

Mary Dinsmore Salter Ainsworth

- Born 12/01/1913 in Glendale, OH; Died 3/21/1999
- Spouse - Leonard H. Ainsworth (divorced in 1960)
- B.A (1935), M.A. (1936), Ph.D. in Psychology (1939) - All from the University of Toronto

Major Employment:

- Tavistock Clinic in London, England - 1950-1954, Sr. Research Psychologist
- Johns Hopkins University - 1956-1975, Psychology
- University of Virginia - 1974-1984, Psychology

Major Areas of Work:

- Psychoanalysis: Attachment Theory, Strange Situation Testing Protocol Development

SRCD Affiliation:

- Governing Council Member (1973-1981); President (1977-1979)



SRCD Oral History Interview

Mary Ainsworth

Interviewed by Harold Stevenson
1998

Stevenson: I have been thinking about the time we were at the Center together and didn't realize that that was a time that you had done some of your major work.

Ainsworth: There's this big gap between leaving Africa and actually getting going on this research. My job at Hopkins was largely a clinical job, and I couldn't really get time to take research of this developmental sort that I wanted to and give two courses a semester; and work at clinical work in the hospital; and teach students how to do personality assessments; and it just wasn't possible. I had to shift over to developmental, which eventually became possible.

Stevenson: So really that year at the Center for Advanced Study in the Behavioral Sciences in 1967-68 was very important to you. That enabled you to bring all that material together.

Ainsworth: It really was, and I didn't realize it at the time that I never did communicate this to the Center. I really worked out the classification system for attachment when I was at the Center.

Stevenson: I look back and see how many things were going on at the Center during those years. Did you have much to do with David Hamburg when he was there?

Ainsworth: Yes.

Stevenson: How?

Ainsworth: As you know, John Bowlby came for a semester, the last semester of David's participation.

Stevenson: So did you arrange that?

Ainsworth: No, Bowlby did. I think he really arranged for me to go to the Center that year. At least, he nominated me. And David said, "Let's establish a weekly seminar over in the psychiatric department on mother/infant interaction, and this is going to be cross-disciplinary and cross-animal versus human and clinical versus research." It was a very cosmopolitan group including students of ex-students of Harry Harlow, Alberta Siegel, and Analiese Horner. I found that very, very stimulating, where the developmental group at the Center didn't have nearly the coherence of that group, and it was in the connection of there that I worked out things.

Stevenson: How did you interact with Bowlby then?

Ainsworth: Oh, we were old, old friends.

Stevenson: Right. But were you collaborating or was he doing his thing and you were doing your thing?

Ainsworth: I was doing my work and he was chairing. He was doing a seminar.

Stevenson: You had a whole bunch of psychiatrists there.

Ainsworth: David and his wife.

Stevenson: Yeah, it's so interesting. Well, of course, the interesting part was that it all comes together in this visit with Barbie Smuts and her relationship with Bowlby.

Ainsworth: I know, I know.

Stevenson: And her relationship to you.

Ainsworth: And with David.

Stevenson: And with David, that's right. And of course you know the Dean at Michigan was also involved.

Ainsworth: That's why I got interested in Barbara because of some of the work she was doing with baboons.

Stevenson: A lot of your story is already available about your relationship with Bowlby and how you interacted. What about Harlow? Did you have much to do with Harlow?

Ainsworth: You know this isn't really SRCD, although I'm enjoying it thoroughly. I did have a lot to do with Harlow in the sense that he belonged to this mother/infant interaction study group that met every two years in London. He attended all of these meetings and presented, and I was very interested in what he presented. He and Bowlby were influencing each other a lot.

Stevenson: Bowlby talked about that.

Ainsworth: And when Harlow devised this little situation, when infants reared by surrogate mothers/cross mothers were introduced to a strange situation, it really WAS a kind of strange situation! And if the surrogate was present, the baby would IMMEDIATELY run to the surrogate and cling, then look around, move away, and explore. This interested me to no end, of course, and I pointed it out to Harry that this already had been initiated by Blatz, but Harry had NO idea of that. He'd thought of it absolutely independently. I had really intended to do a strange situation sometime, to be sure of that. And then Harriet Rheingold, I know, was very impressed. One day, Harriet and I met at a meeting and I

said, "Oh, Harriet, you'd be interested in something I'm currently doing, um, the strange situation..." (Laugh) "You are too? I'm just starting mine."

Stevenson: Oh really?

Ainsworth: And we each had picked episodes of three minutes and met them all. All the similarity fell apart though. It was such a different situation.

Stevenson: So what was this about Blatz?

Ainsworth: Well, he was my mentor.

Stevenson: That's interesting because from the outside it looks like Blatz would be totally different from what you developed later.

Ainsworth: Yes, indeed it was. He got me going on paper/pencil tests and I did my dissertation on security of college students as obtained from the paper/pencil report. It was extremely interesting, and it certainly got me going on patterns. There were certain aspects of the paper/pencil report that I was very satisfied with and still am.

Stevenson: Did you know Molly Mason Jones, who is another of Blatz's students?

Ainsworth: Yes, I knew her.

Stevenson: She seems so different from you in terms of what she eventually did. It seems to me she continued pretty much what I thought Blatz was doing.

Ainsworth: Well, with Blatz I feel that it's an oral tradition largely that he exerted influence. So the people that were involved in the nursery school activities and the philosophy of how two, three, and four-year-olds should be handled, they derived one set of Blatzian beliefs and I picked up something a little vaguely similar, but in a different context. So, you know, it's like the blind man and the elephant.

Stevenson: Well he did different-I mean, didn't he do the study with the quintuplets?

Ainsworth: Yeah, uh-huh.

Stevenson: Did you have any idea that all this would just burst forth when you were there? Did you think, I mean, as you looked to the future, what were you thinking?

Ainsworth: When I was at the Center, I had just completed all the data collection and had done very little of the analyses. I knew it was going to be very interesting, but I didn't know how well it was going to turn out because there was almost no unpleasant surprise in the course of data analysis reduction. I think the one analysis in which there was a disappointment and didn't seem to mean anything, I discovered later was a matter of the coding system not being good enough and I just had some talented associates and students that devised the coding systems and steered away from sheer frequency counts.

Stevenson: Yeah. But to develop a paradigm which then is so widely used. I mean, you just wonder what--

Ainsworth: Oh, I must say I pinched myself. I thought that they were good, and I got very excited about it. But it did amaze me when it started to balloon to the extent it did.

Stevenson: Yeah, 'cause it did. You know the two major paradigms in the last thirty-five-years that had that influence, one is the Piaget conservation study and the other is the attachment work. I can't think of anything else that's had that kind of popularity.

Ainsworth: I attributed an awful lot of it to the fact that I had very good students and they-oh about half my students went on into clinical, which was fairly appropriate from my background. The ones that went into research, with one notable exception, did wonderful things. You know the notable exception.

Stevenson: Yeah, right. I know who the exception is. It's still a great mystery to me, absolutely incomprehensible.

Ainsworth: I talked to MK the other day. He phoned me pretty stressed that he's having an ax job done on him like the job done on me. And he was shocked that, as I felt, there were so many distortions, omissions, and outright mistakes in that thing that it gave a very false impression. Apparently the ax job that's being done on him is even more incomprehensible. I finally was persuaded not to do anything-not to reply.

Stevenson: Yeah.

Ainsworth: I did make a big protest to the editor and he said I could write a commentary, you know, two columns. And you can't say, "Look, I am an honest scientist."

Stevenson: Right, right.

Ainsworth: (Laugh) And, uh...

Stevenson: Well, I mean, everybody wants criticism. How do you progress if you don't have criticism?

Ainsworth: Uh-hum.

Stevenson: And the point is that the goal is constructive to criticize.

Ainsworth: Yeah.

Stevenson: Not destructive.

Ainsworth: And that's what's...

Stevenson: Yeah, and but I think that, I mean he, you know with a critic like that, it is--the goal is never accomplished of even being destructive because it's done so badly.

Ainsworth: Especially after Bowlby died and all the obituaries started to come out. I gave a lot of thought to the things I admired. I couldn't think of anything I didn't admire about Bowlby to tell the truth.

Stevenson: Did you?

Ainsworth: But one of the things I admired enormously was two things that are very interrelated: one was that in order to put his own stuff forward, he didn't find it necessary to tear anybody else's down.

Stevenson: Yeah.

Ainsworth: And the closest he came to that kind of thing was in one of his volumes, I think his second volume, in which he compared the linear course of development with fixations and regressions and

things that you start at the same place and you go forward and all you're interested in--all that you count by is do you get fixated in one stage of development or another? On the other hand, he sort of chose a Waddington model, which incidentally altered with the zygote (and then we go to the genes). But even then, prenatally, there is a constant interaction with the environment and you start at birth with--at a certain departure point, there's a path of development that can get deflected into another path by those international events and so on. And he described the Anna Freud thing, and then he said, "This is the view I have of it and I found it a useful way of looking at things, and maybe you will too."

Stevenson: Yes.

Ainsworth: And he got a lot of very harsh criticism from fellow psychoanalysts: including Spitz, of course Anna Freud, and a couple of other very well-known psychoanalysts. And he never. . . I think I know he was disappointed because he was really writing for clinicians, but he never responded in anger. He just went on to the next step of what he was developing and trusted that he would convince people later on.

Stevenson: Well, other than this one isolated incident, I mean, initiating something new, did you ever feel that the field put obstacles in your way?

Ainsworth: Oh, there were a lot of obstacles to start with, you know. Most of American academic psychology followed a kind of different paradigm theoretically, and I found it really very difficult to get a grant to start with that proceeded with naturalistic observation.

Stevenson: Really?

Ainsworth: And it was not easy to get grants. I worked at it very hard and finally did get enough to do that one study, but then publication wasn't easy either.

Stevenson: Really?

Ainsworth: I got a lot of turn downs.

Stevenson: Really?

Ainsworth: And I think one of the reasons that I published so much in chapters--in which I didn't realize would kind of fade away, whereas journal articles remain--was that I could publish more or less what I wanted without too much editorial interference.

Stevenson: A lot of people are saying that a journal like *Child Development* is full of uninteresting things, and yet, what you were saying--it may be difficult to publish new, innovative stuff.

Ainsworth: It is; it is. And it's very difficult to publish restrictive material even now, which is where you start. You know, the unconscious does wonderful things. I finally did get grants from foundations funding research in psychiatry and Phil Sapir was among them and arranged that. It's a very grudging letter of awarding funds and the budget that was provided, I thought, was really very inadequate for what I said I wanted to do. And so I went ahead and did it anyhow. Years later when I was reading that letter over again, I was being funded for a pilot study. I'd just absolutely blocked out that pilot study idea altogether and that's essentially what it was.

Stevenson: Was it that, you know, somebody like Phil Sapir ended up with, you know, a horrible relationship with the foundation. But yet, he seemed to play that role in a lot of people's lives--

Ainsworth: Uh-huh.

Stevenson: --being willing to risk. And I don't think--where's the Phil Sapir today? I mean, it's hard to find those who'll risk on people who have new ideas.

Ainsworth: The letter I treasured was from the Director of the Office of Child Development, who very happily awarded the grant. And this is really what it's all about, you know. And that was Ed Ziegler.

Stevenson: Yeah, but if you were to apply today it would be very difficult to get one.

Ainsworth: Well, I applied to NICHD. I've never had a grant since. And what I applied for was a grant to, that's what it was--it was really sort of a validation grant to assemble another sample, similar to the one I had assembled, using all the procedures that I had worked out in the first sample, which was POST HOC measures, you see. I'd collected my data, then I realized all the things I said I was going to do ahead of time were not very useful. And so it was all POST HOC measures and to use the same measures on a new sample--to see whether I got the same results--that made total sense to me, but they turned it down. I can remember that pink slip that said um, "Your contribution to theory has been very valuable. Your empirical work is worthless and you better start on a new idea." They were also very critical of the statistical measures I wanted to use. I wanted to use discriminant analysis and factor analysis and I'd discussed this with Bert Green, who is editor of *Psychometrika*, and I think I even discussed it with Al Chapanis, who is a great quantitative person, and they thought it was just great. It's a way of analyzing data that pertain to your sample. It doesn't mean it has a universal generality, but this is a worthwhile way of processing your data. And then I gave a talk, in which I did report on the pretty elaborate quantitative analyses with using multiple factor analyses at the University of Virginia. And the quant people there had never seen such a useful use of the multiple factor analysis. And then comes this pink slip.

Stevenson: But what's interesting is that you persisted. Some people would have capitulated.

Ainsworth: My self-esteem always just plummeted.

Stevenson: But then it went back up. How do you account for that? I mean I see so many big differences among people. Some people get that kind of thing that is that's all they do is throw up their hands.

Ainsworth: Well, I thought I was on the right track (laugh)! And after the plummeting of the self-esteem, in which I questioned myself and my methods and my theories, I thought, "NO! This is on the right track!" So I went ahead with it. But again, I think that if it hadn't been for students being so enthusiastic, I might have gotten discouraged.

Stevenson: What did you think when we came to your conversation hour at SRCD? I mean, nobody could get in the room; it was so packed with all the people out in the hall. What did you think then?

Ainsworth: Well, I'd gotten used to it by then because attachment seems to have an enormous appeal, both at SRCD meetings and at infancy meetings, which always happen to be assigned to a room that's too small.

Stevenson: Yeah. Well, it's so unpredictable--

Ainsworth: Um-hm.

Stevenson: --that, you know, they go there and not this one. It just seemed to me that it was such an enthusiastic group of people.

Ainsworth: Well, I mean it definitely was very heart-warming to say the least, and I certainly can feel very gratified about my career.

Stevenson: Yeah.

Ainsworth: And with lots of people that recognition is much more delayed than with me, although it was pretty delayed.

Stevenson: Um, so, I know you said that there were some of these obstacles, but what other kinds of things do you think about the field that you think holds difficulty? I mean, some people didn't understand what you wanted to do. And all those terrible pink sheets, I mean--

Ainsworth: Well, I had a long and persistent fight with Jack Gewirtz, which you perhaps know.

Stevenson: There is resistance to that much change in that part of the field, 'cause if anything major is required to change...

Ainsworth: Well, it certainly does and the attachment theory and attachment research is exceedingly complex. That's the thing that sort of gives you the cause to start with because what you're really trying to do is to understand personality development.

Stevenson: Yeah.

Ainsworth: And that is a very big issue. I don't think I ever would have contemplated anything that big had it not been John Bowlby.

Stevenson: Why? I mean, why did he--

Ainsworth: Well, our minds went along a similar track. He certainly was the leader in breaking the ground, not much in research.

Stevenson: Yeah, 'cause it seems to me you really bolstered him.

Ainsworth: Yes and it worked extremely well as a combination and a partnership. I'd always thought of him as a theoretician, and I was the researcher, but actually, I contributed quite a lot to that theory.

Stevenson: Yeah, right.

Ainsworth: And there might be a period of time that we hadn't been comparing notes. Then we'd get together and, let's say he'd publish something, and I'd realized that I had been thinking along the same lines or I would send a paper to him and he would never have any difficulty in absorbing it into his system, you know. But coming to SRCD, when I arrived in Baltimore I had no familiarity with American academic science, absolutely none.

Stevenson: Where'd you get your BA? In Toronto too?

Ainsworth: Yeah. Here I came with ideas I thought were interesting, but certainly there was nobody in the--I had two people in the department that encouraged me, but their fields were different. One was in psycho-linguistics and the other in perception, but they both were very encouraging. I needed people that were working with child development research though, and it was Nancy Bayley that encouraged me to come to the first SRCD meeting. Had I realized there had been one before in Toronto when they brought--the whole Society met in Toronto and Blatz hogged the entire program with his stuff on quints.

Stevenson: So when was that, like 1940?

Ainsworth: 1936-37.

Stevenson: Really? Oh that was really early.

Ainsworth: Yeah, and he--

Stevenson: And so that was when Nancy Bayley was in NIMH?

Ainsworth: Yes!

Stevenson: They had the meeting in Baltimore? I mean Bethesda?

Ainsworth: That's right! That was the first time I came and it happened to be the meeting--sort of the union of the founders and Blatz was there.

Stevenson: He was?

Ainsworth: But anyhow, I counted on SRCD as being sort of a support group. Through the meetings and approaching people that had relevant research going or interests, I found it was very helpful and very supportive.

Stevenson: Oh, a lot of people say SRCD--once you make friends, it goes on--

Ainsworth: Yes!

Stevenson: --through your whole career.

Ainsworth: And it has! And when I was asked to run for Governing Council and eventually for president, I gave a long thought about it 'cause I knew it was going to take a lot of work. I felt I owed it to that Society that it had been--on the whole except for the Bowlby connection--the most supportive and useful group I had ever been associated with. I think I would have felt very alone without that membership.

Stevenson: Well what do you think it's like now? I mean it's so big and--

Ainsworth: Well, of course, now there are so many friends (laughing) that I--

Stevenson: Oh the bigger it is the more friends, huh?

Ainsworth: But I agree with you in that it has lost something of the intimacy it used to have. The meeting in Bethesda didn't have parallel sessions.

Stevenson: Yeah.

Ainsworth: And everybody went to every meeting.

Stevenson: Yeah.

Ainsworth: And that in itself was wonderful because you get stimulated by listening to reports from a variety of different approaches and disciplines and segments. And I used to try to do that. I used to teach cognitive development as well as personality development. And now when I go to a meeting, I feel kind of obliged to go to the attachment things because the people that are involved in it are people that count on me for moral support at any rate. And that means you scarcely have a chance to go to anything else.

Stevenson: Yeah. What about Division 7 versus SRCD?

Ainsworth: Never meant as much to me. Although I did attend Division 7 things a lot, and now I really don't go to APA because they have so many divisions that the developmental offerings are very sparse.

Stevenson: Yeah.

Ainsworth: And you feel lost and I just can't feel any affinity for APA anymore.

Stevenson: Yeah. Well, what's SRCD going to do to avoid the kinds of problems that APA has? Do you think it should have more frequent meetings?

Ainsworth: Well, I think there have been a number of things that have been spin-offs, like infancy that is somewhat smaller. And infancy is such a small proportion of the developmental span that naturally we've got a lot of people that have smaller meetings, however, even those are not really small meetings. And I think that there are other, more specialized societies that can fill in the gap and whether SRCD would profit from having divisions and in the off years having the divisions meet, I don't know.

Stevenson: Yeah. What about this clinical infancy group? Have you been involved in that?

Ainsworth: No.

Stevenson: Oh.

Ainsworth: I just finally decided I'm not going to join anymore associations because I haven't time to go to the meetings and I haven't scheduled anything anymore.

Stevenson: Not Brazelton?

Ainsworth: No I don't, no I haven't. Oh! I have--this is another mistake I make. Upon retirement I thought, "Ah, I will have a lot more time available for things that I wouldn't have had time for otherwise. And one of them was that I was invited to become a member of the board of--I think the group you mean is the National Center for Clinical Infants Programs--and once I got on the board, I discovered that this was a working board. It was going to demand far, far more of my time than I was willing to devote at that point. And furthermore, I'm a pretty academic scientist. Although I've had clinical experience, it's been diagnostic and not interventive. And I found that the tasks I would be involved in were really beyond my area of competence and experience. I think it's a wonderful organization, but I backed out. I resigned and then they immediately made me a life member of the board. I can go to the meetings that I want to go to and if I want to go to them, I can do as little or as much as I feel I want to.

Stevenson: They never told me what they wanted in this interview other than sort of a general background. Did they send you any notice about what they wanted to be covered?

Ainsworth: It was Alice Smuts that got me into this, and I think she wanted part of the oral history of SRCD.

Stevenson: Right.

Ainsworth: But she really wanted SRCD and somebody that knew a lot about SRCD to be the interviewer, and you do.

Stevenson: Yeah. But I was thinking that what they wanted was the background of the person in relationship to SRCD. I mean, one of the things about SRCD wasn't even asked about, I mean, the way it flowed, the way it was.

Ainsworth: And what my experience was when I was more involved. I think that was--that was my impression.

Stevenson: Yeah. Well you went to the meetings and contributed to the journals...

Ainsworth: Certainly to the Governing Council as a general member. And one was president-elect, one was president, one was past-president... And so I had rather thought that those were the aspects of SRCD that I can picture.

Stevenson: Yeah. So what years were all that--I don't remember?

Ainsworth: I don't either!

Stevenson: Was it like the 1970's or...?

Ainsworth: Ah, I became President after having been President-Elect at the meeting in New Orleans, the first New Orleans meeting. And I gave it over to John Flavell at the meeting in San Francisco.

Stevenson: So, it was--must have been in the...

Ainsworth: So it was...

Stevenson: In the later '70's?

Ainsworth: One can certainly look up the appropriate times.

Stevenson: Yeah.

Ainsworth: They've gone from my memory and I didn't bring them along. I could have looked them up myself if I had known that we were going to be meeting here (laugh).

Stevenson: Yeah. SRCD, even then, was perhaps what...eighteen-hundred members...more than that?

Ainsworth: Oh, yeah.

Stevenson: 'Cause I thought it was about three thousand or something now.

Ainsworth: It was pretty large.

Stevenson: But how many of those are members and non-members, and how many members don't go.

Ainsworth: I think most members of SRCD try to go to the meetings. 'Cause I think an awful lot of them feel as I do, that this is their primary affiliation, which makes--the sort of thing that makes it difficult for the non-psychologist.

Stevenson: Yeah.

Ainsworth: Pediatricians, obviously in pediatric associations, are their first link and I don't think many of them really want to publish pediatric things in *Child Development*, but they'd like to be able to do it if they wanted to. (Laughing) And the same with anthropologists, the same with sociologists. I became President at a point when there were a lot of hot issues going on and one of them was that commission that they established and used for children's research. The notion was that Kennedy was trying to get a bill to go--sort of like the animal rights extremist--that children simply shouldn't be used for research

and that this was bad for them. And that started up, I think, when Bob Sears was President, and it occupied a lot of Fran Graham's time. Fran kept me in the picture as much as she could to try to be helpful, which it was. And then, it's still what's going on when I was President. So a lot of my time was spent keeping track of the discussions and issues and writing memos and things like that. Another hot issue was that interdisciplinary thing and the fact that, obviously, when somebody is nominated for a position on the Governing Council or President or whatever, the voters are always going to ask, "Have I heard of this person? Have I read anything that this person has written?" And they're very unlikely to have read the publications of the pediatricians and the child psychiatrists and so on, so they always vote for the psychologists. So people just weren't getting elected and, somehow or other, I had the happy thought of looking up the original way it was done early on when it was founded and discovered that they alternated disciplines from one biennium to the next in terms of presidents. And so that was my suggestion to the council that we nominate only psychologists at one biennium and nominate only non-psychologists the other biennium and it's worked very happily. So I thought that was one thing I really accomplished. Then there was the issue about minorities and we did the same thing for minorities on the slates, which helped, I think. And the third one was the need for a Social Policy Committee. I think that the Washington office had started before then. Oh, my memory is getting so bad, but there was a lot of difficulty with the person that was in the Washington office.

Stevenson: Yeah, right, I remember that.

Ainsworth: It was thought that this wasn't good enough and that we should have a Social Policy Committee that met fairly frequently, took their task very seriously, and did something. But it was always left up to the President before whether to enter into some kind of controversial issue on behalf of the society or not. I remember Bob Sears was faced with a lot of that kind of thing. Now, I think it goes to the Social Policy Committee and I think that is helpful. Also, I don't know whether anyone else has done this since the governing council never got to its agenda. They always ended up meeting with a lot of things left hanging and undecided. And also there were very long discussions that went round and round and round and round. And so I decided that the meeting, when I was president, would be kept to its agenda. I brought a timer along (a little kitchen timer), and I assigned a certain amount of time to each item. I warned people ahead that when the timer went off, they would have five minutes to wind up that discussion. It was absolutely amazing that when the timer went off, the discussion just came to an end. So those are the memories that I have.

Stevenson: Yeah.

Ainsworth: On the council there were three people with whom I'd had substantive disagreements about the attachment thing: Bettye Caldwell because she felt that daycare was being attacked; Jack Gewirtz because he was a Skinnerian; and Bill Hartup because his attachment research hadn't turned out to be so productive. I felt very uneasy about these three members of council and whether we would be able to get along. And I must say it really restored my faith in human nature that we could leave the academic disagreements behind and focus on Society matters. The relations on the council, I thought, were great except that Jack was one of the ones that needed to be cut-off more than anybody else (laughing) in my lifetime. Ah, it went on and on. But we certainly did not, as far as I was aware, have any arguments over issues, ever. So I felt close to the members of the council, after all I was on the council eight years and you really feel that you have an awful lot in common.

Stevenson: I think the society's at an interesting point now because everything is going very well, but it's going to get bigger--and how their handling some of these policy issues--should they have a new journal? Should the journals come out every year? What would you tell them if they said, "What are four things that we should do to improve the society?"

Ainsworth: Mavis Hetherington was editor of *Child Development* when I was there and also, of course, a friend and a colleague in Virginia. But I really had disagreed with Mavis about her policy. I felt that if you wanted to attract non-psychologists to publish in *Child Development*, as they have a right to do if they are members, that one should be influenced by the standards of what constitutes legitimate

science in their specialties and not have them conform to what academic psychologists thought methodology should be. This, of course, is particularly pertinent to anthropology because these people go off on field trips and very often there's no way in which they could get duplicated observations to give reliability. That's all there is to it and still there's an awful lot of interest in some aspects of anthropology towards our science. I don't think ever had the courage to tell her that in so many words, "Look, you have certain things as a sacred cow and I don't see any reason why you feel it necessary to adhere to this policy." She had told what her policy was in no uncertain terms and as a matter of your associate editors and your reviewers and not just to choose them, but anthropological papers from within that get judged by anthropologists.

Stevenson: Yeah.

Ainsworth: At least to have some represented, but I think Mavis' policy was one that was pretty much accepted. Mine was sort of a maverick, as far as the society was concerned, but I think it's a way to get stuff published. At least the non-psychologists published as much as they wanted.

Stevenson: What about yearly meetings? A lot of people feel that every other year is not enough these days 'cause there have been two thousand submissions and twenty simultaneous sessions and so if they met more frequently--

Ainsworth: I wouldn't object to more frequent meetings. I think, though, that it's a very good idea to adhere to what we already have in terms of the governing council because if everybody comes in and out on an annual basis, there's no continuity at all...but scientific meetings, yes.

Stevenson: Or the--

Ainsworth: Division meetings that alternate with--

Stevenson: Some people said that what is needed is an applied child development journal because it's very difficult getting published in *Child Development*.

Ainsworth: Applied--I immediately feel uncomfortable because of what happened in APA with clinical versus everybody else.

Stevenson: Sure.

Ainsworth: I'm not too happy about APA. It's almost down to the point I think you have to be very experimental and I think with *Child Development* stuff you'd find it hard to get published in an APA journal now.

Stevenson: What about people who do have interest in anything other than standard research, I mean, some kind of a journal where you were given an assortment such as in *Applied Child Development*.

Ainsworth: (Laugh) Of course I would like to see some innovative approaches be published without requiring all the methodological perfections, descriptive studies that have leads for more intense theories, controlled research, and results. Yes, I would, but I wouldn't like to think it was just in terms of applied because I think of clinical intervention then, but that's off the top of my head.

Stevenson: Yeah, I agree with you, but some people get very, very motivated about particular aspects of *Child Development* or with the society.

Ainsworth: Or there's also the life-span issue. I think people feel that they are really concerned with research that focuses on adults, you know, and that has a developmental perspective of having come

from somewhere and going somewhere. I think that they are choosing who should go to developmental psychology and...

Stevenson: But what about international research?

Ainsworth: Well, that's another thing too, and I know it is largely true because the Grossman's are doing very, very good attachment research. And they have a terrible time getting published in both *Child Development* and *Developmental Psychology*. And their stuff, if it gets accepted, has problems in the matter of format and style that it's just not organized in the same manner that we organize in the journals. That doesn't mean that what is said is any different in its implications, they just have a terrible time. And so I do think that was a point of policy that might be clarified.

Stevenson: Some persons are very critical of SRCD because as you said, SRCD is not responsive to foreign interest. For example, some of the Canadians are saying, "Why should we pay dues to SRCD when they have a Washington office?" But some of them don't realize that it was an American society to which foreign members came.

Ainsworth: I guess Blatz was the only Canadian on the original board when it was founded. I don't think they thought of it as a national thing; it was a disciplinary organization, you know. These were all people interested in child development, regardless.

Stevenson: One alternative would be to purposely get people on the governing council that are representative of different parts of the world.

Ainsworth: That's possible, yeah. But I suppose ISSBD is really the home of that group. I was astonished when I went to that meeting for the first time-

Stevenson: Was that last summer?

Ainsworth: It really is international. I thought it was fascinating, really, but I'm not going to be active in that society because it's too late and as I said, I don't have enough time and energy. But it's a great society and it's small enough that you feel a bit of intimacy. People that go to those meetings, I guess every year, are from the most remarkable dispersed areas and they seem to be on very cordial terms with each other and it's interesting, very pleasant.

Stevenson: But if they were meeting in the same year as SRCD?

Ainsworth: That's tough.

Stevenson: It would be very difficult.

Ainsworth: I know.

Stevenson: Well I hope this all came out okay; it should. We're near the end of this tape. Well, is there anything else?

Ainsworth: Well, I guess there is just one thing. John Hagen is doing a wonderful job. I think John is very interested in it and he has a very, very good person working with him, so I think we'll be in good shape.

Those who inspired and were influenced by Mary Ainsworth:

Mentors

John Bowlby

William E. Blatz

Colleagues

David Hamburg
Barbara Smuts
Harry Harlow
Harriet Rheingold
Molly Mason Jones
Edward Zigler
Nancy Bayley
Francis Graham
Robert Sears
Mavis Hetherington