

Doris Entwisle

- Born in Wilbraham, Massachusetts
- B.S. (1945) University of Massachusetts, M.S. (1946) Brown University, Ph.D (1960) The Johns Hopkins University

Major Employment:

- Associate Professor of Sociology and Engineering (1967-1971), Professor of Sociology and Engineering (1971-1998), and Professor Emerita of Sociology (1998-Present) all at The Johns Hopkins University



Major Areas of Work:

- Research Design and Data Collection
- Sociology of Human Development

SRCD Affiliation:

- Committee on Summer Institutes and Study Groups, 1978 – 1981
- Nominating Committee, 1981
- Program Committee, 1983-85 and 1985-87
- Publications Committee, 1987 – 1996
- Adolescent Panel, 1987

SRCD ORAL HISTORY INTERVIEW

Doris Entwisle

Interviewed by Linda S. Olson
At John Hopkins University, Department of Sociology
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Olson: First of all I would like to talk about your family background and early experiences in childhood and adolescence since it might be of interest. The educational and occupational interest of your parents; where you were born, that kind of thing.

Entwisle: I was born in Wilbraham, Massachusetts, which is still a very small town. I was born at home. My father had about two years of college, and my mother had never worked outside the home. I have one sister who is a little bit younger than I am. I think a very important part of my early childhood experience was the fact that I lived – moved to Springfield with my family --and lived in a neighborhood where almost all the children were girls. And we – the various families did quite a few things together, and the children played together, went to a school within walking distance, and so got a really different attitude, I think, about feminine capabilities and that sort of thing than I might have had if I had grown up under a different situation. Also, because there were only girls in my family, my father spent a lot of time with me - giving me mathematical puzzles to do, and things like that. He liked to do those. He taught me all kinds of things, including giving me lessons in how to play contract bridge, and things probably that wouldn't have happened if I'd had brothers. At any rate, I went to public schools in Springfield, Massachusetts, and at the time I went there they had an unusual set up [organization of high schools]. They had four high schools that were located in the center of the city, so this city had a population of about 150,000 people at that time, and this so-called "Springfield Plan" was ideally suited to a city of this size. All students commuted by public transportation to the center of the city, and of the four high schools one was the College Preparatory High School, and that's the one that I went to. And nothing mattered in this high school except academic performance, and the performance of females was as highly rewarded as that of males, which is kind of unusual. We didn't even have a football team, and were rather scornful of people who did. So that kind of

atmosphere continued, if you will, the kind of feminist - and it was not something that was designed, it was just a chance set of events, in a way, that I think had a big influence on me. That high school was run by very, almost Nazi-like means. There were no – if anyone didn't hold a swinging door for someone else behind him or her they were expelled from school. If you did well in school, the principal every month got up on the stage and read the honor roll, and you could tell from the way he did it; (honor roll 1, 2, 3, 4) you could tell that the people with the highest honor roll got all A's, and people on the next one got all A's but one B, and so on, so everybody knew exactly what everybody else was getting in the way of grades, which made a very kind of competitive atmosphere. At any rate, after I went there I went to the University of Massachusetts, and the competition there was so much less than that to which I was accustomed that I really felt as though I was resting all of my freshman year. But I did take a lot of courses in science, because I liked science. I took math and I took chemistry and I took biology and botany, and I had to take English, and I took German. I had never had any German before. And I continued in that pattern for most of the four years that I was in college. I ended up with a major in math. I tried to do lab work in chemistry and physics, but I was a complete flop at it so I didn't pursue that any further, but I got interested in psychology. At that time there wasn't any real sociology in very many places and there certainly wasn't any there, so I also majored in psychology. And when I finished, the man who was Chair of the Psychology Department at that time, was a product of a man who was a very famous experimental psychologist earlier in this century by the name of Walter S. Hunter, and he was particularly well known for training graduate students. He had been at Clark University but he moved to Brown, and when I was going to graduate school I was admitted to Brown, and I was just twenty-one by the time I finished getting a masters degree.

I forgot to say that when I was in grade school there was a lot of double promoting going on, so that by the time I was in 7th grade I was 10 years old, and that kind of acceleration just kept going. By the time I got to Brown to begin graduate school I was quite a bit younger than most of the other people there, but I also had had a lot more math and science than most of the people there, and Hunter especially valued that kind of background. What he didn't value was later on that year I had met my future husband, who was in medical school, or the fact that I was going to link up with somebody who might want a wife. He told me explicitly, I might as well be a secretary. So, when I did drop out of that graduate school to move to Boston, where my husband was in medical school, I had to work because we were extremely poor. We didn't have any other means of support, and work conditions for women in that year were far from ideal. They have improved greatly since then.

One of the things that has struck me about differences in those two time periods is that almost all organizations, including universities and the investment company where I worked, worked six days a week; that is, they worked five regular days, and they worked half of Saturday. The grocery stores, however, were only open until 6:00 p.m., so that if you were a working housewife who had other responsibilities, it was pretty difficult to try to manage these two things.

Also during that period, the limits on what women could do, and many occupations were quite severe. In fact, a friend of mine who went into a masters program at Radcliff in business management was booted out when she decided to be married, because married women were not allowed to be in the program. So these were very different situations from what we find now.

And after I had been in the investment business for a while working in downtown Boston, I realized that there were no opportunities for advancement at all, so I took the civil service exam, and I must have done pretty well on it because very soon thereafter I was interviewed for a job as a vocational advisor, for which I had no training and knew nothing about, at a local veterans hospital; the fifth biggest one at the time where they had psychiatric patients, mainly people who had survived World War II and who had very serious problems. I went to work there and became quite alarmed about the situation in civil service jobs, because as I said, I had no training really, although I had a Masters Degree in Psychology, I really didn't know anything about vocational advising, and furthermore, these people who had serious problems (like completely incapacitating cases of alcoholism, or were suicidal, and so on) were being tested to see what they wanted to do in the way of a vocation. And the test listed such things as being a playwright or an auto mechanic, or other jobs for which these people had no hope of being able to qualify, and to try to interview these really sick people and see their faces light up when they found out that they might be interested in being a playwright was just too much for me to handle.

Not very long after that, the problem went away in any case because they had what's called the 'reduction-in-force,' and at that point anyone that had a civil service job could be replaced by a veteran, whether or not the veteran had met the - in fact, most of the veterans had not taken the qualifying test – but

they were given additional points, and if they could write down on paper what seemed to be a reasonable biography to support this position, and they were veterans, they took over the jobs of the people that had passed the test, so at that point I joined the ranks of the unemployed. And so I got in touch with the people at Brown and learned that one of my colleagues at Brown, a fellow student named Dick Solomon, had gotten a job at Harvard and they had just started a new department of social relations. So I got in touch with him, and asked him if he knew about any kinds of work in the Boston area, and he conferred with some of his colleagues and they decided to hire me as a mathematical assistant to what they called the Laboratory of Social Relations, and they could afford to hire me because they decided not to buy a calculator, which would have cost the same amount of money at that stage.

At any rate, that was an extremely fortunate thing for me, because the year that I started there the department was very new, it had been established in 1946 at the end of World War II. It had four components: Sociology, Social Psychology, Social Anthropology, and Clinical Psychology, and it had one hundred fifty graduate students. I was working directly for a person named Frederick Mosteller, who was the first mathematical statistician to be hired at Harvard. There was no department of statistics, and he came from Princeton, which was a premier place to train mathematical statisticians. In fact, there were only five places in the country at that time that gave Ph.D.'s in mathematical statistics. He was in the forefront of his field, even as a young man. Many of the people who were in various areas of science, including social science, had spent the war years in Washington trying to deal with some of the problems that the war presented, and they all got to know each other and there was a lot of cross-fertilization going on that enabled him as a mathematical statistician, for example, to then join a Department of Social Relations where most of the people were social scientists. At any rate he was a very talented person and also a very generous person intellectually. And so as his assistant I was able to work with him doing all kinds of things, such as helping people who were doing research in that department, and all those different wings of the department. But also, he consulted for the Massachusetts General Hospital, had to design medical experiments, and he had a wide-ranging set of colleagues all over the country, and really all over the world that were always sending him questions or asking for advice, and he just treated me as a colleague, so sort of by osmosis I picked up a lot of statistics. I had, of course, a mathematical background that helped, and I went to his course in mathematical statistics and, of course, we were getting a lot of articles to referee, and I helped as much as I could with referring those articles that were going to be in statistical journals.

So I was working there for about five years and my husband was continuing his medical training. After he finished medical school he had five years more of training. In Boston the situation was, and perhaps still is, that the better the hospital the less money people make, so after five years out of medical school my husband was making \$75 a month. At that point the Korean War came along and all doctors and dentists were in short supply, so as soon as Eisenhower won the election, he declared that he was going to do something about this shortage of physicians and dentists in Korea and he made it happen that people like my husband, who had been in World War II, but who had not been in as a medical person, could be drafted as privates or they could volunteer as officers. Given that choice, and after checking out the choice rather thoroughly at the first army command, and finding out that they really meant it, he volunteered as an officer. And within a month or so he had left for Fort Sam Houston, Texas, and in nineteen days he had orders to go to the Far East.

Meantime, we had decided to have a child before this juncture, because it meant he would be making enough money so that I wouldn't have to work, so by the time he left to go to the Far East I was pregnant. I resigned my job after I was about five months pregnant, because, again, conditions were very different in those days, pregnant women were very unwelcome around the work place, so it wasn't really feasible. Also, I couldn't continue living by myself very well if I was going to have a baby, so I moved back with my parents for a year while he was overseas. And he worked in a MASH, which was a hospital devoted to fevers, although MASH means Mobile Army Surgical Hospital, his hospital was about two miles outside Seoul and dealt with all kinds of serious medical infectious disease problems. He was trained as an internist, and they were seeing all kinds of things like Typhus and Cholera and exotic diseases that he had never seen in his training in Boston, including one called Hemorrhagic Fever, which at that time was thought to be endemic only on the Asian continent. It turned out later that was present or has been found since in South America particularly, but now we were also thinking that Hemorrhagic Fever is one of the closely related problems next to the Ebola Virus.

When he came back from this tour, he came back with samples of frozen blood and so on, and the first time he came back after he had been gone for a year, I met him at an air base in the middle of the night, and he had with him all these blood samples packed in dry ice. So we had to spend his first day back

after a years' absence running around the city getting enough dry ice so that he could then take off again and go to the Walter Reed Hospital in Washington. He went down to Walter Reed and stayed for a while, and then he decided to take a job back in Boston working for the same group at Boston University Medical School, with whom he had been training. They had a large group practice with about fifty physicians in it, and they hired younger men to do most of the running around and all of the house calls, things like that. So he did that, and I continued to stay home and raise children, and I decided to have another one, and in the meantime his salary just didn't cover our basic needs.

In those days, to be a medical doctor in Boston, it helped a great deal if you were independently wealthy, which we weren't. So, while he had been in Korea he got to know one of the leading people in infectious diseases, a man named Theodore Woodward, who was then at the University of Maryland Medical School, and through him a job came about and we migrated to Baltimore. After we got here with two small children, I was getting very restless staying at home, because I had had such a fine experience working at Harvard, and I was looking around trying to find something to do, and I realized that without more advanced degrees it was going to be pretty limiting circumstance. I said to my husband "I think I'll go back to graduate school" and he said "Well fine. You can always quit if you don't like it."

So I went back, and as soon as I got back in it I really enjoyed it and I ended up getting a Ph.D. at Hopkins in 1960, and then had a Postdoctoral Fellowship with the Social Science Research Council from 1960 to 1961. In that capacity I became busy with a computer on the Hopkins campus, the first computer they had that was being fiddled around with by the electrical engineers and also James Coleman, who had just come to Hopkins to found a new department that was called Social Relations, but which was mainly sociology. It was taking the same name as the Harvard department, and it tried to be broad-minded, but it had mainly social psychologists and sociologists.

When I came to the end of that Postdoctoral Fellowship, I was pressed into service to help the engineers teach computer programming to the undergraduates and also the people in sociology, which was only a graduate department at that time, wanted me to teach a course in methodology to their students. So I agreed to do that. And for a number of years I had a joint appointment in electrical engineering and into then the Department of Social Relations, with about fifty percent of my time spent in each.

So that continued for a while, and after I had been doing this for six or seven years, some of my friends who were on the faculty decided that I really deserved to have my position regularized because I was not in a tenure track position, but I had published quite a few articles and written a couple of books, and they thought I should be brought up as a candidate for an Associate Professorship, which they did. And so in 1967 that was where I ended up, still with one foot in each of the two departments, and being an Associate Professor with both department names tagged after it.

I continued in that situation really until about ten years ago when I resigned the title in engineering because it just didn't apply any more. Well, this doesn't tell much about how I became interested in child development, because really I wasn't particularly interested in child development until quite far along into my career. The way I became interested in child development was that we were devising some projects to be funded in order to get money to do research, and one of the projects had to do with cultural differences in how children learn language. At that time the field of linguistics was undergoing very rapid evolution. It was the time when Chomsky's early work was just coming out. There was no Department of Linguistics here or in many other places, but there was a lot of interest in understanding how children learn language with a view towards understanding the structure of language in general and why languages differ from one another and the like. So I began doing studies of word associations, that is, you say to a child the word "chair," tell me the first word you think of, many children will say "table," but naturally there's a range of responses. And if one samples word associations in this way, there are differences across cultural groups or even socio-economic groups, because the rate of development seems to vary and also the associations seem to vary. At any rate, that research, I don't think has been very important in the whole field, although it did reinforce very strongly the idea that children's learning of words follows a pattern. That is, if you ask a very young child to give you a response to the word "went," they'll say "home. That is they give you a word that follows "went" in a sentence. A little bit later, when they're about seven or eight, they will give you a response to the word "went" that is like "go" or "traveled" or some other replacement word, something that could substitute for the original word in a sentence. So they're indicating a lot of additional knowledge about the privileges of occurrence of that word that indicated they had really revised their whole mental system for filing words.

So I was doing this under the auspices of the Sociology Department and actually spent quite a bit of time tracing-out these facts in Amish children and in rural children, in relatively low SES children; both

African, American/white in Baltimore, and as I became more and more involved in this work that was related to the socio-linguistics of development I became convinced that the kinds of problems that minority children had that were presumably linguistic, that is, everyone was commenting on the fact that some of the minority group children were not doing as well in school as the majority children, and there were a number of people in the area of applied linguistics, who at first thought that this kind of deficit was due to some kind of language learning problem. That is, they were thinking that some cultural groups had cognitive deficits that were leading to these language differences. Well, it didn't take very long before most people became convinced that this was a fallacious idea, that to think that, say, a black child's vocabulary is different from that of a white child because the black child has a cognitive deficit was just impossible, and not only that, it was dangerous. So I began thinking about this whole problem and became convinced that the real thing that was impeding the children who came from less favorable economic circumstances, was that socially they could be easily identified. We could tell from ten minutes of tape - the teacher can tell the socio-economic status of the child. So it wasn't so much that the child spoke a different dialect than a majority child, as it was the way the child was treated or the expectations that school people had for such children really led them to achieve at a lower level.

- and that side of the whole line of research and has continued to this day, so I was in half time in the Department of Sociology, and I was working mostly on problems related to how language and things related to language affect schooling. And I think in some ways all of my work, intellectual work that is, relates to a single question, and that is, "What is schooling?" because schooling is not learning. Some learning occurs in schools, but it is a mistake to think that what happens in schools is entirely something that is related to cognitive changes in children, there are many, many other things that are happening too.

Olson: I'd like to ask you about the political events that have influenced your teaching and writing over the years?

Entwisle: Well, I think certainly the political climate of the sixties, when war on poverty started, and President Johnson had other initiatives that were related to improving the public schools, had a lot to do with my interest in how language or social-linguistic factors affected child development, and how that in turn affected schooling. Since then, I think the funding situation as it's gone up and down has had an influence on both the kinds of topics we did research on, and also how quickly we were able to finish things. Lately, for example, there's less interest in studying some aspects - or I better stop right there.

Olson: Okay, now I'd like to focus a little more on your personal research related to child development. You spoke a little already about how you first got involved with your interest in child development. What continuities in your work are most significant? What shifts occurred and what events were responsible?

Entwisle: I think more than most people, my entire research career has revolved around a single question, and that is, "What explains the effects of schooling, and particularly how social structure relates to schooling?" So the early studies on linguistic development, the later studies, there are more complex longitudinal studies like the Beginning School Study, is basically motivated toward understanding what goes in schools and how attending school changes children's development.

Olson: Okay, could you reflect a little on the strengths or weaknesses of your research and theoretical contributions on the impact of your work and on its current status?

Entwisle: Well, I think the primary strengths of the research at this stage, and looking back, is the whole Beginning School Study that was initiated in 1982. We picked up a pretty good size range of samples, about 800 children who were starting first grade in Baltimore for the first time, and they were picked randomly so that they were fairly representative of the city school population, and then we have been following them ever since. Following them intensively, by that I mean we've interviewed them many times over the intervening years, and in the beginning when they were six and seven years old we were interviewing them two or three times during the school year and also interviewing the teachers. So this project is still the only one that we know of in the United States, in which a fairly large sample of youngsters has been studied from the time they started school, and followed intensively over that period.

The national studies now available start in the eighth grade. Some new interest is beginning; it won't come on line for quite a few years, starting at birth or at age three.

Olson: Okay. Do you have anything more to say about the current status of your work?

Entwisle: Well, we're continuing to follow these people and we're in the field now, they're age twenty-three, and we're trying to bring to bear what we know about these individuals life situations all the way back to age six, to understand what their early adult life circumstances are. For example, we have found that some of the things that we could tell about these people in first grade predicts who is going to drop out from high school. And we expect now that we're studying the early job situations at age twenty-three to find similarly that there are predictors of who does better in the labor market, who was having difficulty balancing roles of going back to school and working at the same time and the like. For example, right now we are working on their employment when they were adolescents; whether or not they did paid work. And we find that circumstances of their work when they were ages thirteen and fourteen predict some of the circumstances of their work when they were fifteen and seventeen. And we fully expect that the whole picture at age twenty-three is going to have some precursors that go back not only to age thirteen but even before.

Olson: Okay. What published or unpublished manuscripts do you think best represent your thinking about child development, which of your studies the most significant?

Entwisle: Well, I think the most significant one is undoubtedly the book that came out last year, in part, because it's also the most comprehensive one to date dealing with the Beginning School Study. It looks at children's development from the time they start at age six through the first five years of school, which would be the elementary years in the Baltimore system. Not all of the children, of course, at the end of the fifth year will have finished grade five, because there's a high retention rate, but over that period in this book we talked about what we see as the socioeconomic background influences on children's development. We have a chapter on family structure and how that may affect progress in school. We have another chapter on how the organization of the school affects children's performance, that is, their grouping practices, the organization of the school-years; that is, some schools only cover years one and two, others cover years one, two and three, and so on, and then we also talk about gender. And we try to deal with these questions or topics not only from the standpoint of what we found in the Beginning School Study, but also in terms of how this work fits into the more general picture of what we know about child development.

Olson: Okay. Could you reflect a little on your experiences with the research funding apparatus over the years, comment on your own participation in shaping research funding as well as on the securing of support for your own research?

Entwisle: Well, I think almost in the beginning, probably in the mid-sixties, I began to serve as an outside reader on some research proposals; those that have been submitted to organizations like the National Science Foundation or the National Institute of Health, but also on internal proposals that were being made by my colleagues. My participation became formal right around 1982 when I was asked to join the Human Development number one study section for the National Institute of Child Health and Development, which is a four-year term supposedly, but actually I only stayed three. During that time I also joined a panel, reviewing proposals for the National Science Foundation, and I stayed on that panel for two years. In both of these cases, hundreds of proposals are reviewed and, of course, a very broad view of the field is taken. All along, before and after, and up to the present, I would say I reviewed at least half a dozen proposals made to private agencies or to government agencies during the year, and they deal mainly with topics that I'm supposed to be somewhat familiar with. In terms of – what was the rest of that question?

Olson: Say something about your experiences serving on these panels, and also about your own experiences in trying to secure funding for your research?

Entwisle: Well, my experience on this panel was extremely valuable. I can't over-rate the value of that experience, because it really allows one to learn about things that are right at the cutting edge, and to be informed about a broader set of issues than one could possibly get by just looking at the current literature.

The ups and downs in funding cycles have been extremely frustrating, because at times it seemed as though it was almost too easy to get funds to do research, and at other times it seemed almost impossible. And there wasn't much that seemed to relate to the quality of the work or the subject matter of their work that explained that. It seemed to be more these government contextual effects that we had to deal with, or perhaps even the personalities of the particular institutions who were giving money to granting agencies. For example, in some of the private foundations a President could take over who was interested mainly in social service work, or ecological studies, or things that we were not very well qualified to investigate.

Olson: Okay. Anything you want to add about your own efforts to support -- getting support for your own research?

Entwisle: It's been continuous since day one, and I've been very fortunate. I can only remember one brief time when I wasn't covered by at least one grant, and usually by more than one. So we have been unusually lucky in that, despite the difficulties that I just mentioned. And now we have multiple sources of funding that we're always working on, through trying to make sure that our work will be underwritten in the years after, when our present funding goes out.

Olson: Okay. You already spoke a little bit about your affiliation with Hopkins and your work there, which has been continuous there over the last, what thirty years? Do you have anything more to say about your experiences at Hopkins, and also your experiences as a teacher of child development and research, or trainer of research workers, or courses you've taught, anything like that?

Entwisle: I really haven't done much in the way of teaching courses or running programs, because I'm in a Department of Sociology, and I have taught many years a course called the Sociology of Human Development, and before that I taught a course along the same lines that was called Child Socialization. I trained a number of graduate students in topics related to the sociology of education, but nothing like the kind of close ties to the field of child development that would be the experience of, some would say, in the big department of developmental psychology, with a training grant that was really focused on training future researchers in that domain. Hopkins is a very congenial place for researchers to work, because there's literally no pressure to work on any particular problem. And it's also pleasant, because in terms of teaching, at least in this department, there's very little pressure to teach one course rather than another, so that that kind of freedom is a boon to someone who does research, because the kinds of teaching that one elects to do can be related quite closely to the ongoing research.

Olson: So I gather you didn't find a lot of tension between the teaching and research experiences that you had?

Entwisle: No, in fact, just the opposite. The teaching load is small enough here so that being forced to do some teaching was actually a benefit. It broadens one's horizon in ways that probably wouldn't occur if one were just doing research one hundred percent of the time. But the teaching requirements are so minimal, in a sense, that it doesn't interfere very much with research, as compared to what the teaching load would be in a regular -- a different kind of university.

Olson: How about experiences in applied social development research. Have you done any work in that area, and could you comment on your role in putting theory into practice?

Entwisle: I haven't done any work in that area, in fact, I specifically avoided it, partly because the topics that we work on, which are related to schooling are so poorly understood, but so widely believed to be transparent to people who have ever attended school, that it's very difficult to make prescriptions or to talk to groups about the kinds of problems that still remain. And the Baltimore City School System, although they've been uniformly gracious and accommodating, especially in the early years to our research, really have shown very little interest in the kinds of things we've found, even though we think some of the things we found could be enormously useful to them.

Olson: Okay. Now I'd like to focus a little on your experience on SRCD, if you could talk a little about when you joined the organization, your earliest contacts and with whom, and the first biannual meeting you attended, and then describe something about the history of your participation in the meetings and publications?

Entwisle: I think the first time -- I was an avid reader of child development almost from the beginning, and especially a two-volume set of books called the Review of Child Development Research that came out in 1969, that was an excellent set of volumes and enormously useful to me when I began teaching child socialization in the Department of Sociology. And as everyone knows, the authors who wrote those various chapters were among the leaders in the field, and that the book was just precious in terms of its help for people like me who were in a close related, but not the same discipline.

I think the first time that I went to a SRCD meeting was about 1975. I never went to any national meetings until my children were in high school, it was impossible, and so I was a full Professor before I went to any national meeting. But I remember being asked to discuss some papers that had to do with linguistic development at the Philadelphia meeting where George Miller was on the same panel, and I can't remember now whether he presented a paper or whether he was another discussant. And then soon after that I was asked to participate, or maybe it was about the same time, in a summer institute program that was being put together at the University of Delaware where young researchers, recent Ph.D.'s and others were spending five or six weeks to become acquainted with special topics, and I was brought up there for a couple of days to talk about our research on linguistic development.

Then, I think largely through the interest and emphasis that the society's always had about incorporating people outside the discipline of psychology specifically, and outside of child development also, I was asked to participate in various society activities. For example, I served on the program committee for a number of years. And then I was asked to serve on the publication committee, and I think I served on that for six years. And as we all know, those committees often can involve being the Chair in the middle years of the six years of service, so then I was the Chair of the Publication Committee for a couple of years. And since 1993 I've been on the Governing Counsel, and in connection with that and also with the other committee work, I participated in things like the Committee on Finance, and the Committee on Social and Economic Affairs, and all kinds of other things. I can't remember all of the affiliations, but they're listed right here.

Olson: What were the major problems and issues that confronted you during your time, especially when you had more leadership positions in these governing wards and things?

Entwisle: Well, I think the biggest problem that I -- well in terms of the Program Committee, the archaic way of organizing the program that was in effect in the mid-eighties was soon and properly replaced by more computerized methods of organizing conventions, so that instead of everybody running around writing on blackboards who was supposed to talk when, and having to kind of do this in a very crude way, we have passed fortunately beyond that point. In the Publication Committee there were some very tense sessions as the Society tried to enlarge the diversity of its publications. In the late eighties, for example, virtually every article in Child Development was based on studies of white, middle-class children, and minority members of the SRCD, and also the rest of us, realized that this was highly flawed kind of research because there were all kinds of groups that this research may not apply to. And so vigorous efforts were being made to encourage a wider sampling of studies, and not only with respect to the samples that were included, but also with respect to the topics; for example, the topics of poverty have been largely neglected, the topic of cultural or ethnic differences have been under-represented and so on. But I think that was the most significant, and also the stormiest period perhaps in the whole Society as these problems were being worked out.

Another period, I think that was somewhat stressful, but for a different reason, was when the top organization of the Society shifted to the single office based in Ann Arbor, and the University of Chicago Press gave up supervising the membership roles of the University. And at that point also, the way that articles or journals were edited changed with a single editor, copier or whatever you want to call it in Ann Arbor, and then the various other editors sending communications back and forth by email, so that the way that the processing that was completely different, and naturally such profound changes in how things are handled, there were some slip-ups here and there, but that seems to have worked out quite well also.

Olson: Do you have anymore to say about what you believe are the most important changes to occur in SRCD and its activities during your association with it?

Entwisle: I think the most important change is a broadening of the topics that are considered topics to be suitable to work on in terms of the diversity, size of populations or bigger samples. We're getting some secondary analysis now of national data sets long overdue. Also an opportunity for more studies that are not strictly speaking experimental, that is, not randomized experiments. Not that I have anything against them, I think they're wonderful, but there are many problems where that methodology cannot be applied, and many of these problems are just the ones that we've been concerned with, the diversity and cultural issues.

Olson: Okay. Now I'd like to focus a little more on the general field of child development. Please comment on the history of the field during the years you've participated in it. Major continuities and discontinuities and events related to these. And have your views concerning the importance of the various issues changed over the years, and how?

Entwisle: My situation at Hopkins is very unusual, because in the Psychology Department now we have nobody that's really interested in child development that I know of on the regular staff. And earlier we had a couple of social psychologists that at least were interested in personality variables or things that could be developmentally relevant, and prior to that, Mary Ainsworth was here and had a very active program. But I would say that certainly over the past five years there has not been even a glimmering of what I would call social psychology, so that department's interests really do not overlap those of child development at all. And so, in a sense, one who is interested in this field is academically isolated in this University. Fortunately, because of my ties to the sociology of education, that hasn't been a great hindrance for me, because I have many colleagues who are interested in schools and schooling, and I'm interested in a developmental segment of that, so that I can continue to have intellectual colleagues and to profit from association with other people who happen to share those interests.

Olson: Have your views concerning what the most important issues in child development changed over the years?

Entwisle: I guess so, but such an imbedded process is hard for me to pull it up and think about it. Certainly my interests have changed, as I mentioned, from those that relate closely to language to issues that now pertain not only to social influences like expectations; the expectations held by parents and by teachers, but more even to social structure. For example, what is it about poverty that makes it such a depressing factor in terms of child development? What about neighborhoods? Some of these social contexts including schools that we couldn't investigate very thoroughly before, because we didn't have either the statistical tools or the technical capacity to develop models that were anything like pictures of reality. Now we can look at children nested in families, and families nested in neighborhoods, which we knew was important all along, but we didn't have the ability to cope with that degree of imbeddedness, and yet, certainly having profound implications for the findings that we come up with.

Olson: Do you have any hopes – particular hopes for the future of the field?

Entwisle: Well –

Olson: Or fears about the direction it's going in?

Entwisle: I think that there's a great emphasis on policy now and applied topics, and I think that's good. At the same time I get a little worried from time to time if some of the kind of basic science of the field isn't getting enough support, so that we can have what you might think of as a group of people whose work over the long run may lead to some new interesting approaches or new theories that we don't have any ideas about, or can even think about at this point in time. I like to have some people that are just doing scholarly work and not subjected to pressures of how this applies in what situation or whatever.

Olson: Okay. Now I want to know if there are any other personal notes you'd want to add? I know you've spoken about your family early on, but are there any things that you want to add about your personal interests and your family, especially ways in which they may have had a bearing on your scientific interests and contributions or on your applied contributions?

Entwisle: Well, I think having my own children, when they were very young, were certainly a strong influence on my interest in verbal development, because just observing them on a daily basis posed some interesting questions. For example, both of my older children spontaneously around the age of four said, "What's tenty?" I remember them saying, "What's tenty?" They're saying, "Eighty, ninety, tenty. What's tenty?" which was clear they were generalizing along a dimension of language that was very reasonable, but you know, English being the way it is, there was an exception to the general principle they were trying to uncover. So that kind of thing I think stimulated my interest in children's verbal development. But in terms of my overall professional development, I don't think that my family life, per se, has had a very close relationship with that. That is, I don't see those as closely over-lapping.

Olson: Okay. I guess that's about it then.

Entwisle: Good. Thank you.