

Frances Keesler Graham

- Born August 1, 1918 in Canastota, New York; died April 16, 2013
- B.A. (1938) Pennsylvania State University, Ph.D. (1942) Yale University

Major Employment

- Professor/Professor Emerita, University of Delaware, Department of Psychology: 1986-present
- Research Associate/Associate Professor/Professor, University of Wisconsin, Departments of Pediatrics and Psychology: 1957-1986
- Assistant/Instructor/Research Associate, Washington University Medical School, Departments of Psychiatry and Pediatrics: 1942-1948 and 1952-1957



Major Area of Work

- Psychophysiology

SRCD Affiliation

- President (1975-77), Publications Committee member (1969-1977), *Child Development* editorial board (1966-68), Governing Council member, Finance Committee member, Social Policy Committee member, Program Committee chair, Nominations Committee chair

SRCD ORAL HISTORY INTERVIEW

Frances K. Graham
Waisman Center
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Interviewed by Carroll E. Izard
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Izard: It is my great pleasure to interview Dr. Frances Keesler Graham, one of the distinguished people of SRCD. We'll begin, Fran, with a fairly general question that I'm sure will be of interest to many, many listeners and readers. Tell us a little bit about your family background, along with any childhood or adolescent experiences that you think might be of interest. Where were you born, where did you grow up, and what sort of schooling, your early work history that might have played a part in your career?

Graham: Well, I'll try to keep this short because I published, in 1988 in the *Eminent Women in Psychology*, something of this sort that was edited by O'Connell and Russo, a fairly lengthy description of my history, particularly from the point of view of being a woman.

Izard: That's a good account; I saw it.

Graham: Which, I mean, it (sex) does have an impact. So I will try to keep to the minimum and mention only things that weren't mentioned in that. I was born in Canastota, New York, which is a small town in central New York state. I was the first child, born in August 1918, and I have a brother four years younger who is still living. My father was Clyde Keesler; he was a civil engineer who worked as treasurer of a family business, a wagon-building company that was in Canastota, that's why we were living there. But it folded after World War I; the demand for wagons was reduced at that point. So when I was five years old we moved, first to Toledo and then shortly afterwards to Philadelphia and my father set up a sales agency for heavy-duty construction equipment. He also designed some of the equipment. And he had a partnership with my mother and then later with my brother and still later

with my brother's sons. My mother was Norma Van Sutam and she was a librarian and also a professional singer for quartets and weddings and church, but she gave up everything except the singing when she was married and she did not work except that she did work with my father. She taught herself accounting and she has served as the accountant for the business for many, many years. Let me see, if there is anything else that I should add about this. Oh well, I want to say a little bit about what kind of parents they were -- I think that's relevant.

Izard: It sure is.

Graham: We were a very close family and my mother and I were, my parents didn't believe in being friends with the children, they thought they were parents not friends. But in fact my mother and I were confidants, there wasn't anything really that I wouldn't talk to her about. My father was more remote, he was a quiet soft spoken, but quite clearly a person of presence, and he spent loads of time with both my brother and me, particularly on sports. He set up all kinds of things around the house. He developed a game which is -- I think he developed it, I've never heard of it anywhere else -- called Pots. Did you ever hear of that?

Izard: No I don't think so.

Graham: Well it's a backyard version of croquet I realized years later. You dig pots in the ground and you use marbles, and you shoot the marbles into these holes. And otherwise the rules are all the same, if one marble hits another -- well it brought the kids from the neighborhood like -- to the house -- like honey for bees.

Izard: We played a game something like that but we didn't have the pots.

Graham: Well I've asked people -- I got curious later in life -- had anybody ever heard of it and nobody ever had. But it was really just great. We took a big hunk of the back yard, he kept the grass out of that, and then he would stand around and referee. He also did a lot of coaching of us in all kinds of sports. He taught us to swim and ice skate and roller skate and basketball, tennis, everything. And I admired him very greatly.

Let me see, what else should we say? In connection with intellectual development we didn't have any overt pressure to do well in school or anything of this sort, but it was obvious that high standards were expected. That was just part of things. And I can remember all of the quotations, you know, "never put off until tomorrow what you can do today," and "anything worth doing is worth doing well." I challenged him later on that; I thought you ought to be able to have a few frivolous activities. There were lots of books in the house and I -- there is one thing that particularly stands out in my memory and you, knowing me, will understand what I mean, I think you will. There was a book on Greek Mythology with just marvelous wood engravings in it. And so before I could read or even knew there really was such a thing as reading, I used to pour over it. There were pictures of Pegasus and the winged horse and Medusa, all of these odd things. Of course, I have -- I don't know whether you know, but I have a great passion for everything Greek today and I've been to Greece four times. Nobody taught me to read or any of the things that parents often do push you into. But I think just before I was due to start first grade -- there was no Kindergarten in this small town. We lived in Prospect Park which is a small town suburb of Philadelphia, and there were lots of woods and meadows and things around, it wasn't like suburbs today. But they gave me a small desk and it had a blackboard and it had a rotating screen. And on that screen were all these fascinating things, you know, like an upside down triangle with a bar through it and two bars connected by a bar. And I don't think anybody taught me, I think my parents expected me to figure out what these items were, but they'd answer questions, but there was never any formal teaching. And I just spent hours because the while alphabet was there. All the numerals, and then there were puzzle things, like two things were the same and one would be different but they weren't always in the same position. So it was a kind of workbook, but with no instructions. And I think that was kind of typical of what they did generally in parenting, which was not so much explicit instruction as providing opportunity and standing back. They really put a strong

emphasis on independence. They wanted children who found their own way. So that's -- those are the kinds of things I especially remember. But there was one, the most important kind of experience in my whole growing up is that I spent alone with my father's father and his only sibling, his sister, who was a biology teacher in high school, and I'd spend the whole summer with them on a -- on what they call camps, and they still call camps in New York state. These are simply summer homes. We spent part of the summer, most of it, on a big lake, Oneida Lake in central New York state. And then in the Upper Adirondacks in a hunting lodge that was way in, we had to take everything in on pack baskets. And this business of being the only child was kind of fun for somebody who was a first child and had to put up with a little brother, you know. And I had the attention, the whole attention, of two adults, both of whom were extremely interested in everything to do with biology and nature, and I could go on and on, but I won't, about those wonderful summers. Really my aunt was almost like a second mother. And it was she who gave me my abiding interest in biology. My father too, but particularly my Aunt Ruby.

Izard: That interest has stayed with you all of your life.

Graham: Oh yes, yes. And my aunt and I remained close until she died at the age of 85 out picking blueberries in the sun at noon day. She had a heart attack and like that was just gone. My father also died the same way, that's part of the genetics of the family, but it affects the men younger than the women. I think that age is a reasonable age.

Izard: It's a fine age to achieve.

Graham: One other thing that I might mention about influences, my mother is the fourth of five children and had a sister just two years older. So she never had to do any housework; she always did outdoor work. And she didn't like the housework, needless to say. Not so many women do. But at any rate, she taught me the sewing and cooking and things like that, but along with it the attitude that these weren't really the most exciting things to do. Being outdoors was much more fun and that again is the same I have felt.

Izard: Over what period of time did you do the trips to the Adirondacks and --

Graham: Oh from the -- I think the first summer I spent with them alone was when I was eight or nine. And I continued through adolescence.

Izard: Oh, so this was a long time.

Graham: Well I guess in the summer I was -- the summers I was sixteen and seventeen I worked in New York City in the stock exchange -- a kind of, essentially a telephone person. But the end of the summer, I went to the Adirondacks then. And I took my children there later.

Izard: It was an extensive experience then in nature.

Graham: It was a very extensive experience. Yes, from nine to, through fifteen. Most impressionable years. Well let me turn now to the schooling, because I think that was one of the questions you were asking about. Well, I went to the public schools in Prospect Park and that was, as I say, a small town. They were relatively un-crowded, there weren't any such things as special classes for gifted or disabled, but I think the school handled them extremely well. There are several things I look back on. We did have a blind boy in the class who came in at around the fourth grade -- the same people go all the way through twelve grades in small towns like that -- and he was wonderful and he was very smart and he had been tutored before they sent him to school. But then he went along with everybody else, but that's a strain on teachers you know, to have that sort of thing. But I thought they were very adaptable generally. The school had a policy of giving you advance placement if you were advanced, and so three of us in the middle of third grade got moved into the middle of fourth grade. Well that was sort of, it was rather fun, but the fourth graders had already had fractions and decimals and we hadn't come to them yet. But the teacher would give the -- while the other kids were doing special

assignments she would give us special tutoring to catch us up. And we caught up, it took us most of the fourth grade to catch up but then it was all right. So, again, there was an ability on the part of the teachers to adapt to a really wide range of abilities. I'm not sure that doesn't have real advantages. You grow up amidst everybody and amidst the spectrum of skills and yet you're not, I was never bored. Another thing that I was thinking about that they did, most teachers -- I remember one exception -- always let you read library books if you were advancing all right in your work. And we had one teacher who gave -- that I remember, there may have been others -- who gave special assignments for children who were advanced. And we had an, I think it was an eighth grade teacher, who later married the foreign minister to Iran and moved to Iran, Miss Powell, and she gave three of us, she exempted us from all study of grammar, and, instead, had us do writing; short stories and essays and poetry. I don't think any -- I think there were just three or four of us -- I don't think any of us ended up doing writing.

Izard: So how did you learn grammar?

Graham: Oh we knew it. I mean we knew enough.

Izard: You came to this writing stage with the basics, so you just moved right into writing?

Graham: Sure, we already had the basics she expected us to know. If there was some grammar we didn't know, we should find it out again for ourselves.

Izard: There may be no better way to learn than by writing and then getting feedback.

Graham: That's right. And then she gave us feedback on everything.

Izard: Now this is all taking place now, you're still in Prospect Park.

Graham: Prospect Park High School, eighth grade. We had six grades of elementary school and then there was the junior high system when you began to change classes and then three years of high school. And there was both a commercial and academic -- or whatever they called it -- track. So that, in the academics, there were fewer than in the commercial. So our classes were even smaller than in high school.

Izard: How much of a part did the city of Philadelphia play in the --

Graham: None. This way -- Philadelphia was even in a different county, we were in Chester County.

Izard: So you were far enough away that you weren't in and out of the various museums?

Graham: Oh yes, we did go in to Philadelphia. That's where we did our shopping.

Izard: Were there any special experiences there that played a role in your schooling or education?

Graham: Well, the thing I most remember is, well, first my parents gave me elocution lessons and they were just as old-fashioned as the word implies. You know, I had to memorize poetry and perform in front of this teacher. I had to take a long street car ride to go to this once a week, I think it was, in the late afternoon and evening and I know I came back in the dark. I wasn't really too keen on that. But also my parents enrolled me in a little theater kind of thing that was at a distance, this was when I was in high school. I was quite keen on dramatics and expected to make that a profession at one time. I don't think we ever went to a museum. It's curious because my parents were very -- thought education was the most important thing in the world. I remember that's one thing my mother said, among many. Music there was fair amount of. My mother still was -- she was a contralto, so she would have the contralto lead in local musicals and operettas -- that's what they were, they didn't perform musicals. And I never saw -- I remember being taken to the theater and seeing Barkley Square. I thought that was fascinating. I can't -- so there wasn't much of -- except the theatrical experiences I

had personally, there wasn't much going to the big theater, which was -- I mean Philadelphia has always had good theater because it gets the tryouts before New York. And I don't think I had ever been in a museum until I went to college and took a course in art and music appreciation and I was -- it opened a new world. So that gets me through my secondary schooling.

As I mentioned the only work history I had was in baby-sitting and those two summers in the stock exchange. And I did have some work experience. I waited on -- no I didn't -- yes I did -- I waited on tables one summer at the seashore, but that was primarily to get to the seashore for the summer. And I was a hostess in a restaurant when I was a graduate student to help add to my fellowship. And then I did some research assistant kind of things there, but I think you want to come to that later.

Izard: Okay. Well, are we ready to talk about college experiences, intellectual development during those years?

Graham: The college experience was just tremendous. I did forget to say that one of the things about my secondary school that was not satisfactory was that I hated science. I had a general science course in eighth grade by a male teacher and he just talked about airplanes and automobiles. He talked to the boys and completely ignored the girls. I didn't even know what he was talking about.

Izard: What about biology courses?

Graham: Well then there were other courses but he taught them. So I was required by state law to take one in high -- one high school course in science, so I took chemistry with him. And that was more interesting, but I decided that science was not for me. So when I entered college, I entered as a mathematics major. I'd always liked mathematics and my mother and father had, were both very good at it. I used to -- my first year in college I substituted mathematics for science courses, but when I got to my second year there was a social science series and they wouldn't let me substitute mathematics for that. So I was forced to take the undergraduate introductory psychology course. I didn't expect to like it at all, but the teacher was fantastic. He was a first, first year out of his Ph.D. from Minnesota, a clinical psychologist named Fred Brown. Do you -- have you ever heard of him? He still -- I see him in the APA directory but -- he's in New York, but I've never met him. He was there only the one year. But he did a lot of things. He conducted little experiments in class and they would turn out to be what he had predicted. I mean, they were ones that worked.

Izard: He was very remarkable for his first year out of graduate school.

Graham: He really was terrific; he even hypnotized a student in class. And he offered free psychological testing to any of the students who wanted to and I went and did that and I found those form boards and things like that really terribly interesting. So before the semester was over I had decided I wanted to be a psychologist. But the main reason that I was hooked was because he taught psychology as something that -- in which there was a whole lot yet to be learned and it could be found out through research. And this idea that I could be a person who would discover new things just seemed to me the most exciting idea I'd ever had.

Izard: So now science has become a part of your life.

Graham: Indeed it has. And immediately, as a result of that, I telephoned my parents and I said, "Could I finish college in three years and take the money from that fourth year and use it for the first year in graduate school?" And they said, yes, I could do that if I wanted to. So I planned very carefully, I had a year and a half and one summer also in between to get enough credits to graduate. And you had to have a whole lot, 132, if you can imagine. I just made the 132 on the line. So then I filled in with all the science courses that I had avoided. I took physics and chemistry and I took them with the engineers and not with the liberal arts baby course. And I took biology and genetics and I even got a course in human anatomy, which was unusual for a college to have, and I thought that was fascinating. And you see, it's funny that I had -- I don't know whether biology was or wasn't offered in

high school, but I wouldn't have taken anything from that teacher again, no matter what it was. So that was sort of, that was the important intellectual aspects of college. So I was going to graduate school next.

Izard: How did you come to that decision, when did you realize that, did this first psychology professor --

Graham: That was it.

Izard: He made it clear that to do this research and to make these discoveries you'd have to go on to grad school?

Graham: That's right. He showed me the way. And I talked to him and to other professors. I became a major in psychology. But then I had -- I thought that was important then. I don't think so now; you don't need to be a major if you've got -- you get all your good psychology in graduate school. And I decided to go to Yale because of Gesell's Child Study Institute. And I also learned that Penn State had a one year fellowship for graduate work which you could use at any university. I think that was really remarkable. And you had to submit an application and tell them what you proposed to do research in. And so I proposed to do a study on effects of infant malnutrition on later development, and that was because of a personal experience. At the time of my birth, my mother had a very severe postnatal infection, and she remained hospitalized for several months. And so my grandmother took care of me during that period, but I was -- really failed to thrive. I didn't regain birth weight until several months after birth. So it must really have been quite traumatic. I suppose, you see, without a wet nurse, it wasn't so easy that day to provide nutrition to a young infant. So that was certainly a profound experience in arousing my interest in infants, because at the time I wanted to do this, I hadn't even seen a newborn, for example. Well I had seen my brother, but I had no memories of that. He was born when I was four. So, shall I go on to graduate school?

Izard: Yes. I think we have some nice highlights from college. You owe a debt to that Fred Brown.

Graham: I do, and I wish I could see him some day, but I never did. OK, well Yale then. And that was another tremendous experience. Yale has always had a good psychology department, but I think it was when I was there in the late 30s was one of the periods of real flowering. We were -- the department was in the Institute of Human Relations, Rockefeller funded. It was adjacent to, or part of the medical school. So again, there was a lot of opportunities to pursue my interest. We took courses in physiology and neurophysiology in the medical school. And again, Yale had the style of education I like, which is your independence. The course work requirements were very minimal. I think there was a pro sem for two semesters, one statistics course, and one experimental course. Those were the only requirements and you -- I never knew how many courses we were supposed to take or credits we were supposed to have. It was never mentioned. But you started research and you started it right away. I guess everybody worked about a fourteen hour day about six days a week. A little different from the way things are now. And it was a very exciting place to be. At that point, there were two general theories that were important. One was psychoanalysis and, you know, Bob Sears work, for example, and the other was, of course, Hullian learning theory. And they were trying to blend those two general theories. And everybody's work was theory-oriented except Gesell's. And I temporarily shelved my interest in child psychology and really worked in straight experimental psychology. However, I did do two things to maintain my interest in child development. And one was that I had a three-year, sort of clinical research experience with Pat Sears in a child guidance clinic in Bridgeport. We went down one day a week. And I also, during those three years, had a kind of internship in pediatric psychology with Dorothy Marquis. Now you remember Dorothy and her pioneering studies of classical conditioning and neonates. And I also had a chance to help her with those and that's when I saw my first newborns. I mean she brought in, you know, her first subject, there's this red wrinkled creature. And I was -- I thought babies looked like the pretty pictures in the magazines, new babies.

Izard: So this must have been a big impetus to your growing interest in child development?

Graham: Oh yes, I mean with the two people -- Patty, that's Pauline Sears -- Patty and Dottie Marquis, that I could do without Gesell and the Child Institute. You know it's (i.e. Gesell's) such a theoretical research and the general emphasis in the department was always on theory and you didn't do anything that wasn't to test a hypothesis. And I think that certainly influenced me, it fit with the way I liked to do things, but it as just wonderful training. So that gets me through college (i.e. graduate school), now where do we go?

Izard: Well, let's see. We responded a little bit to the idea of the origins of your interest in child development, but maybe you could just sum up a little bit on that.

Graham: Well, I think I've covered those. I mean there was my own personal experience with being a malnourished infant. And I guess also my mother was very -- my mother and my father were very child oriented. And we talked a lot. They made explicit some of their ideas about education and raising children and they were certainly -- they certainly followed many of the things we think of today as desirable. And then the -- that sort of was the -- those were the background or underlying factors. And then when I got to Yale the interest with both Patty and Dottie maintained them. I'd had a child psychology course as an undergraduate but it didn't really appeal that much.

Izard: Did you -- were you feeling especially lucky that you had these two women role models before we began being concerned about women role models in the profession?

Graham: I didn't really feel lucky. I didn't really think of it one way or another. I still wasn't that aware of the cultural roles that were going to be imposed on women. You see my mother had worked and she had rejected the obvious women things like sewing and cooking. She did them but --

Izard: It was sort of coming naturally to you and yet you did have the good fortune of winding up at a school where there were women active in research.

Graham: And there were women graduate students in Yale at that time, probably in more numbers at that time because the war had begun in '39 and I entered graduate school in '38. And so there weren't -- yes, I was there from '38 to '42. So there weren't as many men available. Katherine Miles was also there but I didn't have much direct contact with her. There were no women on the faculty, however. So I -- but I hadn't really -- it hadn't sunk in yet. But I began, with the psychoanalytic training, to get some notion that there was a picture of women in psychoanalysis, you know, and anything assertive or even achievement oriented would be penis envy, wouldn't it? God knows I didn't want to be guilty of that.

Izard: Okay. Well is there anything more you want to say about research mentors? You've covered several of them.

Graham: I did. I didn't say what my, you know, my research was, I just said I shelved development, the work with Gesell, and then the other things I did. But my formal advisors were Don Marquis and Clark Hull. Together with those two as advisors I did a white rat study, and it was a mature white rat, mature white rats, and it was on conditioning. So that was pretty standard output for a Yale graduate student. And both Marquis and Hull really -- well all in all it was just a wonderful faculty. Bob Sears too. We, I became close friends with the Sears, because I worked with Patty and Bob taught us. There was a group of young -- they were all assistant professors -- not Hull of course, but Marquis and Sears, and Hovland was there too. He was a great cognitive psychologist but he died very young, in the 50s. He died when he was not yet 50.

Izard: Yes, yet it was young.

Graham: Neal Miller. I don't know how I could forget Neal, because Neal, he didn't teach the graduate students. He wasn't verbal enough to, I don't -- well, he had a research appointment at that time. But he had strong research, and he was like Sears. And Mowrer was there also -- Hobart Mowrer -- who were all trying to kind of integrate psychoanalysis, like the conflict studies of Miller's that are so well known, but to integrate it with Hullian theory. So I think that covers enough. You're pulling me on to say things I want to say but I'm trying not to take too much time.

Izard: Now then, are we ready to talk about political and social events that might have influenced your research and writing? Are you aware of any of these other than --

Graham: Well, the Depression was one certainly, because that really constrained financially what you could do. And then, of course, the women's movement, and I mean, that began in the late 50s and I really feel that I personally owe a great deal to it. I had kind of lost my self-confidence, had been worn down over the years. You know the troubles of trying to get a job, working to find a niche in which you could work. I remember I gave a talk at the -- invited by neurologists -- I can't think of what the convention was, and I walked into this huge hall, full of nothing but men and it was obvious when I went to register that they thought Frances was a man's name. They were not expecting a woman. That was a very traumatic experience. Well, at any rate, the women's movement helped at least to restore self-confidence. It also gave me a much better understanding than I had ever had about social influences.

Izard: I know from our previous conversations that there was a span of years after your Yale Ph.D. that were not easy and you didn't find a niche that you -- that was comfortable for you as a creative scientist.

Graham: That's right, I did. I had to -- I did clinical work.

Izard: How long did this last?

Graham: Well, I graduate in '42, I had a couple of years at home with parents while my husband was overseas, but otherwise I worked all of the time. But I had, I did have three years at Barnard that were great, but Barnard was an undergraduate school. I did set up a laboratory and do some research there. And then when I went back to St. Louis I had, well, then I really began research in earnest. But I did not -- I got a grant, money, and we will come to that.

Izard: Let's get an idea of the age range we're talking about here. This was -- how old were you when you were at Barnard?

Graham: Well I graduate from college at 19 in 1938 and I was at Barnard from '48 to '51. So I'm 29 to --

Izard: So you finished Yale in 19 --

Graham: I graduate from Yale in '42. So I was 23 then. Then in the next ten years I did, first I did the clinical work in St. Louis. I became acting director of the St. Louis Child Guidance Clinic. And I was also in the -- both, no, just the department of psychiatry then, later when I came back I was both in pediatrics and psychiatry. So that's those ten years. But I didn't become a faculty member until 1964, which was 22 years post Ph.D. I finally got a faculty appointment. I would be things like research assistant or research associate on grant money that I myself brought in.

Izard: And you were actively on the job market for a faculty position during some of this time?

Graham: Well, no, because I was with my husband in St. Louis. I did have the job, I had a faculty position at Barnard, but what I meant to say is that in 1964 I got a faculty position with tenure. So I was an assistant professor at Barnard -- no, I was an instructor at Barnard. That was a legitimate

position in those days and I had been promoted to assistant professor, but we left. We went back to St. Louis.

Izard: And your first tenured appointment was --

Graham: At the University of Wisconsin.

Izard: At Wisconsin.

Graham: So that's '64, that's still later if you're trying to do a chronology. I used to watch Jackie Gibson, Eleanor Gibson -- got her Ph.D. from Yale, I think in 1937 or '38. She wasn't in residence at any time that I was there, but she came down, maybe she took her final orals my first year there, because I knew her, and then I met her at meetings and I was very impressed with her. And I knew she was only just a little bit older than I was and I watched what position she held. And I managed to get tenure I think six months before Jackie did.

Izard: Well, her struggle is well known. It took a long, long time.

Graham: It's fantastic. I mean, she was so distinguished and yet --

Izard: She was at the same university for many years, being very productive, before she actually was recognized.

Graham: That's right. That's right.

Izard: So you were many different -- you were several different places before you had the good fortune to get your tenured appointment at Wisconsin?

Graham: Yes, well I'd been at Wisconsin -- we moved to Wisconsin in '57, so I had been there seven years. And I had an -- well, they offered me an assistant professorship when I came, but I didn't -- but I was embarrassed.

Izard: You had been there already?

Graham: I was embarrassed. '57, I graduated in '42, that's 15 years post Ph.D. to be an assistant professor. So I said no, just make me a research associate.

Izard: It must have been a little like a rabbit in the briar patch kind of thing. It must have been fun for you to have the opportunity to get full time into research at such a fun institution.

Graham: Sure. Well, you were asking me about political --

Izard: Yes. We wanted to make sure we had covered whether there was any important political and social events.

Graham: Well, it was the women's movement that was the major thing of concern. I was saying -- I remember I said there were -- that the cultural impact on me. I was very biologically oriented, I still am, and I thought social cultural things of less interest. But the women's movement made me see that they were of interest, and very personal interest. And of course it opened doors with that movement. When I talked about I got tenure in '64, well the movement began in the late 50s and that began to open doors. It hasn't opened them nearly far enough as you may have noticed.

Izard: Yes of course.

Graham: I keep wondering if my granddaughters will be fortunate enough to live in a world where their 50% of the population is represented in Congress and Senate. At least in the advanced democracies one would hope the representation would be more equal than it now is.

Izard: Well we maybe have some reason to hope.

Graham: Well I'm very interested in politics and I always have been, but I do not feel that it has really influenced my research because I haven't -- I just haven't been -- worked in those areas. I don't find them manageable for the kind of intellectual abilities I have.

Izard: Okay. Well then, let's just take a short pause.

This is the second of my interviews with Dr. Frances Keesler Graham. I am Carroll Izard here at the University of Delaware. Our first interview was on February 9, 1993. And this interview is taking place on February 11, 1993. We are about to begin our discussion of Dr. Graham's research contributions. And that reminds me to say a few things about her coming to the University of Delaware. We are very fortunate that Dr. Graham came to the University of Delaware in 1986, just seven years ago. And while we acknowledge that Dr. Graham made many contributions before she arrived here, we are fortunate indeed that it was after she arrived at the University of Delaware that she received the American Psychological Association's Distinguished Scientific Contribution Award and was elected into the National Academy of Sciences.

Graham: And the SRCD Distinguished Scientist Award in 1990.

Izard: The SRCD Distinguished Scientist Award and also noted as distinguished alumni of university.

Graham: Yale and -- oh that was earlier.

Izard: And so these, all of these great good things, the harvesting of these rewards came after she joined our faculty and we are extremely proud of that. I'll begin now by asking you Fran if you would like to add anything regarding general intellectual history, mentors that may have helped shape your career.

Graham: Yes. Yes, I'm sorry but when I talked about mentors last time I was talking really about my teachers, but there were many colleagues and friends who had an important influence. And I'd especially like to mention Helen Tredway Graham, who is my mother-in-law and a very distinguished neurophysiologist and very devoted to basic research. And she was active and she died at the age of 81 but before, when she was 75, Helen Graham got a five year grant from, I don't know whether it was NSF I guess, which when she was 80, was renewed.

Izard: I think that's most remarkable and I'd be surprised if it didn't set a record.

Graham: I think it may well have. And I also want to mention three SRCD members. Two were collaborators on the St. Louis perinatal anoxia project. One was Bettye Caldwell, who was very well known for her Little Rock project and the other was Claire Ernhart, who is known for her lead studies and her current -- she and Sandra Scarr are currently fighting the work of Nesselrode, is it - Pittsburgh -- you must have seen it, any rate. And the third one is Rachel Clifton, who introduced me to the orienting response and is still a close and dear friend. Oh yes, and I did want to mention that there are some male colleagues that were influential in my work too, very supporting, and these were Luke Teuber, who I met when he site visited me, and two colleagues at the University of Wisconsin, Harry Harlow and David Grant. Wonderful people. So that brings to up to the current.

Izard: Okay then, that brings us then to some conversation about your personal research contributions and we can begin by your telling us about your primary interest in child development at the beginning of your career.

Graham: Well I was always interested in infancy, that was one of the things that I discussed I had some personal reasons for that. And I wanted to trace the early development in normal and in brain injured infants and young children. Cognitive processes, again it was cognitive processes that was the major interest. And I thought that by using brain injured to contrast with normal performance that I might get some notion of more, and to learn more of the nature of the processes and also to see how the brain and the behavior were related to one another. And these interests really have continued throughout my career. The -- let's see here, you said what were the primary interests in the beginning of my career, but I wish to emphasize that those continued throughout my career. And I don't know whether you want to resume with other questions, but that's all I have to say about the interest at the beginning of the career.

Izard: Well than, let's talk about -- well, you'd said that those interests continued through your career and maybe you could tell us if there were any shifts that occurred in your research goals or in your ideas and if so what it is that were responsible for those?

Graham: As I said, I really feel that the same goals have continued throughout my career. Since the move to Delaware, however, I have unfortunately given up my infancy work. And this is really because of -- about -- my age and the fact that it's much more difficult to maintain an infant lab than an adult lab and I was trying to do both. And to administer and raise money for both is also not so easy. But I have shifted in other ways, not in the main goals, but I have tried to adopt new paradigms and better methodologies as the methodologies became available.

Izard: That's characteristic of your career to change as times and new technology permits.

Graham: Actually, the biggest shift that I made was when I was part of the reason for the move to Delaware and that was to learn the techniques for recording brain potential from the scalp. And Delaware was an attractive place to come, both because there was a good cognitive area group and cross discipline area group. And because Bob Simons was available as a collaborator and, as you know, Bob also happens to be my son-in-law.

Izard: A very nice happenstance.

Graham: I turn it back to you.

Izard: Well, are you ready to reflect on what you consider to be the strengths and weaknesses, if you see any, in your research and your contributions. And if you will, continue and talk about the impact of your work and its current status.

Graham: All right. With regard to strength and weaknesses, I don't wish to say anything about weaknesses.

Izard: Well this is not the time for modesty. We know that your work has been recognized for its strength and it's contribution, but maybe it's useful for us just to have your vantage point on this and your perspective on this.

Graham: Well it's hard to give, but I can say I'm not satisfied with what I've contributed, what I've accomplished in my career, and I hope I'm going to have enough time to just push it a little bit further. But I do feel that it has been good work, and I have certainly enjoyed it very much. And I do think it has had an impact, sometimes a surprisingly large impact. One of the reasons may be because I did introduce methods that were pioneering at the time, and then they were picked up. If I had it to do over again I might have taken more risks, but that was hard to do if you have to keep getting funding.

Izard: If your whole career is a risk.

Graham: Exactly, exactly. Well that's the, that's the problem that I think, that you were going to ask further about, about funding. And I think that's one of the hardest problems for American research. It depends on grants that are relatively short term.

Izard: Yes, we'll talk about that later.

Graham: So to talk about the work itself, I divide it -- it's easily divided into three main phases. And the first one, the earliest work, I used behavioral methods and I used them to study infants and children who had known brain injury or were exposed to potentially brain injurious conditions, such as the perinatal anoxia project. My first work was with the Memory for Designs test for brain injury in adults and children up to, from 8 years and beyond. And that was published with Barbara Kendell in 1946, updated in '48 and '60, and it is still in use internationally. I think this earliest work has stood the test of time, after all it occurred back in the 40s, 50s, early 60s, is the phase of it that I'm trying to talk about now.

Izard: In a sense I guess this is pioneering work for neuropsychology.

Graham: Yes it was and the brain injury studies were. I mean it seems naïve now, but at the time brain injury was sort of a global concept. And it just seems silly, but that's where the Memory for Designs test was of interest because it tapped a really quite specific process and it had a very low correlation with general intelligence. And that was, you know, the goal; that was why we undertook the test.

Izard: It was part of the beginning of a specialty domain in our field that has flourished in recent times.

Graham: That's right. And then I went -- I was in St. Louis, Washington University, from the time I got my degree until '48 and then we had -- because my husband had a post doc in New York at Cornell, we went there for three years and I was at Barnard College there and did some research, but not especially relevant here, and then returned to St. Louis. And that was when there was a flurry of activity involved in the perinatal anoxia project. And that was -- I got into that in part because of Bettye Caldwell. I was on the job market again and she set up an interview, which she monitored by attending, with Alexis Hartmann, the chairman of the Department of Pediatrics. I had an appointment in psychiatry when I had been there before and I knew that I could have one there again but I was looking around for research opportunities. And so, with the three of us, Hartmann, Bettye Caldwell, and myself, and there was a young woman pediatrician who was also interested in doing a study of perinatal anoxia. She had perfected the method of taking blood samples that would -- tiny blood samples -- that would give you feedback on what the condition of anoxia was during the first 30 minutes to an hour after birth and we wanted to take advantage of that methodology. And Hartmann provided us with money for the first year that he got locally, but after that we had to raise the money for ourselves, and did so. And those studies have also continued to be known and read -- they are cited in a review by Kopp -- I can't think of her first name -- sometime in the 80s she cited it as a definitive study, which it was. I enjoyed that. Brazelton has acknowledged his indebtedness to the infant test that we developed and I know he came and spent a week in St. Louis observing our work. And the tests were modified by Judith Rosenblith and they are still in use as the Graham-Rosenblith scales. A chapter was devoted exclusively to them by Osofsky's 1979 *Handbook of Infant Development*. And then we continued -- well I guess, the story -- I try to think of things that you aren't going to ask me about later. It's hard to keep the continuity. I met Luke Teuber because I applied for another grant -- we did get a grant to do the perinatal anoxia work. I'd applied for another grant in '57 or '58 and Luke Teuber was sent as a site visitor to that, and my husband had a grant in at the same time on another topic and Luke visited both of us. And that grant I wanted to use to develop procedures for the follow-up of older children and the first big follow-up was to be at three years. And so I -- so we got that grant and I both developed the procedures and also studied a group of 70 brain-injured children. And there were two monographs that were published on the tests and the effects of how the brain injured children compared with normal children, and then there was another monograph that did the

follow-up on the perinatal anoxia children. And the most interesting thing, I think, was that the pattern of deficits in the brain injured group was not the same as the pattern of deficits you get in adult psychology. And, of course, that's why Luke Teuber -- why it was especially nice to have had Luke Teuber as a site visitor. And we continued, I visited his lab later, and he had all of this fascinating work on the quite specific wounds, gun wounds, gunshot wounds to the head. And that was classic work that --

Izard: Did you think about this difference and did -- was it -- did you consider that this was just because of the immaturity of the infant brain that the patterns were different?

Graham: Yes indeed. I think it did reflect both the age at which injury occurred and the age at which you were testing for it. But the biggest difference was that the younger ages seemed more vulnerable to language impairment, whereas, with older individuals and adults, perceptual motor deficits were more likely to occur. And then I -- there's one other kind of work I did during this first phase which I characterized as a phase in which you tested human beings who had various kinds of known injury to the brain, or, as I say, had been exposed to probably injurious conditions. And that was work on phenylketonuria in collaboration with Harry Waisman -- and for him the Waisman Center was later named at Wisconsin -- and Phyllis Berman who was also a well-known SRCO member. And the main outcome of that work, perhaps the only work I've done that had immediate practical application, was the -- we established quite clearly, I think, that it was very important to start the diet treatment as early as possible. It wasn't clear -- there was some argument as to whether you should wait for a number of months, but it looked as if the earlier you get it going the better the outcome would be.

Izard: A lot of your work has had applications for application.

Graham: Well I don't know. I guess so.

Izard: It is basic research in most cases. But as it turns out, many of the things --

Graham: Well I believe that that's one of the values of basic research; it's one of my arguments in favor of it.

Izard: That's what makes it basic. It's ultimately found its way into the world of work.

Graham: So that's all I have to say about what I'd call the first phase of my research. The second phase involved discovery of the cardiac orienting response.

Izard: Which is?

Graham: Which is cardiac deceleration and the orienting responses.

Izard: And I know the publication is a couple publications later, but I know that publication was a citation classic.

Graham: That's right.

Izard: And is still widely read and cited.

Graham: Well the orienting responses most people now know is an automatic response to an unexpected stimulus change, but the work began as an attempt to simply measure physiological responses in infants and because I'm always interested in the relationship between behavioral measures and physiological measures. That's the essence of a psychophysicist, which I am, as well as a child developmental psychologist. But I had just submitted the grant, I had only just submitted the grant when I got a letter from Rachel Clifton, who was just getting her Ph.D. from the University of Minnesota Child Institute, and she wanted to come work as a post doc with me. And she wanted to

study heart rate responses in infants. Well, we met at SRCD to discuss this proposal of hers and I mean I was very impressed with -- and that was, I guess, was in March. But we had to get money. I had a grant pending. I had a current grant with a little money left so I paid -- said, "We can pay you from that grant for the summer. In the meantime, you submit a post doc application and we'll see about my other grant coming through." Well, she got her post doc. Now I guess my other grant -- why was it held up so? Oh, I know. So we began our work, and my grant was held up for a site visit, so here we're moving into the next year and Rachel and I are busily setting up the infant laboratory and putting electrodes on babies and watching their heart beat. I remember it was very exciting, the first baby we put into the -- no that was an adult that we put into the apparatus. And the beats were being amplified and it was really exciting when you would give a stimulus and you'd hear those beats come slower and slower and slower. It was one of the most clear-cut phenomena I've had. But when we put newborns in, that wasn't the way it was going. Their hearts were going faster when we gave a stimulus. So this is what -- part of what led us into the orienting response. But Rachel introduced me to Sokolov's work. I had not known of it, and of course that was one of the most important things that happened to me. And so let's see, she came and we were working about a year when I got a notice that the site visitors came. It wasn't a terribly attractive site visit. There were two young men. And then in June or July I got notice that my grant was not accepted. And that's the only time I had that happen. Well, I was quite incensed because we were working away. So I telephone and said I wanted permission to resubmit a new grant immediately, even though the deadline was passed -- I guess it was June 1st to get a grant in. And so we raced to get a new grant in and that grant was much more focused. It wasn't just, you know, a sort of fishing expedition to see what physiology accompanied what behavior. But it was focused on the theoretical problem of why Sokolov and the Lacey's, whose theories both were concerned with the amplifying and reducing effects of autonomic activity on behavioral or, particularly, on this openness to stimulation which Sokolov called the orienting response and the Lacey's conceptualized as stimulus acceptance as opposed to stimulus rejection. And so then we proceeded to write that paper, the 1966 *Psych Bulletin* paper that you just mentioned that became a citation classic. And that was essentially -- we already knew the two phenomena I've told you about, that the young infant, the neonate, was showing acceleration and adults were showing deceleration. And we examined the literature, primarily the adult literature in that paper. And the Lacey's work would have indicated that it (orienting) should decelerate response, but Sokolov was getting acceleration. He didn't talk much about heart rate. He talked primarily about the GSR or the Galvanic Skin Reflex. We used to call it the GSR, it is skin conductance today.

Izard: Well, I don't want to omit anything. And I want to know if you want to say anything about specific publications. Are we ready to talk about specific publications that you think best represent your work? I know we've mentioned several.

Graham: Yes, I've mentioned the anoxia monographs and the brain injured monograph and, in the orienting responses work, the *Psych Bulletin* paper, but there is also a 1970 -- *Advances in Child Development and Behavior* -- a chapter with Jan Jackson that is a good summary of the work. And, I guess, in 1983 I have a chapter in a book by Siddle on orienting and habituation. And I'm still doing my work on orienting and we did a lot. And one of the most interesting, I still -- I guess it was the last -- no, well it wasn't quite the last but almost the last study I did with children with brain injury. We had the opportunity to study several anencephalic infants and we studied one intensively over a six-week period. The child died at 42 weeks -- I think that's correct, that seems a little old -- 42 days, that's what it was. So we were -- it was very -- and the child was actually precocious in one sense in its orienting behavior. The heart rate was slowing and it was, the response was rapidly habituating and then it would return if you changed from Ba to Ga, and I could hardly hear the distinction and here was this so damaged child. We published the *Science* paper on that which referred to precocious behavior in an anencephalic infant. So I think that summarizes pretty much the work of --

Izard: And publications that you would like --

Graham: And publications that include that phase of the work. Then I began with the third phase in the early 70s and this was work on startle, and this work is also still continuing. And I began that -- I

was interested in startle because it really was a nuisance response that interfered with your trying to measure orienting. You see, if you're orienting your heart rate is slow, but if you're startled your heart rate accelerates. So very often people find that (acceleration) when they give an 85 DB or even a 75 DB stimulus in the adult, and that (intensity) will give you heart rate acceleration initially, especially if the onset is fast. And so I wanted to sort out and see if I couldn't separate startle from orienting. And again this business of autonomic activity and its relationship to amplifying or reducing behavioral activity is also seen in startle as well as the orienting response. And I had been interested in some work that Howard Hoffman -- he spoke here on a colloquium if you recall -- had done in rats and later, I may say, did in infants, partly influenced by my work. But Howard was on the same study section that I was from '66 to '70 -- sometime like that -- and he had developed a paradigm in which a very brief pre-stimulus within, oh, maybe half a second or a second of a startle eliciting stimulus would reduce the startle or even knock it out. And I was interested in that phenomena and there again we had a, you know, the first pilot subject; the data were very exciting.

We had a graduate student in computers who was going around with one of my female students and he was willing to be a subject and we hooked him up inside the -- no, we had him -- we didn't even put him in the sound proof room, he was sitting looking at the same scope that we were looking at as we gave him simple tones. And he got so irritated because every time the little tone came before the big noise burst, he couldn't startle. We were measuring startle by blink, I guess it was, and he was watching the scope and nothing would happen. And so he was deliberately trying to undo this automatic behavior, but he couldn't. He didn't like the idea that psychologists could control his behavior. So I had -- we're continuing to study the conditions under which we can facilitate or inhibit, reduce this reflex behavior and attention does affect it. And if you attend to the startle stimulus you can enhance the startle, but if you attend to the pre-stimulus, this very brief change and it can be at threshold, then you increase the inhibition. So it's a very nice tool, with the same behavior, to be able to either increase it or decrease it as a result of your manipulations. And we did find that we got attentional effects on startle as early as four months. Although, and the surprising thing, though the attention effects were present, this strong inhibition that the computer graduate student couldn't do anything about is very sporadically and weakly present or delayed in infants and it doesn't even mature until near adolescence. And that's unusual to find something that is, you know, such an automatic really reflex response itself as inhibition, and to find it so delayed in development. And there is neurophysiological work being done and Mike Davis who came down here from Yale and also gave a colloquium, which you probably didn't go to because it would seem, you know, too neurophysiological, which it is. But his work has -- as well as some of the work of Howard Hoffman had done -- have localized in the brain what the mechanism is and where, and you sort of have a direct path through the human brain from the ear to the startle center and then you've got a little longer path that goes up to the mid-brain and then comes back. Not a lot longer though, a little longer. And that's relevant because now, my latest work which I call part of phase three, we are using brain potentials and we have in press a paper that I think is a very exciting, one in which we have found that the brain shows, the brain potentials show inhibition at about 50 milliseconds and that potential is probably initiated in mid-brain and then reflected on the scalp. So we are able to correlate the reflex behavior with the brain behavior. Well I won't --

Izard: That is exciting though that you --

Graham: It is exciting work.

Izard: And you should --

Graham: And we're continuing -- I mean, but we're, with both the orienting and the startle work, looking at the brain changes that are occurring and the precision that we are getting, particularly with this 50 millisecond P50.

Izard: Do I understand that you didn't get the strong inhibition in infants?

Graham: This is -- and you don't get it.

Izard: Did you ultimately -- I guess, again, we're talking about maturation of inhibitory mechanisms.

Graham: Of course, and the inhibitory mechanisms are slower to mature. I was trying to think, there was something else I think I wanted to say, but I'm not sure.

Izard: Would you like to just pause a moment and reflect on that and we can come back?

Graham: Yes.

Izard: As we begin again Fran, I'd like to ask you if there are any other publications representing significant studies that you'd like to mention.

Graham: Yes, I would like to mention the work in connection with startle. I got started in that as a result of it being a nuisance response and of also being on a study section with Howard Hoffman. And the first publications on that were in '75 and the one that is the biggest, the best summary, was my presidential address to the Society of Psychophysiological Research. And I should say that this work really has been very important in psychophysiology and beyond it. Lang (Peter) is now using it in studies of emotion and a whole bunch of people jumped in and so it's been a lot of fun. And in '79 there was a publication of an international conference and that was called -- the orienting response was the main part of it. Kimmel was one of the editors, at least, of it. And that was a paper in which I considered distinguishing (among) the orienting reflex, a defensive reflex, and the startle reflex. And that paper has probably been as much cited as the earlier paper on orienting. And we did write a chapter in the Minnesota Symposium series. And that was published in '81 and in that I also reviewed both the startle and orienting research in the infant. So I just wish to emphasize that while I have been talking about the sort of theoretical basis of work that was done in adults, all of the work has also been done in infants, just not the last few years; that's the only time I have had to forgo infants. So that's all I have to say about publications.

Izard: Okay, then let me ask you if you think if there were any contributions that were wrong-headed, and you started yourself or somebody else off the on the wrong path?

Graham: Now, I -- there is unpublished work and I -- but if it was wrong-headed then it didn't see the light of day. Some of the papers are better than others, but there is no paper that I have published that I am ashamed of. And I won't -- I just wouldn't publish them.

Izard: And to what do you attribute this good luck? Even though it is not just luck, everybody often has something out there that they kind of wish they could bring back. I know you had very sound training in the beginning as a --

Graham: I had what?

Izard: You had very sound training as a graduate student. You had good mentors.

Graham: The research takes patience and it takes attention to detail. I have parents that say anything worth doing at all is worth doing well.

Izard: Okay, so we go back to very early influences. Well very good.

Graham: The scientific method is a very powerful method. And I have a very powerful brief in it.

Izard: That helps a great deal. Let's go on to the final question in this section of research contributions; and I know you've mentioned already several things about funding. Let me ask you

though to reflect a little bit about that and talk about your experiences with the research funding agencies and tell us if you have any observations for current investigators who are trying to do this.

Graham: Well the federal funding is the life-line, at least it certainly was for me, and actually I supported myself solely on grant funds for many, many, many years and always supplemented with grant funds, I mean, my own salary as well as my research. But the -- my first -- I put in my first grant for, and that was for the perinatal work in collaboration with Hartmann and Betty Caldwell -- no it wasn't with Betty Caldwell, it was with Hartmann and Pennoyer, the woman who had the blood sampling technique in something like '52. And really it was Bob Sears who planted the idea of getting money that way because, you remember the National Institutes, I guess they existed long before that, but they really -- the funding of individual investigator grants was a result of Vannevar Bush and a post-World War II phenomenon. So people didn't even really know a lot about it but Bob Sears was involved in it some way. And he suggested to me, sometime like 1951 or so, "Why don't you get a grant? And then you'll be able to have funding for your research." And so I did and it was successful, but, in fact, they sent a site visitor to look at the grant, okay, because he was an executive secretary and he told us that the reason he wanted a site visit is -- I guess it got all the way to the council -- it was being -- it was the Neurological Institute that was supporting this work, but they were worried because there were two part-time and half-time women on the grant. Well, Dr. Hartmann, he was all right, but it looked as if the women were going to do the work and here they were only working part-time and stuff.

Izard: It was kind of ground breaking it seems to me.

Graham: It was what?

Izard: Ground breaking business I think.

Graham: Well it was, it was a major project. I think I said at the end of it that it's the last one. Just, you know, maintaining the --

Izard: Do you have any comments about your participating -- any influence you think you may have had directly or indirectly on funding policies?

Graham: I did have some, yes. The -- well first I want to say the perinatal, I got the funding for that. I got a second grant to do the follow up and the studies of brain injury in preschool children. And in the meantime, around '56 or '57, there was a big planning conference at Princeton to which I was invited. And there were -- I know Nancy Bailey was there, I don't know, I'm sure there were other SRCD people, but I wasn't a member of SRCD yet; I didn't really know about it. But that was to plan the Collaborative Project, the fourteen year affair. And I remained a consultant on that for as long -- I don't know when I was last in Washington on it, but it was for many, many years (1958-1970). So that was an influence, not so much on the funding mechanisms, but at least it was an influence on a big project that was being funded in-house by, at least, the Neurological Institute. Then my next experience with the national funding mechanisms was my application for a Research Scientist Award. You see, I'm still, not only having to get money for the research, but if I want to get paid I have to --

Izard: Get a salary?

Graham: That's right. I was a -- we moved in '57 to the University of Wisconsin and I was there until I came here in '86. And I'd been offered an assistant professorship but by that time, however many years was it? We went there in '57 and I got my degree in '42. Fifteen years post Ph.D. It did seem a little --

Izard: It wasn't the time for an assistant professorship.

Graham: It wasn't a time for an assistant professorship so I said just call me a research associate. Which they did but they didn't pay me any salary. And I had to earn a salary if I wanted to have any, so I decided when the research scientist program came out that I'd try for that. And I did get that (in 1966 and held it until my official retirement in 1989), but it was shortly after that that I put in for the second phase work. I mean we were finished now with the Collaborative Project, those studies were in press, and I wanted to turn to this physiological work in infants. That's when I got the first grant turned down. And I told you that there were two young men that visited and the site visit wasn't altogether successful. Only I wasn't aware of how unsuccessful it had been until they turned me down. Well, you didn't get pink sheets in those days, but I was able to get hold of a pink sheet because of somebody who was as incensed as I was by the outcome of this. And I personally was described by three adjectives and they were all three, I think, in a book on academic women as stereotypes of what women were called. The only one I can remember for sure is that I was called a dilettante. I mean nobody had to know me very well at all to know that that is the last word that applies to somebody with the focus that I have.

Izard: Sounds like a true misperception.

Graham: Yes, well, it made an impression on me as you can see. Then in 1970 I was asked to be on a psychology study section. It was the experimental psychology study section and I was the first woman they had ever had. They were quite proud of themselves.

Izard: That was long after you got your first grant?

Graham: Oh yes. I got the first grant in something like '53, I guess. And it was a longer course than the usual one but I was one of the early women who did get invited to serve on a study section. So I just did that four years and I did not accept any renewals to study section. That's a lot of work, it's very interesting, but it's a terrible amount of work. And I've done ad hoc work for them sometimes. Then in the mid-70s sometime -- I'm sorry, many of these things have gone to SRCDC archives, so I'm having to rely on memory and so I may be off by the exact year -- it was a very interesting experience. The NIMH director, who was Bertram Brown -- I don't know whether you knew him or not -- and Norman Kretchmer and every SRCDC member knows, who was director of NICHD, convened an ad hoc group to advise them about the distribution of funding in the area of child development. There were six people, and it's terrible, there are two of them I can't remember, but there was me and Julius Richmond and Ed Ziegler, whom you know, and then Sandy Dornbusch, he's a sociologist from Stanford, and we worked hard. And we met quite frequently as we produced a voluminous report. Now I suspect that the un-stated agenda for this ad hoc committee was to make a case for getting more funding, it usually is. And I'm sure that --

Izard: It also helps set priorities or guidelines?

Graham: Right, right. I know that the MR people, the mental retardation people met with us a number of times because that was a major interest in NICHD, but one about which I think there was some uneasiness as to whether they were draining off too much of the funding that might otherwise go to individual investigator research. Then my next experience with funding, I was appointed to the Board of Scientific Counselors of the NIMH. And that's the group that reviews and set priorities for the in-house research at MH and I was chair of that for the last two years (of four). And thus I had a chance again to influence policy on funding and on the kind of research that received funding. And then my last experience -- and this I don't know where to put it but it's in the list of topics that they wanted us to interview about. That was my appointment by President Carter to the Presidential Commission on -- let's see, it was on Ethics in Medicine and Biomedical and Behavioral Research, was the title of the commission and that I certainly had more influence, if not on funding, at least on public policy, than I have had before or since, or than I would, I mean it would be very easy to be seduced into this kind of thing because it's quite heady to consider problems of life and death. We issued a monograph on definition of death because the situation with state law was such that you might get into an ambulance and be dead in one state and not in another because the criteria for whether you were dead or alive

were different. And we considered a lot of the problems of health funding and the inequalities in the availability of health across the general population. I can't think of all, again those are all things I left in Wisconsin when I had to move here, all of my materials on that. But it was an extremely exciting committee to operate on. We met every two months for several years. And then when the new administration came in, in 1980, they let the commission die. Every time they -- oh, and I may say one of the interesting things about that commission was that it had five women and six men on it and that must have been very unique. And this was in President Carter's administration. And as I say, Reagan allowed it to die a slow death.

Izard: February 17, 1993. Continuing my interviews with Dr. Frances Keesler Graham. This is interview #3. Fran, at the end of our conversation last week, we were talking about a possibility that there were some differences in attitudes in the universities and federal agencies regarding the funding of the support of women in psychology. And certainly you are one of the people who might be able to comment on this from personal experience.

Graham: Yes I -- there are several things I'd like to add about the question we had that we were discussing on funding last time, but with respect to the possibility of gender bias, I felt that, at least with grant reviewing and also editing, that there really was not any gender bias in the looking at the applications. But it was interesting that the male members -- and they were all male but me at the time that I went on to study section -- just didn't understand the position of women. They didn't realize that many, in fact most women doing research, were in unpaid positions and the only way they got any salary was through grants. They were quite used to male researchers applying for summer support, but they were bothered and would comment about, well now, why is this person wanting a half-time salary for twelve months? I wanted to make a couple of other comments that I realized afterwards that I'd forgotten. I wanted to point out that my adult studies of orienting and startle were funded by NIMH or NSF and infant studies by NICHD, and this was despite the historical picture of how NICHD was setup. It was set up for much more social and applied kinds of child development than my research was about. And I also wanted to add that I did get funding from the Grant Foundation. Phil Sapir was the director for many years and was a great and good friend of child psychology. And I was particularly grateful, because out of his discretionary funds, in 1972, he just gave me, after a telephone conversation, \$25,000, which at that point was enough to get my lab computerized. And I also later had a regular grant for the orienting and startle work from them for a four-year period. But it was to be able to get the hunk of money to get a computer that made all the difference in the world to me and you couldn't -- it was very difficult to get computers out of study sections and NIH at that point. You were supposed to already have one in order to demonstrate that you could use one. It was something of a bind. Well those are the only things that I wanted to add. We still have lots of cover on the next section.

Izard: Okay. We'll move then to the section on personal institutional contributions. And the first question is in which institutions have you worked and the dates and capacities?

Graham: All right. I'll try to go through that rapidly. My first post Ph.D. positions were in St. Louis. I was half-time in the St. Louis Child Guidance Clinic and I was first a psychologist and then acting director. I was just there for two years, from '42 to '44 and my work was entirely clinical diagnostic, although I did do some treatment of reading disorders and trained a volunteer to help with that too, and did some play therapy treatment. The other half of my time was at Washington (University) Medical School. I had an appointment as an instructor -- no, first I was just called research assistant in the department of psychiatry. I was in St. Louis from 1942 to '48. And that work, again, was mainly clinical diagnostic testing, where I did manage to get a half a day a week for research. And during that period we developed the Graham/Kendall Memory for Designs Test. Then I was at Barnard College for three years from 1948 to '51 in the department of psychology. I was an instructor promoted to assistant professor. And there, for the first time since my degree, I was able to do regular teaching. And I taught introductory, an experimental course with a laboratory, child, and adolescent psychology -- two different courses. We had three courses a semester and I managed to do some research during that. Then I returned to Washington University in St. Louis again, this time in the departments of both

psychiatry and pediatrics as a result of the starting of the perinatal anoxia program, which I talked about last time. I was there from 1952 to '57. I was an instructor and then a research associate. And I did do some teaching of a graduate course in projective techniques for the psychology department, and I also taught an extension course in the Ozarks for elementary school teachers, and I'll talk about that later when you ask me about teaching. Then I was at the University of Wisconsin from 1957 to 1986, first in the department of pediatrics as a research associate. I don't know whether I put this on the records or we mentioned it in talking, but they did offer me an assistant professorship, but I was 15 years post Ph.D. by then and I thought research associate was more dignified. Then, in 1964 when I got the research scientist award, I became tenured as an associate professor and then became a professor within another three years. And I was also invited to join the psychology department. So I was half-time in pediatrics and half-time in psychology from then until 1986. I did get an endowed Hilldale Research Professorship from the University in 1981, I think it was, which was a great joy. And, the kind of work I did was, again, primarily research, but I did teach one course or seminar a year in psychology and some of those seminars were developmental. The course was always psychophysiology, a laboratory graduate student course. My teaching was solely to graduate students, with one exception, I had Linda Smith, who's now an eminent SRCD member, as an undergraduate. And she credits me with getting her interested in developmental. Then, in 1986 I came to Delaware and was there as professor from '86 to '89 when, because of state law, I had to go emeritus. But as you know, I'm still working and doing the research and serving as an advisor to graduate students.

Izard: And continues to provide wisdom for the department on many issues.

Graham: I don't know about that.

Izard: All right, that brings us up to date then in terms of your work history. I'd like to have you now, if you would, comment on your connections and associations. Your well-known research sites and then, perhaps before you finish that, say something about the role you believe that unit played in the history of child development research, changes that might have occurred during your time.

Graham: I'll just talk about the University of Wisconsin because that was the best known institution with which I was associated, and for a considerable time. And there was no real child institute there, but during my tenure we established a developmental area group specialization in the department of psychology. Mavis Hetherington was one of the important forces in getting that organized, and also particularly Leonard Ross, who had gotten interested in mental retardation at that point. We got a training grant for the area group program and also, as part of my work in pediatrics, we got a training grant for pediatricians for advanced training. And at my request we also had a provision for a fellowship in behavioral science. And Lewis Leavitt, who is an SRCD member, was the first post doc we had in that position. Then the other main thing that's relevant to SRCD and child development is the establishment of an MR center. There was a big push at that time to build MR centers -- this is around, say '69 -- and I served on the University committee to plan the money for the building and later on the getting of the first core grants to support them. And one thing I did was to argue the need for computer facilities. There weren't to be any, and I argued that it was very short-sighted at that point to set up a whole research building and not have computers. I had much more trouble trying to persuade people locally to put that into a budget than I did the site visitors. They all thought it was a fine idea. Rick Heber, who's known for the Milwaukee Project for the increasing of IQ, was the first director of that program. And I don't really like to talk very much about this. I, in the end, resigned from going into the MR center because of disagreements, particularly with Heber, about how the administration of things should be set up. As you know -- or may not know -- about ten years later Heber was caught in financial fraud using federal funds. It was a federal district attorney who turned this up, and he (Heber) went to prison. And, at that point, I went into the MR center. We also established at the University of Wisconsin a new school for allied health and I was chair of the first search committee to get the first dean for that position. And that has relevance to SRCD because we have members -- a few, and we ought to have more -- from physical therapy and occupational therapy. And Susan Campbell is one of the UW's, who is both a -- has a Ph.D. in both neurophysiology and a

degree in physical therapy. And she was a student and -- oh yes, she also took a sabbatical with me later. And she's active in SRCD. And she's one of the few representatives of that kind of diversity that I wish we had more of. Well, that's all I think I need to say about UW.

Izard: Okay. Then we're ready for the next question. You've talked about your experience as a teacher a little bit. Do you have anything further you would like to say about your role as a teacher or as a trainer of research workers, or teaching in general? Anything particularly you'd like to say about courses that you've taught? I'm not sure how much you want to say about tension between teaching and research, because I don't think it was ever a big issue for you. But there it is.

Graham: Well, teaching was never my major concern, partly by accident, because I certainly enjoyed it. And I particularly enjoyed the three years of teaching at Barnard. The undergraduates there were terrific and I liked introducing them to the idea of research. I had a requirement that I thought was a good one that they had to observe in a local nursery. Select one child to observe and they would do this three or four times, I can't remember exactly. They'd observe that child and make (write) a report to me on the observation. My purpose was to both give them experience in actually looking at pre-school children, observing them carefully, and also to teach them something about how you go about making objective observations. And that really was quite popular, although it was a good deal of extra work, because I had to comment on these papers, with something like 40 students in the class and each of them was giving me four of those papers, but the department gave me some funds to get some help with it. Oh, I also offered extra credit for research and that too recruited some of the best students. And there was one publication that results, a collaboration with me and three of the students. And all three of those students went on to get a Ph.D. in psychology. One of them was Wanda Charwat Bronson, who was editor of the monographs, SRCD monographs, and maybe still is, I don't remember who is now. And there were two others who are still SRCD members: Frances Fuchs Schachter and Alice Honig. And work of both is well known. And then another one of my students -- this was Vera Polgar, who became Vera John-Steiner. And she is no longer in SRCD but she recently got an award from, I think, the teaching division of APA for her cognition book which was called *Notebooks of the Mind*. So all together I had quite a few students from those three years that went on into the field. And I've always been very pleased about that group.

Izard: Yes indeed.

Graham: And then the other teaching experience that was particularly interesting was the teaching of the extension course in the Ozarks (to elementary school teachers). The class met only once a week, at night, and I had to drive over winding, hilly roads. I dreaded any sign of snow and there seemed to be no semester you could avoid some snow, even in St. Louis in the Ozarks. The students were very highly motivated but they clearly had a much poorer educational background than Barnard students, of course. And it was interesting how much they avoided anything to do with numbers. It was clear that their teaching was very largely devoted to promoting language skills and that they did only the minimum necessary to teach anything about arithmetic. And they were teaching by rote memory. Well, with this I couldn't use the child course that I'd used at Barnard so I had to sort of operate on -- re-think the whole thing anew. And what I did was primarily emphasize just some few general principles. And the two that I particularly concentrated on were the greater effectiveness of rewards over punishments and the greater effectiveness of active self-guided learning as opposed to passive and rote learning. And I did do a good deal to try to make them see how interesting arithmetic could be. It was a matter of having symbols that could represent things and their relationship. Well it was interesting. At first I had a terrible time trying to get them into discussion, which I thought was what the course really needed to be about. They wanted to take copious notes and they seemed quite fearful of public exposure, but we worked that out and I really thoroughly enjoyed the experience. I did it just two years because the hills and the night time driving were pretty hard on me and I did have children at home. And you were quite right on the question about tension between teaching and research, I just don't see any at all. I think that ideally they blend.

Izard: Okay. We're ready to move on to the next question then. I'll ask you to describe your experience in so-called applied child development research. And comment on your role in putting theory into practice.

Graham: I think I've already said enough about that in everything else I've said.

Izard: You have talked about quite a few of the things you did, and your contributions to applied psychology.

Graham: I think the distinction between applied and basic is not always clear cut. I do feel that SRCD has had less of what I would call basic research and it's kind of related to the distinction I made between NICHD funding and NIMH funding. Although, heavens, NIMH has applied interests too, but they're different ones. At any rate, they have a history and have supported basic biological and psychological research.

Izard: Yes, well I think they even had a division called basic research identification, but I quite agree with you in their emphasis. Then we'll go to talk about your experiences with the organization, with SRCD. When did you join SRCD? What were your earliest contacts with the society and with whom? And what do you have to tell us about the first biennial meeting that you attended?

Graham: Well that's interesting. And that's fun to do. But I must say before I start this section that I sent all of my papers to the archives of SRCD before I moved to Delaware. And I find that I seem to have erased or written over a great deal of disc space in my brain because I can't recall many names that --

Izard: Well, just what might you want to talk about now?

Graham: So, I'll just do the best I can, but I know about the first meeting and I know how I joined SRCD. It was immediately after attending my first meeting, which was in 1957. It was in Washington, DC and I'd been invited to be on a symposium that was organized by the Pasamanicks. And that's Ben Pasamanick and Hilda Knobloch. I certainly hope the history project has interviewed them because they certainly should. He was in public health, she was a pediatrician and they had been working for many years on the question of perinatal events and their impact. And so they set up the symposium that I was on and Seymour Levine, the Stanford animal behaviorist, were also on the symposium. I hadn't known much about the Society previous to that invitation and I was really very excited to discover an organization whose purpose was to promote research in child development and particularly interdisciplinary research which, with my work in the perinatal anoxia program, I was much engaged with. I attended a great many of the sessions and I met a lot of people and I remember one lunch in particular that Nancy Bayley invited me to -- four or six people, and I met Larry Frank who was already a figure of history at the point. I think that covers how I got into the Society and my first meeting.

Izard: Well then, describe your participation in the scientific activities of the Society. I know that they have been --

Graham: They started with a bang, to my astonishment. I was invited to be program chair for the 1959 convention of SRCD, having just attended one such convention, having never even been on a program committee. But I was very excited at the idea. And I had some ideas because, although I had been very excited by the 1957 meetings, I had noticed that there were people missing and kinds of research missing that I really thought that the society should be interested in. So I was determined to do two things in the program: to enlarge the representation of interdisciplinary research and animal research, and to get more representation of basic science in fields like perception. And the mechanism I used was to have the program committee invite nonmembers to give addresses or to be on a symposium or even to prepare a symposium. And this way I recruited Juli Richmond and Eleanor "Jackie" Gibson to the society, as well as a number of others, but I thought that those two recruitments would have been

worth a gold medal from the society. After that my laboratory was usually represented in papers or symposia at the SRCD meetings until about the mid-70s. And, by that time, the Society was a great deal larger and there was a much smaller proportion of work in the area that I was interested in. So I shifted and presented more of my research at the infancy meeting and, of course, at the Society for Psychophysiological Research which always had a sizable group of people interested in developmental. I continued, I may say, to try to recruit researchers that were outside of standard child development, including pediatricians, psychophysiologicals, and animal behavior people. And as president, instead of giving a presidential address, I organized a presidential symposium on visual perception, which is not my specialty, but I thought it's hard to think of anything more important for child development. And the symposium included one SRCD member, Phil Salapatek, who introduced and discussed, and then three non-members: Dick Held, you know, whose famous work with the effects of active and passive movement through the environment on kittens, and two young neurophysiologists, Mike Stryker and Peter Spear who were both were doing cutting edge research on the neurophysiology of and critical periods in the nervous system. Then there is another whole area of scientific work at the Society and that's editing, of course. I was quite active in editing for a decade between the middle 60s and the middle 70s formally on editorial boards. I was on *Child Development* from 1968 to 1970 and I was also on the *Journal of Experimental Child Psychology*, on the *Journal of Experimental Psychology*, and on *Psychophysiology*. And my *Psychophysiology* work included two years when I was an associate editor, specifically to try and increase the number of submissions from child people. So, even when I wasn't working specifically in SRCD, I've always promoted development wherever I could get my foot in the door.

Izard: You've done a splendid job of it.

Graham: I declined, I made decision not to take any editorships. I was offered the editorship of *Child Development* at one point, and later of the *Monographs*, as well as three other journals. I did this -- it was not because I wouldn't have liked to do it, but it just seemed to me an editorship is extremely time consuming. And to try to be both a parent and also a full-time research -- I just didn't feel I could afford the schedule. So, after 1973 I no longer had any formal appointments on boards, but I've continued, and still do, ad hoc reviewing. And I can take a greater breadth of journals, can select the areas in which I wish to do reviews. I did serve for eight years, from 1969 to 1977, on the SRCD publication board but that, of course, is much less demanding. You only have a meeting a year and some other things in between. So that covers my work on the scientific activities of the society.

Izard: Well that's quite a bit. A lot of things that make good memories for you. They are certainly significant contributions. The next question is to describe your participation in SRCD governance and maybe talk about the major issues that confronted you during the time of your presidency, and there may have been other offices that you held you might want to talk about.

Graham: Well, my first experience was when I was elected to the council in 1965. There are six members-at-large on council in addition to the officers and they serve staggered, six-year terms. I came on at quite a difficult time for the Society. Bill Martin had just completed a ten-year term as business manager and editor and during that period he had really built *Child Development*, too, making it a very impressive publication. And he wanted to continue in these positions, but some of the younger members of council -- this is just before I came on, this was happening in 1963 and '64 -- some of the younger members felt it was time for a change and they wanted to separate the two functions, which does indeed make sense. I wasn't present at the business meeting when this idea was presented to the membership, but I understand there was a very emotional scene that took place. In any case, Bill was terminated as business manager in May of 1964. He was replaced in November of 1964, six months later, by Robert Hess from the University of Chicago. Well, by the time of the first meeting of the council to which I had been elected, which I think was held in the spring then, of 1966, it was evident that the Society was in very precarious financial condition. I know before attending this meeting I sought legal advice about what personal obligations council members might have. Apparently it was reassuring, perhaps because for non-profit organizations there are different sets of rules. Well, the first order of business was what to do about a replacement for Hess because of the shape things were

in. No candidate seemed to emerge and I had a brilliant idea. Margaret Harlow -- Peg Harlow -- was a colleague, not in psychology, because that would have been -- you couldn't have two members (spouses) in the same department. For many years she couldn't have a job at all. She had been at -- she was an eminent child psychologist on her own. Margaret Kuenne -- I don't remember the name -- but she did some classic work with Ken Spence in the 40s. Anyhow, everybody was excited by that idea, but they wondered whether or not she would be willing to accept. But I had a good idea that she would because she'd be happy to be able to do something completely independent of Harry. She'd have her own niche. But I thought maybe it would be a good idea -- because this is just the business manager now, not the editorship, I'm not dealing with that because I didn't have any role in that -- that we change the title to executive director, because that really was the nature of the position not just business manager. And that too -- we did have to get a by-laws change to do that. But that was done and Peg accepted and she had a wonderful five years with the society. She was terrific, by the time of the next meeting she not only -- well there were things to be done; income tax hadn't been paid for several years and we also had to move. The society was incorporated in Indiana, and there were some unfavorable developments for non-profits in Indiana, I can't remember what they were. But at any rate Peg and I, I serving as agent for the Society, filled out incorporation papers, so we were incorporated in Wisconsin, still were, I think, the last I knew, but I was agent even after I got here, but I thought it was time for somebody else to fill that position since I was no longer even in the state. But Peg had not only gotten the books straightened out and everything on an even keel but she was actually accumulating savings, because she wouldn't let any money sit idle. She was getting interest on things which had not previously been done. So, at that time, I suggested we better have a committee on investment policy and I was willing to chair that, because I wanted to learn something about how to make money work for you. So that worked fine. But Peg, within a couple of years, was found to have a carcinoma which had invaded the brain and was terminal. And so that was a horrid last year while she was still sometimes able to work and often not. She had a very good assistant and we managed to see that things were going along all right. And then the smooth transition was made to Dot Eichorn. Well then that brought me -- that was '71. Then from 1973 to '79 I was again on council by virtue of my positions of president elect, president, and past president. And there was nothing as exciting as financial insecurity, but there were many things that had to be considered. There was the perennial problem of getting adequate representation for the diversity of disciplines. And just before I came on to council they had made an effort to try to improve the situation of psychologists being so dominant, by increasing the number of members on council from 6 to 9. The result -- all nine were psychologists! So I suggested what people were at first taken aback by but then agreed to, that we reserve one of those positions for appointment by the council and that required a by-laws change but it was really very sensible, because then you could -- and that meant that essentially three of the positions for each group of nine would have been appointed by council at their discretion. There were other changes also in how the ballot was presented. You could group people in such a way that --

Izard: Was that your way of guaranteeing diversity?

Graham: That's right. And it seems to have worked very well. I was looking through the directory recently where they were listing council members. There were three to five non-psychologists and I knew some of them were minority members. That's hard to know unless you know them all. The second issue we struggled with was the question of ethics for research. Now, SRCD had long had an ethics committee and had their own code of ethics for research, but we became alarmed when congress mandated the first Presidential Commission to study the ethics of research with human subjects. And they were, in particular, supposed to discuss whether it was permissible even to test, to use as subjects, humans who would be unable to give informed consent, such as children and such as the mentally retarded. There were other groups also of course, like prisoners. Well our concern was intensified because of the, I think, ten or twelve commissioners, only two were from the behavioral sciences and neither of them -- I mean they were both psychological psychologists, they had no experience with work with children. And so we felt we ought to take some action on that. I wrote a quite strong letter to the Commission about the lack of people experienced with child research and offered we would be glad to give our advice. And then I did two other things, I set up a part-time -- I and Dot Eichorn, who provided the wherewithal and located the person, we set up this part-time

position in Washington at first, the beginnings of what became of our Washington office. And the child psychologist who took the position was supposed to monitor the Commission. She attended all of the meetings; she made contacts with staff on the Commission. She and I were in telephone communication as well as writing regularly. And she was also to make herself known in other societies in Washington. So it was a true beginning of our forming a presence as a socially-active organization. The second thing I did was to ask Bill Charlesworth, who for many years had been on the ethics committee and was probably the chair of it, and Juli Richmond to prepare to testify at the public hearings the Commission would have. And Bill Charlesworth was very clever; he polled the whole Society, asking, by means of a questionnaire which he developed, about their actual experiences with unethical behavior. And then he presented this report as part -- he submitted to the Commission the report of this study. And they were very impressed. This, they said, was the only time that they received data, instead of argument. So to make a long story short, the final recommendations of the Commission were reasonable. Child research was not put out of business. And SRCD wasn't overlooked when a second commission was authorized, the one I was on, to cover a broader range of problems. Then a third issue began to receive a good deal of attention during this period. And that was the general role of SRCD and social policy. And Harold Stevenson and Alberta Siegel who was on, she was on council at the time, obtained eternal funding to hold a series of meetings exploring how the Society might be more effective in influencing social policy. I participated only marginally in the movement because I had, and still have, serious reservation about the effect of social activism on a society that is dedicated to promoting research. I think that's all I'll say about that.

Izard: All right. Then you have already talked about some of what might be involved in the next question, but here it is: what do you believe were the most important changes to occur in SRCD during your association with it?

Graham: Well, the social policy movement continued to grow after my term of office. And there became a full-time office in Washington and there was a regular social policy newsletter. And eventually its financial demands became a real cause for concern and led to some reconsideration of what our priorities were. And Fran Horowitz chaired an ad hoc committee on which I served and it included, I remember, Juli Richmond and Eleanor Maccoby. There was a great deal of ambivalence among the committee members, but the final report happened since in respect to that. I still get something that indicates that social policy work is going as it should, but that was the last time I was actively engaged in SRCD government.

Izard: Do you want to talk about these other two points or would you like to --

This is Carroll Izard with Dr. Frances Keesler Graham on February 22, 1993 and we expect this to be the fourth and final interview of Dr. Graham for the SRCD history project. When we stopped last time we were talking about your experiences with SRCD. I'll begin this time by asking you what you believe are the most important changes to occur in SRCD and its activities during your association with it.

Graham: Well I think I covered the changes in SRCD, I think you mean to ask me about the field in general, don't you?

Izard: Well that will be our next general question, if you've covered --

Graham: Oh I believe we did cover the changes in SRCD, that's right the last time we --

Izard: All right, then we'll begin with the second to the last section of the interview and I'll ask you to comment on the history of the field during the years that you were involved in it. And any major continuities and discontinuities and events related to these. Just to give you the whole question at once, has your view concerning the importance of various issues changed over the years? If so, how have they changed?

Graham: Well I don't think I have very much to say on this and my views are somewhat parochial -- tied to my interests. But I think one of the very evident changes that has occurred is in the, not revival so much as the blossoming of an area of research in infancy. There was a little research in the 30s but the problem was that behavioral scientists didn't -- non-medical scientists -- didn't have access to young infants, especially newborns, and that made it difficult. Dottie Marquis was able to do research, but she had an appointment in pediatrics. But there really wasn't much, almost nothing, done in the 40s, at least that I ever knew about. And then in the early 50s there began -- there was an interesting series of psychophysiological studies that came out of pediatrics at Syracuse with Juli Richmond and Al Steinschneider and then a psychiatrist, Wagner Bridger, was also publishing. But I think it was the Collaborative Project that really stimulated the resurgence, or as I say it's not resurgence, the surge of infancy. And that (i.e. the Collaborative Project) was set up in some 14 institutions around the country including, at least for a time, both Yale and Brown. And, of course, both of those departments developed very strong developmental programs under Kessen at Yale and Lipsitt at Brown in particular. They set up the programs in these various institutions and then in the associated hospitals, so that meant that people could get into the hospitals and into the follow-up clinics. So (infant) subjects became available and so, therefore, the project was a pragmatic success; the other part, it was an exciting national stimulus to research. Oh, I did want to say more about infancy. It became so popular that then out-shoots had developed. And we were talking briefly before we started on what some of them were. The earliest I recall was in 1965 getting a letter from Jerry Kagan discussing the formation of a Committee on Correspondence in Infancy. It was later they wanted an acronym and CCI isn't a very good one but CRI is great, you see. Cry. So they changed it to committee -- or Correspondence on Research in Infancy so that was a fine name. And I think the first meeting was in '66 at Harvard, but I'm not sure about that -- I've tried to look at my records and they're incomplete. I knew the second was in New Haven. And this was -- the invitation had gone out to just 30 people, but 30 people, mostly from different labs, was quite a lot. Compared to what had been going on in the, you know, 30s and 40s. And almost everybody came to the meetings so they were very exciting. That kind of small meeting is wonderful. That was -- it still exists but it just meets along with the larger organization that eventually became that International Conference on Infancy -- is that what we decided the name was? And that's the one that's publishing -- began with the editorship of Lipsitt -- the *Infant Behavior and Development* journal. And so then infancy had its own journal. And along with that there were two societies labeled international. One was the International Society for --

Izard: Studies of Behavioral Development?

Graham: A -- something like that. I'm sorry I can't remember and, as I say, most of my files on these things went to wherever they have the archives for SRCD. But I remember that Harold Stevenson was active in setting up ties with the people in England whom I think had organized this, because there was something similar to a collaborative project going on in Europe and so I think that organization also was an outgrowth of that. I remember going to some meeting where there were respiration physiologists from Sweden and so forth. Then there was another one that was started and I don't -- and that's still ongoing and SRCD I think usually has ties with it. But I haven't had many because it was mostly concerned, it seemed to me, with social personality development. But the other international, in its title, organization was set up in this country and it's the International Society for Developmental Psychobiology. And you see that would be of more interest, in terms of my research. And that published a journal also, *Developmental Psychobiology*. And let's see, I think the Lipsitt-edited *Infant Behavior & Development* started about 1977 and I think the *Developmental Psychobiology* did also. I found, to my surprise, this is an organization primarily of animal people and animal physiological developmental types, but they do welcome human research and human researchers in the organization. I've published in it a couple of times and I must say that I have never had a journal that elicited so many reprint requests from so many continents. So I recommend it if you are interested because it's fun to get them from people all over that way. Probably not as many Xerox machines is one reason.

Well, this brings me to the second areas in which I believe that the field of child development has changed over the years and I don't like the changes well. I think it is much less concerned with biological and psychobiological research than it was in the early days perhaps, when it was more

influenced by a medical model than it is today, but obviously I wouldn't like this and it seems too bad because really major strides had been made in understanding the brain and relating it today here and yet they're disproportionately, it seems to me, underrepresented in SRCD and in APA. APS has somewhat, at least in its officers, better representation of the biological. But I hate to think of psychologists turning over to people that are neuro-scientists or developmental neuro-scientists the relating of brain and behavior when they aren't trained, normally, in behavior. I just simply want to repeat that one of the early strengths of SRCD was that it did provide a means for disseminating ideas about development among a variety of different disciplines. There is a single cord that binds us all, and that is development. And I think the biological sciences have interesting things to say about development which, from which, we can profit. All right, that's all I have to say about --

Izard: Well, then you covered some of the next question. But let me ask you what are your hopes and fears?

Graham: I think that's covered.

Izard: Well, then we come then to the final section which is personal notes and we'd like to have you tell us something about your personal interests and your family. And especially the ways these experiences may have had a bearing on your scientific contributions.

Graham: Well that's easy, and I won't say much, because I could say a whole lot.

Izard: I'm sure you could.

Graham: But I had three children -- have three children. One daughter is a professor of psychology at Columbia University, the other is a lawyer here in Delaware and both are married to psychologists. Made very suitable marriages. And then I have a son who's a research chemist and I have 8 grandchildren, and they are wonderful. I don't really know what is appropriate to say about it. The older two boys, the other two are boys, and they are computer fanatics and all the rest except the nine month old are clearly on a route to being. Well what are my interests? I mean, sports are, travel was when I was younger, but now I'm -- well my husband as you know developed Parkinsonism. So travel isn't as attractive at present, but I hope that may change. But I've always had a lot of intellectual activities. I love to read and the -- I've always had a real hobby of archeology and ancient history. I think I told you about the engraving, book of engravings -- at least I think I did -- that I was so fascinated by when I was five years old. And I still am and I have infected at least one of my granddaughters with the same enthusiasm. And so -- but see, that's also like development, you want to know how things start. And then, more recently, I've been -- become quite interested in paleontology and that's another kind of evolution. The evolution of the hominids that preceded the homos that preceded homosapiens, sapiens, which we think is so great, and also just the development of the earth. That's also fascinating, and I like ornithology. So that's about it, the rest of the time is with my family and my work. Well there aren't enough hours in the day. So that's it.

Izard: I can certainly share and empathize with those sentiments. Well if there are no further comments then on behalf of the history project of SRCD, we want to thank you very much for all the effort you made to prepare these wonderful notes and share these experiences with us in this way. This concludes my interview with Dr. Frances Keesler Graham.

Graham: And thank you.