

Lois W. Hoffman

- Born March 5, 1929; died February 13, 2015
- B.A. (1953) University of Buffalo [now SUNY Buffalo], M.S. (1954) Purdue University, Ph.D. (1958) University of Michigan

Major Employment

- Professor, Psychology, University of Michigan: 1975-2015
- Chair, Developmental Psychology, University of Michigan: 1986-1992
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Major Areas of Study

- Sociology, family interaction patterns, motivations for parenthood, child rearing styles, social context of effects of family

SRCD Affiliation

- Program Committee member (1973-1977)



SRCD ORAL HISTORY INTERVIEW

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Interviewed by Twila Tardif
At the University of Michigan
January 31, 1995

Tardif: Okay. So we're supposed to start with your general intellectual history. Can you tell me a little bit about your family background as well as some childhood and adolescent experiences?

Hoffman: I don't really think there was a lot in my family background to predict that I was going to end up a child developmentalist or a professor. I grew up in Elmira, New York, which is a very small town, about forty thousand. It is in an absolutely beautiful area near the Finger Lakes about a hundred miles from Rochester and not near any other big city.

Tardif: Just off of 20.

Hoffman: A bit off from 20, right [Elmira is actually at the south end of the Finger Lakes, near I-86, quite a bit south of 20]. I was the youngest child in a very American, happy, intact family. A very non-intellectual family. And the family was financially comfortable, not rich, rich, but we were financially comfortable. And I really think that much of my ending up where I am had to do with chance and various encounters. And I do think a lot of the prejudices of the times affected me.

Tardif: You said you were the youngest child. How many siblings did you have?

Hoffman: I have a brother eight years older and a sister four years older.

Tardif: And they're not developmental psychologists?

Hoffman: No, my brother did not go to college. My sister did go to college. She got married very young, she had six children, and in her sixties went back to school and got her master's degree.

Tardif: Great. Are they both still in New York State?

Hoffman: No, my brother is still in Elmira, as are two of his children. They have continued to live there and I do have a lot of family there. And my sister is in Decatur, Illinois.

Tardif: And you go back to Elmira frequently?

Hoffman: I have a lake place in the Finger Lakes and I go there to see my family and brother and his children, and I spend my summers there particularly.

Tardif: Actually were there any people who you grew up with or knew at all from Elmira who you've met through the years in odd circumstances like at conferences or anything like that?

Hoffman: Never.

Tardif: Not one?

Hoffman: I went back to my high school reunion, and at one point—it was a twenty-fifth high school reunion, and people asked me afterwards if my classmates were surprised at how much education I had, and I said, “No one ever asked.” They asked me how many children I had and I said, “Two” and they said, “Oh,” because they all had enormous numbers. I sat with a friend of mine from high school and she had seven children and I said, “Oh, Kay, they're giving a prize for the most children, you will win it,” and she said, “Not in this crowd I won't.”

Tardif: Wow. What type of school did you go to in Elmira?

Hoffman: In Elmira I went to a public high school. It was primarily working class. It was very easy to be an honor student. I was not very studious, though I did well in school. I was interested in English, I guess, and my best friend and I co-edited the high school newspaper, and we were planning to go to college and we decided—we picked our colleges ourselves, none of the four parents involved had been to college and gave us no advice. So we selected Northwestern because we wanted to be foreign correspondents like Margaret Higgins. We wanted to go into journalism. And we selected Pembroke College, which, of course, was at that time a women's school connected to Brown, because it had a very pretty brochure. We applied to both places. We were both turned down at Northwestern because it was 1947 and the ex-GIs were coming home and had the GI Bill and Northwestern said they were not taking out-of-state girls. And so we didn't go there. We were both accepted at Pembroke and we were planning to go there, and then in late May I had a dormitory form and it asked if I had any preferences for roommates and I said I would like to have a Jewish roommate because I would like to get to know some Jewish women—I probably said girls—and I got a letter back saying that they were very sorry that an error had been made and that I was no longer accepted at Pembroke.

Tardif: Was your friend accepted?

Hoffman: Well she was accepted, but they did—they wrote to her minister to check on her after they got my dorm request. At that time they didn't actually ask you if you were Jewish, but they sort of figured out from your name and where you were from. And so they hadn't checked on us, and since I turned out to be Jewish they did write to her minister to make sure that she really was not Jewish. And they did subsequently—in the first letter they did not say it was because I was not Jewish, but I wrote and asked about it and so forth and finally they wrote back and said, well, they had a Jewish quota and I might have fitted in under that, but the Jewish quota was filled at that point, and so I hadn't been considered as a Jewish person so I couldn't go there. So there I was in May with no place to go.

Tardif: Before that experience did you consider yourself to identify strongly as Jewish in Elmira, a non-Jewish community to the extreme?

Hoffman: Well, I definitely felt Jewish. I didn't feel prejudiced against before that. I mean I was Jewish, I came from a religious, kosher orthodox family. I don't know that I would really say they were religious, but it was an observant family, and there were some other Jews in town and we definitely observed the holidays and were involved in—I mean, we knew we were Jewish. I thought of it positively, but I really had never had any direct anti-Semitic experiences. That was my first.

Tardif: So that was late May of your senior year in high school, you got unaccepted from Pembroke and you wanted to go to college the next year?

Hoffman: I was very dumb. I mean I really should have done it differently. I mean I did not apply at Cornell, which would have been a logical place to apply to, or maybe I did at that point, in any case I applied—you know, I may have applied to Cornell. I think I did. I applied at several schools and I actually was accepted, though it was very late, but they wrote and said there was no room in the dormitory. And the main reason I was going to college was to live in a dormitory and to get away from home, so one of the schools I wrote to was the University of Buffalo, which is SUNY Buffalo now, and they did not say there was no room in the dormitory, so I decided to go there. And when I got there I learned they had no dormitories. However, going to the University of Buffalo was what changed my whole attitude about everything. I really had a marvelous college education there. It was—and I think that's a lot of what happened to me, I just fell into situations that worked out very well for me. I didn't know what to major in. A friend of my sister was over to the house and said they had a good sociology department, so I put down I would major in sociology. And as a freshman at the University of Buffalo, very inadequately educated in high school, I took this course from Al Gouldner in sociology and the first assignment was an article by Robert Merton, "Social Theory and Social Structure," and I read the article and I didn't understand a word. And I reread it and I still couldn't understand it. And I read it over and over and over and finally had a more advanced student there, an ex-GI, go over it with me, word by word, until I understood it. And that really was the beginning of learning to think and read and stand on my tiptoes to achieve things, and it was just a very exciting education for me.

Tardif: When you said that you felt very unprepared from high school, was that the only experience that you had, or did you suddenly feel that you were confronted with a completely different set of expectations? I guess what I'm trying to get at is what was the sense of the other students at SUNY Buffalo who were entering in their freshman year?

Hoffman: Well everybody was more sophisticated than I was, I thought. I mean even though this was really basically a commuter college, I think, at that point, but there were a lot of ex-GIs there who were much older than I was. By moving into sociology and by becoming sort of a student of Gouldner's in one year I just changed drastically in terms of my whole orientation toward life and became much more involved in intellectual pursuits. He had a group of very, very competent people. They were very politically aware. They were involved in political social actions. They were left-wing, and they had thought much more deeply than I had about how the social structure of society shapes one's development and one's opportunities. And in English too—I took English courses and I dated a man—in those days we called everybody boys and girls—so I dated a guy who was really very, very knowledgeable about English literature and I started reading books that I hadn't read before. I had another friend who was very knowledgeable about music. In fact, I had a lot of friends there who were serious musicians, and suddenly, on all fronts, I became enormously knowledgeable. The other thing was that Gouldner was really a very great and inspiring teacher and expected a lot of you, and I really started working and thinking. And we actually worked on research projects there. At that time there were a lot of young professors, mostly in the sociology department, who subsequently became well known. They were in their first job out of Columbia University and they didn't have much research money, and so they used to have their students conduct their research.

Tardif: Undergraduate students.

Hoffman: Undergraduates. Yeah, there weren't any graduate students. And we would meet weekly at Gouldner's house, and he was conducting a study of red tape—of people's perceptions of red tape. His work was on bureaucracy, building on Weber's theory.

Tardif: It sounds like you were an expert on red tape by the time you got there.

Hoffman: And I did interviewing. I conducted interviews all over Detroit. I specialized in lower class and ethnic neighborhoods.

Tardif: What brought you to Detroit at that point?

Hoffman: I don't mean Detroit, I mean Buffalo.

Tardif: Oh, Buffalo. Okay.

Hoffman: I mean Buffalo. It was like Detroit to me at the time. But it was really—the times were different then, you know. I mean, I would go out all by myself in some very lower class neighborhoods and interview people in their homes for an hour and a half. We learned how to interview, we did coding, and we did research papers. For my senior thesis, I really did a very elaborate research project in which I interviewed executives who had recently been appointed to office, pursuing the hypothesis that succession in industry leads to an increase in bureaucratization, that the anxiety and problems that the executive deals with lead to certain actions which result in increasing the levels of bureaucracy. That was a very good study. I didn't think about it then, but it was probably publishable.

Tardif: So even though you ended up choosing a college that wasn't the college you initially wanted to go to, you chose it because it was—it didn't have a full dormitory, in fact had no dormitory at all. It sounds like what you ended up finding was a little niche for yourself, in which you had an incredibly stimulating intellectual environment?

Hoffman: That's really true. I mean at the end of my sophomore year—I think it was then, anyway—I went to summer school right after my—no maybe it was at the end of my freshman year I went—yeah, at the end of my first year I went to summer school at Cornell and I could have transferred to Cornell at that time, but by that time I really didn't want to. I was really very hooked on Buffalo. It was just an enormous period of growth for me. At the end, anyway, I applied for law school. I was very interested in industrial sociology as a result of working with Gouldner, but I thought I would go and be a lawyer and be a labor lawyer, so I applied to law school at NYU and Buffalo Law School, and I actually was accepted, but when I talked to the people there they said I couldn't be a labor lawyer because to be a labor lawyer you had to work for the National Labor Relations Board but they didn't take women, so there was no way a woman could be a labor lawyer. So I decided I wouldn't go to law school and I decided I would get a Ph.D. in sociology. And that's what I set out to do.

Tardif: So in terms of early adult experiences that were important to your intellectual development, did that pretty much run through the main things that you would bring up?

Hoffman: Well, in terms of how I got into—

Tardif: It doesn't show how you got into child psychology.

Hoffman: Yeah. Okay. Well, I'll go ahead. I applied to Berkley, which was the school to go to at this time.

Tardif: In sociology.

Hoffman: I mean at this point I was finally picking—in sociology—at this point I'm getting better guidance and I have picked the best school for myself, and I was accepted.

Tardif: Who guided you; again I assume it wasn't your parents?

Hoffman: No. No, it was Gouldner. And I was given a fellowship to start in September of 1951 and Lipset and Bendix [leading sociologists at the time who argued that social mobility is an integral and continuing aspect of the process of industrialization. See Seymour M. Lipset and Reinhart Bendix (1959). *Social mobility in industrial society*. U of California Press] were there, whom I wanted to work with, and that was all lovely. But then I realized in January of '51 that I really had enough credits to graduate, so I finished up school and I was going to stay in Buffalo and work there until it was time to go to graduate school, but my parents said I had to come back to Elmira if I wasn't in school and I didn't want to go back to Elmira. And so probably in hindsight I could have written to Berkley and said, "Will you take me early," because I know we have done that kind of thing sometimes here. But in fact, I learned from Al Gouldner, he told me that there was a fellowship available at Purdue, and Lou Schneider was there, who was a well known person in industrial sociology, and he suggested that I go there and that he would help me get an immediate fellowship there. I would go there for the winter term and then start in at Berkley in September. So that's what I decided to do. And I do think that my story at this point puts me where I am by virtue of the practices of the time, that is, I think that I didn't go to Pembroke because I wasn't considered under the Jewish quota, because of discrimination. I really didn't go to Northwestern because of discrimination against women; they were not taking out-of-state women. I didn't go to law school because of discrimination against women in labor law, and I couldn't stay in Buffalo because, as a daughter in the family, I had to stay in my parental home if I was not in school, which was not anything they would have asked of my brother. So off I went to Purdue, never having really, I think, taken control of the direction I was going in, just sort of led by the prejudice and discrimination of the times, and my ignorance. Now I never got to Berkley.

Tardif: What happened? They un-admitted you?

Hoffman: No. When I got to Purdue, I signed up for a course in social psychology and it was taught by a young professor, Martin Hoffman, who was in his first job. And we—about the second night of the seminar he invited several of the students out for coffee and I said I would go, but the other students couldn't go. And we went to the place to get coffee and it was closed and so we went to a bar across town and had some wine and fell in love and got married, and I never got to Berkley. I stayed at Purdue and got a master's there.

Tardif: A master's in sociology.

Hoffman: In sociology. It was with Lou Schneider. And again, incidentally, I did a very interesting master's thesis. It was really very interesting. I did a study at Square D—that's a brand of electrical fuse boxes [see <http://www.schneider-electric.us/sites/us/en/company/profile/history/squared-history-legacy.page>—at the Square D plant in Peru, Indiana. It was on the conflict between the traditionalism of the workers who had very different rural backgrounds and the bureaucratization of the plant and how it affected attitudes and so forth and so on. It was really a very nice study I think, but I never thought of publishing it, although I clearly should have. And, in fact, Gouldner read the paper and wanted to include it in his book, *The Wildcat Strike*. And at that point Marty and I were moving to Michigan, and even though Gouldner had marked it up, I just sort of said, "Oh, I didn't want to be bothered," and I didn't publish it. I was very motivated to do well. I liked doing well in my work, but I was not career oriented, and I did not publish either the undergraduate paper or the master's thesis, which I should have, and I did a paper when I started at Michigan that I should have published and didn't.

Tardif: Did you know at the time that you should have published those papers?

Hoffman: I didn't really, not very much; that is to say I really hadn't thought of them as—I wasn't career oriented. I would have published them if I felt—well publications didn't mean anything to me at that time. I didn't think of myself as going for a career. I just sort of did each thing as it came. Anyway, Marty was an assistant professor; he was offered a job at the Merrill-Palmer School for a new research project. He was not a child developmentalist either at that time.

Tardif: So this is the summer—

Hoffman: This would have been 1953.

Tardif: —when you were supposed to be moving towards Berkeley? Or was this after that?

Hoffman: No, this would have been a year later.

Tardif: Oh, this is after your master's degree.

Hoffman: This would be a year later. This was in 1953.

Tardif: So instead of going to Berkeley that fall you did a master's degree?

Hoffman: I completed a master's. So then in 1953 Marty took this job at Merrill-Palmer and we moved to Detroit. He had gotten his Ph.D. here, at Michigan, and was very eager to get back to Ann Arbor, so that was why the job was particularly attractive. And it was a job to study children at Merrill-Palmer with Irv Sigel. I came here and decided to go—I enrolled to get a Ph.D. in sociology here at Michigan.

Tardif: Were you both living in Ann Arbor at the time?

Hoffman: We both lived in Detroit the first year and then we moved to Ann Arbor. And so I started out as a student in sociology. The sociology department here was very different from anything I'd had before; it was very formal. It was kind of cold and remote. There were some very good people, particularly Ed Swanson. Guy Ed Swanson was here at the time. He was definitely an important influence on me. At that time he had just come out with the book he did with Dan Miller, in which they had analyzed the structure of society as it affects child-rearing patterns and they differentiated mass from bureaucratic society [Miller, Daniel R.; Swanson, Guy E. (1958). *The changing American parent: A study in the Detroit area.* (1958). xiv 302 pp. Oxford, England: Wiley].

Tardif: Is that your first sort of even glimmer of a possibility that you might be doing something related to children and development?

Hoffman: Well, I don't even think I thought that when I took it, but I was certainly very moved by that idea. I really liked the idea of pursuing the study of the industrial structure of society as it affects childrearing patterns.

Tardif: So in retrospect though you might say—

Hoffman: In retrospect, I certainly would say that. And I definitely would say that that work affected my career-line, because all of my work in developmental has taken the approach of how does the structure of society affect parental patterns or family relationships and how that in turn impacts on the child. And that's exactly what that book was and it was consistent with what I had done in my earlier work in sociology, in that I was looking at social structure as it affects individual behavior. But the Swanson approach was—I mean, the research I'm doing right now is exactly of that sort.

Tardif: Now if I remember correctly in this paper on the “History of Developmental Psychology, an Autobiography,” you talk about not wanting to get involved in this project at all. Is this a project that you just—

Hoffman: No, that was a different project. The second year I was in graduate school I got a job at the Institute for Social Research in the Survey Research Center branch and in industrial sociology, and I was on a project there. I was the only female and, although I didn't experience prejudice exactly, it was sort of considered odd. Industrial sociology was considered a male field. Some of the research was on miners and miners did not even want women in mines. It was considered bad luck. About that time Ron Lippitt had received a grant to do a study of the influence of family interaction patterns on children's peer group relationships at school. There was a woman who was going to be the study director on the project who left Ann Arbor at that point. He needed somebody to work on this project with him, and he thought that since it was a study of family life and he was male, a female person should be working with him to study family life. By this time I had had an awful lot of research experience for somebody, you know, in their second year of grad school and had a good reputation for that. And so he wanted me to join him on the project. And at that time I was very reluctant, because I guess I felt that family sociology wasn't as serious or sophisticated as industrial sociology (industrial sociology was what would be called organizational sociology now), but Lippitt's study was just starting out and if I went to work on this project with Ron, it would be a study where I could have input. I was given leave to put in my own measures. I could do my doctoral dissertation on the project and I joined that project with the agreement that when the project was done I could go back to my previous unit. But I did not, because by the time the project was done I was in a totally different place, and it was that project that really turned me into a developmental psychologist. And I did complete my degree—my Ph.D. in sociology, but—

Tardif: And you did add things to that project to do your dissertation?

Hoffman: Well I threw in a variable on mother's employment. It was a study in which we had data very much like what I'm doing now; we had interviews with mothers, data collected from the children at school, peer group ratings, teacher ratings, and mailed questionnaires, as well as personal interviews. And to give myself a unit that would be separate from the others, I decided to add some questions about mothers' employment status to see how maternal employment affected the family structure and in turn the child, and that's what I did my doctoral dissertation on. And that really—but, in addition, I had enormous control on this project, as I say, Ron is probably most famous for his work studying advantages of democratic structures over authoritarian structures. And he was very democratic and he gave me amazing autonomy and trust. And I really had a lot to do with shaping that study and analyzing it and writing it up. And in the course of it I did a lot of analysis that had nothing to do with maternal employment, on things like—I had a paper on the father's role in the family and the effects on the child. That was not a bad paper. I had a paper on different discipline patterns and child outcomes. And Ron was asked to do a paper for the Paul Mussen handbook on research methods, and I think it was called *Research Methods in Studies of the Family* [Hoffman, L. W., & Lippitt, R. (1960). The measurement of family life variables. *Handbook of research methods in child development*. New York: Wiley, 945-1013]. Ron asked me to do it with him and I played a very major role in it, and it came out with me as first author. So by the time I was on the project for a few years, I had a number of publications, all of them having to do with children. I sent a part of my doctoral dissertation in for publication to a journal, then called the *Journal of Marriage and Family Living*. Ivan Nye was the Editor and he sent me back a letter accepting it for publication and asked if I would like to present a paper on maternal employment at a conference in Ames, Iowa. It was the National Council on Family Relations. I did give a paper there, and at that time he asked if I would like to do a book with him on working mothers. There were not a lot of people working in the field at that time. By then I was really a psychologist and he thought it'd be nice if a sociologist and a psychologist did it together, and so we did that book and that was *The Employed Mother in America* [Nye, F. Ivan & Hoffman, Lois Wladis (1963). *The employed mother in America*. Chicago: Rand McNally]. Later, in the early 70s we did *Working Mothers together* [Hoffman L W & Nye F I. (1974). *Working mothers*. San Francisco: Jossey-Bass]. Though I have only seen Ivan Nye two or three times in my life, and only for very brief periods, we worked together by mail and phone. It was a rewarding and stimulating experience that I enjoyed very much.

Tardif: When did you get your Ph.D.?

Hoffman: I got my Ph.D. in 1958. By then I got involved in other kinds of projects too. At that time Fred Wyatt was head of the clinical program here and he gave me a position as a research consultant to clinical psychology. He was very interested in women's motivations for having babies and I was very interested in motivations for having children, because Marty and I were having fertility problems and I was thinking about it a lot. And so we met. We used to meet weekly and we shared our ideas about it, and then I wrote it up as a research proposal with a strong theoretical statement and a design. There was interest in funding it, but at that point I learned I was going to have a baby and I wanted to quit work.

Tardif: This was in 1960?

Hoffman: That was in 1960—a little earlier, but 1960. And so we published the paper and it became very important for me later, because at a later point there was a lot of interest in studying motivations for fertility. Jim [James] Fawcett and Sid Newman were active in NICHD, which had become very interested in studying the psychology of fertility. It was heavily motivated—

Tardif: And what time was this?

Hoffman: Well, that would have been about '70, 1970. And Fred Wyatt and I had one of the few published papers that were dealing with that topic. Jim Fawcett was doing a book on the *Psychology of Fertility Behavior*, and he asked if I would do a chapter on the value of children to parents. I did that chapter and it subsequently led to a very large international research project [the classic publication (the first of many): Hoffman L.W., Hoffman M.L., 1973, "The value of children to parents", in Fawcett J.T. (ed.), *Psychological Perspectives on Population*, New York, Basic Books, pp. 19-76].

Tardif: So, the next two questions I want to ask you; one is somehow you went from almost not going to college or being—

Hoffman: I was going to college but—

Tardif: —but kind of scrambling at the last minute and ending up at some college because their dormitory wasn't full—because they didn't have a dormitory, to getting into a wonderful undergraduate situation. Then, by accident, falling into a wonderful graduate environment that you initially really didn't want to do, and got all kinds of publications, and then you decided to—

Hoffman: To quit.

Tardif: —between 1960 and 1970, and you didn't quit, but what happened? How did you negotiate that?

Hoffman: Actually I had—what happened is that I planned to quit my job and one of the things—I was surrounded by males, at least at that point, I was surrounded by males who were more feminist than I was in that day.

Tardif: So this was in 1960?

Hoffman: So that would be in 1960, and my husband was not enthusiastic about me quitting my job. He believed in my working. He believed that an intelligent woman should actualize her potential and should contribute intellectually to the world. Even Ron was not eager for me to quit. He said, "Well, I could send a research assistant to the house," and so forth. I was supposed to be—I had been approved as a program director at ISR at that time and was supposed to move into that position, which I was very young for. Ron wanted to make it as easy as possible for me so I could do both things. The third man was Orville Brim, Jr. [Orville Gilbert] "Bert" Brim was at that time President of Russell Sage. This is

probably interesting for this tape because it gets into a direct connection to SRCD. Bert Brim approached me with a plan. He wanted to have the Russell Sage Foundation finance a book—a couple volumes, maybe a series—in which there would be articles that would summarize the up-to-date research literature around topics that would be meaningful to people who were closer to the application of scientific knowledge. And he had the idea of a set of chapters that would summarize research around a particular topic. They would be written by someone who was really knowledgeable in the field, but they would be organized around topics that would be useful. Now this—and I can't honestly remember where I changed the idea originally and where he did, but certainly we were both very involved at that time in communicating research to people who needed to use it in their everyday work, but without stripping it of its scientific validity. He wanted me to edit this set of volumes. Once again I was very reluctant and said, "No," the first time. But we talked about it at great length. I talked about it with Marty and we finally—I finally did accept it with the arrangement that Marty and I would be co-editors. And it was going to be something I could do entirely out of the house, so that I would be able to stay home with my baby. It was not to start immediately; the heavy part of the work would be a little later and Marty was going to fill in for me and cover for me to enable me to be with my baby. So for the next six years my major job was editing the first two volumes of the *Review of Child Development Research* [Hoffman, L.W. & Hoffman, M.L. (Eds., 1966) *Review of Child Development Research, Volume 1*. New York: Russell Sage Foundation and Hoffman, L.W. & Hoffman, M.L. (Eds., 1966) *Review of Child Development Research, Volume 2*. New York: Russell Sage Foundation].

Tardif: From 1960 to '66 or—

Hoffman: I should check it out, but it was something like that. Anyway I worked only at home. I didn't work elsewhere. And it was set up as under the auspices of SRCD, so I was in a way employed by SRCD during that period, and the series was eventually turned over to SRCD from Russell Sage.

Tardif: At that point you've had no position at the University of Michigan?

Hoffman: Yeah. Actually until 1967. I did not work outside the home from 1960 to 1967 and during that time my major job was co-editing the *Review of Child Development Research*. We did not actually—I think I didn't actually go on the payroll until 1962, but we did a lot of planning between '60 and '62. So I didn't—I stayed home. It was 1967 when the second volume came out. At that time I was absolutely a child developmental researcher and developmental psychologist.

Tardif: No longer an industrial sociologist?

Hoffman: I was definitely a developmental psychologist by then. It had been a marvelous job really, and again something I very reluctantly did that just turned out to be marvelous, because I was totally up-to-date on the research. One of the things that used to happen when women would take off when the kids were little is they would go to go back to work and be totally obsolete. I, on the other hand, was not at all obsolete. I was as up-to-date as anybody in the world because I had been editing these books and working very closely with the authors. One of the books won an award. They were both very successful, and so my name was known. And in 1967 I got a call from the psych department here [at the University of Michigan] asking if I would be willing to teach the child psychology course. Lorraine Nadelman had started the course here before me and she had so many students she couldn't deal with them, so the department divided them up into sections.

Tardif: When was child psychology first taught here?

Hoffman: Well, I'm not sure when they first started teaching it, but it hadn't been taught for much before that really. I think she started the course in '65.

Tardif: So before that there had been no—

Hoffman: There was no child psychology course.

Tardif: And then there was no—

Hoffman: The child psychology area was set up here around that time, I think in 1966, but Lorraine taught the course before the area was established. They offered me a lecturer position, which, I assumed, she also held. It was the position that most mothers who were also teaching held. You were paid virtually by the course, you were not eligible for TIAA, you didn't get any sabbatical, and you didn't get benefits. I was perfectly delighted to do it.

Tardif: This was after you were already out with your Ph.D. for twelve years?

Hoffman: Well, almost. By the time I took this position as lecturer I had three books and many journal articles, and at the time it seemed just great. I used to give my children breakfast and see them off to school. They went to school across the street in the park [Burns Park], I mean the school was there. I'd wave them off to school, jump on my bike, go teach a course, bike back, give the kids lunch—they didn't have lunch at schools in those days—see them off in the afternoon, bike back, and be there when they came home. Looking back at it now, it was exploitation, but I didn't know it then. Okay. Do you want to go back to your questions to see what we missed?

Tardif: Well, I think you've sort of said what your primary interests in child development at the beginning of your career were, that was more the social systems of family influences on the job.

Hoffman: I think at that point all the threads of my career in research were laid out. That is to say, I have continued to do many reviews and research projects on the effects of maternal employment on the child. All of my work has dealt with social setting context variables as they affect family interaction patterns and the child. And then this early work I did on motivations for parenthood has become another theme where I have studied the psychological satisfaction parents get from children. I have done some articles on how the needs children satisfy for parents affect their child rearing styles. So basically, by that time I think—I mean all the major themes. I suppose the only other one would be that in about 1970 I wrote a paper on early childhood experiences and how they affect women's achievement motivation, and that paper also affected a lot of my subsequent work.

Tardif: You talked a little bit about the continuities and you sort of talked about the shifts, but once you decided to be a child psychologist what events do you think were responsible for any shifts in your research focus?

Hoffman: Well, certainly I think this Value of Children project, which was the research that preoccupied me for a very long time, and was affected by several things. I think one of the big ones was that NIH was very interested in population issues. There was a lot of concern about population problems, particularly in Asia and in developing countries. And I was contacted by James Fawcett at the East-West Center to join him on a project that would, in effect, turn this article I had done on the value of children to parents into a research project and it would be a cross-national study. And we did do that. There was a national study in the United States, which I was principal investigator of, and then I was one of the principal investigators on the cross-national project, which was eventually carried out in eight countries with national samples in most of them. And that was certainly on a very large scale, very exciting project, which took me to many places. And now you see I am involved in one project that's underway, which is very much like the first one I did with Lippitt, and about to start another project in China on the value of children to parents. At least I hope so. I'm working on the proposal now. So in some ways I don't think my research has changed that much. I think the field has changed a lot. I think the field has moved more toward the kind of research that I do than that I've moved. I think that now studying social context of the effects of family is very in. It wasn't then.

Tardif: So you see your themes as being continuous and your particular focus at the moment being shifted only because funding agencies or the field in general is more interested in that at the time?

Hoffman: Yeah. As you know, I was chair of the developmental area here for several years, until about four years ago. And during the time that I was chair I couldn't go after research grants because the administrative responsibilities were so time consuming. But as soon as I was done, I reapplied for some money to—actually I had done a review of the literature on the effects of maternal employment on the child and I was very impressed with the fact that, although there were some rather clear theories about how maternal employment might affect family life and child outcomes, there really had not been very much data that had actually tested the basic theories. And so I really wanted to do that and wrote up a proposal for that. But basically, my themes and interests haven't changed that much. I feel that when you're at a place like Michigan you keep very up-to-date. Teaching keeps you up-to-date, and so I've incorporated the other research into my thinking and work.

Tardif: I'm interested as well in your reflections on both the strengths and the weaknesses of your research and theoretical contributions. In this other autobiographical paper you paint this horrifying story of trying to analyze data with some sort of a sorter that goes through and counts on one variable and then you have to—what? Do you think that influenced—and certainly it had to influence the types of analyses you could do and how many you could do, but what about the theoretical contributions?

Hoffman: Well, on the whole and ideally, I think that we're in a much better position now to do really good studies and test very good theories. There's no question that the most major change in doing research, I think, is the computer. The kinds of analyses that we can conduct now, the efficiency of it, the kinds of statistical procedures we have now, enable us to test much more complex theories. I do sometimes think that because of the love affair that, particularly, young researchers have with computers and statistics, that sometimes there is not enough sitting back and thinking about things theoretically. I think that there has been a move towards a kind of empiricism in the field that has overtaken some of the actual thinking involved, but potentially it's just enormously advantageous. If one could keep one's head and keep things in control, I think that basically statistics and computer work will enable us to do much more effective testing of theories.

Tardif: The next set of questions are about which manuscripts, either published or unpublished, best represent your thinking about child development *per se*?

Hoffman: Well, I think I probably have the most affection for my reviews of the literature or theoretical papers, because very often single studies are very limited because of the fragility of the measures and the limitations of the population. Now I think that the most creative thing we do is to look over research and try to pull them together and make a theoretical story. So I still like some of my early papers. I have a paper in 1972, "Early Childhood Experiences and Women's Achievement Motives," [Hoffman, L. W. (1972), Early Childhood Experiences and Women's Achievement Motives. *Journal of Social Issues*, 28: 129-155. doi: 10.1111/j.1540-4560.1972.tb00022.x] which I think is a nice kind of theoretical statement, tying together how early childhood experiences can affect later motivations. I think some people think of early events as affecting later motivations in some magical way, in some Freudian mode where something happened that has a later effect, but they lose sight of the process that is involved; how the early experiences affect the next experiences and then the next, so that sometimes later motivations are affected, not in some direct way from time one to the later time, but through the intervening events that go on. I guess I have another paper, which I like a lot, which was in the *American Psychologist*, "Changes in Family Roles, Socialization and Sex Differences," [Hoffman, Lois W. (1977). Changes in Family Roles, Socialization and Sex Differences. *American Psychologist*, Vol 32(8), 644-657] in 1977. I liked that because I think it's mainly a theoretical paper, but it does a nice tie between seeing how macro-social experiences can affect child rearing patterns and child outcomes. I mean in the paper I try to trace how changes in technology like birth control and improvements in longevity and differences in technology, which change the kinds of jobs there are out there, have changed the social situation. And one of the big changes is the fact that women bear

fewer children and spend a larger portion of their life in the work place than they do in child rearing and this can affect child-rearing patterns. I think that—and this again ties back to Miller and Swanson—I think that child-rearing patterns over many years tend to adapt to what the adult role expectations are, that we rear our sons and our daughters, we encourage those traits and qualities and interact with them in ways that are consistent with the adult roles that we think they will be filling. And as these adult roles change, child rearing patterns change. And I think that a lot of the big social changes have made the adult roles of men and women less different from one another than they used to be; and that that has significance for eventually changing child rearing.

Tardif: So of the two papers that you talk about as being your favorites, I find it interesting that one of them reflects your sociological background in terms of looking at these larger variables, societal variables, and the other reflects your strong developmental psychology background.

Hoffman: That's true, I think. I do like many of my reviews of the maternal employment literature. I liked one of the early ones I did, I guess '74, which was in *Developmental Psychology*, and some of the more recent ones too. I also have a recent paper in *Psych Bulletin* [Hoffman, L.W. (1991). The influence of the family environment on personality: Accounting for sibling differences. *Psychological Bulletin*, 110(2), 187-203. DOI: 10.1037/0033-2909.110.2.187]. It's partly an answering back to the present behavioral geneticist position, which is that the family environment does not have very much influence on kids and that siblings are not very similar. And it also, I think, tries to describe the complex interaction that goes into the socialization process, and I like that because I think one of the things that people miss a lot, inside the field and outside, is that what a child turns out to be is a result of many, many, many different inputs, and the complex interactions that are involved in socialization, I think, are often obscured. And I think to some extent our research fails, in that if you look and show this variable affects this variable but you haven't accounted for a lot of the variance, people say, Well, what good could it be if it doesn't account for a lot of variance? To account for a lot of the variance, you often have to move to a level of explanation that is very close to the end product, because if you're trying to examine a dynamic theory where you're trying to see what led to this end product, you're really dealing with a bunch of inputs and different paths. You have this influence which moves a developing person in a certain way and then another influence that can change it. Or a given influence can affect one person one way and another person a different way. The whole complex interaction of the socialization process is an aspect of research that I think we rarely do justice to. And I'm not computer expert enough to do that, but I do feel that the computer sophistication and statistics we have now could be used more effectively toward trying to get at that kind of complex interaction that is involved in socialization. And I like that paper because it at least tries to point out that.

Tardif: What year was that published in?

Hoffman: It was fairly recent; I think it was '91, '92.

Tardif: You're talking mostly about your review papers. If you were to pick one of your more empirical studies, interviews, or surveys, which one would you see as being most significant?

Hoffman: Actually, I think I like my current one the best. It's not done yet, but I do like it because I think I've had—I've been able to give it more attention and sort of shape it and think about it longer, and the analysis will be a long time coming. I mean we're starting to come out with things, but it's a very complex study. But I do like it very much and I think that—

Tardif: And the current one, not the one you're about to start in China, but this one—

Hoffman: Well, it's originally a study that I set out to do to look at how maternal employment affects child outcomes. And I'm trying to test a number of different theories. I think there are certain basic theories that are involved in how maternal employment is connected to child outcomes. One of them is kind of a mom's morale theory, that the mother's working or not working affects her psychological

state in some way, which in turn has outcomes for the child. And I have in this study tried to—I'm using a depression measure and a stress measure and outcome measures, and I have a very good heterogeneous population representing different ethnicities, different social classes, and I'm really trying to look at how some of these processes work in different groups. We have single moms and married moms and kind of look at differences there, and a nice spread of measures covering things. Another theory is looking at the father's role so that we're trying to see if the—does maternal employment affect the father's role in the family, and how does that affect child outcomes? This tape's getting long and we're getting tired, but it has many different steps involved examining how the father's role affects the child. But we know certain things, for example, like most of the data do show, and our data certainly shows, that if there's a crude statement about the maternal employment effects it is a positive one on daughters, particularly in achievement areas. And that can come about in different ways, and so we're trying to cut them apart and see how they work. And so I like it because it's quite theoretically driven and we're trying to measure all the steps in the path. The study is based on an article in 1989, which was a review article, and in it I tried to lay out some of the different pathways.

Tardif: What about the flip side of that? Which contribution do you consider to be the most wrong-headed of all of the things that you've done?

Hoffman: I really don't feel that way. I mean I think that if I had—I don't really think I have done many wrong-headed things, because I've never been overly ambitious. I've never undertaken a research project I didn't really want to do. I have been awfully lucky in my jobs. I mean I have been here at the University of Michigan for a very long time. I have been tenured faculty for quite a while now, and my pay comes in whether I have a research project or not. And I've only really undertaken research when I really had something that I wanted to do, and I think that's made it better for me anyway.

I would say this: I think that I have found my very large scale study of the value of children to parents a very difficult one because of the amount of time that went into gathering those data. There were many, many people involved, so the project was probably worthwhile, but different people used it for different things. For me, I'm not really sure that the intellectual yield from it was worth the effort, because it tried to do too many things for too many people. And because I think it's very difficult to do hypothesis testing with different cultural groups of this sort. I mean, say you find a difference between mothers in Indonesia and the United States regarding their attitudes toward children. Well, what is the reason behind this? Is it because the mothers in Indonesia have infants at a much younger age than they do in the United States? Is it because they're less educated? How can you control for that? How are you supposed to control for the education of an Indonesian woman compared to the United States?

Tardif: And there's also no way in which you could go back and get additional follow-up.

Hoffman: Well, but see even if you could, it's—well, for example, none of the Indonesian women in this huge sample had any college education. And is it the relative educational position of the woman or the absolute education that counts? What is the variable that carries an effect? Is it something about the culture, economics, or family structure? I just really found it very difficult to really zero in on what was the real variable that carried these relationships.

Tardif: And yet you are going back to China to do—

Hoffman: Well, this is going to be just China and I'm going to articulate more clearly the separate components of the study. This study will cover what the investigators in China are most interested in, and I'm also going to make a little section that will be particularly of interest to me, and it will be within one country. It's not a cross-national comparison. I did some nice things with the U.S. data I think, but I think it was when I got involved in the cross-national analysis that I felt uneasy about identifying the key variable.

Tardif: What about funding? You talked briefly about NIH, and their interest in population as driving this sort of huge project as opposed to, say, maybe only a U.S. project. Have you had any role in shaping research funding policy or implementation? What types of things have you had to do to secure support for your own work?

Hoffman: For my own work I have written a few proposals and they have been funded.

Tardif: And you pretty much know ahead of time which agencies? It's not—

Hoffman: I mean you call around and find out. At one point I was very motivated for a project that I would have liked to have seen funded and I didn't quite find the right agency. This was actually for the area, not for me personally, but I wanted to start a longitudinal study going here for the developmental area some years ago and had written a proposal which I was trying to get the funding for. It was very difficult to get a longitudinal study started. I wanted it really as a resource for the area so that when developmental students came in they could hook into it. And I didn't want it to be that specifically hypothesis driven, but sort of to get a framework of families set up in certain ways to provide a basis for subsequent developmental research. It's very hard to do research in social development, particularly for the graduate students who come in and are going to be here for only a certain number of years. They can hook on to somebody else's project that's ongoing or they can do their own, but in both cases they're rather limited to what the time period allows. Starting a longitudinal project is something that is hard to sell. It's big bucks and long-time commitment. Michigan was very helpful to me, but I never really did get that one going.

My involvement at national policy level was not much. I was on a review committee for a long time, but it was dealing with training grants; it was not funding research projects. I've done reviews of proposals and so forth, but I don't think I shaped policy a lot. I have indirectly had an effect maybe in the sense that I was president of Division Seven for a while and Division Nine. Nine is the Society for the Psychological Study of Social Issues.

Tardif: I think you've pretty much gone over the institutions that you've worked for and the capacities in which you've worked for them.

Hoffman: Yeah, I've been a Michigan person basically all—

Tardif: Except for some pretty important early experiences at Purdue and—

Hoffman: Well, I've had associations with other organizations for brief periods, but I've basically been employed by the University of Michigan.

Tardif: One of the questions here is to describe the changes in the unit that occurred during your time. And one of the main things you talked about is that there was no area of developmental psychology, and women were not hired as professors, or at least not women with children.

Hoffman: Yeah.

Tardif: But another question I think might be interesting is the role that the unit, the department and the University in general, played in the history of child development research?

Hoffman: Well, developmental psychology or child psychology are really quite new. Not many schools offered training in them in the 1960s. So much so that here was the huge University of Michigan, and it was huge even back in the '60s, but there was no developmental psychology program. And actually most of the people even younger than I am who are in social development were never trained in child psychology. Most of us were trained in social psychology. Although I have said my degree was in sociology, back in the old days Michigan had a social psychology unit that straddled both sociology and

psychology. The faculty included people like Ted Newcomb, Dan Katz, and Ed Swanson. My training was the same as being trained in social psychology. Other developmental psychologists also obtained Ph.D.s in social psychology: Marty Hoffman, Eleanor Maccoby. Jacque Eccles was also originally social psychology. Most of—

Tardif: What role do you see the institution having as fostering or hindering the establishment and growth? I mean, now you have one of the strongest developmental programs.

Hoffman: Well, I think that this unit has grown into its present size largely because it was in a very solid institution which was financially solvent, which managed to escape some of the—I think we had very good presidents at the University of Michigan and I think we've escaped the bad times some schools have gone through. This has been a growing and very solid and mature department all along, and we've gotten a lot of support for developmental's growth. I mean, at one time John Hagen and Marty Hoffman were the only people in the unit, and then I think Harold [Stevenson] was the next hired. But in any case, we'd gotten a lot of freedom to hire new people. I think we made very good choices. The students have been excellent, and when you are a big school and you have lots of good graduate students that are brought there, they send back their undergraduate students to get their Ph.D.s here. The school itself was so large that even when there were few developmental faculty, a lot of the people who were not in developmental worked with students. I mean somebody like Libby Douvan, for example, and Joe Adelson study adolescence. They're not in developmental psychology, but they studied kids. And there have been others. Joe Veroff has done work with kids. The area was able to build up with the help of faculty in other units and then hire additional people, and is now certainly quite strong. I think we've had good leadership over the years. And Michigan attracts good people. I think Marty and John and I made some good hires.

Tardif: How about when you're touching briefly on students? What about teaching? How do your experiences as teacher, as a trainer of future researchers, how have they impacted your own research?

Hoffman: I do think that being in a large, active, stimulating environment is absolutely for me very essential for keeping yourself going and alive and up-to-date. I think that being here it's impossible to fall behind the times. I mean, you have to teach, keep your research always up, and always know the latest things. The students that you teach are taking other courses, they're very sharp, they've had a lot of psychology, and you have colleagues all around you. You hear new research at brown bags, colloquia, and informal presentations all the time and it gets you going.

Tardif: Do you feel any sort of tension between teaching and research in child development?

Hoffman: Well, on the one hand, teaching keeps you alive and stimulated, but teaching is also an enormous time-consuming and demanding task. I do almost all my writing in the summer time or vacation time. I do most of my writing in the Finger Lakes and it's just right for me. I need big pieces of time to get a lot of work done. If you want to teach well you have to go out of town to get your research and writing done. Graduate students similarly, on the one hand graduate students are wonderfully helpful, wonderful to work with, and always, again, keep you very up to date. It is hard to keep up on all the literature in the field, but when you have students doing dissertations, or writing prelims, or presenting in class you really feel very up to date more broadly. And statistically and computer-wise, I think all the young students are better than any of us older people are. We just haven't kept up with the latest computer possibilities and statistics, and you get updated through your students. On the other hand, it takes a lot of time. I mean, I'm chairing a lot of graduate student committees; I have five dissertations I'm chairing at this time who are finishing this term. Do we still have things left?

Tardif: Yeah.

Hoffman: Let's go over them very fast.

Tardif: Applied child development research. You've talked about your initial experience editing, pursuant to the books—

Hoffman: Oh, there are some changes in the field that I mentioned, things I did earlier that have now become very popular. I don't think I made them popular; I just think that they have become popular. Take the importance of context. If there's a person who's brought that forward, I think Urie Bronfenbrenner has been a powerful voice in that direction, not I. He has definitely got those of us that study social development much more attuned to the social cultural context. But I also think that the relationship between applied or policy-building research has changed over the years. Earlier in my career, if you were doing things that were very applied, you practically hid it, and now the idea of research having at least an indirect relevance for policy is extremely important and encouraged.

Tardif: You said you were on SRCD payroll. When did you actually join SRCD?

Hoffman: Oh, did it ask you about the first meeting you went to?

Tardif: That's part of this.

Hoffman: Yeah, I remember that very clearly.

Tardif: So you joined in—

Hoffman: I don't remember when I joined exactly, but I do remember the first time I ever went to a meeting. It must have been in 19—I think it has to be an odd year, doesn't it, so it must have been 1959.

Tardif: So, while you were still a sociologist?

Hoffman: Well, no—I don't know.

Tardif: No, No. I'm sorry. I'm getting decades mixed up here.

Hoffman: No, I don't know what I was then. I was sort of in between.

Tardif: You just got your Ph.D. in sociology.

Hoffman: Yeah, but see I got it in sociology, but I really wasn't in sociology on my job. I was working at that time with Ron Lippitt at Group Dynamics, which was the Kurt-Lewin-inspired division of ISR.

Tardif: On the job—

Hoffman: I mean, by then I finished it in sociology just because it was easier. I really didn't think of myself as a sociologist. Anyway, I went to the SRCD meeting with my husband and the meeting was in Bethesda. And what was amazingly different was we all stayed in this one—there was this one little hotel that housed the whole group, and it was such an intimate group, I just loved it. And for me it was like meeting all kinds of great famous people; I mean Urie Bronfenbrenner was there. Jerry Kagan and Howard Moss were just finishing their book, and I remember we sat and talked about that sex differences finding in the—where the boys showed continuity over time in aggressiveness but girls did not, and the girls showed continuity in dependency but the boys did not, and so forth. That was the finding they had and we talked about possible interpretations of it. And Alberta Siegel was there. She was—her husband was still alive at that time. The meeting was just such a contrast to the enormous thing it is now.

Tardif: There are questions here on your history or your participation in the scientific meetings and publications. You talked about editing a series you've been presenting at the conferences since 1959, or after—

Hoffman: No, I didn't present then. I have given various papers and been on symposiums at SRCD. I've actually probably done more major presentations at APA through Division Seven.

Tardif: Have you participated in SRCD governance at all?

Hoffman: Well, I was on the Program Committee when it was in Denver. I was on the Program Committee from about 1973 until about 1979. That would be the closest thing related to being governance. Other than that I haven't played a very major role.

Tardif: Are there any major problems or issues that confronted you during that time in that role or—

Hoffman: No, it was a fun role. I mean I really liked it very much. I liked being part of the process.

Tardif: Besides size, what do you see as the most important changes to occur in SRCD in its activities?

Hoffman: Well, I think it's probably gotten more applied and more policy interested. It has also, I think, tried with some success to broaden itself from just psychology; that is, there was always an interest in trying to get medical people and people in other fields involved in it, and I think they had some more success in that over these years. They have done a lot more in the policy area; for example, these policy fellowships in Washington and things like that. I think it has tried much more to become an actual influence on policy development and to encourage training in applied areas and influence policy.

Tardif: A couple of questions on the field in general. I think you've talked a little bit about the history of the field. What about your hopes and fears perhaps for the future of the field?

Hoffman: Well, my hope would be that we would get our computers and stats well integrated with theories and with respect for new complexity of the way in which the environment operates. My fear is sometimes that we are becoming too enamored of the new toys and letting the statistical computer techniques dominate what research projects we actually investigate. I think that sometimes students come in with marvelous ideas and then get so caught up with techniques that they quit thinking and sort of, Well, I'll do a LISREL. And sometimes when you're on committees where somebody has done a LISREL you will see some very exciting relationship that has been revealed by it, but the student doesn't want to pick it up or be side-tracked by it. They basically stick very close to the model whether they demonstrate it or not. And I think that people think better than machines and we shouldn't lose sight of that.

Tardif: They want to know something about personal interest in your family, especially the ways in which they've had a bearing on your scientific interests and contributions or on your applied contributions. You've talked, actually, and sort of threaded that throughout. Are there any additional things you wanted to add?

Hoffman: No. I would just say certainly my long marriage to Marty Hoffman was extremely important and moving to me and my career direction; that is to say that he was always very, very committed to my career and encouraged me in every way. A lot of our students here marry other developmental psychologists and I think it's quite wonderful to have your husband be your colleague also, and that it's enormously beneficial. And I think having children and watching them grow has certainly stimulated my interest and delight and fascination with the socialization process. I mean that I really think, despite the fact that I fell into this career accidentally, by my analysis anyway, I think that the most

exciting question to pursue is how a human being develops and I do think having children for me made it all more exciting and sort of gave it a kind of special intimate sense of—we're probably done right?

Tardif: Yeah, unless you want to take a quick look and see if there's anymore—