Eleanor Emmons Maccoby
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Introduction

This oral history was conducted by the Stanford Oral History Program, sponsored by the Stanford Historical Society and the Stanford University Archives. The Stanford Oral History Program is under the direction of the Oral History Committee of the Stanford Historical Society. The interview was conducted in 2011 by Marion Lewenstein, Professor of Communication, Emerita, and Stanford University Academic Secretary, Emerita. The transcript and recording are part of a collection of oral history interviews documenting the history of the University and the experiences, accomplishments, and viewpoints of members of the Stanford community.

The transcript was lightly edited by program staff and by Eleanor Emmons Maccoby to correct grammar and occasional inaccuracies and to aid in overall readability, while maintaining the interviewee’s voice as well as the substantive content of the interview. As a result of this process, the transcript does not match the recording verbatim. In the case a substantive deletion was made, it would be so indicated where appropriate on the transcript. The oral history itself is an authentic account of the remembered past, but as memory and meaning vary from person to person, the interview may inadvertently include factual errors or discrepancies.

All uses of the interview transcript are covered by a legal agreement between Eleanor Emmons Maccoby and the Stanford Oral History Program. The transcript is thereby made available for scholarly purposes in the Stanford University Archives. The copyright to the transcript including the right to publish is reserved to Stanford University. No part of the transcript may be quoted for publication without the written permission of the Stanford University Archivist or his/her representative.

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This oral history should be cited as “Eleanor Emmons Maccoby, Stanford Historical Society Oral History Program Interviews (SC0932). Department of Special Collections & University Archives, Stanford University Libraries & Academic Information Resources, Stanford, Calif.”
Stanford Historical Society Oral History Program
Purpose Statement

The Stanford Historical Society's Oral History Program explores the institutional history of the University, with an emphasis on the transformative post-WWII period, through interviews with leading faculty, staff, alumni, trustees, and others. The project furthers the Society's mission "to foster and support the documentation, study, publication, and preservation of the history of the Leland Stanford Junior University."

The interview recordings and transcripts provide valuable additions to the existing collection of written and photographic materials in the Stanford University Archives. In addition to scholarly use, information from these interviews may be shared more broadly through print articles and campus lectures. The interview materials are also made accessible online.

Like any primary source material, oral history is not intended to present the final, verified, or complete narrative of events. It is a unique, reflective, spoken account, offered by the interviewee in response to questioning, and as such it may be deeply personal. By capturing the flavor of incidents, events, and personalities, the oral history approach provides details and viewpoints that are not often found in traditional records.
Abstract

In this interview, Eleanor Emmons Maccoby offered great insight into her career in the Psychology Department at Stanford University. Much of the conversation focused on her research into behavior, gender, and linguistic development, from the study of how young children behave to the ways in which language changes based on circumstance and age. She described the Psychology Department’s faculty and administration, as well as the ways in which it has changed over the years. Dr. Maccoby also spoke of the interdisciplinary research efforts that took place on campus during her tenure, and briefly touched on the difficulties faced by female faculty in the middle of the 20th century.
Eleanor Emmons Maccoby

Biography

Born in Tacoma, Washington, in 1917, Eleanor Emmons Maccoby attended Reed College and the University of Washington in Seattle, obtaining her BS from the latter. She earned her MS and Ph.D. from the University of Michigan in experimental psychology, focusing on topics related to the psychology of gender differentiation. Her research has encompassed the socialization of children, developmental change in personality and behavior, relationships of couples after divorce, parent-child interactions, and child-rearing practices. While working at Harvard, she conducted some of the first studies on the impact of television on families and children.

In 1958, Dr. Maccoby transferred to Stanford, where she became a Professor of developmental psychology and chaired the department from 1973-1976. In 1966, along with Robert Oetzel, Maccoby published her first book on sex-based differences, *The Development of Sex Differences*. Her most influential book was published in 1974, entitled *The Psychology of Sex Differences*, co-authored with Carol Jacklin. These publications stressed biological, rather than cultural, influences. Dr. Maccoby has published many books, articles, and papers on her research, and has received awards from the American Psychological Association, the Society for Research in Child Development, and the American Educational Research Association. She is a member of the American Academy of Arts and Sciences and was elected to the National Academy of Sciences in 1993. Division 7 of the American Psychological Association offers The Maccoby Award to the author of a book making contributions to developmental psychology.¹

Lewenstein: All right. Today is Wednesday, February 16th, 2011. I am the interviewer, Marion Lewenstein, and I am interviewing Eleanor Maccoby.

Maccoby: [00:00:19] Hello, Marion.

Lewenstein: Because you gave one of the talks at the Emeriti Council, and you so succinctly put together the history of your movement through psychological research, I think rather than starting with your personal history of your academic career, we will start with your Stanford experiences. I know that you came to Stanford in either 1957 or 1958.

Maccoby: [00:00:59] '58.

Lewenstein: And I'd like you to talk about how you came to be at Stanford and how you found your department. That's what's lacking from our history of you. How you found the department and how you moved through the department. So would you like to start there?

Maccoby: [00:01:19] Sure. Bob Sears brought me to Stanford. He and I had been working together at Harvard in the Laboratory of Human Development there. When he came to take the chairmanship at Stanford, he established a Laboratory of Human Development here at Stanford and raised some money
for it. And he wanted me to be part of that, and also to join the Psychology
Department and teach there. We had jointly written the book, *Patterns of
Child Rearing*, which had been very well received. And so he knew
something about my research style, what I could do, and what my limitations
were.

**Lewenstein:** When did you write that book together?

**Maccoby:** After he and Pat moved here, but while I was still at Harvard. We
did all the data collection in the early '50s. The people we were interviewing
and whose children we were measuring were in nearby Newton and
Watertown. We included 400 families in our study -- by those day's
standards a very large-scale study. Nowadays people have thousands of
cases, of course. And we also introduced ourselves to the new machines for
data analysis. We had an IBM tabulator, which was the state of the art
machine at that time.

**Lewenstein:** I remember those.

**Maccoby:** It was big enough to take up practically a whole room. We were
very proud of it. I think Bob saw me as being up to date when it came to
technology. Turns out I wasn't entirely.

Bob and Wilbur Schramm (a Stanford specialist in communication theory)
were doing a joint project on parents’ utilization of information concerning
child-rearing, and they invited my husband, Mac, and me to come out for a
sabbatical year to work on the project with them. We arrived at Stanford
with our little girl who was almost three. Our baby boy arrived in that first
year that we were here.
Bob asked me to teach the introductory Developmental Psych class that first year. When I attended the first faculty meeting in the Psychology department, I couldn't help being somewhat awed. There were people there who were already of considerable stature nationally in psychology. Psychology itself was a fairly new field at that time and in some universities still hadn't split off from philosophy. But it had been an independent department for quite a period of time at Stanford. Developmental psychology had its roots there in the work of Terman— that is, not the provost Terman, but his father, who had developed the American form of the Binet Intelligence Test. That's what we were known for more than anything else. Bob Sears was nationally prominent because of his work translating psychoanalytic concepts into behavioral concepts, and he had just finished his term as the youngest president of the American Psychological Association. Ernest Hilgard was also very well known in the field. He had written an important textbook and—

Lewenstein: And Hilgard was here at Stanford?

Maccoby: [00:04:40] Hilgard was here at Stanford. Farnsworth was here. Not as famous, but certainly very well regarded. Quinn McNemar also had a national reputation. If I remember rightly, I was the only woman present at that first faculty meeting. No, Edith Dowley was probably there. She was the director of the nursery school, which was available for developmental research projects. That was before Bing Nursery School had been built. The little nursery school was located next to some Quonset huts where returned
veterans who came back to Stanford to school were housed with their families.

My office was in Owen House, which is where Bob Sears had organized his Laboratory of Human Development. In those days the Psychology Department was, I'll have to say, ill-housed. Its headquarters, its main office, were in Cubberley Hall, which was the Education Building, of course. But most of the senior psychology professors had their own little bailiwicks. They would have a house somewhere out around the main campus. Bob's was Owen House; Hilgard had a research house nearby.

And I know that the few people doing animal work had their own laboratories away from the psychology building. We did do much of our teaching in Cubberley, however.

That first year that my husband and I were here, we were supposedly here just on a one-year sabbatical from our Boston and Cambridge jobs, working with Sears and Schramm. We hadn't realized that they had invited us to look us over for faculty positions, but that became clear fairly soon. Bob got me started teaching in the Psych Department immediately, because as it turned out, Lois Stoltz, who had been teaching the large introductory Child Development class for undergraduates, had just retired. And so her spot was open, and I began to teach her course.

**Lewenstein:** For continuity, let me interrupt and ask you what your husband's position was. Was he a psychologist, too?

**Maccoby:** Yes. Mac was a psychologist. We both got our PhDs in psychology at the University of Michigan. And he had been head of the
Psychology Department at Boston University while I was working at Harvard. So when we came here, the awkward part about any possible appointments was that there was a rule that husband and wife could not be part of the same department at Stanford. It was obvious that I was going to go into the Psych Department because I was needed for that developmental slot, and there was no opening available for Mac because at that time there was no Communication Department at Stanford. But Provost Terman had made up his mind to see that one was created. He thought it was needed.

[00:08:29] And so he talked with Schramm and my husband and asked them to establish a Communication Department, which they did quite soon.

Lewenstein: When you were hired, both of you, were you full professors?

Maccoby: [00:09:11] No. My tenure status was rather ambiguous because that first two or three years that I was here, since I had such young children, I asked to be only on half-time. There was no such thing as a tenured professor on half-time, I think, at the whole university at that time. Bob was willing to do anything I wanted in the way of trying a part-time appointment, except he wanted to make sure of one thing, that I would continue teaching the Child Development course. Oh, and the second year I was here, he asked me to teach Psych I, which is, of course, a great big course, and it's a big responsibility. You have a cadre of teaching assistants to help with it, of course, but you give the lectures.

[00:09:59] And I agreed to do that, too. And he said if I would do those teaching chores, that he would let me off of any committee assignments or anything else that might be considered part of the job to be a faculty member
in the department. So as the kids got older, I spent more and more time as a faculty person and—

Lewenstein: But you were tenure line.

Maccoby: I was tenure line and—

Lewenstein: You were assistant professor?

Maccoby: No, associate. But there was some ambiguity as to whether I was on tenure for associate. It was—that's peculiar. But anyway, I got a formal letter about three years later saying, yes, you have tenure. [Laughter] It's funny. I didn't really worry terribly much about all of that at the time. We were so busy, you can imagine, and very happy in our work, I must say. I loved the Psychology Department. I was very proud of it, proud to be a member of it.

[00:10:57] I knew that everybody in it was a very well-regarded person nationally. And I guess, even at that time, we were considered the leading department. No, we were second to Yale in psychology. But we were a very good department, and came to be for many, many years considered the top psychology department in the country.

Lewenstein: How many people do you recall? Approximately how many professors?

Maccoby: Well, seven or eight when I came in, and of course, that number increased. Fulltime equivalents. Isn't it? We grew to twenty-two or twenty-three by the time I became chair in the '70s. So I'm not sure—I'm thinking of how many people were sitting around that table when I went to faculty meetings. But it occurs to me that in those days, assistant professors didn't come to faculty meetings. It was the tenured people who did. [Laughter]
Lewenstein: I'm astonished. [Laughter]

Maccoby: [00:12:03] Well, I think I have that right. [Laughter] But that changed, of course.

Lewenstein: You were talking about working with Sears. He was chair. Am I correct?

Maccoby: [00:12:15] Yes, that's right.

Lewenstein: Okay. And you were working with him.

Maccoby: [00:12:18] That first year when we came. And then after that, I remained a member of his Laboratory of Human Development. We never did any joint research after I came to Stanford though. When I came here, he was just in the middle of conducting another study of parents’ child rearing methods and their effects on kids. But it had much more sophisticated methods than we had used for the Harvard study. Although the book, Patterns of Child Rearing, was very well received, it was entirely based on the parent interview work that my group had done, because the assessments of the children that he and Pat Sears had done together didn't pay off and—

Lewenstein: Pat Sears was his wife?

Maccoby: [00:13:08] Yes. And she had an appointment in the School of Education. It was a bitter disappointment to him, and to all of us, that we couldn't find a really good story to tell about relationships between our parental measures and the measures that he and Pat were taking of the children. And as we reassessed that whole thing, I think most of us came to the conclusion that either the theory was wrong, which is what I really thought, but what Bob really thought was that the methods of measuring the children's characteristics had been much too weak.
So he had started a new study when he came here to Stanford while I was still at Harvard. He had a smaller sample of families, but much, much more detailed and interesting measures of the children, and he used behavioral measures instead of the projective measures that they had used at Harvard. And I thought, too, this ought to be a very much stronger test of the hypotheses.

**Lewenstein:** Can you explain the difference between projective measures and behavioral measures?

**Maccoby:** Well, projective measures are the ones they use for doll play, where they would give the children a little dollhouse to play with, and they had a father doll and a mother doll, a little boy doll, a little girl doll, and a baby doll, and a dog. And one hypothesis was that if the parent interviews revealed that they were very, very strict about any shows of aggression on the child’s part, the child, during the doll play, would displace their anger and be angry or mean toward the baby or the dog. Didn't turn out to be true in the projective test of doll play. “Projective” meant, in that case, that the kids—they were five years old—would project upon the toy dolls in the dollhouse their own feelings.

**Lewenstein:** And you're saying that didn't prove to be true?

**Maccoby:** No. That is to say the hypotheses didn't work out.

**Lewenstein:** Okay. And the developmental methods that were being used at Stanford?

**Maccoby:** Would be much more behavioral. So that, you would want to get a measure of children's guilt, for example, because the hypothesis was
children would develop strong senses of guilt if they were punished by withdrawal of love.

**Lewenstein:** And how could you measure a sense of guilt?

**Maccoby:** [00:15:34] Well, that was just what his interesting innovations were. He would ask--the research assistant would ask--a child to hold a mouse, a live mouse on its lap and take care of it while the researcher was out of the room. And everybody knew from testing it ahead of time, no child could keep that mouse on their lap. [laughter] And the mouse would get away and then the child would feel terrible because they'd been asked to hang onto it. And you saw how they answered, how they dealt with guilt. Did they lie about what had happened, which some of them did?

[00:16:14] There were several little behavioral tests of that kind that elicited presumably the behavior that they were interested in seeing. Resistance to temptation was another one. There would be an opportunity for a child to cheat on something, and you would see whether they did when the research assistant left the room. I might add that those hypotheses tested--the way he tested them--also didn't work out. The book that he and the two people that were working with him at the time wrote on that study was marvelously written, as everything Bob wrote was. [00:16:56] He had been a lit major at Stanford. He was a very good writer. And it was a wonderfully written book, and it just practically made me weep when I saw how weak the results were. And he understood that. There was this hypothesis that boys who had been strongly punished for aggression by their fathers would become aggressive to their peers. And what they became instead was scaredy cats.
Well, I think Bob was working almost entirely with psychoanalytic hypotheses. And that was, in my view, what was wrong from the start. But that's another story. [laughter] In any case, I went off working on a different set of problems entirely.

Lewenstein: Which were?

Maccoby: [00:17:45] Well, I wasted two or three years on some studies of differential abilities, kids who were very good at math and very bad at verbal, or vice versa, to see if I could find some of the antecedents—why the kids would be like that. And I couldn't. As I say, I wasted time on that. We wrote a big report which actually was never published. But I got accustomed to the Stanford environment, and I was doing a lot of teaching and a lot of child rearing at that time. So—

Lewenstein: When did you have time for research? [Laughter]

Maccoby: [00:18:24] That is a good question. And the fact is I didn't publish anything during that first three years.

Lewenstein: And still Stanford considered you seriously and put you on—

Maccoby: [00:18:34] Yes. They liked my teaching. And I had a very good publication record before I came here from Harvard days.

Lewenstein: What was your position at Harvard?

Maccoby: [00:18:44] Lecturer. I was not on a tenure track. I was very easily dispensable by them, except they never did replace Bob after he left. They had asked me to teach his courses for one year until they got somebody else. And they never did, and so I taught his courses all the time—well, seven years until I came out to Stanford.
Lewenstein: How were you treated by the other people who were already in the department?

Maccoby: [00:19:13] At Stanford?

Lewenstein: At Stanford.

Maccoby: [00:19:15] I think they were kind of ambivalent. That is, they knew I was a good teacher, but they knew I wasn't publishing, and they thought—they didn't have very much respect for the kind of research I had chosen to do. And they were right. But then I got into several other lines of work which turned out to be much more interesting, more useful. I turned to studies of differential auditory attention. The people who were studying perception in those days almost universally studied vision. They were interested in visual illusions.

[00:20:00] Well, you can imagine all the things, the focus of the eyes on a particular thing. And I thought, well, we do an awful lot of attending with our ears, and particularly in parenting. We have our ears tuned all the time to where a little child is and what they're doing, and if there's too much silence, we immediately go into action to find out what they're up to. [laughter] Or we listen to what the child is saying and answer them while we've got our back turned. And you get very peculiar dysfunctions of parenting when a child is deaf and the mother doesn’t know it, which will happen in the first year and a half in some families. And she just thinks the child is willful, won't listen to her, and so on, gets very angry, and the relationship really develops badly until she understands that she has to be face-to-face with the child in order to communicate.
Anyway, all of that hadn't—I'd learned some of those things while studying parenting as we did in the Harvard study. I wasn't studying parenting when I was studying listening, but I was asking kids to—I would put earphones on them and put two different messages into their ears at the same time, one in a man's voice, and one in a woman's voice.

Lewenstein: And how old were the kids at that point, more or less?

Maccoby: These kids were now—how old were they? Seven, eight, and nine, something like that.

Lewenstein: And you were putting the headphones on them.

Maccoby: Yes. And asking them, "Tell me what the woman's voice said." And they had to ignore what the other voice was in the other ear. And then after they told you what the woman's voice said, you asked: "Now, can you remember what the man said?" And so you have to—you can measure whether they can hold one message in their mind while they're producing another one. And we actually did one study where we were comparing children, preadolescent kids with young adults with elderly folks. I think the oldest person we had in our old age sample was eighty, and at that time, that seemed to me to be absolutely the end of a life [Laughter]—

Lewenstein: Quite old. [Laughter]

Maccoby: —which it doesn't now. [Laughter] So—

Lewenstein: Are you willing to tell us how old you are now?

Maccoby: Yes. I'm ninety-three. [Laughter]

Lewenstein: All right. [Laughter]
Maccoby: [00:22:30] And what we found then was that there—everybody believed that old people were more distractible. And we found they weren't. That is, they could easily ignore one message while reporting the other one. What they couldn't do was report the second message. And so we thought, oh, well, they can't remember the second message that long. It's a short-term memory problem. But it turned out it wasn't, because we did a control condition where we asked them to simply wait.

[00:23:06] They got two messages in their ears, and they had to wait, what, ten seconds before we asked them to report one. And just waiting did not cause them to lose either message. What caused them to lose a message was saying the other message. And after I got that result, I was so surprised I got in touch with professional audiologists. And they said, oh yes, that's destructive readout that you've found. What? Destructive readout? I never heard of it. Yes. Reporting one thing—the act of saying it wipes out the memory of the other.

Lewenstein: Oh, yes? Was that true of the children, too?

Maccoby: [00:23:49] No, not as true. That was the thing that was the defect for older people.

Lewenstein: What did you learn from the part of the study that dealt with the children?

Maccoby: [00:24:02] I got big age changes in the ability to concentrate and not be distracted by the second voice. And so we compared several ages. We have a monograph on this. So that was interesting work. And the rest of the department really respected it more because it was experimental. And all the stuff that is just simply correlational between what parents do and what kids
do left them very cold, for the most part. Psychologists are deeply experimental. That is, the scientific side of psychology.

[00:24:38] I'm not talking about clinical now, which is an entirely different thing. I might add that one of the issues that our department went through not too long after I came was the question of whether we should continue to have a clinical psychology training program in our department. We had one when I arrived. And after maybe—when was it? I came in '58. I suppose it must have been the late '60s that we decided to abandon our clinical program. It was a big issue and it meant—we had only one tenured professor in clinical psychology. And what had been happening was that we had hired one after another, two or three a year, assistant professors in clinical psychology. They never made it to tenure because they never published.

[00:25:30] They really weren't researchers in any way that our department respected. That was the first problem. And then the other problem—well, I remember Al Bandura, at that time, was head of the clinical psychology program. And he said he kept going to the airport to meet an incoming new assistant professor in clinical, and then a few years later, driving the person to the airport to leave. He was really tired of this revolving door. He said you're telling me we have no clinical program, really, and you are the head of it. [Laughter] He said I don't like this position at all. But there was one senior tenured clinician that was a problem for us in the sense that he would be—if we abandoned the program, he had tenure, and he would stay on
essentially with no role in the department. There's no point in going into all those ups and downs and how the—

Lewenstein: Well, but in fact that's part of what is wanted with this oral history.

Maccoby: [00:26:34] I believe he left. And I'm just trying to think what his name was, but you don't need that. So that was a big issue. The other reason why we wanted not to have a clinical program anymore was that we realized, with our very empirical point of view in the whole department, we spent the first year of our clinical students' training explaining to them how the Rorschach was an invalid measure. It doesn't work. The second year we spent teaching them about the TAT—what the heck is that?

[00:27:14] It's another projective test—and how it didn't work. We trained them on the weaknesses of all the existing measures that clinicians used, and then patted them on the shoulder and said, here's your degree, now you're a clinical psychologist. [Laughter]

Lewenstein: Was this a master's program, or an undergraduate program?

Maccoby: [00:27:31] No, no. It was a PhD program.

Lewenstein: Oh, it was a PhD program.

Maccoby: [00:27:34] And they weren't doing good research because none of their professors knew how. Their theses were disastrous, many of them, and people laughed when they were sitting on the committees working with these students. It was totally unfair.

Lewenstein: Can you describe why—since the Rorschach test is so well known by laypeople who might be listening to this in the future, can you explain why the Rorschach test doesn’t work very well?
Maccoby: [00:28:04] Oh, gosh. I wish I could remember any of that research. It's plentiful.

Lewenstein: Okay. I just thought—

Maccoby: [00:28:12] It's just that—well, for one thing, two different clinicians administering the Rorschach to the same person on different occasions would come up with completely different conclusions about the person's characteristics.

Lewenstein: Okay. That's good enough.

Maccoby: [00:28:30] That's one reason. [Laughter]

Lewenstein: So the clinical department was done away with—what—maybe within five years of when you joined the department?

Maccoby: [00:28:39] I just don't remember the timing. It might have been a little longer than that. Probably was.

Lewenstein: Is that when you started to add more research psychologists to the department?

Maccoby: [00:28:48] Yes. Yes. There was a gradual shift over to—well, first of all—let me think. There was a good deal of research of the Skinnerian sort when I first came dealing with animals. And they were studying something that I had studied as a graduate student for my own thesis, different schedules of reinforcement. Suppose you starve an animal and then feed them if they perform a given response (like pressing on a lever). Or, you could give them food for every fifth response instead of every response. If you do that, you build up a habit in them that's much more resistant to extinction. Meaning when you stop giving them food altogether, they will keep on pressing the
lever much longer than if you had been rewarding them for every press.
And then you can do random reinforcement schedules so that the animals
can't tell when the reinforcement is coming.

[00:29:47] That very much lengthens the period before they stop responding
altogether. And those kinds of issues about reinforcement schedules were
still in everybody's mind because when I moved to Stanford, the dominant
theories in psychology were behaviorist learning theories. And that gradually
changed. It was partly in response to input from linguistics where children's
learning of language just simply can't be understood or explained in those
behavioral terms. You can't think of a word as a response of the sort that a
rat is giving when it presses the lever. Right? [Laughter]

[00:30:31] So it wasn't very long before the rat labs closed up entirely. We
had a joint professor from the Psychiatry Department, Karl Pribram, who
was running a monkey lab. And, well, I don't know how to put this.
[Laughter] But this turned out to be a big pain in the neck for the
department. I guess I don't want to go into the details of why Karl was our
problem child. But it certainly led us to feel that we didn't want those joint
appointments any longer. And I remember somebody saying our department
is full of courtesy, but out of joint. [Laughter]

[00:31:23] We were willing to make courtesy appointments across
departments. Now, Bob Sears had a joint appointment both in psychology
and education. And there was one man, Jim Marsh, who had an
appointment between psychology and the Business School, if I remember
rightly. But those joint appointments became frowned upon.
Lewenstein: But from your history that I've read previous to this interview, you personally started—and maybe others in the department, too—began doing a lot of work with people from other departments, joint work.

Maccoby: [00:32:07] Yes, we did. We did. And this was another—Bob Sears was an institution builder. He knew how to put together a structure where things would get done. He started, as I say, the Laboratory of Human Development. When he was promoted to dean of H&S [School of Humanities and Sciences] where he had the job for at least seven years, I think, he said, I want you to take over the Laboratory of Human Development. I didn't know what that meant. I had never raised money for a research institution. I didn't know how to do it. I didn't do it. And the laboratory pretty much just quietly went out of existence. That was the time when our new building was built. And at 1970 we moved in to the building that the department now occupies.

Lewenstein: At the front of the campus.

Maccoby: [00:32:57] Yes. Jordan Hall. So I was really a failure in terms of running an institution like that. But Bob, meanwhile, had joined with some other people to go and talk with the board of Boys’ Town [Boys Town of Omaha]. And Boys Town, the board at that time decided that they wanted to support some research. And they put up a very big chunk of money to establish an interdisciplinary center for child study at Stanford [Stanford's Center for the Study of Families, Children and Youth]. And I got very active in that, and jointly—that's when I did my joint research with Wald in the Law School—he and I began to teach.
Lewenstein: I've forgotten his first name. Was it Bob?

Maccoby: [00:33:49] Mike Wald.

Lewenstein: Mike Wald. Yes. Okay.

Maccoby: [00:33:51] I later worked with Bob Mnookin, you're thinking of from the law school. But Mike Wald and I taught a joint symposium on Children and the Law for—what—ten or eleven years.

Lewenstein: When was that? Over what period? 70s? 80s? 60s?

Maccoby: [00:34:11] Late '60s and '70s, I believe. And both of us enjoyed that enormously and learned a lot from it. What else did we do? Well, the Center for Child Study was plugging along very successfully. We had people from sociology, people from linguistics, people from anthropology came in there. It was a little bit unstable in the sense that a full professor wanting to bring in money for his research felt the pressure to bring it in through his department rather than through the child study agency, which was not a department. [00:35:04] There was a kind of a centrifugal force, I guess you would say, toward departments, because if you brought your grant in through your department, you were able to support a graduate student or two and some secretarial help. Now, you should understand, in those days, we all had secretaries. [Laughter] This was before the days of the personal computer.

Lewenstein: [Laughter]

Maccoby: [00:35:26] And a number of people, whose names I won't mention right now, never learned to type and couldn't use computers. So it was necessary to raise enough money to support those functions in your department. If you put your money through the Center for Child Study, Children and
Families I guess it was, that deprived your department. Now, I did that on a couple of occasions, and when Bob Mnookin and I developed our big study on divorce and its effect on kids, we put that through Child Study, the center, rather than our departments, and were frowned upon for that reason.

[00:36:20] So I came away with the understanding that actually creating and managing interdisciplinary stuff is very hard to do. I've been very impressed with the program on Human Bio [Biology], for example, which attracts huge numbers of undergraduates. But getting it funded and keeping faculty in it when they are all members of other departments to whom they owe allegiance is not easy. And I have heard recently from somebody who was very active in that program that it wasn't until a new dean came in that they recognized Human Bio as worthy of support from the central office and raising some money to support it specifically, because it was completely running out of resources in spite of the fact that it was enormously successful on the campus.

[00:37:13] They had huge numbers of kids who got major awards and who went on to brilliant careers and so on, more so than most other departments at Stanford. But they were struggling.

I loved interdisciplinary work. Still do. And I recognized that problem.

Now, where were we? Let's see. Back in the department.

**Lewenstein:** I'm not sure you had gotten to who you were working with in the department, but did you tend to work with certain people more than others?

**Maccoby:** [00:37:49] Not among my department colleagues. No. My closest—what—after I became chair, I was busy in recruiting another senior
developmentalist, and was successful in recruiting John Flavell to come to the developmental program. But his interests were completely different from mine. He had been the—he'd written that wonderful big book on Piaget, and he was interested in stages of development.


Lewenstein: —G-E-T. Okay.

Maccoby: [00:38:26] Bob Sears was completely contemptuous of Piaget's work. And so when I started teaching his courses at Harvard, I sort of started with that bias, but I began to read Piaget and I got very interested in those theories of cognitive development. I think Bob's interest really focused on social development in kids, how they develop relationships with other children, with their parents, and so on.

Lewenstein: And the cognitive development focused on when children learn certain behaviors, or certain attitudes?

Maccoby: [00:39:02] Well, most cognitive people have been interested in how they learn, people who study developmental linguistics, for example. How is it that children are able to come out with sentences that they've never heard anybody else speak? It turns out to be quite obvious quite soon that they're not learning just by imitation. And people had assumed that's how they did it. So you'd get somebody—a child running in from outdoors and saying, "Daddy, would you please higher the swing", because they knew the word lower.

Lewenstein: I see. So they now wanted it raised. [Laughter]
Maccoby: [00:39:48] That's right. So they were generalizing into categories. What categories? Well, you can imagine how interesting and intricate that whole thing can become.

Lewenstein: And John Flavell was in this area.

Maccoby: [00:39:59] Not exactly. He was mainly interested in age changes in what children know, what they assume, how they interpret situations. He would compare three year olds and five year olds, or three year olds and four year olds. And he was extremely inventive in thinking of things to study. He was particularly interested in how kids understand other people's minds. How do they know what other people intend, or expect. How do they know how much somebody else knows already so you don't have to tell them things? [00:40:40] And he did wonderful little experiments on these things. I just was lost in admiration of what he did. But it was totally different from what I was doing, obviously. We never collaborated. We didn't need to. There was no purpose in it. But we were very good, close colleagues all those years.

Lewenstein: Was it a collegial department in general?

Maccoby: [00:41:01] Totally. We did not have the crossfire. I know some departments at Stanford where graduate students were at terrible risk if they worked with one person, then the other person was going to flunk them, or come to their PhD oral and try to see that they didn't pass it. I mean really awful things like that that happened. We never had that kind of crossfire, and I was so proud of it and felt so fortunate in being in a harmonious department.
Lewenstein: How did you get to be chair in the sense of how did you progress through associate professor?

Maccoby: Well, for one thing, our chairmanship rotated. And I had only one four-year term. It was my turn. Now, it was well understood that there were certain people who shouldn't ever be asked to be department chair [laughter] because they couldn't administer, they couldn't fight their way out of a paper bag when it came to getting things done smoothly.

Lewenstein: Were you a full professor by the time it was your turn?

Maccoby: Yes. And I have never quite understood how we did choose the next chair. I know when I was chair, I had a little executive committee as the leaders of the different sub-programs in our department, and they would know as a group who could and couldn't do the job of being chair. Now, at that time, we had very strong department administrators. The chief person in the secretarial office, you know, who understood the budget better than most of the chairs did. Now, there were some chairs who knew the budgets up and down and were very, very good. I ran the money of the department, its financial side, very much like I would run a family budget.

I wasn't going to promise that I could establish a new professorship in this or that if I didn't have the money. Now, Dick Atkinson was one of our most successful chairs. You know who he is?

Lewenstein: I know the name but I didn't know him personally.

Maccoby: He became chancellor of the whole system of universities for the state of California. And he was a good administrator, [Laughter] as you may guess from that. He moved from our department up to the deanship, and so
on, which quite a lot of people from Psychology had done. It seems to me
we—

Lewenstein: That's true.

Maccoby: [00:43:36] We produced good deans. [laughter] So Dick always spent more
money than we had because he knew that by bringing in a very strong
person, he could raise more money, and we would have enough money then.
I would never take such a risk, and that was foolish on my part.

Lewenstein: You said that you were chair for seven years?

Maccoby: [00:44:03] No, only four.

Lewenstein: Oh, sorry.

Maccoby: [00:44:06] And that was my rotational obligation. Now, some people stay on
for a second term if they're asked to. Now, if they're asked to, who does the
asking? Well, officially—

Lewenstein: That's usually the provost, doesn't it?

Maccoby: [00:44:21] The dean.

Lewenstein: Or the other deans?

Maccoby: [00:44:23] The dean is the person who's supposed to really make that
decision. And it's usually engineered quietly by the people who are known to
be the leaders of the department, some of whom are more influential than
others, and deserve to be.

Lewenstein: You've said that Stanford at the time you joined it, the Psych Department
was number one or two in the nation. How was it rated within Stanford
itself?
Maccoby: Highly. But I can't tell you whether other departments were rated more so. I know that the dean would sometimes—when Bob Sears was dean, he picked out a couple of departments that looked to him to be extremely weak. One of them was the Drama Department. And he brought in some big stars from elsewhere to do different kinds of functions for the Drama Department. He put resources into a poor department and built it up. People always treated our department as though we should be treated well, but we didn't have to be singled out for improvement. [00:45:42] And we pretty much got what we needed and what we wanted. We were very pleased with our new building, and had enough space, although, I must say that now that the neuroscience side of psychology has become so dominant, that has required lab facilities that are over and above what the building was ever planned for.

Lewenstein: Are the lab facilities there in the building now?

Maccoby: I believe they are. When we wanted to do CAT scans and PET scans, we did originally have to go over to where there already were existing machines to do that. I'm quite sure the department now has them.

Lewenstein: What were the other—well, currently, you said neuroscience is becoming a strong component of the Psychology Department. What I was going to ask you is if you could go through the progression of what were the strongest areas within the department through the years you were associated with it.
Maccoby: [00:46:49] Well, after the decline of the behaviorist approach to learning theory, I think the strongest part of the department has been ever since our cognitive program.

Lewenstein: That started before John Flavell came?

Maccoby: [00:47:09] Yes. Oh, yes. It's one of the reasons that they were happy to have him as a choice. Roger Shepard was one of the people who was very, very good in that. And we were very fortunate to be able to hire Amos Tversky. Tversky and Kahneman, you may have heard, won a Nobel Prize in economics. I mean, there is no Nobel Prize in psychology. [Laughter] But their stuff was so good and so important, and it permeated—permeated isn't the word—it grew in psychology. But then its implications spread out into other disciplines.

Lewenstein: Could you describe it from a psychology point of view just briefly?

Maccoby: [00:48:17] Yes. I wish you'd asked me this ten years ago, Marion. [Laughter] I've been so out of psychology for quite a few years now. Let me see if I can say that.

Lewenstein: What did Tversky study?

[Interview paused and resumed]

Maccoby: He was interested in the way people make decisions. And one of the important things they discovered was strange varieties of risk aversion so that, for example, you would say to somebody who was sick with a particular problem, "If we treat you in the way that we need to treat you, you'll have a ninety-five percent chance of survival. Are you willing to accept this treatment?" And lots and lots of people will say, well, sure, of course, if I've
got that big of a chance. Now if you say instead to them, "There's a five percent risk that you will not survive it," far fewer people will accept the treatment. It's the same risk in both cases. But it's the question of how you frame your question in terms of what the good chances are versus the bad chances. And that people make their financial decisions in exactly that way, making that same mistake all the time.

**Lewenstein:** Yes, I've seen some frequent references.

**Maccoby:** 00:50:36 Okay. That's the kind of stuff that permeated to economics very quickly, as you can see, because it certainly complicated the assumptions that very many people made when the economists had always thought of the consumer as a kind of a rational weigher of benefits and costs of just about anything. I know I talked to Amos one time when I learned that he was working for the Office of Naval Research and asked him what sort of work he could do that was relevant to the Navy. And he said, “Well, of course, lot of it is classified. But I can tell you, I have been working with people concerned with submarine captains and how much leeway they should have to make a decision as to whether to make an atomic attack.”

00:51:27 They're carrying atomic weapons, after all, these submarines. And what kind of chain of command does there have to be. We have to be prepared for the possibility that the naval office in Washington is going to be wiped out with the first strike. What kind of decision capacity do we want to leave with the captains of the submarines? And I thought, Amos, I don't think anybody could solve a question like that. [laughter] But that's exactly the kind of heavy and difficult issues that he got himself involved in. I
thought he was one of the most admirable people because he had a very deep
social conscience in addition to being so incredibly smart and very creative,
innovative in terms of the way he would formulate issues. I just thought it
was marvelous.

Lewenstein: And he was in the Psych Department.

Maccoby: [00:52:20] Yes, he was. And he died quite young. Another very important
person in our department for many years has been Gordon Bower. He came
as an assistant professor. And I think he's probably the best teacher of
graduate students I have ever seen in action. Gordon can—he's a speed
reader. He taught himself to do that. So he gives a graduate student
assignment, wants him to write a short paper on X, Y, or Z. And he's
supposed to bring it in and give it to Gordon. The student arrives and hands
him the paper, and says, "Here it is. I'll come back." And Gordon says,
"No, no. Stay right here." The student sits down, Gordon reads the paper,
one page, the second page, the third page, and he knows what's in that
paper. And then he starts grilling the student. Students are scared to death
of him, as you can imagine. [Laughter]

[00:53:15] But they learn. Oh, my word, the number of wonderful
psychologists that have come through his hands and out into the big world is
really quite astonishing when you think about it. So this is one of the reasons
I'm so proud of our department. And I know that our cognitive program has
been exceedingly strong all these years, and it has not only changed with the
temper of the times in terms of what's important in psychology, it's been one
of the places where those changes have their roots because that's where the
research has been done that has brought those changes about. Next question.

**Lewenstein:** [Laughter] Since you've been at Stanford, what do you think have been your greatest contributions to the field?

**Maccoby:** [00:54:09] I don't think my contributions to the field have been so enormous, to tell you the truth.

**Lewenstein:** Then why are you so well-known?

**Maccoby:** [00:54:17] I'm overrated. [Laughter]

**Lewenstein:** How nice. [Laughter]

**Maccoby:** [00:54:23] Well, part of it is accidental in the sense that a young woman walked into my office and she wanted a post doc. She'd been trained at Brown by somebody I knew and so I had agreed to see her. This was Carol Jacklin. And she and I began to chat about things that we had recently read, and we both were kind of outraged about some of the claims that were being made about sex differences—women were from Venus, and men were from Mars. Actually, that book [*Men Are from Mars, Women Are from Venus*] came out after she and I started working together. She came in the late '60s, just a new PhD (with a new baby, by the way). And we started talking about who had found this about gender differences, and who had found that.

[00:55:11] We said, wasn't it time for us to begin putting together a compendium to see if we could add up all the research we could lay our hands on and find out what it really added up to. Now, I had actually started this kind of thing before Carol came into my office because I'd been part of a Social Science Research Council work group on that subject.
Lewenstein: On the subject of—

Maccoby: Of sex differences and how well-established they are, and that kind of thing. So I'd already put out one little book of readings on that and was trying to go ahead when she brought this up. And I just had one question for her, which is, suppose we find something that we don't like? Women turn out to have a certain weakness that has been confirmed or something, what would you think we should do with that? [Laughter]

[00:56:09] I said this because I had been challenged by another member of our department whom I will not name, that if anybody found out anything that was detrimental to the interest of women, they should not publish it. And I had a very strong contrary opinion. And so what Carol said was, "Well, the truth shall make us free." And I said, "Okay. You're the one for me." And we started working on that book, *The Psychology of Sex Differences*.

Lewenstein: But she wasn't with the department officially.

Maccoby: No. She came in as a post doc, and she didn't have any other kind of an appointment except my post doc. And we jointly raised the money to do the work and hire research assistants and whatnot. So that was a big enterprise. It took quite a long while. We delved through the existing literature and we found a real problem with the so-called file drawer problem, which is, you do a study, you test for sex differences, there aren't any, so you put that part—that computer printout into a file drawer. Nobody ever knows that you didn't replicate what somebody else had claimed to be true.
[00:57:24] So we did—I know we couldn't have been thorough about this, but we tried hard. We would look at the literature. We would know that somebody could have made a test for sex differences in this particular thing, and if it wasn't reported we'd call them up and find out did you test for it? “Yes, we did, but we didn't find it so we didn't publish it, because the journal editors didn't want to publish non-replication.” So we found out a lot of instances of non-replication. I remember one particular thing that was so striking. There was one report on this little test called the prisoner's dilemma.

[00:58:02] The prisoner, if he confesses, he's going to implicate his partner in crime. If he and his partner both remain silent, they can be safe. But if one of them speaks up and tells the truth in order not to go to jail, his partner will go to jail. So it's that kind of thing. Will you sell out your partner in order to save your own skin? And the first report was women would do that more readily than men would. We found something like twenty-eight replications that did not find that and that hadn't been published. [Laughter] Isn't that weird?

**Lewenstein:** That's very weird. Yes. [Laughter]

**Maccoby:** [00:58:42] So our book came out mainly as a myth buster. It was a big seller I have to say, even though it was dense. And the last third of the book is all descriptions of studies, little abstracts that formed the foundation of the conclusions we came to, which were that the only things that were really substantiated was that men were more aggressive—boys, too, and that women were not passive. The two sexes were astonishingly similar
intellectually with one little exception. There's a test of spatial ability where you have to think about the ways things move.

[00:59:33] Like sets of gears. You turn this gear this way, which way will that one turn? [laughter] You know, that kind of thing. And men were better than women on that. Now, it's interesting that these two things have held up very well. We argued at the time—now let me think. How has this gone? The verbal differences, that certain of them, have stood up. One of them is that girls talk more than boys do, particularly with each other. But on the whole, our message was all these things that have been said about the differences between the sexes, that most of them are nonsense. They're not really there.

Lewenstein: You've done a lot of research involving young children and sex differences.

Maccoby: [01:00:28] Yes.

Lewenstein: Were you a leader in that field?

Maccoby: [01:00:32] Yes, I suppose so. We certainly weren't the only ones doing it. And one of the interesting things about a career, as you get some name recognition, you get onto groups who are interested in the same issue. And you go to conferences together, or somebody organizes a consortium on this or on that and you meet, you go to other people's universities and whatnot. You find that you get chances to build the reputation of young people coming along that you see as promising, have done a really good piece of research and you make sure to cite that and write letters of recommendation when they are trying to get a research grant. That kind of thing.
And you form a very strong network of colleagues who are pretty much at your level. Some of my wonderful friends—I just got a letter yesterday of one of them who's having a Festschrift and wants me to come to Texas. Sometimes they still come here to talk to me, which is a great thrill when they do. But these professional connections are deeper really than things that were within my own department for the most part. I love that part of my career.

**Lewenstein:** How did you end up towards the end of your career at Stanford? What were you working on?

**Maccoby:** Well, the last thing that I did was that joint research project with Bob Mnookin in the Law School on divorce. Now, here—this was kind of a throwback really to my very early life when I was doing survey research, and to the work that we did on the *Patterns of Child Rearing* book where I was responsible for interviewing the parents and quantifying what it is that they told me.

**Lewenstein:** That was the study with Sears.

**Maccoby:** Yes. And we were doing that again. We had a sample of eleven hundred families who filed for divorce within a particular one-year period in this area.

**Lewenstein:** In this physical area, the Bay Area.

**Maccoby:** This physical area. And we took the eleven hundred families—yes—they were all families who had children under the age—what was it—ten—no, eleven at the time the parents divorced. And Bob Mnookin, his interests was in conflict resolution. And he was interested in how it was that
people worked out the division of authority, or I should say custody between—for the children. This was in a period of time when there was a big press for joint custody. And people were saying, well, is this good for children, or isn't it? And we hoped to be able to get some answers to that.

So my interest was in the effects of custodial arrangements on children. And Bob's was in the process whereby they arrived at their custody agreement. So the first book that we wrote, called Dividing the Child, was on that process of choosing custody, mostly on that. Then there was a follow-up for each year for three years. We interviewed the parents again—and did some assessment of the children themselves. As they were growing older, they were more able to answer questions and be interviewed. And then that second book, I did jointly with Christy Buchanan. Who was our third author? Sandy Dornbusch.

Lewenstein: Oh, Sandy Dornbusch, did you say?

Maccoby: [01:04:34] Yes.

Lewenstein: And where was Christy Buchanan?

Maccoby: [01:04:37] She was a post doc.

Lewenstein: Oh. Within the department.

Maccoby: [01:04:41] Yes. And she actually took the lead in writing that book. So she's the senior author of it, Effects of Divorce on Adolescent Kids I think is what it's called. Most of them were preadolescent. So in other words, what I was doing was turning my interests more toward issues of social concern instead of the more theoretical issues in psychology, which I had been interested in when I was studying auditory perception, for example.
**Lewenstein:** What sorts of social concern?

**Maccoby:** Whether you should have joint custody or not.

**Lewenstein:** Oh, I see. Within the study with Wald—oh, with Mnookin.

**Maccoby:** With Mnookin. That's right. And I developed those interests during these long years that I was doing the seminar with Mike Wald. And for that seminar, we invited in family judges, family court judges. One of them came and took our course, as a matter of fact, and contributed enormously to the discussion. This was so fascinating.

**Lewenstein:** You were teaching graduate students, or undergraduates, or a mix?

**Maccoby:** All graduate students.

**Lewenstein:** All graduates.

**Maccoby:** And some of them—most of them were law students, but quite a few would come from sociology and one or two from psych. So that was an interdisciplinary seminar.

**Lewenstein:** Did you have other interdisciplinary seminars? You know, in other areas?

**Maccoby:** Well, when I taught my graduate course in developmental [psychology], students would come to that from the School of Education and from the Department of Communication. Henry Breitrose and—

**Lewenstein:** Don Roberts?

**Maccoby:** Don Roberts. Exactly. He's the one I was trying to think of.

And of course, he went on to do work on the effects of media on children. So I consider him a really good interdisciplinary product of that course.

**Lewenstein:** You retired in what year?
Maccoby: [01:07:11] 1987. I was—is that right? Yes. I was required—I was one of the last people where you—there was mandatory retirement at age seventy. So in 1987 I was seventy.

Lewenstein: But you continued doing some research or writing, did you not?

Maccoby: [01:07:33] Oh, yes. We published the second book. Well, let's see. The first book, Dividing a Child, came out in 1992 after I was retired. And the next one came out about two years later. And then my book called The Two Sexes I wrote and published in 1998.

Lewenstein: Describe that book a little bit.

Maccoby: [01:07:59] Well, up until that time, I had been pretty much the darling of the feminist movement because they liked the message that there really aren't very many differences between the sexes, and they felt that I had let them down when I came out with this book. And they knew beforehand because I'd been giving papers about it. But in that book, I found a very solid sex difference that people had just simply not paid any attention to. And that is the way kids behave in groups. And the very strong tendency there is for kids when they're playing by themselves outdoors—I don't mean by themselves—playing among other children, segregate themselves.

[01:08:47] You see it on school playgrounds astonishingly strongly, where boys play with boys and girls play with girls. And Carol and I had done one experiment where we brought in 92 kids, each of them thirty-three months old, all of them. They were within two weeks of that age. And we chose that age because we wanted kids who hadn't really yet firmly established much knowledge about what boys and girls were expected to do or be—they all
knew what their own sex really was. Although when you asked them about it intellectually, they couldn't talk about it very much. But if you say to a little boy of that age, "You really are a girl," he'll stamp his foot on the floor and deny it.

[01:09:39] Anyway, they're on the verge of a lot more understanding about sex roles, but they don't have it very strongly in their minds at age thirty-three months. But they are old enough to play interactively with other children. Younger children tend to play either by themselves or side-by-side, you know, where they're doing parallel play but not really interactive play. So we brought in either two girls together who had never met each other before, or two boys, or a boy-girl pair, and put them in a playroom. And then we would have a series of things. Like you would bring in two exactly similar toys and give one to one child and one to the other. Or you'd bring in one attractive toy, like something you could ride on, and then they had to take turns, or fight over it, or something in order to decide how to play with this toy.

[01:10:39] And we watched what happened. And it was simply astonishing to us how much more interaction there was in same sex pairs. When you brought in a boy and girl who'd never met each other before, quite often the girl would stand off at the side of the room and watch the boy play with things. She never grabbed—or I won't say never—very seldom. She would go over and stand next to her mother. And the mothers were there, but they were instructed not to interfere unless something was necessary. Stand next to her mother and sometimes cry when her partner was a boy. In fact, boys
cried more often when their partner was a boy than when she was a girl.

[Laughter] But for girls that was most outstanding. Two boys might each
grab hold of it and pull against each other and do tug-of-war, that sort of
thing. They were also—their activity level went right up.

[Interview paused and resumed]

**Maccoby:** So, in that last book I was focusing on a real sex difference which doesn't
appear really when you assess kids one at a time. This is a social
phenomenon. And apart from the work on sex differences, the last research
I did before I retired was part of a longitudinal study where we had three
cohorts of children whom we started working with the day they were born.
We gave them tests on how well they could hold up their head to see how
strong they were, that kind of thing. We measured strength. We measured
all kinds of stuff as they grew older, and repeatedly saw them in interaction
with their mothers. Occasionally, at certain ages, we got fathers in as well.
[01:13:18] And we were trying to trace development and to—we had sex
differences in our minds, but in addition, I got extremely interested in how
interaction between mother and child changes over time, and in what way
each person influences what the other will be doing six months down the
road. And I became firmly convinced that that is very difficult to measure—
when you watch a parent-child operating together, to measure the
characteristics of either one, because what you get is a picture that is a
function of both of them. Each one is responding to what the other just
did.
Now, I have to put that against the backdrop of the fact that if you were measuring people, as we were doing every five seconds—making a checkmark on certain aspects: what was their affect? Were they happy? Were they crying? Were they—so on. And other aspects of what they were doing. If you measure instant by instant, you find that the best predictor of what anybody's going to be doing at this particular moment in time is what they were just doing the moment before, because most of our behavior is continuous over much longer time spans than five seconds. But there is also an input from the partner. And what we were finding was that the way a mother behaves when the child is eighteen months old does depend upon the nature of their interaction six months earlier.

So if that child was resistant to being taught at twelve months old, the mother wasn't doing as much teaching effort when the child was eighteen months old. She'd sort of begun to give up on trying to influence this kid. And if the mother, however, at twelve months was a strong, well-focused guider of something the child was trying to learn to do, it turned out that the child was more task-focused six months later. So you could see over time each person was influencing the nature of the interaction that they would have, that each person would engage in, how they would engage in it six months later. Now, these get to be very complex things, those over time things, because you've got growth, you've got developmental change in the child, that even if you weren't concerned with the role of the mother in all of that, you would have to take that into account because there's going to be real change.
So it got to be very intricate kinds of analysis that you had to go in for, but I found it fascinating, and I became very much concerned and convinced about the role of each in influencing the other. And I always hate it when people focus right in on the mother's responsibility when the child gets to be like this or that. Well, you all know that the older stuff about the schizophrenogenic mother who wasn't very nurturant toward the child, it turns out you can't be nurturant with a severely autistic child because they just will not let you hold them close or anything like that. So I came away at the end of my research career as far as—over and above concern with sex differences and other intensely social issues like divorce—I came away with a focus on how parents and children influence each other over time. That became the focus of my intellectual interest during the last part of my career. I had some wonderful interactions with other people, and some of my former graduate students who contributed, I thought, really quite wonderfully. Mary Rothbart at Oregon became the nation's leader in assessing temperament in very young children, and finding out how that influences their interactions with others as they grow older. So to my mind, social contexts are the name of the game. And so that's why people have said, oh, she's in social development. She's not in cognitive development. [laughter] I think cognition has something to do with it.

Lewenstein: Would you say that your active research career ended in about 2000?

Maccoby: [01:18:07] Yes.

Lewenstein: Okay. So you were busy doing your research and working well into your eighties.
Maccoby: [01:18:15] Oh, yes. [laughter]

[01:18:50] Marion, there's one thing you didn't go into that I expected you to. And that is my role in the university outside the department.

Lewenstein: You're right. I did not. Do you have the time to do that now?

Maccoby: [01:19:03] Yes, I do.

Lewenstein: Go ahead.

Maccoby: [01:19:07] All right. When you got tenure as woman in those days, that puts you in a rare enough category, and the university was getting very sensitive about the fact that it didn't have very many women in tenured positions. They over-utilized us, shall I say. I got a little bit of notoriety around the campus because I was the first person, as far as I know, the first woman to teach in slacks rather than in a dress or a skirt which was—

Lewenstein: That's an intriguing one to be first in.

Maccoby: [01:19:45] Yes. My statement of something or other. But they began to appoint me to university committees. I was appointed to the committee on assessment, I remember, because I knew something about test taking and test making. I was on a committee to deal with the people who had been accused of—the faculty members who had been accused of sexual harassment, mostly with their graduate students, or with some younger faculty member. That was, to me, an absolutely fascinating thing.

Lewenstein: [Laughter] To learn about your fellow faculty members, yes.

Maccoby: [01:20:26] Yes. And the thing that struck me was that the men who were accused were absolutely unable to believe that their advances had been unwelcome. And they had a kind of quality of ego [Laughter] that was
remarkable to see in action. I was also on a committee to look over the
salaries of lecturers. And many departments had come to use lecturers—I
think this was often—maybe more often, too, in the—

**Lewenstein:** Language?

**Maccoby:** [01:20:59] —humanities than in the sciences. What were you saying?

**Lewenstein:** Frequently in the Language Department.

**Maccoby:** [01:21:03] Yes. Yes, in the Language Department—but other humanities
departments, too. The freshman English courses and so on. And these
people were abysmally treated, really. And also, there were women who had
appointments as research professors, let's say, or big time research associates,
who had fully the kind of publication record that the regular faculty had, but
simply weren't being considered for faculty appointments. Partly it would
happen when they were the wife of somebody who was a prominent person
in a field. And they were getting passed over for faculty appointments who
were absolutely well-qualified and already sometimes had been functioning in
the role of teaching in the department. So there was a committee that was
set up to try to rectify these things. I think one of the main locuses of that
kind of problem was in the Med School. And we handed over to a

wonderful Med School committee of women—of whom Doty Goldstein was
one, to see to it that these differences were ironed out. And we succeeded in

getting promotions for deserving women at the time. [01:22:35] This I found
to be a very rewarding activity, I must say. There was one funny aspect of

being a member of university-wide committees—oh, I know another one

that I was on, was what should be the fate of the Hoover Institution.
[Laughter] There had been a previous episode in which SRI, Stanford Research Institute, had been more a central part of the university and was split off. And this I believe, if I remember rightly, was because they wanted to take contracts in which their research results would be classified. And that was against the rules of Stanford. I think rightly against the rules.

**Lewenstein:** And there had been student demonstrations against Stanford because of that.

**Maccoby:** [01:23:25] That's right. There had been. And so the answer was to split off SRI. It was no longer to call itself the Stanford Research Institute. It simply used the initials SRI, which I thought was kind of a funny solution. But anyway—

**Lewenstein:** If you drive by that building now, just in the last couple months it once again is called Stanford Research Institute.

**Maccoby:** [01:23:46] It is?

**Lewenstein:** Well, the sign is there. I'm very confused by it.

**Maccoby:** [01:23:50] I'll be darned.

**Lewenstein:** I haven't asked anybody about it.

**Maccoby:** [01:23:52] Well, the committee that I was on was the faculty committee made up of people from several departments, it included David Kennedy from History, included Sandy Dornbusch. And that was a fascinating enterprise to me because I learned things about the Hoover that I had never learned before. One of the things that they were the most proud of and said was their great contribution intellectually to the university was their library, which had a collection of documents about the inner workings of a number of communist countries, states. They were particularly interested in those
things. And so we interviewed people who knew what the library had in these things. And it was a big eye-opener to me.

[01:24:46] One issue concerned appointments. Hoover people kept saying they are AT but not OF Stanford, and they make appointments without any consultation with the departments who might be concerned. And then when they appoint somebody, they want them to become a joint faculty member in the department. And some departments rigorously refused to take these people, and other departments accepted them happily. So it came to be a question of whether appointment through the Hoover then into the departments could be a kind of a channel that would in any way distort the nature of appointments, because the Hoover was offering a lot of goodies to support an appointment that was going to be partly theirs.

[01:25:35] And they would offer big computer possibilities, and summer institutes, and goodness knows what. So we saw some issues there. In that particular instance, the funny part that I've noticed in my other committees wasn't present. The other committees, they wanted always to have one woman on one of these university committees, one tenured woman, and one tenured black person. And there weren't very many of either. [Laughter] So Jim Gibbs became the token black. I became the token woman. And when we'd walk into the first meeting of a committee, we'd look at each other and burst out laughing. And the funny thing is, too, that we were both fellows at the Center for Advanced Study for Behavioral Science up on the hill the same year. And they elected a group of three to represent all of the people who were there that year and do various planning for events. And who
should be elected but Jim Gibbs and me, [Laughter] and one other person who was a wonderful legal scholar from Harvard. So we felt that we were tokens. We were. But that was part of our duty in a sense. Yes. So you had to do it, even if it interfered with other things you wanted to do in your life, it was necessary to be on those events, those things. And for some of them, you felt you were no more useful than others. But it had to be done, and we did it.

Lewenstein: That's the end of your tale of participating in the greater university?

Maccoby: [01:27:21] Well, are there any others? Can you think of anything you could ask me about that?

Lewenstein: No, in terms of relations, what I had concentrated on was your doing cooperative research, you know, with Mnookin and Wald. And I hadn't thought about administrative university-wide activities.

Maccoby: [01:27:44] Oh, I meant to say the one thing that came out of the Hoover thing that surprised me the most was to learn that ninety percent of the budget for the Hoover Library came from the general fund at Stanford.

Lewenstein: Hmmm. I didn't know that either.

Maccoby: [01:28:00] It was one of the things that had come up when we entertained the thought that maybe Hoover should split off in the way that SRI had because it had its own agenda and it wasn't subject to the rules of tenure and appointment and all those things that applied through the rest of the university. It was at Stanford, but not of Stanford at that time. I think the links have become closer since that time.
**Lewenstein:** Yes. There were, as I recall, a time when Don Kennedy was involved and got the rules changed in terms of faculty appointments.

**Maccoby:** [01:28:35] There have been some changes, yes. Yes, there have been.

**Lewenstein:** That's my memory anyway.

**Maccoby:** [01:28:40] Right. But this was before those changes. And maybe some of them came out of our committee report. I don't know. But the Hoover people would always say, if the very idea of a split came up, you would lose our library. We would take that with us. And then we suddenly realized, wait a minute, we've been paying ninety percent of the budget, why don't we just keep your library. [Laughter] That didn't go down very well. [Laughter] Okay. End of story.

**Lewenstein:** All right. We are now ending this interview. [Laughter] You can't think of anything else I neglected to pursue? [Laughter] All right. Again, thank you, Eleanor Maccoby. This is Marion Lewenstein.

[End of Interview with Eleanor Maccoby]
Eleanor E. Maccoby
Curriculum Vitae

Education
Undergraduate:
Two years, Reed College, Portland, Oregon
Two years, University of Washington, Seattle, Washington (B.S., 1939, Phi Beta Kappa)
Graduate: University of Michigan, Department of Psychology (M.A., 1949; Ph.D., 1950)

Employment
1958-1987  Professor of Psychology, Stanford University
1973-1976  Chairperson, Department of Psychology
1950-1958  Harvard University, Instructor, then Lecturer, Department of Social
  Relations, and Research Associate - Laboratory of Human Development
1942-1947  Survey Research Center, University of Michigan, and its predecessor
  organization, the Division of Program Surveys in the U.S. Department of
  Agriculture. Study Director.

Research Interests
Socialization of the child; gender differentiation in childhood; parent-child interaction in a
developmental context; family structure, social change, and public policy; vis-a-vis children
and families.

Awards
Fellow at Center for Advanced Study in Behavioral Science, 1969.
Elected to American Academy of Arts & Sciences, 1974.
Elected to Institute of Medicine, 1977.
Barbara Kimball Browning Professorship, Stanford University, 1979.
Walter J. Gores Award for Excellence in Teaching, 1981.
G. Stanley Hall Award, Division 7, APA, 1982.
AERA (American Educational Research Association) Award for Distinguished


Andrew W. Mellon Professorship in the Humanities, Tulane University, spring 1990.


Jesse and John Danz Lectureship, University of Washington, 1990.

Alumna in Residence, University of Michigan, 1991.

Distinguished Publication Award, Association for Women in Psychology, 1991.

Elected to the National Academy of Sciences, April, 1993.


American Psychological Foundation Gold Medal Award for Lifetime Achievement in the Science of Psychology, 1996.


**Activities**

Member: Committee on Genetics, Health & Behavior, Institute of Medicine


Member: Governing Board of the Social Science Research Council, 1979-1985. (Chair 1984-85).

Member and Vice Chair: Committee on Child Development and Public Policy, National Research Council, 1977-1983.
Member: Institute of Medicine, Section 11, 1977 to present.
Member: American Academy of Arts & Sciences, 1974 to present.
President: Western Psychological Association, 1974-1975.
President: Division 7 (Developmental Psychology), American Psychological Association, 1971-72.
Member: Committee on Socialization and Social Structure of the Social Science Research Council, 1962-1967.

Books
Monographs

Chapters and Papers


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