Psychology's Feminist Voices Oral History Project

Interview with Mary Esther Brown Parlee

Interviewed by Alexandra Rutherford and Jacy Young
Somerville, MA
May 24, 2017

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Interview with Mary Esther Brown Parlee*
Interviewed by Alexandra Rutherford and Jacy Young
Somerville, MA
May 24, 2017

MP: Mary Parlee, interview participant
AR: Alexandra Rutherford, interviewer
JY: Jacy Young, interviewer

*This transcript was undergoing editing by Mary Parlee when she died suddenly on June 27, 2018. It includes all of the edits she made up to the time that she died. These edits are indicated in **bold**. In many cases, the edits reflect Dr. Parlee's concern to clarify and polish her speech. In other places, there are additions and asides. These are placed within [square brackets]. This transcript therefore does not exactly correspond with the video recording. Many thanks to her daughter, Dr. Elizabeth Parlee, for retrieving this edited version from her mother's computer.

AR: Ok, now we are recording. If I could ask you to say your full name and place and date of birth for the record.

MP: Mary Esther Brown Parlee. I was born in Oak Park, Illinois February 11th, 1943.

AR: Great. So, thank you so much, and again thanks for agreeing to sit with us today.

MP: I look forward to it.

AR: Yeah! One of the ways we typically get started… this interview will go back and forth chronologically, it is not going to be a straight line, part of it is [that] I want to learn - *we* want to learn - more about your life in general, but also, we obviously have some questions about your specific work as a feminist psychologist.

MP: We were just about to get to something, I forget what it was.

AR: Sure.

MP: Oh! People who are very involved in activism – did you get Leonore Tiefer?

AR: Yes.

MP: Oh, ok.

AR: And I actually want to talk to you about some of the stuff that she has been working on and how you see it in terms of the processes that unfolded around PMS [premenstrual syndrome].

MP: **In fact, I was just doing an email to her when you came. We are still in touch.**

AR: Ohh! Good, good. Well, let us definitely talk about that. One of the ways we like to get started is actually just to have you speak about your relationship to feminism, sort of how did you become a feminist?

MP: Ok. Well that is a word of course, that has many different meanings now than it did back then.

(File 1 - 1:25).

MP: But, I was a graduate student at MIT [Massachusetts Institute of Technology] and got my degree in 1969, and [during] the four years I was there, the graduate student population was [approximately] 3% women. That has since changed, but that is what it was then. But my department had about 50/50 [men and women]. Most of the people in that program either left or flunked out, but three of us finished, and I was one of them. Marty Steinbach, actually, was one, he was in your department at one point…

AR: Yes!

MP: Yea.

AR: Of course! Oh, interesting! So, there were 50/50 women and men in experimental psychology?

MP: Well, it was a new program, and it had graduated maybe two people. It was very, very experimental, and it was not a typical psychology department, it did not take typical undergraduates.

AR: Okay.

MP: **Almost no one had a psychology undergraduate degree back then, they had science degrees, and the department aimed to focus on physiological psychology and experimental psychology and psycholinguistics.**

AR: Okay.

MP: **And when I finished and was looking for a job — I was stuck in the Boston area because my husband was a medical resident — my thesis advisor (who was Marty's theses advisor too) said, "Well, why don't you teach one course out at Wellesley?" I didn't even know you *could* teach just one course, I had always seen people teaching full-time. So I went out there and found one, and when I told my advisor they had offered me a job he said "well, it's probably the best you can do as a woman." Now, the guy I was sharing an office with, Don MacKay, got his degree a little before me and the department chair said to him, "So, Don, what part of the country do you want to live in?" Don said, "the West Coast," so he picked up the phone, called UCLA, and got him a job. He got tenure within 5 years!**

AR: Right, right.

MP: **When I went to Wellesley, a good friend and mentor there was Laurel Furumoto. She told me that when she finished [her degree] her thesis advisor at Harvard said "I'll call Wellesley and see what they have for you."**

AR: Wow.

MP: And that is how it was (chuckles). But I was assigned a course in the psychology of sex differences, and since I had only had experimental psychology in the past, and no psychology courses like that, I didn't know anything about it, but by that time feminism was in the air, sort of.

AR: Mhmm.

MP: I was not happily married and not happy teaching at Wellesley, so I was interested intellectually because the literature there - I used to say, 'it was like a sitting duck for a feminist with a good scientific training' - it was just bad. And so much of it, in interesting ways, was well, was just not true. So that was sort of the intellectual impetus. But I actually started feeling like an activist [when] I did volunteer work

as an undergraduate at the Metropolitan State Mental Hospital, which was one of those big, old warehouse-y, horrible things.

(File 1 – 4:40)

MP: But there is something in Cambridge, I do not whether it was elsewhere, too, called the Mental Patients Liberation Movement. And a friend of mine, Nancy Henley, who became a good friend during those years, and she was very active with that group. They put out a paper called, 'On Rough Times', which was sort of the voice for the Mental Patients Liberation Movement.

AR: Yep.

MP: And then that became radical therapist, and Paul Brown was a person who took some of those same issues about psychology's role in oppression, and so that is what got me interested in activism, and feminism felt like that.

AR: Ah.

MP: I was also in a consciousness-raising group, which was wonderful.

AR: Oh wow.

MP: But it was as a social movement, my way was through that.

AR: Okay. So really, it was through your involvement in patient liberation stuff, and then kind of also being asked to teach the psychology of sex differences course.

MP: And seeing how the patient liberation movement involved a lot of psychology and the role of psychology, and that was oppressive. So, it was sort of easy to make the transfer.

AR: Yeah.

MP: New York was very active with radical feminism at that time, but Boston - we had the Boston Health Book Collective and my consciousness-raising group had one of those people, and I have one of the first newsprint versions of *Our Bodies, Ourselves*. It was an exciting time.

AR: Yea. Do you… you are welcome to jump in at any point (to Jacy). I hope that our sound is [okay], you have a soft voice.

MP: I will try to speak up, I have been told I have a soft voice.

AR: I'm worried. We do not use a lapel mic because this has a pretty good microphone, but…

MP: My husband tells me, he is slightly deaf, but he tells me to speak up. I'll try to speak up.

AR: (adjusting recorder) I have so many different threads from what you just said that I want to follow up on, but I wonder if we could go back just a little bit before your PhD work. You were an undergraduate at Radcliffe.

MP: Mhmm.

AR: That was immediately before you went into your PhD?

MP: Yeah.

AR: What were your experiences like there at Radcliffe?

MP: Well, Radcliffe, when I was there was a very different [place] (now it is [called] Harvard-Radcliffe), but when I went, there were like 300 people in a class, so it was very small.

(File 1 – 7:09)

AR: Yeah.

MP: There was no faculty, no Radcliffe faculty, but we were admitted to Radcliffe and lived in Radcliffe dorms.

AR: Okay.

MP: **And we went to Harvard classes and it was a Radcliffe degree until the early 1960s.**

AR: Right.

MP: **The year before me was the first year Radcliffe students got Harvard degrees *countersigned* by the president of Radcliffe. We had separate graduations, so it was quiet different. And I did not like Radcliffe. I remember one time I wanted an administrative issue resolved for me and they were not very happy. They [Radcliffe] were just not my intellectual home. And friends! Because I was a a biology major (having switched from Biochemistry) it meant I had four labs a week, and all my friends practically were males. It was different than being and English major living in the dorms where you had female friendships. I kind of missed out on that, but I know now because my friend up here, Susan Milmoe, was one of those and we often compare the differences. But I had a really good scientific education. I was always interested in psychology — in high school I read all the books on stress in the public library. But when I told my biochemistry tutor I was interested in the biological basis of mental illness he sort of laughed because that was the heyday of psychoanalysis. You just did not [think about biochemistry and mental illness together]…**

AR & JY: [chuckles]

MP: **But I did take two courses on mental illnesss, one was psychoanalystically-oriented and one was biologically-oriented (that Brendan Maher taught).**

AR: Oh yeah!

MP: **Maher taught [a course called] the psychosomatic basis of pathological behavior. It was a great course! And Robert White taught his course called (I think) Abnormal Behavior. I took them side by side and was really interested in that comparison [of the different approaches]. So, actually I took experimental psychology, statistics, and a lot of math. I completed the requirements for a psychology degree, but I had done a biology thesis. [Added later by MP: I did it at Massachusetts General Hospital with neurosurgeon Frank Ervin; he implanted electrodes in a cat's hippocampus and we looked for changes in brain waves during learning.]**

AR: Ahhh. Okay.

MP: **So that was my degree, biology. I never really felt like a psychologist, I always thought of myself as someone interested in that [i.e., psychological] subject matter. But I didn't identify with the discipline — I think you don't think in terms of disciplines as an undergraduate, I didn't.**

AR: Yeah.

JY: And how did you end up at MIT then?

(File 1 – 9:43)

MP: **Well, the person who taught physiological psychology at Harvard the year I took it was Charlie Gross who was visiting from MIT. He went to Princeton after Harvard, he was not at MIT when I was there, but he told me about the program [and] suggested that I apply. Interestingly enough, and I think this was actually formative for me, the summer after college I worked at the Center for Cognitive Studies which [Jerome] Bruner and [George] Miller had set up. I worked for Norman Mackworth who was a vision person. But Richard Herrnstein somehow got wind of the fact that I was going to this new program — I had taken two of his experimental psychology classes, so he had taught me and was aware of the fact that I was going to MIT. And he took me into his office and he said, "Now, you're going into physiological psychology?" and I said, "Yes," and he said, "Well…when you know about the underlying brain activity, what more does it tell you about the *behavior?"* And I thought to myself, "What a dumb question to ask," but actually it proved to have a very profound effect on me. I now see cognitive neuroscience in pretty much those terms: "If you learn your behavior changes, and of course your brain is going to change because there are correlations between brain and behavior — the causal question is still open." So, it is much more complicated than the newspaper version of cognitive neuroscience, and I think the scientific version begs the same question.**

AR: I think that question remains a pretty good question.

MP: It does, it does. **Herrnstein** was a strict behaviorist in that respect.

AR: Yeah, it sounds like a very Skinnerian question.

MP: **Yes! But it was kindly meant. [He wanted me to think about] What does it mean?**

AR: Yeah. So, Charlie Gross recommended MIT. Who did you work with at MIT?

MP: Peter Schiller for one unhappy semester. Then Whitman Richards, who is now a computational neuroscientist. Then I went to Dick Held. He was doing some very interesting work on sensory motor coordination, raising baby monkeys without sight of their hands, and sort of classic experiments. That is actually where I met my now-husband. He was sort of the inside person in the lab. We were married to different people at that time, and his wife subsequently died. But, Dick was good because he left you alone, but he also had weekly meetings and they were very stimulating lab meetings. But I think, at the time I knew, I was trying to choose between three strong people in the department. Walle Nauta, who was a world-renowned neuroanatomist. Remarkable, very wonderful man. And Jerry Fodor, who was a philosopher and he had some psycholinguistics. I liked both of them. I actually did very well in neuroanatomy, and I thought about that, [but] I did not want to kill mice (laughs).

AR: Yeah.

MP: And so, I thought experimental psychology is the middle, in a way - it does not have the advantages of either, but that is what I did.

AR: So, I noticed at some point that you mentioned a minor in linguistics.

MP: Yes.

AR: So, was that the influence of Fodor?

MP: Well, no, that was Chomsky's heyday.

AR: Ahhh.

(File 1 – 13:06)

MP: He had just come out with *Syntax* **[Syntactic Structures]**, well, not JUST, but it was still, that was the new kid on the block in linguistics and [was considered] very controversial. I audited a course that he gave on the sound patterns of English. But the courses I took for credit were with Morris Halle, who is a phonology guy, and such a nice man, a *good* teacher, and he just explained the linguistic theory as it was at that time. And I had heard Chomsky, as an undergraduate, give a talk at the Center for Cognitive Studies on stress patterns in English, and I thought that was wonderful. That with a few rules, you could get all this complicated stuff. So, I still am still interested in language but in a different way.

AR: Yes, that certainly comes through in a number of your publications.

MP: It is a paper I wish I had published (chuckles) - [on] language, yeah.

AR: What was your dissertation research on?

MP: I think there was a fight between the department chair and Dick on what I should call it. It was on the perception of… (reflecting on the topic) [it] sounds silly [now]. At that time, it seemed as if in different animal species - maybe it was the rat (I do not know), and the goldfish and then people – [that] the underlying mechanism for locating an object in space is separable from the mechanism for form figure analysis. Everybody thinks this [now], but then it was new. And so, we were trying to see whether you could show **[there were two separable visual systems in humans]** by having people point to the **[ends or the middle of]** a line **[which bent in the middle at an]** angle **[that]** could be adjusted, then **[point to the same locations]** if you had [**only]** dots there. You could not see your hand **[when pointing, but the locations were]** the same **[when]** you were pointing to a dot or to a line **[stimulus]**. And, it turned out, you **[did not point to the same locations].** was, at that time, very exciting to see so many different paradigms converged on that idea.

[Added later by MBP: I cleaned up the description of the experiment a bit because the spoken version was so vague; the key finding of my thesis was never published for reasons I won't go into, but another part of it was.]

AR: Right.

MP: At Wellesley, there were no facilities for research. Zero. They closed the library in the summer! I mean, it was death.

AR: A couple of questions about that, but I am also trying to reconfigure in my own head the time that you were working. You came just after Naomi Weisstein would have come through, I take it? She did her PhD…

MP: At Harvard!

AR: At Harvard, and you would have been an undergrad at that time, so those worlds would not have collided at all.

MP: No, I am trying to think where I first met her. We used to correspond. I read all her early work and she was in this rock band. She called me when I was living in South Carolina about the menstrual cycle stuff and she said, 'you know, I wonder if Stanley Schachter's work is really what is going on? You know, with the labels.' This was in 1973.

AR: Oh wow.

MP: And so, she was very acute.

AR: Mhmm.

(File 1 – 16:45)

MP: I saw her when I was directing the Center for the Study of Women in Society at The Graduate Center, she needed a place to be, so I was able to give her an office. She had a site visit that was just… (trails off). When you are equipment-dependent and you are trying to make do, it is tough.

[Added later by MBP: as the Center's Director I later asked her to give a big public lecture on her work as a feminist in psychology; she was terrific! Smart, funny wise; I suppose there's a tape of her lecture somewhere at the Graduate Center.]

AR: Yeah.

MP: I do not know if she got that grant or not, but I saw her the last time professionally in that setting, and then I saw her after she became ill.

AR: Yeah. So, the move to – well, the move. The time at Wellesley was clearly not a happy (laughs) time. One reads about the first generation of American psychologists being funnelled into the teaching colleges, but clearly this extended forward in history for quite a while, it sounds like.

MP: Yeah.

AR: Yea, I am still astounded that…

MP: Well, the one benefit was Edna Heidbreder had just retired. I had taught seven different courses at Wellesley in a very demanding **[teaching environment with bright students and high expectations].** If you want to kill a research career, that is the kind of job to get. But one of the courses I was assigned and books [that] I used was Heidbreder's *Histories and Systems* **[actual title is Seven Psychologies]**. Now that is a wonderful, wonderful book. Just wonderful. And that was really when I started…this is when I learned psychology - all the courses I had to teach, but also to [able to] see women's historical context. She was a remarkable intellect.

AR: Yeah. Everything I have read about her, people say that that book is just a real classic.

MP: She makes [points] people should notice today. She distinguished between action and behaviour, which philosophy – I took philosophy with Jerry Katz and semantics – but she distinguished between actions and behaviours, which cuts right to the heart of conceptual difficulties in psychology.

AR: Yeah.

MP: Systems and theories. She was retired, but she did not come in to the department. She had a great deal of integrity. She was not going to be this hovering presence, but she used to read down at the public library at Wellesley, so I would see her in the stacks sometimes.

AR: And, you were asked also among all these courses to teach the psychology of sex differences.

MP: Yes.

AR: This was pretty early in terms of teaching anything about psychology of what we would now call gender, but was then called something else. What resources did you draw on? Do you remember?

MP: I think it was part of the individual differences field in psychology. I think Ann Anastasi had a book. Did she have a book? I don't know.

AR: Possible, yeah.

MP: It was some book. Maybe there were special issues of journals, but this was an undergraduate psychology major honours seminar. So, we could not really bring in literature that could (inaudible).

AR: Yeah.

(File 1 – 20:00)

MP: So, whatever it was… Maccoby and Jacklin had not come out yet, so.

AR: No.

MP: I think Anastasi had a chapter **on sex differences** in one of her differential psychology books.

AR: Okay.

MP: And that was when I mentioned Horner's *Fear of Success* was big. And one of the faculty, Thelma Alper, made a big deal of it, and a lot of her students studied that.

AR: Oh really? Well, let me ask then about 1969, the year you got your PhD, but it was also the year the Association for Women in Psychology was formed. And I wondered - you are starting to teach the psychology of sex differences, you are recognizing that there is a lot of feminist critique that could take place here, and AWP is starting. Were you becoming aware of that?

MP: Yes.

AR: Ok. So, tell us.

MP: It would have been Nancy Henley…. The first time I went to an **APA** meeting was in Toronto.

AR: Okay.

MP: (inaudible) You could look it up, I do not know what year it was. It must have been when I moved from Wellesley to South Carolina, so it might have been '71, '72. About then.

AR: Okay.

MP: And we all heard… (trail off). So, the AWP used to **(**inaudible**)**

AR: Okay.

MP: And people would sleep on the floor in sleeping bags (chuckles) **(**inaudible**)**

AR: Yeah.

MP: Katherin Grady **[and Nancy and others (I forget) organized meetings in the hotel, word somehow got around.**

[Added later by MBP: see Leonore Tiefer's brief history of AWP's early years, PWQ, 1991.]

AR: Mhmmm, yeah. I know the name from looking in the archives of AWP. They also have papers at Schlesinger, but I never… I do not know of her.

MP: And some others…(Inaudible) Subsequently got involved in Division 35. I have always been more of an activist, **[don't like working for change from within]** (inaudible) in institutions **[though that's] v**ery valuable (inaudible).

AR: And you have written about some … Can I just stop this for one second? (stops recorder to address background noise)

[END OF FILE 1 -22:08]

FILE 2 BEGINS

AR: Ok, so now we are recording again.

JY: Yes.

AR: Okay.

MP: And it might have been at that APA that we heard Ethel Tobach had been refused entry into the bar when she went with Charlie Gross and all her Division 6 colleagues. She was refused entry! So that was a source of outrage.

AR: I cannot see Ethel putting up with that. (laughs)

MP: I have much experience with Ethel. But Nancy and Bob Brannon, **[Cathy],** I, and maybe one other person (I am sorry if I forgot her), we did a statement on sexist language. I mean, the big thing was to get some statement **[from AWP]** in the general meeting. And actually, that was, as you probably know, the focus of a lot of the early work, was to get the journals to be edited anonymously. Yea, anonymous review.

AR: Masked review?

MP: What is it?

AR: Masked review.

MP: Yes, yes. And, not to list in the index of the authors and the bibliographies. They used to list the initials for the men and the whole names for the women.

AR: Right, right.

MP: Just little things like that. Really irritating (chuckles).

AR: Yeah.

MP: And I think some people did that business of submitting two papers with a masculine and a feminine name. Wendy McKenna was active in … and Sarah somebody… in documenting some of the differences between the way men and women were treated in psychology. Of course, APA used to meet on Labor Day weekend, which in the United States is right before the school year. I mean, what a … (trails off).

AR: Yeah. There were a lot of problems… (laughs)

MP: There were a lot of problems!

AR: -that were gender-based, yeah.

MP: Yeah. But AWP [Association for Women in Psychology] was always more linked to activism than the science world.

AR: Yeah. Can I ask then about how your own feminist, for lack of a better word, consciousness or feminist involvement started to then directly impact the kind of work you were doing?

MP: Well, I mean, I think I said in that paper I sent you that there was an article in the *New York Times* headlined, 'Doctor Asserts Women Unfit for Top Jobs Because of Raging Hormones', and I knew enough about hormones, and I knew enough about [the topic to think] that this was crazy! And I had written a critique of Broverman et al.'s thesis about gender differences in cognition being the result of hormones, which was just nuts. It came out when I was in graduate school, and one of the male graduate students said, 'You should be sure to read this', and I did not, of course.

(File 2 – 2:56)

MP: I critiqued it later **[in Psych Review]**. So, that was a topic that I knew about, I was mad about. At Wellesley, the one thing that they did give me - Claire Zimmerman was the department chair, and she gave me the assistance of a student that they had supported by a NICHD or whatever, a fellowship.

MP: And so, Ellen Zimmerman went to **[Harvard's]** Countway Medical Library on her motorcycle (laughs), as I recall and Xeroxed everything from the journals that she could find on that, and she did an experiment **[on sensory motor changes during the menstrual cycle]** as her senior thesis.

AR: Okay.

MP: And I had all this literature review, so when I left Wellesley and went to South Carolina, I had this trove of stuff, and I wanted to make some order out of it, and that is when I did the first review paper. Barbara Summers had done a paper before – I think hers came out before mine – it was of a different sort, but they were both sort of the same time.

AR: Right.

MP: And that was important because before that, the only paper on the premenstrual thing **[in APA journals]** was Georgene Seward in 1948 or so.

AR: Mmhmm, that 'code of menstrual invalidism', as she put it.

MP: Ah!

AR: Yeah.

MP: I have to go back and re-read it!

AR: Yeah.

MP: Well, Leta Hollingworth did.

AR: Mmmhm. Functional periodicity. Yea, it has been…

MP: Well, then I got interested in rhythms in men because there is some data about beard growth. It is hard to do – it is hard to get funding for, but even if you can, questionnaires are not sensitive. I mean, it was hard to get the right instruments.

AR: Yeah.

MP: I think Alice Dan was the most successful in that.

AR: Okay.

MP: I corresponded with a lot of people when I was in South Carolina because there was nobody really to talk to. Although, I did have a position at the Social Problems Research Institute very generously given to me by the director Robert Heckle. I, of course, wanted **[to stay in touch with feminist psychologists, so I corresponded with** Naomi and Nancy and Carol Jacklin and Anne [**Peterson and Alice Dan and Karen Paige and Barbara Somer and Julia Sherman, and, though maybe this was later, Randi Koeske, and others.]**

AR: Right.

MP: We were talking about details of research, and I think Alice was the most effective in finding a way to study… I used the… (trails off trying to remember). I forget the questionnaire. I really have! **[Added later by MBP: it was the Thayer Activation-Deactivation Check List]**

(File 2 – 5:32)

AR & JY: (laughs)

AR: This was not the MOOS one? That came later.

MP: No, no. This was (inaudible) Mr. David.

AR: Oh.

MP: Yeah, I should remember it. It was a measure of moods. Alice had a more global measure, which she would do daily **[to get at something]**, like, "stress". And that seemed to actually be more responsive over time.

AR: Okay.

MP: Well, **[it seemed to]** track the menstrual changes better. So, those were exciting times. And if I had had the ability to have a research position after that, I would have been golden. I did get a grant**;** I applied for and got, an NICHD grant during those years. Actually, I **[left Wellesley in 1972 and]** went to South Carolina, because my husband was **—** I am trying to think of what year [it was]- he was like a **[third-year medical student** when we got married. At that time, doctors were drafted, but they could be deferred through the Berry Plan, which meant that if they went into a specialty **[they would]** be drafted **[at the end of their residency]** in that speciality — he was a radiologist. So **[we learned where the Army would send him only]** three months before the assignment was made, too late for me to find a place. They sent him to Fort Jackson in **Columbia,** South Carolina.

AR: Okay.

MP: Which does have a university, and there was the Social Problems Research Institute, so I was fortunate in that respect. And he typed my first grant proposal (chuckles). There was time pressure, we had a baby!

AR: (chuckling) Yeah!

MP: He kindly typed it. I had typed his autopsies in medical school, so I figured he owed me.

AR: It was payback! (chuckling)

MP. So, I did have a grant, which I could defer a year. When we came back to Boston, I was offered a post-doc **[in the Dept of Psychiatry at Harvard Medical School]** with the help of people, really supportive people, Helen Mahut being one of them. She was a student of Frank Beach. No, not Frank Beach…Donald Hebb! And [she] was on the faculty at MIT when I went there, but then was let and go and she went to Northeastern and had a very productive career as a comparative psychologist. And she was a French lady with great style and verve, and she helped connect me with a source of a post doc fellowship, or I would not have known what to do. But after I got that, I was offered a place at UMass Boston, [which] was just opening their campus. I did not want to teach, so I took the risk of one year versus three.

AR: Right.

MP: But I did have this grant that started in advance and I could postpone it a year. So, when I was recruited to the **Psychology** Department **at Barnard** by Lila Braine…

AR: Ohhh yes!

MP: She was a wonderful person. Yea, and she was a real feminist and meant it.

(File 2 – 8:35)

AR: Yeah.

MP: And, so I took my grant with me.

AR: Ok, we've interviewed Lila Braine for the project!

MP: When did you do that?

AR: This was years ago.

JY: Maybe five years ago? Maybe even more.

AR: When you do a bunch of these, you start to see patterns, of course, and she talks about working in the lab at NYU [New York University] and thinking it would be a good thing to get some teaching experience, and asked if she could teach and the department chair said, 'yea, you can teach developmental'. And she said, 'well, I have never taken developmental.' 'Well, you are a woman, so you will learn it.'

MP: Really?! Who said that?

AR: The department chairman said to her, 'yea, you are a woman, you can learn how to teach developmental'

JY: It will come naturally.

AR: So that was one of her pathways into it. I am interested in the ways that women who are in very experimental 'hard science' areas of psychology, end up 'being funneled into' [these types of more feminized disciplines]. I think that it is a combination of things, but these things happen, right? So, you get asked to teach the psychology of sex differences, even though that is not your field.

MP: Right.

AR: And then that begets other interests, and so on. But Lila says that part of the reason developmental was attractive to her was because she went to meetings with developmentalists and many of them were women, and she found it a much more congenial atmosphere than the all-male physiology meetings she was going to. She was like, 'I actually felt comfortable talking'. And so then why would you not want to then pursue that, right?

MP: Yea. The person who was brought in to found the psychology department at MIT was Hans-Lukas Teuber and he had had a lab at NYU.

AR: And she was working there.

MP: And she was working there. So, when he wrote a recommendation, she had some context from that.

AR: Yea. There are so many interesting connections. I know that you have written extensively about this, but part of the project is to have some of this on tape, partly so that students can connect the people who are doing this with [their work]. So, would you mind telling us in a bit more detail about what you were finding when you reviewed the literature on what was just beginning to be called premenstrual syndrome? I think a lot of students these days, never knew that there was any time there was not such a thing as premenstrual syndrome.

(File 2 -11:05)

MP: Yeah.

AR: So, if you could maybe talk a little bit more about what you were actually finding and what your critique of the research was, and so on?

MP: Oh sure. Well I did look back a little bit into the '40s. Before, in the '20s, '30s and '40s, when women talked about menstruation, they were talking about menstruation, and they had a different vocabulary than men, and women learned theirs from women, and men learned theirs who knows how. But then, I think it must have been obstetricians and gynecologists got their hands on this topic. I mean, what I found was most of the articles would begin in those days, 'It is widely agreed that…' As soon as you see that, you *know* it is *not*. I actually learned that from Marty Steinbach (laughs). His thesis – he studied eye movements in owls - and all the articles until his time said, 'it is widely known that owls do not move their eyes, even if you use pliers', and they would repeat this phrase over and over. Yea, 'it is widely agreed that…'

I said, well, show me the data! And one set of data was reports of airplane crashes by women pilots at the time they were said to be menstruating. Some of it was Catherine Dalton's work. At that time even, the Women's Menstrual Distress Questionnaire had come out and that was probably the first systematic effort, which is fine, but it was contaminated by the fact that some of the women were pregnant! (My memory for that kind of thing is kind of weird). So, there were not too many studies of changes over time.

And so, when I looked at the literature that Ellen had gathered, it fell into three methodologically distinct categories, and that is actually a helpful way to distinguish because you get different kind of knowledge from those different methods. And so that was sort of the contribution I made, [which] was to clarify what there was (it was a thorough review), and what the differences were among the literature that was available. And I concluded that there was no good scientific evidence that mood changes are related to premenstrual phase of the cycle, but many women spontaneously say there are. And, so that does not mean it is not real. I said it more tactfully [than that].

But after that, I have always judged people **[by whether they]** would **say**, 'you said it is not real!', **[when]** all I said was 'there is no scientific evidence.'

AR: Yeah.

MP: Subtleties are not always appreciated. Or, honesty is not always appreciated.

AR: Yeah.

MP: I got over 500 reprint requests, and that was in the old days in South Carolina. So, I paid for the postage, mailed them out. It was reprinted seven times, eight times - it was an influential article. And Barbara Summer had an article on [this subject, though] I think hers was on **premenstrual** changes in moods, and that was also an important article.

AR: Mmhmm.

MP: I do not know how Alice Dan got into **menstrual research, but before she and others founded** the Society for Menstrual Cycle Research, it was not important.

AR: Yea. When did that Society start?

MP: Well, I think that paper I sent you was the twentieth anniversary

(File 2 – 14:53)

AR: Okay.

MP: So, I do not remember (leafing through papers). Oh, that was the twelfth conference. So, it was the twelfth conference in 1997.

AR: Okay.

JY: So '85?

AR: '85, yea. And I think they have just started their own journal, have they not?

MP: Oh, I bet.

AR: Yea, Joan Chrisler has been involved. Can you talk a little bit about the transition from speaking about menstruation in terms of changes and so on, and then talking about PMS?

MP: How did that happen?

AR: Yea. (laughs) It is a big question.

MP: Well you see, when I sent the article to *Psych Review* that I had written about the literature, I put premenstrual syndrome in quotation marks because it was an odd and unusual concept given the evidence, and they took it out. And I think when the *New York Times* headline was talking about the raging hormone influences in the menstrual cycle, that may be where premenstrual syndrome attention came in. Things become a 'syndrome' when they draw the attention of the medical community.

JY: Mmhmm.

MP: So, 10 years later at a conference in Kiawah Island in South Carolina, the medical gurus funded by whatever drug company 'discovered' PMS, this terrible problem that had been neglected, and now we are going to treat it. And so, they knew nothing about the history of the research, [and] could not have cared

less. I think what drove PMS for sure **[was what I described in]** the article 'The Good Body'. Do you know 'The Good Body'?

AR: Yes.

MP: That is the one that I think laid out most of **[my thinking about how PMS became a Big Thing].** What drove it was the economics of fighting between obstetricians and psychiatrists over who would get this insured patient population that is not really very sick. And drug companies wanted to market Prozac as a treatment for this. I would do literature reviews time after time, [the drug companies] would come out with these trials of **Fluoxetine** and there was no difference between the **[placebo]** control and the experimental group. Again and again they would find no difference, no difference. But they were determined to get that, because that would expand the market enormously.

So, I think the economics **[of drug company efforts to make markets]** and the **[turf]** fight between gynecologists and psychiatrists **[were key]. Gynecologists already had a definition of the problem in** the ICD [International Classification of Diseases] definition, which was different from the psychiatrists' which more mood symptoms **[best diagnosed by a mental health professional.]**. **[The scientific evidence supporting a definition was secondary to its usefulness,] a**nd that was a very dispiriting thing to be involved in. Very dispiriting because it was so irrational, so irrational.

(File 2 – 17:52)

AR: Well, you have written very eloquently about all of these different forces coming together, and also, of course, another thing that happens in all of this is late luteal phase disorder in the DSM.

MP: That is just another name they did not want to get right (laughs).

AR: Exactly, yeah. And you were asked to be an advisor on the working group.

MP: The first time they tried to put that in as…

AR: LLPD.

MP: Right (laughing). That was in '83 and they introduced it with two other controversial diagnoses. I do not know if you remember that.

AR: Paraphilic coercive disorder?

MP: Something that is like wife battering but the battered wife brings it on herself kind of thing…

AR: Self-defeating personality disorder

JY: Yes, yes!

MP: That is the one, yes, and something else. So, it was three controversial diagnoses, and they got a lot of activist stuff. I was not involved so much in that one, but the next time around, they wanted to avoid the public controversy, so the whole thing was going to be done on the basis of the literature. **[They set up a committee of]** APA [American Psychiatric Association] members, and a board of scientific advisors for each diagnosis to review **[the scientific evidence and]** decide what it showed. And Paula Caplan has written about that. She got discouraged before the end, but I stuck in there and actually **[was invited to do the]** commentary when the results of the literature review were presented the APA convention. It was just such a bunch of hooey. I mean, there was nothing that showed (now, this may have changed)…**that a bunch of symptoms cluster together and change during the premenstrual phase of the cycle.**

Sally Severino, she was not a psychologist, she is a psychiatrist, but she was on the committee (I was just an advisor), and was one of the authors of a journal issue that was published after the diagnosis had gotten into the main body, which was the goal of the Diagnostic and Statistical Manual of mental disorders. She and I were just on the same page and very discouraged, we both just walked away because a syndrome is a collection of symptoms, and all they showed in this **exhaustive** literature search was that Prozac helped some of the symptoms, but we knew *that*. I mean, they did not show anything, they did not find any treatment for the syndrome, they found a treatment for some of the symptoms, which they already knew. What it gets them - to lump them together as a syndrome and then market PMS - is more money, but it does not actually do very much scientifically. Or clinically. I mean if a woman feels yucky and Prozac helps, then prescribe it. You do not have to have this pathologizing label, which can be disabling, and it has some dangers in court cases and custody cases. It is not a neutral thing.

AR: Right. You have written about this, and I think it is something hard for students and anybody to really understand - when something is socially constructed, it does not mean it is not real. Can you tease that out for us?

MP: Well that is when I got interested in science studies, actually.

AR: I know, I wanted to ask about your transition into kind of sociology of science.

(File 2- 21:45)

MP: Well, 'socially constructed' is used in so many different ways. I mean, Rhoda Unger wrote a paper in the *American Psychologist* about the difference between sex and gender as one of those biological (inaudible). That never made sense to me because it begs the question of, 'Which is which?' I think Wendy McKenna has talked about that.

Some of it **["socially constructed']** refers to a Schachter-like phenomena, that you have got this diffuse bodily arousal, you look around to give it meaning, the culture provides a meaning and it fits well enough, it works. The postdoctoral fellowship I had **[at Harvard, with Elliot Mischler]** was in social labelling theory, which was somewhat similar. **Once** you get a label **[for what ails you from a doctor you are put into what sociologists call the "sick role."]** You are given certain social responsibilities and absolved from others. But in the social labelling theory literature on mental illness, once **someone is** labelled schizophrenic, for example, they never **[stop being "sick," at best they are "in remission."]** I mean, it is a very powerful social thing.

So, some of social construction refers to the labelling business, but everything we know is through our human experience. Everything - it is in response to something that is *really* there. So, it is the difference between, in my view, epistemology and ontology. You know that there *is* a world out there, and you can only know it through your own perspective. So, what is the problem? I mean, if you try to get closer to the truth, and I think that one of the feminist insights (I think Donna Haraway was really explicit about this) is that if you look at the world from as many different perspectives as you can and you cooperatively try to figure out what this is, you get a better view than when you see it from just one. That is just a vey basic point. It does not have to be feminist, but feminists made it best, I think (chuckles).

AR: I love the quote that you include in your *Theory and Psychology* article from Donna Haraway, which was exactly what you just said. I was reading this earlier, and I [am thinking], 'This is just perfect.'

MP: It is common sense! But somehow people want to think socially constructed means [that] it is not real.

AR: Mhmm.

MP: And to say there is no scientific evidence does not mean it is not real. 'Real' means different things to different people.

AR: 'Real' has many different meanings. So, this is obviously a related question, but can you talk about some of your transition into more sociology of science, science studies-kinds of ways of thinking? Obviously, the labelling theory postdoc must have been influential.

MP: It was very influential. And, [I was] just talking to Leonore in our email exchanges about medicalization. She sent me a review where feminist contributions are pretty well invisible, and she thought it was a shame that I was not there and she was not [either]. But many sociologists did. Sophie Laws and Catherine Reisman said medicalization is one form of social construction. It feels real, it gives you certain benefits, it gives you a social role. There is a lot to it.

But after seeing what the APA (the American Psychiatric Association) did with their literature review and their conclusion about the scientific evidence, I was so pissed off [that] I called the main office finally at one point and said, 'could I talk to someone about this?' And he said, 'Well, what do you know about the literature?' And I said, 'I was on the review committee, I probably know it as well as anyone in the country' and he said, "well, what is your degree in?' (laughs) If I did not have a medical degree, he was not going to take me seriously.

(File 2- 25:50)

MP: Now my experience is that many MDs [medical doctors] have to work a little harder to get up to speed on research.

AR: Yeah.

MP: So, I walked away. I mean, the American Psychological Association was going through the same old, same old struggles, between the scientists and clinicians. I was not interested in that and other iterations of that.

MP: But, I was interested in how this PMS diagnosis had gotten to this and what the forces were because I had participated in that. I remember **being** a speaker at a press breakfast, where they had people come and talk **to reporters and editors** about PMS. **It was** funded by a drug company! The **company** would never tell **us** what to say, they must have known in my case [that] I was not going to be especially pro-drug. And when I was doing my research at Mount Sinai in Jerry Knittle's lab (he is an endocrinologist and fat cell and metabolism guy), he was asked to speak at some conference on women's health (laughs). Anyway, he asked me to do it, and so I talked about PMS, and some guy got up and said, 'I found in my practice in Colorado that such and such treatment (the drug company's treatment) works very well.' And Jerry, the most uncynical man I know, said 'he's a plant' (laughs). It was that kind of thing. Drug companies are *very* sophisticated. My daughter is a doctor and she gets these things all… [the drug companies] know what medical residents need – they need sleep, they need good food – and they know how to make themselves attractive.

So, drug companies make markets for their products, and one of the markets is the scientific aura around it; I was interested in that. Then I had my first sabbatical, twenty years after my degree (laughs). In the year 1991, I went to the University of York in England, which had a master's program in women's studies, which was unusual back then. They were reading all the same things we were, it was really fun. And Michael Mulkay and Trevor Pinch were in the sociology department. Michael Mulkay is sort of the

father of science studies, a gentleman of the old school. And, so I just read a lot of stuff there, I was really interested. I started going to conferences on science studies and the history of science and the histories of contemporary science. The other influence that came a little bit later (but was related) was Evelyn Fox Keller, whose work I knew personally from a Columbia seminar on man and society. We had crossed swords and then crossed paths and then became good friends. She was just an enormously smart woman.

AR: I was going to say that she was an incredible intellect.

MP: And **[when I was a Visiting Professor at MIT]** I would go to the History of Science **colloquia at Harvard and colloquia at MIT's Science Technology and Society Program, and at the Dibner Institute.** It was just such a rich field of studying things that felt right from the inside. And I like historians, I do like historians.

JY: (laughing)

MP: They believe in facts!

AR: It is true! It is an empirical discipline.

MP: Yeah, you cannot get too far, and that is what I think is the intellectual stimulation and challenge for psychology of interdisciplinary groups - that if a psychologist says something, a historian is likely to pipe up and say, 'well, it was not like that in Elizabethan England, it was the lace trade or something.'

(END OF FILE 2 – 29:55)

(FILE 3 BEGINS)

MP: And it, it keeps you honest, I think that that is the benefit **[of interdisciplinary groups]**. And once you have been bitten by that bug - Leonore and I have talked about this - you cannot go back to **[mainstream]** psychology, you just cannot go back. I could not, and she could not. I am still interested in psychological subject matter enormously, but not really in the way it is institutionalized within the discipline of psychology.

AR: Right.

MP: But I did get interested in how neuroscience became a discipline.

AR: Mmm.

MP: Because I saw it at MIT for sure. And it was happening during the years I was becoming aware of how these things happen, and it is just such an interesting story. I actually gave a paper – a couple of papers – at history of science conferences on the role of psychology and psychologists because… I forget his name…who is that man?! (laughs) It will come to me. **[Robert Galambos]** [He is] a very distinguished physiological psychologist **who** said that when he looked at what is **[today]** called neuroscience , each one of those is a chapter in physiological psychology - there was a discipline already doing that. So why did neuroscience not inform and enrich psychology? Well, the answer is quite gendered. Psychology because of this clinician-scientist split, was increasingly seen as feminine – gendered, feminine, weak, soft, we do not want to be part of that! So, some of the faculty at MIT (most of the faculty at MIT when I was there) – subsequently disavowed psychology, **left APA, and became** cognitive neuroscientists,'real' scientists.

AR: Right.

MP: None of them became social cognitive neuroscientists (there is a bastardized field if there ever was one (laughs)!) But psychologists actually played an enormous role **[in the rise of neuroscience as a separate discipline]**. I looked at the participation in some of the first neuroscience conferences, and it was half psychologists! And some of the most distinguished neuroscientists today were trained in psychology. Donald Pfaff was with me in the MIT program, Larry Squire…

AR: So, they needed to flee the feminized discipline by becoming hard scientists.

MP: I think Don Pfaff was… Psychology was not feminized then; I think it depends on when, but yeah, yea. I think it is very gendered. Otherwise, physiological psychology does cover **[the same topics as neuroscience].**

AR: Yea.

MP: But it could be because they're equipment-dependent, maybe, that the subfields of traditional disciplines **[which came together to become interdisciplinary neuroscience]** have a lot of clout. They are connected because they had that clout.

AR: Yea. Margaret Rossiter has some interesting things to say about establishing the hardness of a discipline by making it mathematics-heavy and instrument-heavy, which I think has been interesting to see throughout the history of psychology, actually.

I have a couple of things to pick up on. So, you said you crossed swords with Evelyn Fox Keller (chuckles). Clearly, I wanted to know more about that! (laughs).

MP: At the Columbia seminar, she gave her paper on, 'science is cold and hard and some other field was soft'.

(File 3 – 3:25)

AR: Okay.

MP: The stereotype **is that** science is cold and hard and impersonal. I said, 'But they [**scientists]** all know that's not true!' And she knows they know, but somehow her argument did not look at that. Michael Mulkay studied how scientists talk when they are adopting the rhetoric of science **[as cold, impersonal in order]** to persuade funders and **[how they talk when]** talking among themselves, because we all know it is hot as hell in science. That is what is fun about it! And Evelyn knew that.

AR: Okay.

MP: I think she actually took it onboard and went on to great... She was remarkable, just remarkable. And I like the way she keeps gender as an analytic tool in her studies on microbiology and everything. She is just amazing.

AR: Yeah.

MP: And she did not have an easy academic career either. Like any really smart woman of her generation in physics, she can be…difficult. But I always found her very supportive and helpful.

AR: Yeah, right.

And the other thread that I wanted to follow up on was [that] you have mentioned Leonore Teifer several times. And I mean, I am reading some of your material on PMS and so on, and I am thinking this is exactly what has been happening with this whole hypoactive sexual desire disorder in female sexuality, or

whatever you want to call it (inaudible), and so I was wondering if in fact, what you thought of all this and if you have also been following this, and clearly you have if you have been in touch with Leonore.

MP: Not really. I mean, she sent me a paper **(I forget by who)** which was a sort of an archeology of menstrual cycle research, but it was written **[from a very traditional, old-fashioned perspective]**… you know, Foucault was the star; Susan Bell was mentioned, but no other feminists. Leonore was saying it was too bad because my contributions had been important. And I wrote back and said, 'well, I don't much care, actually.' But I did admire the fact that she has been persistent, she has stuck with it. She has been on that topic **[medicalization, etc]** since forever and it is just amazing to me that she can keep doing it. I do not know how she can keep doing it because it is a very parallel case **[to MPS]**, and I think it is a big market, especially if they can get people to take it everyday! The fact that it has side effects does not bother them at all. In fact, I think Leonore was at a conference - there was a conference on menopause, which [I think] was published in the **New York** Academy of Sciences where all the same forces were lined up, as had been lined up for PMS - and there were a few activist groups. I think there was one from Canada that had a nice network of women, a support network. Susan Bell was there, and I was there, a couple of feminists, and sitting next to me was Richard Wertman from MIT who was working **the** drug company rep, I mean it was just **frightening**… we gathered in the hall during the break and were just quivering with rage at this **familiar and** impenetrable **[alignment of forces]**, 'oh, here it comes again - menopause!' You know, that is the ideal market, if you can get women 49-89 taking this stuff every day. The rationale, of course, is that **[if we don't]** there will be a terrible burden on the medical system - all these broken hips. Oh, it is just horrible. Again, I don't **remember** if Leonore was there **for that one,** but I don't know how she **has continued to fight the good fight** in the face of that. It comes back and it comes back whenever there is a market **to be made**.

AR: Well gosh, if you can get people started with PMS and take them all the way through menopause, you can get almost the whole lifespan!

(File 3- 7:36)

MP: They used to just do it with valium; back then it was straightforward. (laughs)

AR: (laughs) But yeah, [in regards to] Leonore, the parallels are incredibly striking in terms of these two cases, and her larger campaign to put the focus on this particular issue did hold drug companies at bay briefly.

MP: Yeah, well, I think she has the intellectual and clinical authority to be able to make **the feminist case effectively**– and she writes very well –

AR: Yes.

MP: So, she can make the case.

AR: Yeah. And she, I think, is very aware of leveraging some of the very forces that you have talked about, that science has authority, or *scientists* have authority, and she has been, I think, pretty aware of that and able to leverage that for *her* side of the case.

MP: You know, that was a fight in the British journal (*Feminism & Psychology*) when they were founding the equivalent of Division 35 **in the BPS [British Psychological Society]**. Celia Kitzinger was really against doing what I did and what Leonore does, which is using the rhetoric of pseudoscience to fight on scientific terms. She, I think, felt that that was not going to be effective in the long run. It probably was not.

AR: Using the master's tools, so to speak.

MP: Yeah, exactly.

JY: I am curious, too, you have spoken a lot about these sort of encounters or discussions you have had with medical professionals or psychiatrists that have not been productive, and have not been receptive to your point of view, but I was wondering where people have come from that *are* more receptive to that discourse on PMS and where you were having conversations.

MP: Well, after my postdoc fellowship was ending and I still had another grant, and I needed a lab, I went to B.U. [Boston University] where Bob Rose was (he was a psychiatrist known for his work in psychosomatic medicine), **had a huge grant to study stress in air traffic controllers**. He trained at NIH [National Institutes of Health] **where pioneering work on stress was done.**. He was, in my view, nuts; but he is a very smart man and he is open to ideas, so he was interested. And we actually – Judy Abplanalp,he and I – wrote a grant proposal to do the hormone assays **[in a longitudinal study of mood changes over the menstrual cycle].** But, **NIH** cut it down so far, so it did not really do anything. I think Judy did continue and make a study out of it, but **NIH** cut **[the budget]** down so far it did not work out. But he was open to it **[empirical psychological research on the menstrual cycle].** I think **when** people are smart, they are not going to say no, but if they have some other agenda, or psychological-whatever. I mean, it just depends on the person, I think, and how institutionally-captive they are.

AR: Mmhmmm. We are out of sync now, in terms of the chronology of your career, but I wanted to go back to make sure we covered the brief time that you did spend at *Psychology Today*.

MP: Oh yes.

AR: I am not sure exactly where that fits given where we are, but maybe you can orient us.

MP: Well, when I was at Barnard, I was brought in as an Associate Professor with the understanding that likely I would be tenured, and Lila had hired me to teach the psychology of women.

(File 3 – 11:32)

MP: And, at that time, Barnard and Columbia were in institutional flux, and during those years **[1975-78]** they subsequently negotiated an agreement with Columbia. Effectively, Columbia had veto power over **[tenure for Barnard]** faculty slots - and what they wanted to do was fill them with their guys. I mean, this is how I see it from my hurt and vulnerable end, but also what the Dean of Barnard wrote to the president of the Graduate Center when I was going there – that it was an institutional battle, and I was clearly not going to make it.

So after, Barnard at every level of the review process wanted me unanimously; Columbia did not, and the ad hoc committee was 3:2 **[Columbia:Barnard]**, and it took them three votes to kind of come to **[a negative decision]**… Yeah, it was brutal for me because I was living in Barnard housing, I had a five-year-old, and no job, and was **[getting]** a divorce. I knew that some people had gone from academia to Wall Street if they were good at statistics, but I was not that good at statistics. Some took **jobs in** publishing as acquisitions editors. Columbia University Press would have hired me, but it was too much travel with a young kid, so I did not want to do that. But then, I forget how I heard about *Psychology Today*, [but] I went there when Bob Nestle was the editor and Ziff-Davis owned it. Bob Nestle was one of the generation of journalist-editors who trained with Gloria Steinem; Clay Felker was their guru, he was a real pro. The other two editors (one was from *Time*, and one was from *Newsweek*, I think), they were

professionals, and they were good editors. They had an eye and an understanding for what the mass market wants, and this was attractive.

Psychology Today before that, when it was first founded, was a remarkable journal, actually. I don't know if you know its early history. I don't remember the editor, you could look that up, but they published really key papers in psychology, like Matina Horner's 'Fear of Success', Karen Paige's 'Menstrual Blues' a cross-cultural one – they did a lot of well-written articles by psychologists. And if you look at the Social Science Citation Index, Psychology Today from those years was very highly ranked among scientific journals. By the time I got there, that was not so much the case, but it still was very professionally edited and that was the peak of its readership (it was about 800, 000 at that point). I was supposed to find people who were doing interesting things and coax them into writing. I also wrote what they called 'News Minds' – little summaries of articles that I thought might be interesting.

And that is where I learned that a lot of social psychology sounds really interesting, but when you translate it into English, there is nothing there. Their expression is M.E.G.O. – My Eyes Glaze Over. 'Well, there is absolutely nothing new here!' (laughs). It is just trivia. And that really made me interested in how we avoid seeing that in the classroom reading it. So, I had a great respect then for people who can write well, and I think Carol Tavris is the perfect example.

AR: I was going to ask, because I could not remember when she was there. Was it after you were there?

MP: I do not think she was ever there.

AR: Psychology Today? She wrote…

MP: She wrote *for* them, yeah.

AR: Ok, but she was never on staff.

MP: I don't think so. No, no, they would have grabbed her.

AR: Yeah, ok. Yea, I am sure you are right.

(File 3 – 15:35)

MP: Yeah. Because she went to writing as soon as she got her degree, and she has done beautifully. I used her book as a textbook.

But I had this **NIH** grant **[which I had gotten at Barnard]** – it was a big grant – and when the Graduate Center **[was looking for a Director of a new research center]**… actually, Harold Proshansky is that a name you are familiar with?

AR & JY: Yeah.

MP: He was the president of Graduate Center. I do not know if you are familiar with the City University of New York's structure, but he was the president of the Graduate Center, and he actually understood my situation (the Barnard Dean explained it to him), because he was denied tenure at Brooklyn because they said environmental psychology was not a field (laughs). And he thought the fact that it had been absorbed (when it did become a field) into the paradigms of mainstream social psychology was a complete disaster. So environmental psychology **at the Graduate Center —** a program that he started — was a wonderful one, very diverse, very… It is really, really good. And, so, he had sympathy for me. The only thing he could do was give me half-time teaching, and half-time administrative. So, I became an unwilling administrator **(as Director of the Center for the Study of Women and Society).**.

AR: Okay.

MP: And I got, I think, two grants from the Ford Foundation **[for mainstreaming feminist scholarship into traditional disciplines]**, so we were able to keep going.

AR: You have on your CV that you became the director of The Center for the Study of Women and Society. Was that a new centre?

MP: Yes. The founding director was Susan Saegert, a feminist psychologist.

AR: Ahh, of course!

MP: I guess she did not want to do it, she was Director for only one year, and then I was. I was there for five years, and then Sue Zalk became the director.

AR: Oh right!

MP: And they kept trying to develop certificate programs, and a master's program in women's studies at the graduate level because they had a wonderful faculty. And then a PhD program. And they wanted to combine the Research Centre with the curriculum, and I did not want that, all the administration. They did combine it after I left.

But, as a non-elite university, CUNY [City University of New York] had some of the really smart women who were early pioneers in women's studies. Feminists. Joan Kelly and Renata Bridenthal, Blanche Cooke. I mean, it was just wonderful, wonderful faculty. And **al**so, at the Graduate Centre, I was really fortunate (because of the Provost, who is probably no longer there) to be able to co-teach at the graduate level on feminist topics — it was marvelous, just marvelous. So, I co-taught **courses** with Rachel Brownstein and Kate… I forget her name.

AR: Kay Deaux?

MP: No, no. She came **[to the Graduate Center]** later, and she was not an interdisciplinary type.

AR: No.

(File 3- 18:44)

MP: I taught twice with Rachel Brownstein, a literary critic, and once Rachel and I taught with Setha Low, who is an anthropologist. I **also** taught with Ana Celia Zentella, who is a sociolinguist. That was really interesting. So, it was such a stimulating environment. Graduate students who are mature are always wonderful, just wonderful.

AR: Yeah.

MP: So, I loved that part. When I resigned (once I got tenure, I resigned my administrative role). (Chuckling) I did not want to do that anymore.

AR: Yea, yea.

MP: It was fun.

AR: What did your, in terms of your own research program during those years. Can you talk a little bit about what you were up to?

MP: Well, the Graduate Center **[then]** was on 42nd **between** 5th **[and 6th]** in this old building, which had no space for labs. It was not laboratory-oriented. But I did have **some money for research** — as most places do, **CUNY had** faculty grants (small ones) — and I really was interested in sensory changes related to the menstrual cycle. (That was the second **[menstrual cycle]** review I did for Psych Review, because that was something that was puzzling.) At least you could figure out the mechanism was if you were working with the right people. So, I found a place at Mount Sinai School of Medicine in Jerome Knittle's lab of nutrition and metabolism, and studied changes in food intake over the menstrual cycle. Actually, we laid out food, **measured what they ate,** and it was kind of fun. That is the paper that I wish I had persevered and published because we did find changes.

AR: Uh huh.

MP: They said, 'accepted pending this revision', and at that point, my life was in chaos, so I did not do it. But that is one paper I wish I had published and did not. So that was what I was doing **in research**, I was trying to do that, but it never got off the ground. I do not really have the persistence and the ambition and the competitiveness to be in big science, to make a success in big science. And those were the only models I had, you know. It is worthy, it is very worthy, I just could not do it.

AR: Right. Well, as you point out, it requires a lot in terms of institutional support, and infrastructure, all that.

MP: NSF [National Science Foundation] had a program where the proposal had two components: a research component and then a 'how would you bring women's studies to the faculty of the institution you were proposing to go to?' **component.** And I was going to go to Rockefeller because Don Pfaff was there and Carl Pfaffmann and they would have supported the scientific part, but the reviewers thought there really was not much you could do in the way of providing information about current feminist scholarship to the Rockefeller faculty. **Because**, if they were interested in it, they could go down to CUNY. (laughing)

AR & JY: (laughing)

(File 3- 21:48)

MP: But I just missed it **[the cutoff for funding that grant]**. That would have been a game-changer for me, I would have had laboratory facilities with good people for a few years, but I did not.

AR: Right, right, right. And of course, somewhere in this time period, too, you were president of Division 35, in the mid-80s, I think it was.

MP: Yeah.

AR: Can you tell us a little about that experience?

MP: Let me see. I think it must have been the fairly early days of the division, it must have been about the tenth…

AR: Yup!

MP: So, the journal got straightened out.

[added later by MBP: Nancy Henley was doing wonderful work as PWQ Editor, but the publisher was terrible; thanks to my college friend Susan Milmoe, then an editor at Cambridge University Press, we were able to solve the publishing problems by moving the journal to CUP.]

AR: Okay.

MP: I think some new sections were formed within the Division the year I was President – Psychology of Black Women and I think maybe Gay and Lesbian… I am not sure about that.

AR: I do not know. I think that came not too long after.

MP: Later, yea. I do not know.

(I was also Chair of the Public Information Committee at the APA **[for two years]** because of my experience at Psychology Today, but I just did not like that kind of thing. I just do not.)

So, I do not know whether it was a success or failure, but we kept going, we kept all the records, we had the programs. I have, I guess, obliterated it from my memory.

AR & JY: (laughs)

MP: It was not a disaster, but it was not something that I care much about, in retrospect.

AR: Yea, yea. Well, one of the articles that you sent me was the article you wrote in what must have been the inaugural issue of *Feminism & Psychology*, and you talk about the politics of remaining within a mainstream organization versus maintaining your outsider status, and so on, and how that might play out for *Feminism & Psychology* vis-à-vis the BPS [British Psychological Society], and so on and so forth. So, I wondered if, in reading your reflections on that, because the journal was founded in 1991…it has been twenty-five years…

MP: Yea, how are **they** doing?

AR: Well, that is my question! My question is now with twenty-five years of hindsight, what do you see when you look at *Feminism & Psychology*?

(File 3 – 24:06)

MP: I have not looked at it.

AR: (laughing) Okay.

JY: (laughing)

MP: I knew two **founding** editors quite well, and so they asked me, but at that point I was thoroughly uninterested in institutionalizing.

AR: Okay.

MP: The English tradition in psychology that I was familiar with in social psychology, in particular, is much more reliable and incisive than a lot of our social psychology. I do not know how someone like Michelle Fine manages to breeze along! She is not at all traditional, and yet she does wonderful work within psychology. She is an example of someone who can do it.

AR: Yeah. I heard her say once that she is a psychologist, that is how she was trained, and so whatever she does is psychology (laughs). I am like, 'Well, that is one way of looking at it!'

Okay, again, I have a million threads that I want to pick up on. The CV that you sent me kind of leaves off after the mid-1990s, and you talk about how that was a time that you became much less interested in organizational/institutional stuff. So, I guess my question, in terms of rounding off some of the chronology of your career, would be to kind of bring us up to date over the past twenty years or so.

MP: Yeah, it **[the CV]** did end in '95. I think this is where the personal and professional intersect. My daughter was born in 1971, and I went to New York in 1975. The intention was that my husband could easily get a job as a radiologist there, but this was my one chance. He did not, in the end, want to do that. So, I stayed with my daughter as a single parent on an academic salary, which was not pleasant. I went back for a reason, but I forget what the question was.

AR: Oh, just in terms of… what after the mid-1990s, kind of…

MP: Oh, okay. Yeah, so, it **[Manhattan]** was a great place to raise a kid. She went to wonderful schools. She went to the ethical culture schools in the Bronx, and they were wonderful. But when she went off to college, I was lonely, and there had been some interesting men, some not-so-interesting men, but I was lonely. And so, looking around Manhattan, many of the men were gay and not interested (chuckles). So, I was going up to **my cabin** in Maine for the summers, and there was Joe Bauer **(who was still in Held's MIT lab) in his cabin on the same lake** with one of his sons, and we sort of reconnected. In fact, we reconnected rather rapidly. Within the space of six months, I sold my apartment in New York and moved up here **[to Boston]**, bought a house, got married. But **after commuting weekly to the Graduate Center for a couple of years** I gave up my tenure, and that was a major step. I did not want to stop working, so I was a full-time Visiting Professor at Bowdoin College for the first year (commuting weekly again). I think that was the first one, and then a Visiting Professor at Clark University. Clark had just started a Women's Studies PhD Program, and I led the pro-seminar, which was interesting. They had some interesting students, they were more mature. And then I was a Visiting Professor at MIT, which was undergraduates. At MIT I taught a **Psychology of Gender** course that was cross-listed between Women's Studies and Brain and Cognitive Sciences, which was the name of the department that I got my degree in – it went from 'Psychology' to 'Brain and Cognitive Sciences'. And, I taught that, and **with one exception** that was all I taught.

(File 3- 28:13)

MP: So, I was interested then in doing some archival and interview research on how the MIT department had gone from a little pychology department **(with roots in a nucleus of psychologists in the 1950s, George Miller being one)**, to become this huge, behemoth **Brain and Cognitive Sciences (aka neuroscience)** – it is huge! It is institutionalized, and takes over buildings, and I wondered how that happened. So, I interviewed some wonderful people, that was really fun.

AR: Oh wow!

MP: Really fun. And I have written some things on that, and I still think there might be something if I were to want to write again that could be said about that. So, I was really doing both archival/interview research and teaching, and then I got involved in the gender community.

AR: Yes!

MP: Because I used to start my class by saying, 'This is the cultural part, the stereotype – there is masculine and feminine (we know it is not true, but there is this stereotype). But we know it is not true! It is a huge overlap, but the one thing that seems to be dichotomous is this sense of whether you are male or female.' And, one of the students in the back of the room shook his head (chuckles). He came up afterwards and it turned out he had been working at MIT as a woman until fairly recently, and some highly sue-able doctor got hold of him and gave him hormone shots, which masculinized the appearance, so that he went with a male presentation.

(FILE 3 ENDS - 29:43)

(FILE 4 BEGINS)

MP: And I spent a lot of time with him talking about this. He had a friend, an MIT graduate who had graduated as 'John' but had become **Nancy. Nancy Nangeroni** actually became a leading force in the International Foundation for Gender Education and its videos and publications; she was a guest speaker in every class I taught.

AR: I saw her name among the references on the paper.

MP: Yea! She is quite a character, **admirable**. And then I met others. Other guest speakers were a couple from Brookline who were raising their child without gender at all; they simply would never tell anybody what gender this child was. And that caused a lot of issues … you can imagine – bathrooms, sports teams,… And then somehow I connected was a sociologist at Brandeis who was transitioning from female to male, and he came and spoke in my class.. Students were interested, I was fascinated.

And I went to a couple of conferences. One I went to was a **male-to-female weekend conference** – residential conference – and it was quite enlightening, you know. And I went to a **day-long** female-to-male conference. There is a big difference between the two groups that I went to. The female-to-male would often be lesbian… well, not necessarily lesbians, but activists, [and they] were very smart politically. And they had their act together. Those at the male-to-female were not quite as plausible, some of them, in appearance, it was harder **(for them to pass as women, which is what they wanted to do**). This was a time **— mid-1990s —** when, medical care was terrible, it was physically dangerous even in the most civilized suburb… it was bad. Really bad.

(File 4 – 2:10)

MP: And they were trying to change laws, and one thing they did change was a Cambridge Housing Law that you could not be discriminated against on the basis of gender presentation. One of the activities at the male-to-female conference was to try to come to terms with some different conversational patterns that men and women have. So, you would each get a token and after you had talked in the group, you put your

token back and you could not talk… well, it was very hard to get some of these women to shut up! (laughs)

AR & JY: (laughing)

MP: It was like being with a bunch of… It was very strange. So, some things go deep, and they were not very political. But they were generally technical, and highly trained, so they had good jobs. They were richer, and protected in some ways by class.

AR: Yeah.

MP: So, I found that absolutely fascinating. And the literature about it **is terrific**– the personal accounts, the cross-cultural literature…

AR: Well, one of my questions was going to be what was the development in your own thinking and so on that lead to the article on transgender activists and psychological perspectives on sex and gender? This sounds like it is part of that path.

MP: Yea, yea. Well, I think Rom Harre sort of laid down the groundwork.

AR: Right! Now, I do not know if it has made its way down here, but one of the things that came to mind when I was reading this article [was] have you followed any of the controversy that is going on in Toronto over Jordan Peterson, who is a psychology professor at University of Toronto? He has publicly and forcefully stated that he refuses to use the preferred gender pronoun – it is all hooey, basically, he is not into it. As you can imagine…

JY: He was just testifying in front of the Senate in Canada.

AR: And, he was invited to Harvard recently to give a talk!

JY: Yes.

AR: Because now this is all being couched as academic freedom issues, too, right? I mean, transgender activists have been very, very upset by this, and so on, as you can imagine.

MP: Well, they are pretty well organized up there, too; I know because I have a cousin who is an Anglican bishop who lives in Hamilton and is very supportive of inclusiveness in the church. (He teaches at UofT [University of Toronto]).

(File 4 – 5:10)

AR: Yea. I mean, the things that you are writing here about all this are extremely relevant.

JY: Yeah, to the current conversation.

MP: It is interesting, at one point I wanted to submit something to *Feminism & Psychology* about this, and the reviewers were in a very different place. There **[in the UK]**, the goal was still to pass **(whereas here the idea of a gender continuum was beginning to take hold).**

AR & JY: Mmhmm

MP: That was their goal, and to say the celebration of these inter- and non- **genders just didn't fit.** But down here, my younger grandson goes to a Quaker school, Cambridge Friends School, and there the teachers always give their preferred gender (chuckles). I think that when kids are 9 years old, at that point they are not ready to understand, but that is what they do.

AR: Yeah. So, I realized you are especially - I mean, I realized it before, but especially now that we are talking – that your career has become much larger than psychology. You have been interdisciplinary, multidisciplinary, whatever you want to call it, for a long time. So, some of my questions are more about psychology because I implicitly, sort of come from a position of critique, right? So, I was hoping (I hope it is not to far out of the realm of things) to ask you about some of your perspectives on psychology as a field, given that that was your home field, but now you have moved beyond.

MP: Well, I have always taught in the psychology department.

AR: No, that is true.

MP: I have always taught psychology.

AR: Yea, yea. But, some of our questions have to do with things like what is your view on what feminist psychology has done successfully in terms of resisting, critiquing, changing psychology, and where has it not been that successful and, sort of, what you see that remains to be done. Those kinds of questions. I mean, do you care to take that on?

MP: Well, they are very reasonable questions, and if I had a view, I would share it. I really have not read… I moved out of the psychology literature pretty much when it became… the journal *Psychology of Women* was a big disappointment to me…

AR: Okay.

MP: … a big disappointment. I did not see any forces likely to make it different, better-minded.

AR: Can you elaborate on that? What was disappointing to you about it?

(File 4 – 7:59)

MP: Well, it became social psychology of the most unimaginative sort in my field. I mentioned Michelle Fine published there, and I think she is wonderful, her work is wonderful… and I think Kay Deaux's work is very interesting. She was a very pleasant colleague, and she is also a very good graduate teacher – she trains her students well, she gets them jobs, I mean, she knows how to do it, and that is a skill that is important. But I am having second thoughts about whether we made a mistake setting up a women's studies program. Well, a Psychology of Women section. I don't know, I am conflicted.

AR: Okay, okay. So, in your view, the kind of studies that are more likely to be published in places like PWQ [Psychology of Women Quarterly], or that PWQ have chosen to publish, do not shift the ground significantly enough away from mainstream epistemological positions.

MP: Methodological.

AR: And methodological, yeah.

MP: Well, I think somewhere I documented who cites who.

AR: Mmmhmm, yeah.

MP: And I think that it shows that everyone is talking to themselves, which is all very fine if you need to be a big fish in a little pond, but it is not advancing the field. But, everyone has their own view. My interest and my identity has always been someone who is interested in the subject matter of psychology.

AR: Okay.

MP: But that was formed in graduate school. I mean, the head of the department, Hans Lukas Teuber, was a neuropsychologist (they did not have a name for it back then, but he was a neuropsychologist), and he always talked about how his department was going to be different, it was going to be what psychology ought to be. He was a master publicist, so within five years this new department was ranked number eleventh or something in those **[national]** rankings. He was a salesman, but he also had a vision that had some integrity. So, the idea that psychology is perfect as it is and we should not be thinking in other ways was not part of my training, actually.

AR: So, what was his view of what psychology ought to be?

MP: Well, human action. We could use any **[animal]** model, but **his own research focused on** human actions and understanding ho**w they were correlated with underlying brain function**, **particularly when normal brain function was disrupted.** One of his big studies **(Lila Braine worked with him on this)** was to study brain-injured Korean War veterans **to see what behavioral** deficits they had, and what inferences you can draw about **relationships between behavior and the brain**. He was always thinking very clearly about **[brain and behavior] and the faculty he recruited reflected this. [MBP: I have no idea what the rest of the sentence, which I've omitted, was trying to convey! so I added a bit to finish the general thought about Teuber's vision of the department.] One of the senior faculty he brought in was a world-famous neuroanatomist, Walle Nauta.** But he **also** saw a place **for faculty who worked on sensory-motor coordination, memory, and psycholinguistics; it was a bit of a stretch sometimes, but he managed to fit it into his vision.**

AR: Mmhmm. So, sort of a more expansive vision of what psychology could be?

MP: Well, looser.

AR: Yeah.

(File 4 – 11:22)

MP: Many, many psychologies. **[MBP: next sentence fragment omitted, no idea what I was thinking about at the time!]** Chemistry has many different kinds of chemistries.

AR: I mean, do you care to speak about methodology at all? You have alluded to it, and that methodologically a lot of feminist psychology pretty much used the kind of traditional approach; but on the other hand, experimental methods have been quite generative, right?

MP: Yes, exactly!

AR: So, yeah, what is your… how do you situate yourself?

MP: Well, one of the things I was interested in was studies that were done or reports that were made of psychologists during the second world war – what did they do? Mathematicians were scattered all over the place, working in different sub-areas of defense research where scientists and engineers were getting together to solve problems. Psychology was like that, too, and especially the experimental psychologists. For example, they worked with pilots on vision problems at night – that is the kind of data that is useful, it is solid, applicable. Some of the work in cognition and in personality research proved useful too. **The point is psychologists worked with others [scientists, engineers, military], contributing skills they brought from their sub-field of psychology, not "psychology" as a whole.**

But I think if I were to glom onto one area, one direction that I wish psychology **[did]** more of, or would take, it would be what Sylvia Scribner did using activity theory. (Michael Cole, I think, is one of the **[best known]** proponents of that; he and Scribner were together in a group at Rockefeller in the 1970s**(?)**, George Miller was part of that but he was sort of separate.) Sylvia Scribner made a very coherent research program with her graduate students which was grounded in activity theory. It comes out of **Vygotsky and Soviet Psychology: t**he **basic** idea **being** that human activities are socially organized around a goal. This changes the way you think about **very** basic concepts. For example, one of Vygotsky's best-known books (I encountered it as an undergraduate **in a course taught by Jerome Bruner and George Miller**) is *Thought and Language*. That's the English translation: in Russian the title was more like *thinking* and *remembering*. When you think about remembering, it is not the same as memory, it has a different feel. And so, remembering for a purpose in a context of the actual physical environment is part of thinking, part of what you needed to include when you describe "cognition." **Take**, as an example, **one of Sylvia's** students who studied bartending (he went to bartender school). Part of their knowledge of how to make drinks is physical location. Where are the right bottles? You just have to know that. And if the environment changed, your thinking about how to do that would change. **[Added later by MBP: would you say your "thought: changed?]** So, **activity theory** is much more integrative and purposeful, and there is a human actor at the centre of it. A subject in the real sense. I think PWQ and I split ways when I sent in a paper and I wanted to use "subject" to mean the human the actor. And they did not want to do that; editorial policy was that "subject" meant "object" **[the object of the experimenter's manipulations]. In interdisciplinary conversations this just doesn't make sense!]**

AR: Ohhh.

MP: There is something wrong with that, you cannot really do that.

AR: So, for you, as a scientist, how has feminism informed how **you** have worked?

MP: Well, it provides a wonderful community to start with. It also provides a different, and productive, and fruitful perspective. If you put women's experience at the centre, and think about that, it looks (inaudible)

AR: Right, right.

(File 4- 15:41)

MP: Now, when you come down to actually using **[as I did]** spit tests to do saliva assays of hormonal cycle phase, it is not exactly…

AR: We have been going for at least an hour and a half if not more, so I do not want to completely tire you out, but I also feel like there is more to talk about.

MP: Okay.

AR: Is there anything I have not asked about, or we have not asked about, that you would like to talk about? Any other people? You mentioned in your email to me that you hoped there would be an opportunity to talk about some of the influential people.

MP: I think the feminist scholars [**Kate Stimpson, founding editor of SIGNS,** calls it the 'new scholarship on women'] in New York who were in the **interdisciplinary** Columbia Seminar on Society **were very influential. We met monthly to discuss a paper by one of the members; discussions were great, no-holds barred (though usually tactfully phrased).** They were (are) such wonderful people, pioneers in their field **[Gerta Learner, Nancy Miller, Blanche Cook, Evelyn Fox Keller, Joan Kelly, Nell Painter, Honor Moore… so many others, too many to name or remember.]** Those were very exciting days. **Joan Kelly** was a pioneering feminist historian who was a part of the City University system; she was very supportive of me **when I was** Director of CUNY's Center for the Study of Women and Society. She died of cancer, it was a terrible loss for the community. What was your question again?

AR: Oh, just that is there anything we have not asked about your career?

MP: I think one thing that is different about my record than many others is the range of places I have published. Now, that can be a sign of dilettantism, or it could be a sign that part of what I do speaks to that group, but I have published in (I don't know if I have those encyclopedias on **my CV**?)

AR: I do not think so.

MP: Ah, well! At the end of about 2000, publishers were doing encyclopedias -

AR: And still are (laughing)

MP: Two are in the *Encyclopedia of Neuroscience* , two in the *Encyclopedia of Cognitive Neuroscience*, and two in international encyclopedias …I should have updated my CV but I never did… *Routledge International Encyclopedia of Women and Global Issues*, *Women's Issues and Knowledge* (which was translated into Spanish), and *Routledge International Encyclopedia of Women's Studies*. And I've published book reviews in ISIS, which is the history of science journal journal, in *Science*, in the British Journal of Psychology, in *Contemporary Psychology*. I've published in *Vision Research*, which was an experimental journal, and in *Theory & Psychology* and other psychology journals. **My review essay on Psychology and Women in *SIGNS*, vol 1, no1 was translated and reprinted in Italian, and my 1975 review essay (in *SIGNS* vol 5) no 1 was translated and reprinted in German.**

[Added later by MBP: I'd like to add a couple more points here to round out the picture: In the 1970s I was awarded two NIH research grants (in a very competitive environment); in 1986 I was nominated as a potential candidate as Editor-in Chief of *Psychological Review* and in '89 was proposed to be the Editor of *SIGNS*. (Since I said I wasn't interested in either of those positions, I

don't know if I would have been selected.) So, I think I have a broader **range of interests and professional connections than many feminist psychologists. Which is why I never really felt wholly identified with psychology as a discipline, although as I've said psychological subject matter has always been central to my work.**

AR: Right, right.

MP: I did not think in terms of career. You asked about mentors, and as I said, I do not think that was a concept **then.**

AR: It is a fairly recent idea

MP: I think Carolyn Sherif was someone who was supportive.

AR: How interesting. How did you cross paths with her?

(File 4 – 18:40)

MP: Well, she was active in feminist stuff… maybe even in AWP, certainly in the Division [35], and she asked me to be Program Chair the year she was President.

AR: Okay.

MP: There was a meeting of the **[Division 35]** Board when she was president in Colorado – I am hoping you have heard about this from someone, because I was not there (I had a new child, I could not come out) – but apparently a lot of personal and meaningful experiences were shared at that meeting. I know Alice Dan was there, and (inaudible). So, I was not as close to her as some others.

AR: Okay.

MP: And actually, Florence Denmark, she was at the Graduate Center and published the proceedings of the Madison Conference. You have heard about the Madison Conference, I assume?

AR: No.

MP: It was in 197- (trails off trying to remember the exact year). It was funded by NIMH, and I later saw it referred to by some people as the founding of the field of psychology of women. And it was – I forget who all was there – Irene Frieze, Rhoda Unger, **Julia Sherman**, Kathy Grady, I think. Nancy Henley, I think. Me, Florence Denmark. There were a number of people, and it was not a good conference. We had prepared papers beforehand and then people just read them. I mean, it was ridiculous. But I think those papers were published.

AR: Okay.

MP: Florence Denmark overrode **[Rhoda's]** objections, and got her husband's publisher to publish it, so it did not get much distribution. But that was an important conference, I think.

AR: Florence says hello, by the way. I saw her last week, and we serve on the board of the Cummings Center for the History of Psychology, and I mentioned that I was going to be coming to interview you, and she said, "Please give her my warm regards." So…

MP: She is a very sweet woman.

AR: Yeah, yeah. But I also wanted to follow up with Kathy Grady, because of course I have read her work, but I have not run across her.

MP: Well, what she did – she was really active and constructive in getting AWP going, for sure, and some of the issues in the early Division APA. But then she figured out, 'If I want to say something, I have to get a million dollar-grant, do the research for five years, write a paper, and in the last line, I can say what I want. And that's not worth it.' So she was going to go into politics. She moved to **the University of Connecticut Medical Center in Farmington and lived in** Massachusetts - Longmeadow (this was a long time ago, I do not know where she is now), but she just decided that she wanted to be an activist. Being a scientist-activist was not the most direct way to…

AR: Not the most efficient.

(File 4 – 21:34)

MP: Right! Not the most efficient or pleasurable.

AR: Yeah, fair enough.

MP: Then there is Bob Brannon. Did you ever interview him?

AR: I have never met him, but I have heard many people speak of him. Rhoda and I became interested in the very few men who were really involved in organizational feminist stuff in the early days, and his name came up, yeah.

MP: He was certainly one of them. And Arnie whoever.

AR: Arnie Kahn. One of only two men we have interviews with on the Feminist Voices website!

MP: He is still at Brooklyn, is he not?

AR: Arnie?

MP: No, Bob.

AR: Oh, Bob! I do not know where Bob currently is.

MP: But I do know from one of the people I met through the more scientific side of things at the Brooklyn department that Bob Brannon said at one point quite early on that he was not going to write anything that was not **already assured of** publication.

AR: Ah.

MP: And my scientist friend in the department said, "I am willing to support with my grants people in the department who have not been able to get any, but you at least have to play the game," and Bob was not playing the game **[trying to publish articles in peer-review journals].**. So that is the other view.

AR: Well, maybe in terms of wrapping things up a little bit, I was sort of intrigued about the seminar you were teaching at MIT that was Women's Studies and also brain and cognitive science.

MP: It was an undergraduate course.

AR: Undergraduate course, okay. So, I am actually just really interested in how you were approaching that, what you are drawing on, and how you are bringing it together because the feminist relationship to the brain and the body, as we know, has been a very interesting one, right? It has been an ambivalent one.

MP: And a long history of it.

AR: A long history, and I think there is really interesting, fairly recent work kind of reprising the feminist relationship to the body and to the brain, and sort of saying, "we cannot just say biology is bad and culture is good", right? It does not work that way. So, I am curious about your take on all of that.

(File 4 – 23:39)

MP: Well, it was dictated by the institutional setting, so I don't really have an answer to your question. The Brain and Cognitive Sciences Undergraduate Program had a requirement for distribution, and one of them was a sort of social psychology component. And Steve Chorover, who was on the faculty when I was a student, taught social psychology, a very popular course. But they needed to take two courses, so the Psychology of Gender – he would recommend that students take that, and so it was really more of a Women's Studies course. They knew enough about the brain, I do not think we even touched that. The way I focussed it, because it was at MIT, they had so many international students (it is a wonderful student body to teach, just wonderful. It was half women, half men). So, I did use some data from the UN about women and work in different countries, child rearing in different countries and mortality rates in different countries. And then we did some of the social psychology stuff on relationships, and they did observation studies, it was kind of a fun course. I did really enjoy it. But I must say, I think I am the only Women's Studies professor in the country, and possibly Canada, where I had students in full military dress in the class.

AR: Yeah. Wow.

MP: Occasionally. Usually they were in ROTC [Reserve Officers Training Corps], usually they were not there in their uniforms.

AR: Right.

MP: But, once one came and sat at the front of the class, he participated. So, you know, it was a class that was welcoming to men and women because that had to be that way, and it was quite effective, I think.

AR: Right.

AR: As you look back on your career, which of your contributions are you most proud of, or feel are the most meaningful or impactful?

MP: I think i**n terms of citations, there is no doubt that the 1973 *Psych Bull* article on premenstrual syndrome had the greatest impact. In terms of reprinting (and maybe citations) it was the 1972 *Psych Rev* critique of Broverman et al's hormonal theory of sex differences in cognitive abilities and the two review essays of Psychology and Women in *SIGNS* (1975, 1979).** I think there are two papers I *wish* I wrote, that is wish had been published. I think they are the ones I am proudest of (chuckles). One was an analysis of language used in social psychology where I compared social psychology and vision research, the titles of the abstracts. And I really thought I was getting at some of what makes social psychology weird (laughs). It got mixed reviews, and ultimately, they did not publish it, and I think they should have.

JY: What did you find in your analysis?

MP: That the linguistic contortions that social psychology goes through in the abstracts and the titles (their language use) **have** the effect of disguis**ing** the active role of the experimenter. And so they have used as agents in sentences things like 'results' or 'surveys'. There is no agent, it is not clean. You would think the same thing might be true in vision research, but it is not. And these are both fields that have passed through crises at the same time; I just thought that there was something there to be gleaned about languages, that if you try to say it clearly… You know, what is it that they say? 'The experimenter…' But they do not do that, they are so busy being scientists. I do not know what the impulse is, but…

(File 4 – 27:34)

MP: And the other one is a paper I gave, which Evelyn Fox Keller thought was really good, and was actually accepted, but I could not revise it in time. It was an analysis of a special issue of *Science*, where they had papers on sex differences at the genetic, chromosomal, gonadal, hormonal, and behavioral levels, so there were four papers. And it turns out that you do not actually have to use 'male' and 'female' as words **or concepts** to analyze what is going on. So, the people **studying** the gonads would say, 'there are male and female genes, but we have all this variety', and the people at the hormonal level would say, 'there are male and female gonads, but we have all this variety'. You could describe the processes without ever having to use a dichotomous male/female. Even if you wanted to expand the number of sexes to eight, or whatever, you do not need to talk about sex to talk about what is happening. That was a nice point. And it was accepted – it was the proceedings of a conference – but, I was moving to Boston and I just did not get it in time.

AR: Are the unpublished manuscripts in your papers in the archives?

MP: Yes.

AR: Oh good! (laughing)

JY: They are there to be found! (laughing) I am wondering if you want to tell the story of *Psychology Today* and the APA that we had done off camera, or whether that is…

MP: Well, I do not want to tell all the details, for sure, but…The purchase of Psychology Today was not discussed among the membership at all. And the reason given for that was that it would lower the stock price, or the value of the enterprise if it became… I do not know exactly, but it was some economic argument. Maybe it was true what they said. The conclusion should have been well, then a membership organization could not buy it, for sure. And they went ahead and did it. **[Added later by MBP: I was**

President of Division 35 and had been Chair of APA's Public Information Committee, and the first I heard of the proposal to buy *PT* was when Kathy Grady, Division 35's Council Rep called me at home in Manhattan to ask how I thought she should vote — that day!]

(END OF FILE 4 – 29:53)

(FILE 5 BEGINS)

MP: And then, it was just Virginia O'Leary and whoever was the guy…I forget his position title, but APA then.

AR: Ohhh. Was this Chuck …

MP: That sounds…

AR: What was his last name? I think I know. The CEO…

MP: I think so, yeah.

AR: Oh shoot. I know who you are talking about.

MP: Not the president.

AR: No, no. That was Chuck…

MP: Chessler? Chessen? No.

AR: I know.

MP: Yeah? Okay. Anyway, they had quit

AR: Kiesler!

MP: Kiesler, yes! Virginia and he worked quite closely together. Virginia had talked to me about, but I did not get what was behind it, about editors at Psychology Today **when it was still owned by Ziff-Davis]**. Well, at one point, two editors that were there, but not in the highest levels, had edited it by themselves when the **senior editors** were busy. And so she thought that they would be good people to run the journal after APA bought it, and [that] they would take direction better than a more senior professional editor would. So, they got people who did not have the same editorial **experience**, confidence and judgement that is needed, and you just cannot assume that the psychology is well-presented and it is going to grab a **mass readership, even an upscale one**..

I adore George Miller (I took his **psychology of** language course and think he's wonderful!) and he's known for saying psychologists should **[think beyond their journals and] "**give psychology away" **[to the general public].** In my Div 35 Presidential address **[this was when Psychology Today was coming under the APA umbrella]** that 'sometimes you just can't give it away.' You just cannot.

AR: (laughs) That is good.

MP: That **[purchase]** was very expensive and destructive because I think that furthered the clinician-scientist [divide], which was [**heating up for other reasons as well].**

AR: Yea, yea. Well it was not… I mean, APS started in … was it '84 or '88? I think I am messing up the dates, but it was right in that period.

Ok, I have one last question because we are really infringing on your time here, but…

MP: It was very pleasant!

AR: You have talked about the various, [and] we have probably already covered this, but one of our questions is about your experiences of the ways in which maybe gender and being a feminist and so on have resulted in discrimination.

(File 5 – 2:38)

AR: Yea, have you ever been discriminated against because of your gender, and so on? You have mentioned some of these already in terms of getting shuttled off to Wellesley when your male colleagues were getting arranged tenure-track positions, that kind of thing.

Any other episodes, or maybe times when you felt that your gender was an advantage?

MP: I mean I think we have all experienced that situation where you are in a mixed group, and what you have said is not taken seriously until a man says it. I mean, that happens; it has happened to me a lot.

AR: Yeah.

MP: I think what feminism did is [provide] a sense of a community. You can just sort of roll your eyes and go somewhere else when that happens. I think that sense that you are not alone, that this is not personal is a big, big help. I think the discrimination against me in academia was not because of my gender, but because of my subject matter. Absolutely, absolutely.

AR: Ohhh.

MP: Absolutely. It made people nervous.

AR: Menstruation, you mean?

MP: Yeah.

AR: Yeah.

MP: (laughs) Women's health, in general.

AR: Right.

MP: In fact, I think I said somewhere that when I first submitted a paper on sensory changes related to the menstrual cycle to the JPSP [Journal of Personality and Social Psychology], they said it was more suitable for a clinical journal. So, I sent it **to another [non-clinical!] journal.** They bounced it around in a complete circle, nobody wanted it in the APA journals, and of course, APA journals are what count for tenure.

So, I don't know what the future of psychology is. I think it was jumpstarted in the '60s when it fulfilled a science requirement

AR: Uh huh.

MP: And then everybody who was not really quantitative, and [who] wanted to be activists, was drawn to it. I think that is when psychology, in the United States, anyway, ballooned **[in terms of undergraduate majors]**. And psychology faculties.

AR: Yeah. Okay, is there anything else that you want to…

JY: I think we have covered a lot. Is there anything else…

MP: I should say just as the subject matter also helped me. **Barnard [College] — that is, Lila Braine — was definitely looking for someone like me, a feminist psychologist with a track record of getting grants, and so was CUNY; my Visiting Professorships at Clark, Bowdoin, and MIT came because of my record and reputation as a feminist psychologist.** So, with a lot of help from other people **[women and men]** with feminist sympathies, **and hard work on my part**, it became something of an advantage. But in some circles of course, I am sure many circles still today, it is not an advantage to be identified as a feminist scholar. Even though the best work…

So I guess the advantages and disadvantages depend on who is involved and which generation it is.

(File 5 – 5:37)

AR: Yeah. And we have certainly heard through this interview project, people who today will only identify as a feminist psychologist among certain communities within psychology, and in other circles will still not because they feel that their work will not be taken as seriously, or would be seen as political, as opposed to scientific.

MP: Yeah.

AR: So…

MP: Well, it depends on how career-oriented you are. I never was, to my detriment in some ways.

AR. Yeah. Alright, let us stop there.

(END OF FILE 5 – 6:15)