station other than to say that that was

Charles Spiker's first year as a faculty member—I'm talking now about 1951. I think I was mistaken before, it was 1951 that I began graduate school so I started undergraduate work in 1948, completed it in 1951 and we started graduate school in 1951 and that was Charles Spiker's first year as a faculty member in the Station. And he instituted—I'll come back to that, it doesn't have to do with my personal history, but I think for purposes of this history it's probably well worth noting that it was in 1951 that he introduced one of the first graduate programs in experimental child psychology and my husband, Gordon Cantor, was in the very first class of the curriculum.

I, in the meantime, was in the psych department taking courses in experimental psychology with Kenneth Spence and Judson Brown and I.E. Farber and Arthur Benton. We all took our first year courses with these professors and also Harold Bechtold and the Child Welfare students took a great number of these same courses with us, but of course they had their own courses. Boyd McCandless was the head of the Child Welfare Research Station at that time and he taught a course on child development for first year students. So you either made it—and you knew by the end of your first year whether or not you were going to be allowed to stay in these programs, and not everyone did. The grade of C at that time was a failing grade and it did occur and students who received Cs in their first year found themselves out of graduate school, but if you got through that then everyone finished in three years. That's just the way it was done, it wasn't something special; everyone did it. We all did.

And I was working with Don Lewis who was a general experimentalist and he had worked for the Navy during World War II and had contracted the Navy and was studying motor learning, and he had a very elaborate laboratory in the psychology department with complex motor tasks including the mark up of the aircraft controls, the rudder and air ground controls. They were called complex coordinators and he was studying susceptibility to motor interference so we had undergraduate students who came in and performed a large number of different motor tasks involving pursuit rotors and mirror drawing and all kinds of complex motor tasks, and we did large correlational studies to see whether individuals were susceptible to interference when the controls on all of these paths were reversed on them. So we were training them to fit a relatively high level of proficiency and suddenly reverse all the controls and determine how long it took them to get over the interference. So that was what I did, we all worked twenty hours a week on our assistantship and we really worked twenty hours a week and it wasn't on our own research, it was on our mentors' work. The same thing was going on in Kenneth Spence's lab, so for twenty hours a week we did our assistantship work. Not many of us had the opportunity to teach in those days, we were mainly research assistants, but we took our courses and I think toward the end of our second year I did a master's thesis and that was on verbal factors and motor learning. I got interested in those days in the effects of verbal labels and the effects on the motor process. I was never that interested in complex motor learning, it seemed too complex for me, I really wanted to get down to the basics of learning and so I got involved in the whole area which was extremely active at that time on verbal pre-training and the effects of verbal pre-training on very simple motor tasks, button pushing tasks. So my master's thesis was in that general area and at the end of our second year I believe we took written comprehensive exams and spent our third year doing our dissertations and taking additional courses. So I did graduate then in 1954 in general experimental. My dissertation was very centrally involved with a verbal pre-training study. In that case it was on what's called the star discriminator, a sixth response task. I was studying the effects of verbal labeling on that.

We both graduated in, I think, June of 1954. In those days couples did not look for jobs together, there were nepotism rules virtually everywhere and so, of course, it was almost a given but it was perfectly acceptable to me, I was perfectly happy to have my husband go on the job market, which he did, and the plan was for him to seek a job and then I would try to follow up with it and find something in the same area. Well, he received a job offer from the George Peabody College in Nashville, Tennessee, now part of Vanderbilt University, but it was independent at that time. Nicholas Hobbs, whom I think is pretty well known in the field—former president of APA—was just setting up a program in mental retardation at George Peabody College and he received a very large grant, kind of an institutional grant, for studying mental retardation. So my husband was hired to work on that and be a faculty member at George Peabody College. And Nick Hobbs was very good to me, he was very eager to have

me in his department and he went to the president of this small college and talked him into paying me a thousand dollars a year and gave me an office and a laboratory and I was then free to try to make my own way by getting grants, which I did. I applied for a grant from the National Science Foundation and was not successful the first time, but within a year I had a grant to support myself and my research from the National Science Foundation to continue the verbal factors research. Actually I was pretty consistent; I stayed in that area pretty much for my entire career moving from one problem to another in that area.

So for several years I supported myself on my own grant research there and I also began to teach in the department. I've forgotten what my first title was, might have been research associate or something along those lines, but I did begin to teach courses in statistics and experimental psychology there and eventually worked into a half-time faculty position there. By that time we had started a family and I really did not want to work full time and the arrangement that I had was that I was working half time and was at home with the children half time which I thought was an ideal kind of arrangement. It worked out extremely well for me. So we were there for six years from 1954 to 1960. During that time I also did some contract research for the Navy at Great Lakes Naval Station; they wanted Peabody to do a study on trying out different methods of teaching sailors to trouble shoot electronics gear and they needed someone to come in and actually compare doing evaluation of these methods, so I had taken courses with Judson Brown here at Iowa who was an expert on electronics and taught courses that I had taken and he was a consultant, and I would go back and forth to Nashville to Great Lakes and did that research. Other than that I was pretty much committed to my own research—by the way, I was working with rats. I had worked with rats starting at Iowa. When I was still a student at Iowa I took some courses with Kenneth Spence and learned how to do laboratory work with rats and was studying some of the motivational effects, the inner play between habit and drive within Hull/Spence Theory and I was doing that work with rats. So I had a rat lab at Peabody right from the beginning and was doing quite a bit of research on that. In fact, I think probably that NSF grant may have been on that particular topic, I'd have to go back and check. But I ended up teaching general experimental courses and stat courses and doing research with rats and with adults and with the college students there. So I really didn't have any background in child psychology until my husband was offered a position at the Child Welfare Research Station in 1960 by Boyd McCandless and he accepted that position and we came back to Iowa.

For my first year back at Iowa City I worked on a grant that I.E. Farber had in the psychology department and he hired me to help him with that, and later in that year I began to become affiliated with the Child Welfare Research Station. Charles Spiker was acting director at that time because Boyd McCandless was in Pakistan for a year, and so Charles Spiker had taken over as acting director and I believe—I'm trying to remember the chronology of this—my husband and I had a research grant at that time from—I can't recall if it was NSF or NIH but we were studying novelty effects and the motivational effects of novelty in children. And that's how I happened to get involved in child research through that collaboration with my husband on studying the effects of novelty and trying to tease out whether novelty was a motivational factor. Again, we were always very theoretically oriented within the Hull/Spence framework so most of our research had theoretical implications derived from Hull/Spence Theory and that was true of that joint research that we were doing at that time. Shortly thereafter Charles Spiker offered me part-time teaching positions and I began—again I'd have to check on the details of this—but I began to teach a course, took over a course that had been taught in the Child Welfare Research Station on children's learning. It was a natural progression for me because the theoretical questions I was interested in at the time really could only be answered by getting back to very basic learning and so it was natural that you would want to study them in young children rather than adults, where so much of the learning in adults involves transfer of training and I was really more interested in the original acquisition of the learning processes. I became well versed in that learning literature and then began teaching at the undergraduate level.

And about that time—I might just talk about some of the faculty. We are talking about the early '60s; Ruth Updegraff was here, Bill Hartup was on the faculty at that time. Howard Meredith was a central figure in the now-called Institute of Child Behavior and I might as well, because it was around that time

that the name was changed. Ralph Ojemann was on the faculty and retired about that time. Charles Spiker became the director when Boyd McCandless did not return to the University of Iowa; Charles Spiker was made the director and was the director for many years there. By that time Spiker's experimental child program was flourishing and in fact he, here at Iowa, and Harold Stevenson at the Institute in Minnesota were certainly two of the major forces in the whole development of the field of experimental child psychology. It was a major change in the field and quickly took over many of the child psych programs around the country. Prior to that time, there had been a flourishing field of child development, but it wasn't even really developmental psychology at that time, it was called child development and people were interested in studying changes in behavior that occurred with chronological age and there was a great deal of description of how children change both socially and in terms of learning process, how they change in terms of motor behavior. Of course we have all the Gesell norms and things from that period, but it was a very dynamic and flourishing area, but the whole thrust of it was to describe how children changed as they developed and until the period of the early '60s not that much concern with what were the causes or what were the factors that produced these changes with age; I don't think anyone really believed that just the mere passage of time was doing this. There were environmental factors and genetic factors and experiential factors that were producing these changes, and that was the thrust of the whole experimental child movement was to try to find out, not just how the behavior changed but why it was changing, what were the causes of the factors, and so the experimental method was brought to bear in answering questions about what factors produce changes in behavior. So many of the studies were cross-sectional rather than being developmental, in fact, in the early years of the experimental child movement they were virtually all cross-sectional and they were mirrors of research studies that had been done on adult learning but they were being done with young children. Of course Spiker's work evolved that way. And I did not mention Al Castaneda because he was gone by the time I came back, but he had been there during the time that we were in graduate school and was a major factor in the Child Welfare Research Station. And both Castaneda and Spiker were doing experimental work within the context of Hull/Spence Theory; they were testing implications of the theory as they applied to young children.

So the rest of my career really was very much along that path, starting in the late '60s. Spiker and I began to test some implications of Spiker's own extension of Hull/Spence Theory. Spiker extended Spence's discrimination learning theory and he had a very well elaborated theory that grew out of Hull/Spence Theory and with many testable implications, and so Spiker and I began to team up at that time on studying discrimination learning in children and later the effects of verbal factors on discrimination learning. It was all done within the context of the theory and we did a lot of research at that time in the late '60s. We obtained grant support from the National Science Foundation in which we were testing some implications of his theory he had actually derived from prediction equations for various types of discrimination tasks, and we did a lot of research mainly with kindergarten-age children, but to some degree with preschool-age children, but we went around to all of the local school districts around here. We bought a little research trailer and we would park the trailer next to the school and we would run through the kindergartens, between seven and ten children a day and I think we, together over the period of years, probably did twenty or twenty-five studies each of which involved anywhere from 90 to 200 children in each study. So we collected a lot of data and really used it to our advantage to extend and test Spiker's theory. I also did some rat research that was pertinent to the Spiker interaction hypothesis, stimulus interaction hypothesis and that needed, I thought, to be tested with animals as well as with children. So I did some parallel work there and actually collaborated with Kenneth Spence on some of that work. He was very good about allowing me to work within the confines of his rat laboratory.

So that brings me to—and as I said I became a—well maybe I didn't say—I became a faculty member in the Institute of Child Behavior over the course of that time. Much of the time I was supporting myself on grants, but began to work into a regular faculty position in the Institute and, again, I was working part-time by choice but as my children grew up and required less time at home, I increased gradually the teaching that I was doing and I was teaching children's learning at the undergraduate level and some methodology courses. I've always done a lot of teaching in methodology: statistics, experimental design, and that sort of thing. Although I think primarily most of that I did later after I was back in the

psychology department. Before I go on I'll just ask, because at this point I need to talk about what happened to the Institute and rather than my own personal history, but kind of at this point more on the history of the Institute. So is there anything I've left out in terms of my personal history that should be included at this point?

Hartup: Not that I can think of, I think you've been pretty thorough.

Cantor: Okay, well, then toward the end of the '60s the university became concerned really about the administrative functioning of the Institute. I don't think I have to elaborate on here that the Institute had a worldwide reputation; it was an absolutely outstanding unit. It had, I think, moved in the direction of becoming more psychologically oriented and less involved with some of the more social aspects of child development. The faculty were very much, by the end of the '60s, very much into pretty much straight psychological research, various aspects of it. We had people doing physiological work—I don't mean it was all Hull/Spence theoretical work—but it was really very much into psychology as compared with the whole physical growth area in which Howard Meredith had been very active. Well Meredith retired—I've forgotten what year but probably about—well actually he was acting director in the early '70s so it was right after that that Howard Meredith retired.

Hartup: And the context of that is that originally the institute started to be an interdisciplinary—

Cantor: That's right, it was originally very interdisciplinary, but really under Spiker, I think, well it moved away more from—it was still interdisciplinary but not nearly as much as it had been originally and it became known as a child psychology department rather than an interdisciplinary unit. And while that could be considered one of its strengths, and I think it was one of its strengths, it by some people was considered a wrong direction for the Institute, that is, it had become too specialized for some people. At any rate toward the end of the '60s—I should probably first say that administratively the Institute had been a separate independent unit in the university. It was not in the College of Liberal Arts and it was not in the College of Education, it was just unto itself, a separate institute that had been established by the legislature by the state of Iowa and it was administered by the Provost of the university and that was both a plus and a minus. It was a plus in that you didn't have to go through any Dean, the money came directly from on high and that was very nice. By the way, I should mention that the Institute had been supported by the Laura Spellman-Rockefeller funds throughout its early history. So I'm not sure who in the university—well I have some ideas about who in the university but I don't really have any hard facts so I'll just say that there were people in the university who felt that the Institute should be brought under the wings of the Liberal Arts College and so at that time Spiker had been director for quite a number of years—I've forgotten, let's see we said at least nine or ten years or maybe more—and he was tired of administration. There were beginning to be a lot of frustrations associated with administration and so he decided to step down as director and that precipitated a review of the Institute by the central administration. Now there were people who suggested that the administration instituted this review but they had in mind all the time what they wanted to do with it and that was to put the Institute in the College of Liberal Arts. Unfortunately it didn't turn out that way. The Institute faculty did not want to do that, and it wasn't so much that they didn't want to be within the College of Liberal Arts—that would have been okay—but they wanted to be a separate unit and part of going into the College of Liberal Arts would have involved becoming a wing or a part of the psychology department and the faculty definitely did not want to do that, they wanted to be autonomous. There had always been very good cooperation between the Department of Psychology and the Institute of Child Behavior, the graduate students, particularly from the Institute, had always taken a lot of course work in the psychology department and that was a two-way street to some degree; a number of students from the psych department took courses in the Institute too, but it was mainly the other way. And there was a fair amount of envy among the psychology faculty for the faculty positions in the Institute because they were twelve-month positions to begin with. We were supported for twelve months, we were a research institute, we had very light teaching loads, probably half that of the average faculty member in psychology, so we had our summer salaries paid during which we could do research and the rest of the year we each taught probably one course per semester.

Hartup: And what funds supported that?

Cantor: Well, that was supported by the state of Iowa, these were hard money lines. We had a president at Iowa, Virgil Hancher, back in the '60s when all of the universities were running after federal money for the first time. Hancher did not push his faculty of get their salaries put on grants. He felt that it was important that the state support the teaching and research enterprise and so, while he didn't object to people getting grants, he was not pushing people to get grants to support themselves and so these were hard money lines and the research side in the Institute was supported by the Spellman-Rockefeller funds.

Hartup: So there was enough there to support—

Cantor: There was enough money there to—research was not as expensive in those days. You didn't have high tech labs and you went in the lab and with wood, you built your apparatus, and with a little bit of electronics relays and relay circuits, and so on you could build an apparatus and we did to take care of our research so our research was not expensive. Mainly the cost was for hiring research assistants and the Spellman Funds really did cover that. There was enough in the Spellman Funds pretty much for every faculty member to have one research assistant. As always child research is very labor intensive and we had that work, we had plenty of support. We had our computer person who—in those days you had to write your own programs; Spiker wrote all kinds of programs and he wrote analysis variance programs that are still in use for the new computers—computers were just coming out. Spiker taught me to use the first IBM 650 which we had downstairs here in a room and you went in and you ran it yourself with cards; it was fun. So there was plenty of money in the Institute to support the research, and you can imagine why the psychology department would be envious of these faculty positions given all of that and the Institute did not want to go into the psych department and have to live by the rules of the Liberal Arts College, so the Institute fought these developments. Unfortunately there developed a schism within the faculty within the Institute and that was very unfortunate. There became two camps, one of whom pretty much wanted to continue along the lines that—the same direction the Institute had been going in, and others who said, No, we really need to start going in some different directions here, we need to become more general, we need to serve a broader constituency, we've become too narrowly focused, and it was the divisions within the Institute that I think basically lead to the breakup of the Institute. I think had the faculty of the Institute hung together, probably what would have happened is that the Institute would have gone into the College of Liberal Arts—I think that was preordained, I think the central administration had decided long ago they were going to do that, they didn't like having this separate little unit out here, it didn't fit their charts very well and so that was going to happen—but if the faculty would not have broken up I think that these two would have survived.

Hartup: As its own department?

Cantor: Either as its own department or as a kind of an independent sub-unit somehow affiliated with the psych department. Now the psych department wanted naturally to get what it could from the Institute but the review that came down on the Institute was quite negative, I think incorrectly negative, as these reviews often are. Some of the people who were on the committee had some axes to grind for many, many years in the university. The psych department was not the only department that was envious of the Institute, there was envy of the Institute all over campus because there were many psychologists—and still are—in a wide variety of places and they had always wanted to have collaborations with the Institute which just never happened. So there were people who felt that the Institute was too insular, there was not enough interdisciplinary cooperation with other departments within the university, and some of those people were involved in the evaluation of the Institute and so a lot of these complaints came down in this review. And with the faculty fighting among itself, the final upshot was that it was decided—actually, the proposal was sent by Howard Meredith and Charles Spiker to the central administration—that the Institute be broken up, that there was really no way to salvage this thing at this point, there was no way to agree on—and so their proposals were accepted by the university which many people would probably find very surprising, but it had just gotten to the

point of no return in the view of these two people. And so they suggested how the academic programs in the Institute should be broken or should be divided. I haven't mentioned previously the preschool program which was an outstanding program, always was from the very beginning at the Institute. The preschool program was outstanding and it continued to be, but that aspect of its functioning went into the College of Education partly, partly it went into the home ec department and various faculty members went either into the College of Education, department of sociology, and I think six people from the Institute, six faculty members came into the department of psychology. I was one of them, although I did not initially come into the department. My position was somewhat—well, for one year my position was somewhat in limbo, but eventually I did end up in the psychology department and at that time there were several different program areas in psychology, one of which was general experimental. Spiker and I went into the general experimental area which was appropriate given our interests and a new area of developmental psychology was developed with the remaining faculty members from the Institute, there were three or four.

Hartup: But you were not a part of the developmental—

Cantor: We were not, Spiker and I were not; in fact, because of the divisions within the Institute the other group of people who had been arguing for broadening the scope of the Institute wanted to go really more in the developmental direction and Spiker and I were pretty much continuing along the lines of the experimental child program. In fact, we developed a curriculum in experimental, a sub-curriculum in experimental child within the general experimental program of the psych department. The developmental program, in the meantime, developed a curriculum more along developmental lines. So that was in—actually I came into the psych department in 1973, the others had come in in 1972, and I began teaching undergraduate and graduate courses in children's learning in the psychology department. I was teaching experimental psychology laboratories in which I had maybe 15 students at a time doing experiments on children's learning at the preschool. We actually took them into a preschool. Then I was also teaching courses in statistics and later a course called Evaluating Psychological Research which is a course developed for consumers of research rather than for people who are going to do research, and I continued to teach that course to the end of my career and had a hand in the early developmental course.

At that time, during the '70s, that was part of the time when Spiker and I were doing a lot of research on discrimination learning in children. We had grant support and we got also interested in more complex forms of problem solving in young children and started studying hypothesis testing among children, so we had a series of research studies on the development of strategies of young children and the effects of dimensional labels and how that affected problem solving. We were interested in at what ages children began to use strategies and did quite a number of studies in which we taught very young children to use basic strategies and also studied how they developed along the way. I think at this point you no longer hear much about experimental child psychology because the field has changed to the point where virtually all developmental psychologists are using experimental methods. They are studying age related changes and then trying to do experiments to explain why these age changes are occurring and we were too, that's what Spiker and I were doing, we would do some developmental—

(tape paused)

Hartup: Okay, maybe you can back up just a little bit.

Cantor: I was talking about how Spiker and I along with the rest of the field, I think, began to use both developmental and experimental methods; certainly the Handlers did, I mean virtually everyone did. We looked at age related changes and the use of strategies and then we said to ourselves, well why are older kids, you know, seven-year-olds, using strategies and five-year-olds aren't? And then we went to try to figure that out and did experiments to try to see if we were correct about what seven-year-olds were doing and five-year-olds weren't, such as rehearsing and so on, and then we would teach the younger children to use these same methods and see if they were capable of doing it and I think we were able to show some of the earliest use of strategies—by that I mean youngest use of strategies

through given strategy training. So that was pretty much what we did through the '70s and '80s, we were following that line of research, and toward the early '90s Spiker retired and I retired a year later. He retired in 1990, I retired in 1991, but pretty much I think the research that we were doing at that time was involving the strategies for that period of time. So let me just stop right now and see what questions you might have about things that I have—

Hartup: Well, one thing that I was curious about in retrospect: do you see anything that the Child Institute could have done differently at a certain critical point that might have changed the course of its history?

Cantor: Yes, I think with 20/20 hindsight that we did become too insular. It would have been better to maintain more contacts around the university with other people who wanted to collaborate with the Institute, who wanted us to be more things to more people. We had a very central mission and I think we did it very well but, you know, we were too much unto ourselves and, therefore, we made enemies. Now, from a scientific point of view I don't think we made mistakes, but from a political point of view I think we did and so I don't think the direction the Institute went was the only direction it could have gone. I do think what it did, it did very well but it was fairly narrowly focused, and to survive during that period of time I think it needed to have broadened its scope. And probably the Institute at Minnesota did maintain a broader scope. Although they were very much into social development, for example, our Institute really didn't have a very strong emphasis on social development so, even within the field of developmental psychology, we tended to be fairly narrow. We didn't cover all the bases and had we maintained maybe more clinical interests, more interest in social development and so on we might not have run into the buzz saw that we did run into.

Partly it was the accident of this administrative arrangement, it really was. There is no question in my mind that it was a big mistake to break down the Institute. For one thing I think it was inevitable that the psychology department would take the resources of the Institute, which were sizable, and apply to other things which it did. Many of the resources, for one the Laura Spellman-Rockefeller Fund, were lost with the Institute. We tried very hard to get the psychology department to get those funds for support of child research but it didn't happen and that money has ended up being used for interdisciplinary research in the university and, unfortunately, much of it goes to the medical community rather than to the behavioral side, so that money is pretty much down the drain. It's been a long time since the psychology department has been able to really support developmental research with the kind of funds that it needs because it is so labor intensive and this has been a long educational process within the psychology department. I'm very encouraged at where we are today as far as that goes and, I think at this point in time, finally, once again developmental psychology is beginning to get where it needs to be. Child research which is extremely time consuming, it's very expensive to do, and it needs very special care and tending. A general psychology department does not understand that, or at least very few do and I think for the last fifteen years—well really more, maybe twenty years—this department did not understand that and therefore developmental psychology did not flourish in the psychology department during the period of the '80s, it just didn't. In fact, at one point in time, the developmental area was disbanded. Fortunately it has been rejuvenated and I think it's headed in the right direction and is going to be very strong, but because the institute was disbanded the reputation of child psychology or child development or developmental psychology at Iowa has suffered greatly and it's been a long, long road trying to bring that reputation back. It takes a long time to build something academically that's strong and it takes no time at all to tear it down, and so it's been very difficult to convince the developmental psychology population in this country that anything good is going to happen at Iowa given that the administration at Iowa ruined one of the best academic units in the world. You can understand why people would be leery about wanting to come here either as graduate students or as faculty members. Now I think that's beginning to turn around but it's been a long road back.

Hartup: I'll definitely attest to that being in the position of one of the developmental faculty members here.

Cantor: Well, it's because of wonderful young people like you that it is coming back but, you know, that's been very hard and the period of the late '70s and '80s in the psych department were extremely hard on developmental psychology. It really just about died, it really did. It suffered greatly and there were a whole lot of reasons for that which I won't go into here.

Hartup: It's interesting, actually, talking about making links and being broad and things like that, that it is important even within the psychology department today because I think developmental psychology flourishes partly to the extent that we can make a case that we benefit other areas in the department.

Cantor: That's right and I think the field is unique in the sense that child psychology or developmental psychology cuts across all of the basic areas in psychology. It's unusual in that it is an area that is banded together by a developmental level, by a subject population. You don't normally talk about rat psychology or sophomore psychology; you talk about social psychology or clinical psychology or learning psychology or perception psychology and, of course, developmental cuts across all of those areas and I think it's finally been recognized that developmental psychology makes a contribution to all of those areas, it's not just the other way around and that is its strength.

Hartup: Yes, it's interesting.

Cantor: Yeah, it's a very different thing. So I'm trying to think of what other aspects of all of this might be of interest for our history. The Institute was a very unique place—I might just comment on that—it was a wonderful place to be a graduate student, although I wasn't. The psych department in those days was a wonderful place to be a graduate student—I'm talking about the early '50s now. It was a very exciting time; there was no place that was more exciting than the psych department at Iowa and the Child Welfare Research Station. Kenneth Spence was a truly remarkable outstanding scientist. Gustav Bergman, a great philosopher—a philosopher of science—was collaborating with Spence at that time. You had Judson Brown here, you had I. E. Farber here, you had Spiker here, you had McCandless here, you had Updegraff here, and it was a really exciting time in the history of psychology. As a graduate student you became really caught up in this great mission—at least in the psych department—in the mission of advancing theoretical work, specifically Hull/Spence Theory, but it could have been any theory and it was exciting and it was challenging and we worked extremely hard. Moral was extremely high, we all worked from morning till night. We were here from 8:00 in the morning until midnight every night including weekends with the exception of Saturday night when we partied. Iowa has a reputation for heavy drinking and faculty were very heavy drinkers, so were the graduate students and we had really very wild parties. That's what we did to blow off steam, the rest of the time we worked. We ate our meals at restaurants and were here in what is now Seashore Hall, but was then East Hall, from morning till night. We played handball in the corridors and so it was very much a family of graduate students. I think it's really changed a lot now. Graduate students have lives of their own outside, but then this was our life, I mean we were all in it together and it was one big family.

Later then in the '60s in the Institute that was a different experience, it was a small faculty, we had twelve, maybe not even that many, ten or twelve faculty members and it was a very cohesive group. Every morning we drank coffee together and we didn't chit chat, we talked psychology, we talked about our research, we talked about academic issues. And the graduate students were invited and they came. In later years they didn't, but in the early years the graduate students came and it was very much an intellectual community and very exciting and the Institute was run in a democratic way. A handbook was produced and they pretty much followed a democratic process. The director of the institute, however, was appointed by the president of the university and served at the discretion of the president. So, in another sense, it was an autocracy of a kind. I happen to think, because of what I've seen over the course of my career, that electing chairs of departments has not been in the best interest of the academy. I know it's nice to be able to vote your leader in and out but, on the other hand, it becomes a political process and I think it's very hard for the elected leader of an institute or of a department to withstand the political pressures of getting reelected; much of the decision making

is based on trying to stay in office rather than on what is academically correct or will produce the best academic results. So in viewing what's happened to universities and departments over the course of, what are we talking about here, 35 or 40 years, I think a lot of the problems that we see in universities and the decline in academic excellence can be attributed to the fact that the hard decisions that sometimes have to be made to maintain standards and to maintain quality cannot be made in a political fire. So you could get a director or a department head who was doing terrible things and the only recourse would be to go to a central administration and try to get them out, which you could do, but if you had a good strong leader you had the best of all possible worlds. Kenneth Spence was such a leader and I think Charles Spiker was such a leader. I think Boyd McCandless was although I cannot speak from personal experience because we didn't overlap and I was not on his faculty. But Spiker provided very strong leadership and so did Kenneth Spence and I think they both did outstanding things for their departments that they could not have done had they been trying to maintain themselves in an elected office.

Hartup: That's an interesting observation.

Cantor: So for what that's worth, I have noted, from my point of view, a decline in the quality of education and the quality of the student body over the course of my career and it isn't that we don't have outstanding students as we did in the early days, but rather that at the undergraduate level we have a lot of people in college now who are there for the wrong reasons or who really are not qualified to do college work and so what our country's going to do to bring that back to a higher level again, I don't know.

Hartup: It's a difficult question, I think.

Cantor: I will have to say that seeing those changes over the years took some of the fun out of teaching. As the years went on and students seemed less motivated, and were there not so much because they were interested in the material but because they felt they needed a college degree.

Hartup: Yeah, absolutely.

Cantor: So I enjoyed the teaching earlier in my career rather than, I think, later although there were some exceptions; I thoroughly enjoyed teaching psychology courses. By and large it was a fine career, I have no complaints. I think I devoted more time to my family than young women probably do today. I have no regrets about that whatsoever. If I were doing it again I would do it the same way.

Hartup: Well, it's wonderful that it worked out that way for you that you could do the half-time stuff and that ultimately it resulted in a full time career.

Cantor: You know, life is a matter of choices and I chose that way and I did not—I think I probably could have had a somewhat greater career along some lines had I worked at it full time or had I gone independently of my husband at the time but you can't really have it all. You just have to make some of these choices. I never felt that because I was a woman I had to do it that way and if I were doing it again now I would do it differently; I wouldn't. I worry some about how much time both fathers and mothers have with their children these days.

Hartup: Well, in academic families I think it's very different than it used to be.

Cantor: Very difficult, my hat is off to you young people who are doing that. I just don't think I could have done it.

Hartup: I sure don't know how some people do this with young children.

Cantor: I really don't, but they do and they are wonderful at it. But, you know, with those reservations I probably could have done more, you know, if I hadn't had a family, but I wouldn't trade that for anything.

Hartup: Right, right, that's right.

Cantor: So that's pretty much all I have to say, unless you can think of anything that you want to hear more about.

Hartup: No, actually it's been very interesting for me to hear this and I have been looking forward to interviewing you, even though it's taken a while for us to set this up, because I was interested in hearing more about the history of the Institute and about your personal career.

Cantor: Is there anything more on that, I'm sure I could probably talk more about it but I would probably need to be prodded with questions. Do you have anything about the history—I didn't say that much about the preschool program but I do want to emphasize it was really an outstanding program.

Hartup: Was that run by teachers who were affiliated with the—

Cantor: Well there was always a faculty member, it was Ruth Updegraff initially, and I'm blocking on the name of the woman who ended up as head of the preschool, but she was wonderful. In the early years—well actually I think even toward the end there was always an academic program in the Institute on preschool education, you got a degree in preschool education and it was those students who were teaching. Their assistantships were in the preschool.

Hartup: Like Lucille must have donated the money—

Cantor: Probably.

Hartup: I think she got a degree in teacher education here because she worked with Gustav Bergman.

Cantor: And that was a very large program and a very good program, and really Ruth Updegraff was very central in establishing and maintaining, well I don't know about establishing it, but maintaining. Bill Hartup was actually very involved in it too. We were very sorry to lose Bill.

Hartup: Well, I guess one thing I was curious about is that you said that this schism developed in the Institute during this time. Was that brewing for a long time?

Cantor: Well, it's a little hard to tell about those things. It might have been brewing but I think what happened is that when the review of the Institute came down and was negative in many ways, and I think personally very unfairly negative, a lot of it was just really off target. Some of it was on target but the majority was really off target, but at any rate I think people got frightened. I really think that's what happened, especially among some of the younger people; they felt like the way to hold on to the Institute was to agree with what was being said by this review committee. To accept that and say, Yes, you are right, we'll do whatever you say, you are right, we need to broaden out, we need to do this, we need to do that, and so I think they felt that was the way to go to save the Institute. And there were some of us who didn't feel that way and felt these were not necessarily good directions and that these were, in many cases, unjustified criticisms and I don't think the faculty would have divided if it hadn't been for this review and this negative tone. I think if the review would have come back supportive and highly positive the Institute would still be in existence today. I don't have the slightest doubt, in fact we had proposed—we had a replacement for Spiker as director; Hayne Reese was to be the next director of the Institute and they were all ready to make that appointment when this review came into the picture. Someone up there—and I can probably tell you who, but I'm not sure so I

won't—decided to do this institutional review first, I think that's what happened; people got frightened.

Hartup: It does seem unusual how that all came down; there is usually a probationary period or some time when recommendations can be acted upon and then they review again or something like that.

Cantor: Well, you would think so too. Two things happened, I think. As I mentioned before, the central administration had already decided beforehand what was going to happen. Now I don't think it quite turned out the way they wanted it but something major was going to happen. They wanted it and they wanted to do it now and the other thing was that I do think Meredith and Spiker were extremely influential and because of the divisions in the faculty and it got really very acrimonious they felt there was no way to pull this group back together. It had just gone too far for anything to be salvaged and so that was the only thing that they could see that was going to work at all but trying to put this thing, Humpty Dumpty, back together just was not going to work. That's a comment on how bad it got. And I think they had, you know, a lot of influence and so pretty much exactly what they proposed was what happened.

Hartup: That's interesting because you would think for the sake of self-preservation that a group would try to pull itself together.

Cantor: You would think so, it was hard to believe what was happening, it really was. But you know when you say was it brewing, it must have been, because I personally couldn't believe the things that were happening and they had to have a history but it must have been kind of varied or at least I was not aware of it anyway. There were these dissatisfactions brewing for months or years prior to that; I wasn't aware of it and maybe I should have been. Sometimes, you know, you are kind of caught up in your own—

Hartup: Well, at that time in the '60s there was the whole Piagetian movement that was going on—

Cantor: That was not represented at Iowa at all.

Hartup: And that was not accepted.

Cantor: Right, and even the more cognitive side of things. I think we were at fault for not welcoming more of the cognitive development movement. Now the cognitive development movement has turned out to be very strong scientifically.

Hartup: And information processing?

Cantor: Yes, but the early form of it in developmental psychology was not very experimentally oriented and was very fuzzy I think, fuzzy headed in many ways.

Hartup: Well, certainly the Piagetian approach, I mean methodologically, was going back a lot.

Cantor: Right, and there's no way that would have been accepted by the majority of faculty members at the Institute.

Hartup: Right, so in developmental psychology there was the Piagetian revolution while in the adult world it was more information processing.

Cantor: Exactly, but that took a while for information processing to become dominant. Now, had that been in the picture early on or had we had the foresight to see it coming and we were sort of moving in that direction.

Hartup: Right working on kids' strategies and things like that.

Cantor: There were some timing things that just came together and ruined an awfully good thing.

Hartup: It's weird how some things just happen. Well, all of us who are here now, developmental psychologists were really pleased to be here [the100th anniversary of psychology at Iowa] and hope very much that Iowa can be brought back into the world of developmental psychology.

Cantor: Well I think you are well on your way. I am just delighted with what's going on in developmental psychology in this department now. I think you and the other young people are just terrific and I've been telling everybody that I talk to, you know, that you are well on your way to bringing it back. I think it's too bad you don't have the Institute and the resources in the Institute. On the other hand you have gotten some very good resources in the department now so you can be finally cognizant of how important you are to the field so I'm extremely optimistic about developmental psychology at Iowa. I just wish you still had the Institute to do it in.

Spiker and I worked and worked and worked on trying to get that money [the Rockefeller Funds] into the psych department. There were a lot of political things going on; the reason that I, for instance, was held out of the department for a year, I was kind of a pawn because the psych department was eager to get more of the resources. The psych department did get a lot of assistantship lines and a lot of faculty lines from the Institute and I was a kind of bargaining chip in all of that. So there were just a lot of political machinations going on at that time. Well you can imagine with the breakup of the Institute, a lot of departments involved, as I said, we were the envy of the university as well we should have been, because we were in the best academic positions I've ever heard of . They were absolutely fantastic. We couldn't have found a better academic position and so it's not surprising that there was a lot of competition for the resources.

Hartup: Well, anyway, thank you very much for sitting down and doing this.

Cantor: Well, thank you for doing this, it's nice of you to take the time; I'm sorry it's taken me so long to do this.

Hartup: That's okay, I'm glad it's done.

(End of interview)