SCIENCE HISTORY INSTITUTE

SANDRA HARDING

Origins of Science and Technology Studies

Transcript of an Interview
Conducted by

Joseph Klett and Jody A. Roberts

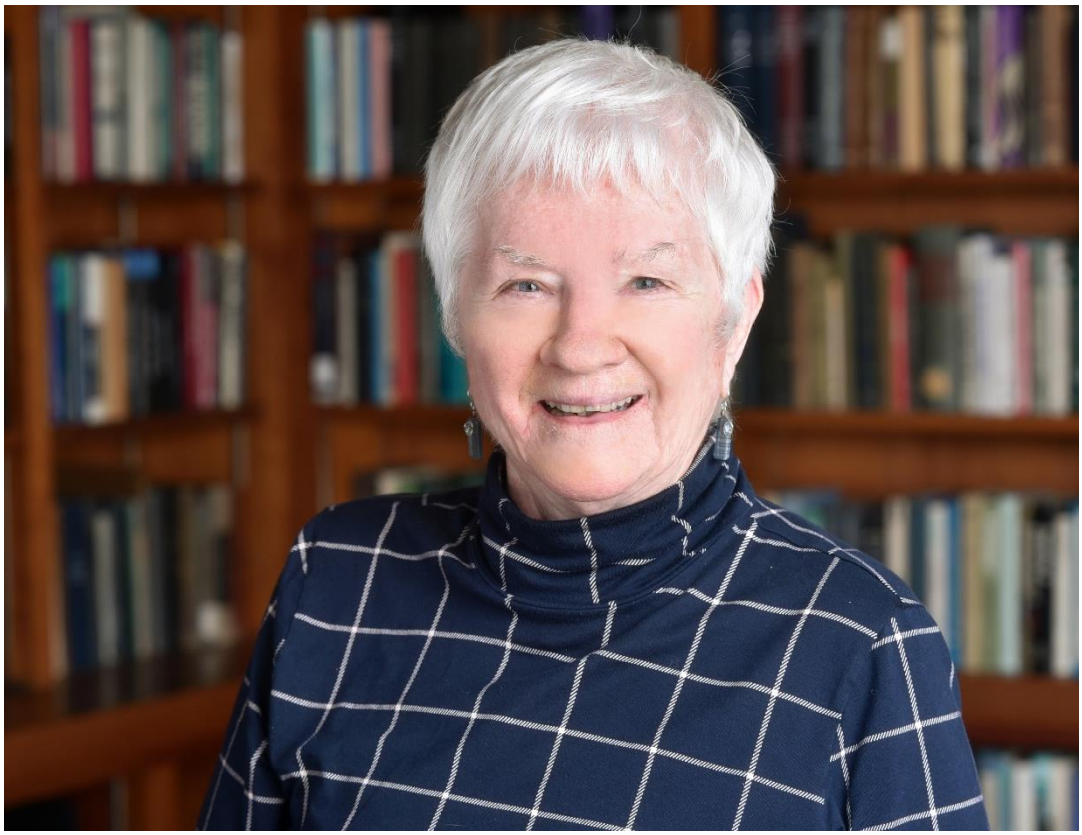
at

100 Harris Street and Harding's home
Sydney, New South Wales, Australia, and Playa Vista, California

on

28 August 2018 and 23-25 April 2019

(With Subsequent Corrections and Additions)



Sandra Harding

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> Sandra Harding, interview by Joseph Klett and Jody A. Roberts at 100 Harris Street and Harding's home, Sydney, New South Wales, Australia and Playa Vista, California, 28 August 2018 and 23-25 April 2019 (Philadelphia: Science History Institute, Oral History Transcript # 1101).

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SANDRA HARDING

1935	Born in San Francisco, California, on 29 March

Education

1956	BA, Douglass College, English
1973	PhD, New York University, Philosophy

Professional Experience

	The Allen Center, SUNY-Albany
1973-1976	Assistant Professor of Philosophy
	University of Delaware
1976-1979	Assistant Professor of Philosophy
1979-1986	Associate Professor of Philosophy
1981-1996	Joint Appointment to Sociology
1985-1991	Director of Women's Studies
1986-1996	Professor of Philosophy
1992-1993	Director of Women's Studies
	University of Amsterdam
1987	Visiting Professor of Philosophy and Women's Studies
	University of Costa Rica
1990	Visiting Professor of Philosophy
	University of California, Los Angeles
1992	Visiting Professor of Women's Studies and Philosophy
1994-1996	Adjunct Professor of Philosophy and Women's Studies
1995-2000	Director of the Center for the Study of Women
1995-2012	Professor of Education and Women's Studies
2012-2014	Distinguished Professor of Education and Women's Studies
2014-	Distinguished Research Professor of Education and Women's Studies Emerita
	Swiss Federal Institute of Technology, Zurich
1993	Visiting Professor of Sociology and Gender Studies

Signs: Journal of Women in Culture and Society

2000-2005 Co-editor

Michigan State University

2010-2014 Distinguished Part-time Visiting Professor of Education and Women's Studies

Cambridge University

2017 Diane Middlebrook and Carl Djerassi Distinguished Visiting Professor of Gender Studies

Tapuya: Latin American Science, Technology and Society

2017 Member, founding editorial team, and International Advisory Board

Honors

1986 *The Science Question in Feminism* nominated one of five best science books of 1986, *Los Angeles Times*

1986 *The Science Question in Feminism* named one of the five best books of 1986, *The Socialist Review*

1987 Jessie Bernard Award, American Sociological Association

1988 *Feminism and Methodology*, Susan Koppelman Award of the American and Popular Culture Association

1990 Woman Philosopher of the Year, Eastern Society for Women in Philosophy

1994 *The 'Racial' Economy of Science*, Outstanding Book Award of *Choice*

1995 *The 'Racial' Economy of Science*, Critics' Choice Award from American Educational Studies Association

2007-08 Phi Beta Kappa Visiting Scholar

2009 American Education Research Association (AERA) Award for Distinguished Contributions to Gender Equity in Education Research

2013 John Desmond Bernal Prize for Lifetime Achievements, Society for the Social Studies of Science (4S)

ABSTRACT

Sandra Harding was born in San Francisco, California, the first of five children born to Lloyd and Constance Harding. Her father's struggle to find work during the Great Depression led the family to Los Angeles, where they operated a roadside diner until the outbreak of World War II. At that point, her father got a position in the civil service and the family moved once again, this time to the East Coast. Harding recounts experiencing sexism in her elementary and secondary schooling in New Jersey, but recalls a warmly loving family environment which included encouragement for the children—both daughters and son—to pursue their educational aspirations. Earning tuition through summer jobs as a waitress and at the telephone company, Harding attended Douglass College and studied literature.

After graduation, Harding moved to New York City where she worked at ABC and held soirees with her friends at her Greenwich Village apartment. She met and married Harold Morick, at that time a graduate student in philosophy at Columbia University. Morick was writing his dissertation on Wittgenstein, and Harding contributed her efforts as typist. After he completed his PhD, they settled in Albany, where Morick got a position in the philosophy department at the State University of New York. There, they welcomed two daughters, a year apart. With the women's movement gaining momentum, Harding found herself dissatisfied with the role of faculty wife and decided to join the ranks of wives and mothers returning to school for graduate degrees. She began coursework in sociology at SUNY-Albany. When she decided to switch to philosophy, she transferred to New York University, partly in an effort to keep some separation between her budding career and Morick's. She elected to focus her dissertation on the epistemology of Willard Van Orman Quine.

Harding's first faculty appointment was at SUNY-Albany's Allen Center. There she began work on feminist standpoint theory. Following an amicable divorce from Morick, Harding accepted a position at the University of Delaware. She was drawn to the University of Delaware because of its active philosophy department, which included a master's program and a focus area on the philosophy of science. She also appreciated Wilmington's proximity to Philadelphia, New York, Baltimore and Washington, DC, which presented her with many opportunities for networking and for involvement in research and writing work for programs run by the United Nations. Harding recalls some strife within the department, especially in the form of vituperative anti-feminist critique of her work, and recalls that the critical tone of her tenure letter belied the 100 percent vote in favor of tenure for her. While at the University of Delaware, Harding began expanding feminist standpoint theory to incorporate perspectives from the feminisms of Women of Color feminism, and she relished her contact with the Black intellectual community in the Northeast.

After a period of splitting her time between the University of Delaware and the University of California, Los Angeles, she accepted a full-time appointment at UCLA's Graduate School of Education. There she continued her active engagement in professional societies including the American Philosophical Association, the Society for Women in Philosophy and the Society for Social Studies of Science. She served as editor of *Signs* and worked with colleagues in Latin America to create the journal *Tapuya*.

Throughout this multi-session interview, Harding often reflects on the influence of social justice movements—the women's movement, the civil rights movement, and the independence and post-colonial movements in nations around the world—on her work, and her steadfast

commitment to producing work that furthers those movements. She emphasizes the practical and managerial approach she has taken towards her writing, teaching and mentorship of students. She describes herself as a "rogue philosopher," and delights in Sharon Traweek's characterization of her as someone who "plants herself on the borders of institutions and refuses to go away."

INTERVIEWER

Joseph Klett is a sociologist of culture and technology. His research focuses on sonic interactions between people, places, and things in social organizations. At the Science History Institute, he served as principal investigator for the Community History Platform, a digital resource for connecting scientific communities to researchers and archives. Before joining the Institute, Joseph was a visiting assistant professor in the department of sociology at the University of California, Santa Cruz.

Jody A. Roberts served as the Director of the Institute for Research at the Science History Institute. He received his PhD and MS in Science and Technology Studies from Virginia Tech and holds a BS in chemistry from Saint Vincent College. His research focuses on the intersections of regulation, innovation, environmental issues, and emerging technologies within the chemical sciences.

ABOUT THIS TRANSCRIPT

This interview was conducted as part of the Origins of Science and Technology Studies project. These oral histories and research interviews focus on scholars who first applied knowledge from the social sciences and humanities to study the social, political, and cultural aspects of scientific practices and technological development.

The Center for Oral History, Science History Institute, is committed both to preserving the recording of each oral history interview in our collection and to enhancing research use of the interviews by preparing carefully edited transcripts of those recordings. The preparation of interview transcripts begins with the creation of a verbatim typescript of the recording and proceeds through review and editing by staff of the Center; interviewees also review the typescript and can request additions, deletions, or that sections be sealed for specified periods of time. We have established guidelines to help us maintain fidelity to the language and meaning of each recorded interview while making minor editorial adjustments for clarity and readability. Wherever possible, we supply the full names of people, organizations, or geographical locations mentioned during the interview. We add footnotes to the transcript to provide full citations for any publications that are discussed, to point to extant oral history interviews, and to clear up misstatements or provide context for ambiguous references in the transcript. We use brackets to indicate the addition of material that was not in the audio, and bracketed ellipses to indicate the deletion of recorded material. The transcript also includes time stamps at five-minute intervals. We omit without noting most instances of verbal crutches and all instances of nonlexical utterances. We also make small grammatical corrections where necessary to communicate

interview participants' meaning. Finally, staff of the Center create the abstract, chronology, and table of contents. With the availability of online full-text searching of our transcripts, the Center for Oral History opted to discontinue the practice of preparing a back-of-the-book index for each oral history transcript in 2020. **The Science History Institute is committed to the responsible presentation of the history of science by addressing evidence of inequality and oppression as well as the subsequent silences in our collections. To that end, we recognize there may be language in our oral history collection that is outdated, offensive, or harmful, such as, but not limited to, the following: racist, sexist, Eurocentric, ableist, and/or homophobic language or depictions.**

At the request of the interviewee and due to legal or privacy concerns, some material has been redacted from both transcript and audio. Textual redactions are indicated in the transcript and the redacted audio segments have been replaced with silence.

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INTERVIEWER: **Joseph Klett**

LOCATION: **100 Harris Street**
Sydney, New South Wales, Australia

DATE: **28 August 2018**

KLETT: So, if we just start, have you say your name and your institutional affiliation.

HARDING: [Yes]. I'm Sandra Harding, and I'm retired from the Graduate School of Education and Information Studies at UCLA [University of California, Los Angeles].

KLETT: Wonderful. And when did you first recognize yourself as a scholar of science and technology?

HARDING: Well, I was trained to be a good analytic philosopher, a philosopher of science, and an epistemologist, but the minute I got my doctorate, I went rogue. And so, my teaching experiences, even though I'm trained as an analytic philosopher, I have always held a joint appointment in a social science department, for forty-one years. I think there was one year when I didn't.

So, for me as a philosopher of social science, for example, I get to live in my laboratory, and I get to watch my colleagues and my graduate students struggle with incomplete data sets, with interviews that miss the important question, with surveys that don't add up to what they want them to, and the struggle they have to make facts out of these data that they collect are just wonderful for me to watch.

So, I taught first in—briefly in an experimental social science college, and this would have been in 1966, '67, something like that, at SUNY-Albany [State University of New York, at Albany], the Allen Center. So, I had sitting in on my classes physicists, economists, sociologists, and when I taught my well-prepared analytic philosophy of science, they took me aside and they said, "Sandra, you're not going to pollute these fresh minds with that, are you?" And I had to rethink what I was doing. I had no idea that that was a bad thing to do.

Then I moved to the University of Delaware, which was a big science campus, because of DuPont, primarily, in 1976. And within a year or two, I had a joint appointment in the graduate department in sociology, and I also taught African-American studies, African-American philosophy, when there was one book available to use for it.

And now for the last twenty-five years or so—I mean, until I retired in 2014—I taught in the Graduate School of Education and Information Studies at UCLA. There are fifty-one line faculty in the department, me and one other philosopher, and forty-nine social scientists. And they have seven hundred graduate students. That's a lot.

So, I was always working with graduate students who were collecting data. They were social scientists. And so, it—this is to say that coming to the "science in society" part of this has been with me from the very, you know, early days of—before [Thomas] Kuhn was Kuhn. It was way back there.

KLETT: How did you find your way into a social science joint appointment in the first place?

HARDING: Well, my—[yes], that's another story. So, as one of the four or five people who developed standpoint theory, feminist standpoint theory, most of the others were social scientists. Dorothy Smith and Patricia Hill Collins were sociologists, are sociologists. Nancy Hartsock is a political scientist. And we would—in the early days, one of us—we knew each other's work. I mean, it was a tiny little group of people working. And one of us would capture a position on an association program, the History of Science [Society] or AAAS [American Association for the Advancement of Science] or something, and invite the others. And we'd have a panel and talk about objectivity.

And in retrospect, it was like watching two-year-olds play. We would sit and look at each other, and then do our own thing. And we didn't know how to put it together in the kinds of ways we subsequently <**T: 05 min**> developed.

So, this is to say that in my own work I've worked with social scientists all the way through, and what I initially referred to as standpoint epistemology, in the last decade or more I've come to call standpoint methodology, because that works better in the practical social sciences. So, that's the best I can say for that. It's just always been part of my life, both institutionally—my students have been coming from the social sciences—and also in my own personal scholarly work.

I can tell you, though, that—I've been told this story several times by several people. When in the sociology department, people say, "What is it that Sandra Harding's doing in your department?" And they say, "We have no idea. It must be philosophy." When the same question is asked in philosophy, they give the same answer. "We have no idea. It must be sociology."

So, I think standpoint theory is one of a number of social theories that have brought together—have made huge changes in research thinking, that have consequences for several disciplines. So, sociology of knowledge. Hilary Rose is another person who was involved, and she's always been a sociologist of science in particular.

So, I don't know how to answer that question, except to say that I went rogue when I got out of graduate school.

KLETT: And so, did—you were developing methodology. How did it touch then the science? Like where did those two worlds come together?

HARDING: Well, I'm going to talk about that later when I talk about the practical implications of STS [science and technology studies]. But let me give the short answer to it now. I think there are two areas that have been and remain crucial. One is public education. We're all educators, and bringing STS issues into general education, not only as a specialty for scholars, but training the next generations how to think about science and technology in the kinds of ways that literary criticism teaches people how to think about poetry and novels and so forth, art criticism does the same, but there's been—STS is as close as it gets in the sciences and technology. And of course, the word criticism scares the scientists, precisely because they don't think of their work as social, having any social context.

But I think the need for that becomes especially clear right now in the wild and awful political context of the US [United States of America], but also other countries, where issues about what counts as truth are on the front page of the paper every day. And when it gets to science and technology, some people treat them just as if they're individual preferences, and as if there's no rational reasons for holding them with the kind of respect and the kind of understanding that STS brings to the project.

So, I think just on this issue alone, this answer to the question alone, many scientists are very happy with STS work and the kind of work that I do. I mean, physicists tend not to understand it, but biologists sure do. Anybody working in the medical area does. Environmentalists do. So, I think the natural sciences are as diverse as any other field, and that's before we get to the social sciences, which are supposed to be doing this, but don't always do it.

KLETT: Great. [. . .]

KLETT: Which text was most important to your early understanding of what STS is?

HARDING: Well, I'm going to—I'm going to pick Thomas Kuhn's *Structure of Scientific Revolutions*, because it just has been so important in the—in my history **<T: 10 min>** here.[1] So, when I was being trained as a good analytic philosopher, my absolutely favorite, very adored philosophy of science professor had been a student of [Rudolf] Carnap's. And so, Carnap was part of the Vienna Circle, the group in Germany and Austria, that in the late thirties and early

[1] Thomas S. Kuhn. *The Structure of Scientific Revolutions*. University of Chicago Press, 1962.

forties fled Europe, and many of them ended up in the US, and indeed, Carnap's office was at UCLA. I was in his office for a while. That's another wonderful story. I'd walk in the door, and that's where he'd been.

And so, my professor just loved Carnap. And we had a moment of silence every October or whenever it was to commemorate Carnap's birthday. This is my first course in college—in grad school. I thought, wow, this is kind of a club or something. This is pretty fancy.

But meanwhile, Kuhn's *Structure of Scientific Revolutions* came out in '62. I went back to grad school in '67, in sociology, for a year, before I got myself over to philosophy the following year. And I read *The Structure of Scientific Revolutions* in some kind of social theory course, I forget what it was. So, Kuhn was being treated as a sociologist at that point, which is where many people have commented he made his greatest contributions, in a way.

So, when the next year I got into—I went to grad school in philosophy, switched to philosophy, and wanted to ask questions about Kuhn from this favorite professor, no way. He said this was ephemera, it would disappear, and he was not going to entertain any questions on it in the class.

So, of course several of us graduate students said, "Wow, let's have a Kuhn reading group. This is pretty exciting, to have somebody that this guy feels so strongly against." So, we did, and we talked about it, and from there I went on on my own, not with the help of the philosophy department, that's for sure, and probably a few years later, I can't quite date this, to read David Bloor and Barry Barnes, and I followed the Kuhn disputes of [Paul] Feyerabend and so forth.

So, the rest is history. I mean, I just—I got into it. Kuhn was really important in that, and I—

KLETT: I love that you willed it not to be ephemeral.

HARDING: Right. We—

KLETT: You yourselves took it up.

HARDING: Yes, we did. Yes, it was—he was at a particular moment, and I'm currently writing a paper for a collection that's trying to bring Feyerabend into the present moment. He's kind of been neglected in that history. And both of them had a kind of—what's the word? A spirit of the era they lived in. I mean, this was the sixties, right? And revolution was in the air. And you can find those kind of reflections of it in both of their work, that a thousand flowers, theories bloom, and Kuhn's very title, *The Structure of Scientific Revolutions*, of course,

scientific revolution has its own quite different history. But it was—I mean, he was living in a liminal space before and after a huge shift.

And my dissertation, the guy I focused on in my dissertation was also—this was Willard Van Orman Quine, who I wrote on—a dissertation on Quine's epistemology. And at that point, nobody thought he had an epistemology. He was treated as a logician, and maybe even a metaphysician. But Quine was clearly trying to pull out of positivism, unsuccessfully, in my view. But he was one of the transitional figures who became important for other thinkers down the road.

So, yes, it was—I still find Kuhn's work very energizing.

KLETT: I don't know if you've—this is neither here nor there, but there's this new book by Errol Morris, who was a student of Kuhn's, and it's called like the—it's called *The Ashtray*.[2] **<T: 15 min>**

HARDING: Oh, really?

KLETT: Because apparently, they had an argument, and Kuhn threw an ashtray at Errol Morris, and this is the—

HARDING: Oh, I remember reading a review of this somewhere.

KLETT: [Yes], I haven't—I don't know anything about the book yet, but I'd be curious to see what his angle on—because it is very much like—Errol Morris is really kind of capturing the zeitgeist.

HARDING: [Yes].

KLETT: And who's just—who digs into the zeitgeist and turns it back on.

HARDING: [Yes].

KLETT: And I think it would be interesting.

[2] Errol Morris. *The Ashtray:(Or the Man Who Denied Reality)*. University of Chicago Press, 2018.

HARDING: I met [Karl] Popper once at some kind of philosophy conference, and my recollection is it was in Canada. And, I mean, those philosophy meetings were fiery in those days. I mean, whoa. I mean, we haven't seen that kind of excitement in a long time. There was just real anxiety. And in some ways—I'll come to talk about it later—those issues of the sixties haven't gone away. They're as—what's the—a Brazilian anthropologist, Eduardo—what's his first name? Viveiros de Castro said [that] Kuhn and Feyerabend couldn't have imagined what different worlds we'd be talking about today at that moment in history, but those issues of working across worlds have only gotten more intense.

KLETT: [Yes]. Absolutely.

HARDING: And with a kind of—same kind of large political consequences that haven't been seen since the sixties, in a way. It's another form of anti-authoritarianism, is the way I think about it. Of course, it goes too far here and there. They always do. But they're important moments.

KLETT: There's also a similar informatic critique from someone like Marshall McLuhan then—

HARDING: Yes.

KLETT: —and we've got, you know, the sort of filter bubble, and—

HARDING: Right.

KLETT: —information streams going on now. It's very much—we're living in it, I think. [. . .]

So, how has—how have you found STS has altered your disciplinary training?

HARDING: Well, as I indicated, you know, I think I've managed to do some of this already. Sorry about that. As I mentioned already—disciplinary training. Okay. I'm sorry. Forgive me.

KLETT: And feel free to repeat yourself if you want to re—

HARDING: Excuse me. I'm just going to take a minute to look here. I renumbered these and forgot that I renumbered them. [Yes], I did some of this already. [Yes], I sure did. Sorry about that.

KLETT: That's okay. No, feel free to repeat yourself, because it's really—

HARDING: Okay.

KLETT: —like per question, whatever the answer is you'd like to give.

HARDING: Okay. So, this is about having always had a social science appointment. So, do it again?

KLETT: [Yes]. Or maybe—if you maybe have an example of a—of a riddle or something you were dealing with, and how you were able to tackle it by having this social science input, or a view to how science gets it done.

HARDING: [Yes]. So, pick up some stuff from later. That's all right. Okay, so ask the question again.

KLETT: Sure. How has STS altered your disciplinary training?

HARDING: Well, from the beginning, I was a rogue philosopher. I've always been involved with both teaching and social science contexts, having joint appointments to sociology departments or, as in the case of recent—the last couple of decades, of the Graduate School of Education and Information Studies at UCLA, the whole department is social scientists of education, plus two of us philosophers.

And also, in my own work, I've worked through—especially standpoint theory was developed by, of course, originally Marxists, but got radically revised by us feminists, beginning in the probably very late sixties, early seventies, we began. And I was a philosopher, but the others were mostly sociologists, such as Dorothy Smith and Patricia Hill Collins, later, and Nancy Hartsock, who was a political theorist, and Hilary Rose, the British **<T: 20 min>** sociologist of science.

So, both in my scholarly work and in my institutional work, I've always been working with social scientists. And one—just to give you one example of how that kind of—I think of it as a borderlands position. I never give up my philosophic training, people tell me, even though that narrow analytic positivist influenced training I think is incredibly narrow, even though it's still immensely popular in many departments in the US and elsewhere.

But I'm never fully in the social sciences, either. Face it. I've never systematically collected data the way social scientists do. But on the other hand, the social theory part of the social sciences has always been important to my work. And recently, it's become especially important, because one of the very practical projects that I think STS is important for is to contribute resources for science and technology policy; that is, to do a kind of educational project for policy makers who hear complaints—let me give you an example.

Just before the Beijing [China] conference on the year of women, whenever that was, back in the nineties, maybe—nineties sometime, I ended up being recruited by a number of UN [United Nations] organizations to help them organize how to present their women, science, and technology issues.[3] They had many, many decades of complaints from women, back to the League of Nations they'd tracked them, about how unfairly women and women's issues were treated in the sciences, but they had no way to organize it that made a compelling argument of the sort that they wanted to make.

And then appeared standpoint theory and strong objectivity, which was the part of it I worked on, and they loved it, because it gave them a way to insert their—not to be a side issue of complainers, but to insert their work positively on scientific grounds into science policy.

And so, I worked with the UN—various UNESCO [United Nations Educational, Scientific and Cultural Organisation] meetings. As a matter of fact, the first time I met Hebe Vessuri, who I know was on your list of people to interview, was in Paris three or four decades ago, and it was one of those meetings, not directly focused on feminism, but there were several feminists there. It was some UNESCO group that was preparing for Beijing.

So—and I met with UNIFEM [United Nations Development Fund for Women], which is the—I forget exactly how you work that acronym out, but it's a very old organization. It had existed under the League of Nations and was collecting all these complaints and disorders.

And so, this proved a wonderful opportunity to show the importance of STS for policy, and that was a couple of decades ago, preparing for Beijing. I've been asked to get involved other times since then, but rarely have, because there are plenty of other people who can do it now.

But somebody who is a perfect example of how important this work has become, Londa Schiebinger, who's a well-known historian of science, of French science in the seventeenth and

[3] The Fourth World Conference on Women: Action for Equality, Development and Peace was a conference convened by the United Nations in September 1995, held in Beijing, China

eighteenth centuries, as I recollect, she heads an institute at Stanford—she has a whole listserv that tracks—and she must have a team of a lot of graduate students, because they track what's happening in public institutions all over Europe and the US, and they're just beginning to reach out to the rest of the world.

And they—I get mailings maybe every two weeks, **<T: 25 min>** and they list three new science and technology programs in Belgium, France, and the Netherlands that have now inserted into their requirements for applications that they explain the relevance of this work for gender inequalities, what effects is it going to have on it. And so, she has—you can get one—you can get the backlists. It's just an incredible project. And while it's a bunch of separate data pieces that she's collecting, they're tied together by this larger understanding now that science and technology are in society, and consequently, society is inside science and technology, whether it likes it or not. It's there.

So, I think that's been—in terms of how my borderlands disciplinary position has turned into a kind of more general policy position that's working back and forth between philosophy and the social sciences, theories about science and scientific practices.

One of my good friends at UCLA is a well-known 4S [Society for Social Studies of Science] person, Sharon Traweek, an anthropologist and historian of science institutions. And she and I knew each other, have known each other for a long time, but we both arrived at UCLA I think the same year. And we went to each other's events. I went to her events in the history department, lectures and so forth, and she went to mine in philosophy and wherever I was going to. She's here at these meetings.

And she finally said to me one day, she said, "Sandra, now I understand you for the first time, what you do. You plant yourself on the edge of an institution, and you refuse to go away." And I think that's true, because I'm located at UCLA in a graduate school of education. Most of my friends in the arts and sciences think that's an inferior place to be. I think it's highly superior. It's interdisciplinary. It's a very well-established one, highest rank in the US. And it has as its mission improving education, and science and technology education are a lot of that, as well as the research methods for their own work.

So, I think what looks like a strange liminal position at one moment is the harbinger of some kind of—frequently can be the harbinger of a more general shift in an institution. After all the departments that most colleges have now were established in the late nineteenth century, and there's no reason why those departments should be the only ones relevant.

And administrations, if you look around, go to a lot of effort to try to bring breaths of fresh air into their colleges, which they find difficult to do in those older departments. So, they're very happy to establish these cross-disciplinary groups, because those are ones that can bring in—that's certainly the case at UCLA.

KLETT: [Yes], it strikes me that between both your example of Londa's work, and, you know, working in the capacity of educating the educators here, it's dissemination, and it's disseminating information in a very systematic way—

HARDING: That's right.

KLETT: —a codified way, as opposed to the sort of, for lack of a better term, the ivory tower idea of pinnacles of knowledge that could be a beacon of light if you so choose it, but who's going to choose that when it's so off point?

HARDING: Exactly. Exactly.

KLETT: Fantastic. [. . .] Shall we move on to four, discuss 4S?

HARDING: You know, I don't—let me just tell you, I don't—I only have two little things to say about it.

KLETT: Okay.

HARDING: I would rather—how much more time do we have?

KLETT: We have as much time as you need.

HARDING: I'd rather take more time to talk about the other practical application. Which would be journals.

KLETT: Okay. I would like you to discuss your time at 4S.

HARDING: Oh, okay.

KLETT: Just, you know, if it's not a significant role or part of your career, that's fine. I'm just—to get a sense of—

HARDING: Okay. I'll just say a little something.

KLETT: Sure. [Yes], if you could—

HARDING: They're very obvious things to say, but—

KLETT: Sure. If you could just tell us the story of, you know, **<T: 30 min>** your first meeting, maybe your first impressions of 4S—

HARDING: I can't even remember the first meeting. But anyway, we'll give it a try.

KLETT: And how you feel like the field has changed since then, whenever that is.

HARDING: Yes. Okay. So, I can't even remember my first STS meeting—I mean, 4S meeting—but I think there were probably about 150 people there, maybe. So, the first obvious change is 4S has gotten huge. How many people are here? Is it two thousand?

KLETT: I don't know.

HARDING: I think it's around—something around that. I mean, if you look at that humongous program, and look at the huge breadth of topics and approaches being covered, and look at all the stuff going on on the side, and especially all the efforts this year that—to make STS honestly international, instead of simply claiming an internationalism that really is not grounded in reality.

So, I would say the increase in sheer numbers has been accompanied by an increase in areas of work, and one obvious one is that at the beginning, my recollection is there weren't that many policy people. It was mostly—maybe not even historians. The historians have tended to keep their distance from 4S. I think that's unfortunate, but that happens. But it was mostly sociologists and anthropologists. It wasn't people hanging around the edges of Washington, DC, but there are a lot of them now.

And they don't—frequently don't share the background of STS work in particular. So, it makes I think very interesting exchanges, and constant self-education, so to speak, going on, through the 4S meetings. I always love to look at the program and see, wow, I had no idea they could bring STS perspectives to bear on that. That's great.

So, for me, it's the one conference I try never to miss. Some of these foreign ones are too much travel for me, but I managed it this time, and I'll be glad to go to New Orleans [Louisiana] when it's back in the US again.

KLETT: Do you feel like the policy orientation, does that exist solely—is 4S a nexus for that, or is policy also finding its way into STS the departments and journals and the other kind of spaces that STS goes these days?

HARDING: Well, the journals I've been involved with, it's definitely—I've been heavily involved with, it's definitely getting in there, and that would be—I edited *Signs*, S-I-G-N-S. I have to spell it, so people don't think I'm saying science. *Signs Journal of Women in Culture and Society*, which was not the first big international women's studies journal, but it was the biggest and best established. I think its first volume was 1976, University of Chicago Press.

By the time I edited it, which was from 2000 to 2005, it had an eight percent acceptance rate, and you could—if there was going to be one feminist journal that some institution was displaying on their coffee table in the waiting room, it would be *Signs*. I ran into it in the Romanian Ministry of Technology and Railroads or something like that. You know, there it would be. And there—definitely policy issues about gender and science were important in that, but more so as the years went on, I would say.

For the last two years I've been involved in the—I'm on the editorial team for a brand-new journal that's brilliant. It's called *Tapuya: Latin American Science, Technology, and Society*. And it's in English, though all the editorial decisions are made south of the Rio Bravo, the Rio Grande. They're all made in Latin America. The rest of the editorial team is a group of brilliant young Latin Americans. A number of them are here. They're on the editorial team now. They're from Brazil, from Argentina, from Mexico, **<T: 35 min>** from Colombia.

And of course, *Science, Technology & Society* for the peripheral countries, if I can put it that way, that have not been strongly represented in let's say 4S, is always a matter of policy, because they've had to suffer the consequences of development, modernization theory that has science and technology at its basis. They've had to suffer the misperceptions and mistreatment of their own science and technology systems, which have been very powerful and frequently are very old, but virtually unknown, or treated as merely myths and superstitions by the North. And so, for this journal, the policy issues are very important. They're not the only ones, but they're important.

I don't know what to say about the role of—I'm not close enough to big graduate programs in science and technology studies in the US to say much about them, but the people I know are interested in policy.

KLETT: [Yes]. It does seem like both the truly transnational spirit of this year's 4S, journals like *Tapuya*, that they—they don't enjoy the privilege of duality, right? They're forced to confront these issues in very practical ways. Like you're talking about, right?

HARDING: [Yes].

KLETT: So, policy doesn't become, oh, [yes], let's include some policy papers in the journal.

HARDING: [Yes].

KLETT: It becomes, policy is what is relevant. You know, maybe it could become a qualifier of what would make a suitable article to include, or a topic to address, because it's got impact in that sense, right?

HARDING: Right. [Yes]. I mean, with their agricultural policy, their medical policies, their internet communication policies, you know, all of these are affected by the resources they can get from various places, but including the places the North gets them, like the United Nations, UNESCO, and various other agencies, and what the World Bank and IMF [International Monetary Fund] think about what they're doing.

And so, there are a lot of very interesting studies going on about how not only, so to speak, the bad guys from the North, the openly colonial, extractive practices of Northern corporations and so forth, but also a lot of us good guys, a lot of us progressives, have nevertheless failed to actually understand how science and technology works in Latin America. The journal is committed to turning Latin Americans into subjects of scientific knowledge rather than suffering the historical position of being the objects of other people's knowledge and extractive policies, using them as data, using Latin America as data.

So, I think things are changing, and the policy—the way policy issues figure in—outside the North, and in subjugated communities in the North, are becoming much more understood by the centers of power in the North. So, that's a good thing. It can't happen fast enough. And it is always blocked by the vested interests in those still present colonial practices of extractive—they don't want to—Northern corporations don't want to share the profits with the poor. Right? They're highly invested in keeping a whole bunch of poor people around, whether it's working in their mines, or agricultural fields, or in service industries in Los Angeles, or wherever. So, it's the usual interesting political situation.

But again, I think this is a particular historical moment, when issues about the role of science in society are on the front page of the *LA Times* every day, because of the politics of distruth that have become not only a scandal in the US, but in many other countries, too. <**T: 40**

min> So, it's a particular moment when global forces that we can't control, but in which modern Western sciences and technologies have played a big role, for better and for worse, at least, there's a lot of communication now between—in the internet and cell phones and so forth. But this, you know, change is happening.

And 4S needs to, as it is doing hugely at this conference, reach out to incorporate those issues into the ongoing thinking of 4S people, because we're pretty powerful educators. We know how to do it. And a lot of us are doing it in different ways. I'm happy to work with a journal that's published in English, but presenting the Latin American perspective on the kinds of issues that *ST&HV* [*Science, Technology & Human Values*] hasn't until recently begun to pick up on.

So, we hope to be a resource—the journal will be a resource for STS in the North, but also for science and technology policy anywhere.

KLETT: Fantastic. I feel like we've already previewed question number five, but just to kind of recapture—or recapitulate this in terms of practical application—

HARDING: Okay, I'm going to stop for one second and cough.

KLETT: Go for it. I don't know if you have a particular way you want to rehash this conversation in terms of the STS—

HARDING: I thought I'd maybe talk about *Tapuya's* structure, which is kind of interesting.

KLETT: [Yes].

HARDING: Because it's very innovative. And I've got a page on it somewhere. Where the heck is it? There we go. Excuse me for a second. Let me eyeball and remind myself what I thought I was doing. [Yes], I've pretty much said it, but we'll try it again. [Yes], I'll recapitulate it, and step in whenever you want to.

KLETT: Sure. It gives me more to work with on editing and trying to do this on the fly. So, I'll just—I'll ask you the question.

HARDING: Okay.

KLETT: What do you think has been the most important practical application of STS?

HARDING: I think that there have been—there have been two kinds that seem extremely important to me. One is public education, and I'll say something about that, and the other is working within the kinds of institutional resources we know how to work within a second way, namely journals, which are—which can reach big audiences, larger than we perhaps have in mind.

So, first, public education. We are educators. We know how to do that. And it has been very interesting to me that STS is beginning to develop undergraduate programs. There aren't a lot of them, but there are a couple of them, maybe more than that, already starting, and certainly undergraduate courses are starting up. And I think that's an excellent place to bring the insights that STS offers. After all, we have literary criticism that helps us understand how to read poetry, plays, novels. We have art criticism that helps us understand how to look at Picasso's *Guernica* or Rembrandt, and understand it in society and in its own terms.

But it's only STS that begins to do that for science and technology. They've always been left outside that legitimate interpretative framework. History, it's only scientists who have been legitimated to tell the story of science. After all, professional history of science is not that old, right? It was usually the aged, wonderful old scientists reflecting on—there's a couple of volumes out there that pick that up, and they're very amusing, and very lovely in a certain way, but they feel far away. They feel like something that is not done in literature or any other field.

And I think this is a particularly important **<T: 45 min>** moment to take on that problem, project, of public education, because issues about what counts as truth and what counts as mere opinion, which are central to science thinking and STS thinking, are on the front page of the *Los Angeles Times* every day, and everywhere else around the world, too. So, I think STS and 4S in particular have an especially rich set of resources to contribute to these discussions.

So, public education I think is one very important place for it. A second one, as I mentioned, is journals, the disciplinary journals, which have not usually been thought of as public education. They've been thought of as communication between us scholars. And that's of course extremely valuable, but they can also be resources for public education.

And I'm now involved in helping to get founded a wonderful new journal called *Tapuya: Latin American Science, Technology, and Society*, and it's published in English, but all the editorial decisions are made in Latin America. I—I don't make editorial decision. I got it incorporated in the US. I got it a bank account in the US, which the publisher in England and all of the South American editors insisted the bank account could not be in Latin America, because the political situation makes that very vulnerable. So, it has a Bank of America account in Los Angeles. But all the editorial decisions are made in the South.

It is electronic only. It is open access, which is terrific, because people who can't afford subscriptions—and of course, the whole publishing industry is a riotous chaos these days, as libraries and individuals try to figure out how to deal with the costs of publication—but for *Tapuya*, for it to be available anywhere in Latin America where you can get on a computer is great. It's open access. It's free to those who want to read it. But authors pay to publish, which is a pattern that got established in the natural sciences.

However, in the case of this journal, the fees are very low, and they're waived for anybody who does not have paid research. So, I think they're—[eight] hundred dollars is the top, but that's waived [for] anybody who needs it waived.

So, the journal is indeed available for public education, and it has already attracted a wonderful set of articles. We're about halfway into volume one, and one can go online and read them. And when we get a full volume one, we will start approaching other institutions, particularly the Latin American Studies Association. We're now affiliated with 4S. It shows it right on the literature—I don't know if you can see it here, but here's the 4S symbols. And also ESOCITE [Sociedad Latinoamericana de Estudios Sociales de la Ciencia y la Tecnología], which I get—bungle the Spanish, but it's the equivalent in Spanish, in Latin America.

But we want to approach also the Latin American Studies Association, which for the most part exists only in the US. In Latin America, they don't have Latin American studies. There's a little of it, and there certainly are some programs elsewhere, in Japan and Norway and so forth. I have to take a cough. I'm going to swallow a cough drop while I'm at it. That should do it for a little bit. Okay. Where was I?

KLETT: You were forging alliances with Latin American studies.

HARDING: Yes. Latin American Studies, if you check out your local university program, rarely has anything on science and technology. It ends to be entirely literature and history, **<T: 50 min>** which is, you know, wonderful. There's not even very much in the social sciences, occasionally an anthropology course, but not a lot.

And so, we want them to know that they can now offer science courses on their campuses, because *Tapuya* will be providing a steady supply of first-rate articles on science, technology, and society, the equivalent of four issues of a year of a regular journal. In year one, we'll probably only have twenty full articles, plus a bunch of book reviews, but we hope to move up to thirty or maybe even if we get really smart and lucky, forty, very quickly. So, there'll be plenty of articles on topics about science and technology in societies, plural, the different ways they function in different parts of the world. And this makes possible kinds of courses that weren't possible before.

So—and we hope that will have an effect, as the educational project does, on new generations coming up, that they will be able to understand the importance of STS for their

lives. So, I hope 4S has ten thousand people attending, and that *Tapuya* is one of the journals and projects that's made it possible for many of those people attending to be coming from other parts of the world than the former British Empire and Europe.

KLETT: And I'm struck by your point about the sort of siloed nature of Latin American studies, the idea that the development of STS is almost as like a strain of cultural studies.

HARDING: [Yes].

KLETT: Where we have American studies, we've got Latin American studies, AF-AM, and that STS was almost another kind of cultural study that didn't ever harmonize with these other cultural contexts.

HARDING: [Yes].

KLETT: And I think what you're suggesting with *Tapuya* is that, you know, there's a particular Latin inflection on STS that matters—

HARDING: [Yes].

KLETT: —and that you can't just take the two parts and smash them together.

HARDING: No.

KLETT: That you need to understand—and in many ways, the recognition that STS has been a very Northern, very Western kind of construction, hasn't acknowledge the other kind of constructions that have happened outside of that quadrant.

HARDING: That's right. And that's also young. I mean, STS is relatively young, and we've needed to be talking to each other, so we knew what we could be saying to larger audiences. And so, I think the very fact that STS has so expanded itself, that it's made itself important for policy, for example, and now has this huge outreach to other parts of the world, I mean, *Tapuya* is not the only such journal from other parts of the world. The *East Asian Science and Technology Studies* is out there, and there are a few others. There's an Italian one, and so forth, that are developing.

And there are going to be more. We're already talking with some people in China. There's a wonderful Columbia [University] graduate student who got his doctorate in the very best STS program in China, knows all the top people there. They're thinking about starting a journal in mainland China. And he and Columbia—it's going back and forth between 4S and the Chinese people. And there's already a 4S—is it a 4S? An STS listserv for Africa. And I know there's talk within 4S of trying to get a conference in Africa sometime soon.

Meanwhile, I believe—you might have to cut this if it turns out to have been a wrong thing to say, but I believe that in 2022, there'll be a joint meeting in Pueblo, Mexico, [between] 4S and ESOCITE. So, 4S has really been moving on this, and that's important.

KLETT: Great. Have you covered everything you wanted to cover? Is there anything you wanted to add? Before I ask you my curveball final question?

HARDING: I don't think so. I think I've gone [over] everything twice by now. **<T: 55 min>** I'll take the curveball.

KLETT: Okay. So, interpret as you will. Do you know any STS jokes?

HARDING: Oh, do I know any STS jokes? Oh, isn't that a good question, and why don't I? Are we too serious?

KLETT: I don't think we are. I think we—I think there's lots of humor in what we do, and I think, you know, just even the tradition—the methodology, which is built in the ironic humorous—I feel like there's something there, and I want to know what STS people find funny, and if they know any jokes.

HARDING: I really don't know. You know, it's interesting, because I think STS, unlike some other disciplines, we're always a little self-critical, because we have to position ourselves always with people from every side saying, "What? You're doing that? How dare you do that?" from every side. So, I think we get used to kind of maneuvering our positions. I mean, I'm sure this isn't true of everybody and so forth, but it's not—I mean, we're a new field that's intruding into—I mean, there are sociology of science courses, of course, in many departments, but not as widely as there could be. And ethnographies of science and technology. You know, obviously, histories have been there, but I think we're still in the earnest stage, and I don't—I'm going to have to think about that now. I don't know any jokes, and that's sad.

KLETT: That's okay.

HARDING: Have you heard any?

KLETT: No. Mostly the reaction is similar to that. But again, you know, we're also talking to—this series of interviews are very sort of generational. You talk about the Dark Ages. The Dark Ages aren't very funny times, right? And like you're saying, this sort of—the sort of politeness of pushing into someone else's space.

HARDING: [Yes].

KLETT: I think, you know, maybe if I asked that question of a recent generation of STS scholars, they might point to examples from the work that you've been leading in, you know.

HARDING: I'm sure they could make fun of us oldies. [laughter] There's a start.

KLETT: Absolutely. But that's a fantastic answer. So, the final thing I'd ask you for is as a—you know, these are really thumbnail histories.

HARDING: Yes.

KLETT: So, I'd like to give you a chance to, you know, if we gave you a byline, Sandra Harding, what would your byline read, other than your UCLA affiliation and your standpoint theory? What would be the way you'd like to be introduced to the world?

HARDING: I don't know if I'd like to be introduced in the world this way, but it certainly is something I keep in my mind, and that's Sharon Traweek's statement that I plant myself on the borders of institutions and refuse to go away. And I find that a very fun place to be. I mean, I've managed—I have to be very careful not to recommend it to graduate students, because they say, "Oh, I want to do what you did." I say, "I don't know about that. You have to start out somewhere else, I think."

It was kind of an accident of history that I got drawn—that I went rogue, coming out of philosophy into the social sciences, and without any training in social sciences, more in political philosophy and social theory. So—

KLETT: It also took someone else to point that out to you, right? It wasn't a deliberate strategy, to be incalcitrant.

HARDING: No. Well, no, it wasn't, but—

KLETT: It's in your nature.

HARDING: It's in my nature.

KLETT: You're a stubborn individual.

HARDING: I am. I'm stubborn and I'm loud, but I'm polite. So, it turns out to be a great combination, as far as I'm concerned.

KLETT: [Yes], it worked for you.

HARDING: [Yes].

KLETT: Fantastic. Well, I think we're good. Thank you very much.

HARDING: Well, thank you for the interview.

KLETT: Of course. Thanks for making the time.

HARDING: Thanks. It was great. [. . .]

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

INTERVIEWEE: **Sandra Harding**

INTERVIEWERS: **Joseph Klett**
Jody A. Roberts

LOCATION: **Harding's home**
Playa Vista, California

DATE: **23 April 2019**

KLETT: All right. This is Joseph Klett with Jody Roberts, and we're sitting, beginning the oral history with Sandra Harding. We are here in her home in Los Angeles, in the Playa Vista neighborhood, and it is April 23, 2019. So, if we could begin, could you tell us about where you were born and raised?

HARDING: [redacted 00:00:24 to 00:01:02] I was born in San Francisco in the depth of the Depression, March 29, 1935. And my father had already had trouble finding work. He had had many different jobs. He was a plumber. He was an electrician. He was an assistant to Miss Jane Pittman, of Pittman Shorthand. He was an assistant to Frank Galbraith of time-budget studies. He was a man of all trades, but couldn't find stable employment during the Depression.

And so, in San Francisco, he worked various jobs. I don't know exactly what—plumber, electrician, something. And then things got really bad, and my sister was born. And so, in 1937, they moved down to Los Angeles, to Sunland. And they moved in with Great Aunt Ruby. So, this was my father's aunt or great aunt. I think it was my father's great aunt—my father's great aunt. Her father or grandfather, I can't track it exactly, had been a forty-niner, who hadn't found any gold, but had settled in Sunland. And so, it was a beautiful orange orchard up against the hills. I have a very romantic watercolor painting of it in my bathroom. I'll show you.

So, my sister was a couple of months old, 1937, and so my father worked as a real estate agent. I mean, he did various catch-me-as-you-can jobs, but was fundamentally unemployed for a good part of the time. And built a café on the side of the road there. It was on—not 210. Is it 210? Foothill Boulevard, going through Sunland. Where we lived is now condominiums, but the park across the street was just as it was.

And I don't know, maybe twenty years ago I went into the park community room or something, and there was a caretaker there, and I started chatting with him, and it turned out his wife and I had been in the same kindergarten. [laughter] He was a Latino guy in there. So, that park was there.

So, to move a little bit quickly, they ran this coffee shop for, I don't know, five years or something like that. And it was on the main trucking route into Los Angeles from that direction, so they had a steady supply of clients. And my mother cooked, and my father eventually built a

little kind of cottage on the back for my sister and me, and Great Aunt Ruby was—I have a wonderful photo of her. I'm five, and she's ninety-five or something like that. And for her, fashions had hit their absolute peak around 1880, so she's holding my hand, and we're looking at these palm trees in front of the cottage. She has on her 1895 clothes, and I'm five years old, and the photo's from behind. And we inherited the equivalent of that photo at ten-year intervals, from 1850 on, when those palm trees were first in coffee cans, and then we see them growing up through the years.

So, then the war starts in 1942, and my father got a job in the civil service on the East Coast.

KLETT: Can we pause for a second?

HARDING: [Yes].

KLETT: What is your father's name?

HARDING: My father's name is Lloyd Edward Everett Harding.

KLETT: And what was your mother's maiden name?

HARDING: My mother's maiden name was **<T: 05 min>** Constance—I forget her middle name—Kimber. [Victoria—Constance Victoria Kimber.]

KLETT: Kimber was the last name?

HARDING: [Yes].

KLETT: And the diner, was this just Depression-era ingenuity, or did they have a background in running a restaurant?

HARDING: Nope. Nope.

KLETT: Had he worked in one?

HARDING: Nope. Just they figured everybody needs food, so—I mean, that's the way it was in the Depression.

KLETT: [Yes].

HARDING: Let me just enter a couple of interesting things about my parents. My mother grew up in an orphanage from the age of seven, I think, with her three siblings, even though both her parents were alive. And her father was already a very distinguished financier. He became an Undersecretary of the Treasury in the Wilson administration and taught finance at NYU [New York University]. And I just lent out a book of his that I ordered from the Library of Congress. *[Foreign Government Securities: A Text-Book for Banker and Statistician*].[4] It reprinted important books that were in the public domain already. And a couple of his were among them.

And that was very interesting to—I never even knew his name, he was so hated in our household. He was only referred to as AW. It turns out his name was Albert William Kimber.

So, my mother grew up without a family in—not in a traditional family, and in a way, so did my father, because his father died in a diving accident in Illinois when he was eight, I think. And his mother remarried, and the new husband was not fond of the kids by the first marriage, so they were sent out to live with Aunt Ruby. And I have a photograph of his older sister, who died as a teenager—I never met her—teaching school when she was, I think, twelve. And there's her [class of] four six-year-old students, [standing next to her].

So, both of them suffered the vagaries of [broken] households of that particular historical era. And so, when they came to do family, they didn't exactly have models, so they kind of did it the way Hollywood would do it. They were the best parents. All the other parents—all the other kids were so envious of us and our parents. It was—they so loved family, and so tried to make it work. So, it was—it's a lovely inheritance, in a certain painful—you know, kind of painful way.

KLETT: I was going to ask, why was Aunt Ruby out there?

HARDING: Why was Aunt Ruby?

KLETT: [Yes], in California.

[4] Albert William Kimber. *Foreign Government Securities, a Text-book for Banker and Statistician*. New York City, A.W. Kimber & Company, Incorporated, 1920.

HARDING: She was a descendant of the forty-niner.

KLETT: Oh, okay. Right.

HARDING: I think she was the—could she have been the daughter? She would have been born around—my father was born in 1895, and so she would have been—[yes], she could have—she could have been the daughter of the forty-niner.

KLETT: Okay.

HARDING: I have a family tree somewhere, but I can't remember what generation that is.

ROBERTS: Is there more story on your mother's side of—that they shared about how they ended up in the orphanage, or—

HARDING: Yes. My grandfather, AW, as he was referred to in the household by my mother and grandmother, ran off with a secretary. My grandmother had been his secretary. She had been the first woman court stenographer in New York. And so, they lived in Brooklyn [New York]. There was an older brother, Adrian, my mother, Constance, and then two younger sisters, the youngest of which was born in a reunion between them when the rest were already in the orphanage.

And so, she had no way to support the children, and they were picked up off the streets of Brooklyn [when they were begging] by, as my grandmother always referred to them, "the papists." Catholic charities. [laughter]

That was intolerable to my grandmother. And so, she got them into this orphanage up outside of Poughkeepsie [New York]. And at one point in my years of camp following my professor husband, he was teaching at Vassar [College], and the orphanage was about twenty minutes away. And this is—you know, my mother is [at this time] sixty [or so]. She was visiting, and we went over to it. And when we got there, the young woman who when she went in at age eight, this young woman had been fifteen or something, she'd been the **<T: 10 min>** daughter of the owner of it, and this little family came in, [three] girls and a boy, and then a third girl arrived, and she kind of adopted this—this fifteen-year-old, kind of adopted this family. And I'm going to get weepy.

My mother and they hadn't seen each other, for [forty-five years, or] something like that. They threw their arms around each other. She was now the director of it. And it was no longer an orphanage. It was a home for court appointed kids or something – I forget what it was. But, you know, that—I think this—I've always had a deep melancholy that's kind of related to my mother's deep melancholy. I mean, it's not anything surface. I mean, she was never depressed, that I knew. I mean, maybe she was, but—and I've never been depressed. But there's—you carry something of those kinds of painful events that your parents can't talk about. And so—

KLETT: Was the—I think you said your grandmother—your grandmother had a problem with the Catholics. Was it—was it the religion of the orphanage that was the issue, or was it just the fact of sending kids off to the orphanage?

HARDING: The orphanage was fine. The orphanage [was sponsored] by Harry Emerson Fosdick. Do you know who he is?

KLETT: [No].

HARDING: He was very famous in the 1920s and '30s, and he appears back in my life at a later point, several—two later points. I'll tell you. [. . .]

KLETT: Oh, sure.

HARDING: And he was a pastor who'd been head of the First Baptist Church in New York, and founded Riverside Church, and turned it into an ecumenical Protestant church, which was the first of the sort.

And so, the orphanage was run by—had been sponsored by this guy who was ecumenical. My grandparents were just prejudiced against Catholics. And my mother carried a little bit of that. I mean, it wasn't a visible thing, except that the lady next door to us liked very black tea, and my mother always said, "She drinks tea the way the Irish do. I wouldn't drink it that way." I don't know—you know, that's the Blacks of the early part of the twentieth century, remember, the Italians and the Irish, and so they shared that, to some extent.

On the other hand, in their overt kind of civil rights selves, they were very progressive. My father appointed the first Jew and the first Black to the local school board. He was very disappointed when I brought home a couple of those guys as [steady boyfriends] in college, because it was not his idea of how it should work out, but he was polite and kept his views to himself, so . . . [laughter] If kids can figure out a way to really annoy their parents, they'll figure it out.

KLETT: [Yes]. In your—you had written this piece in *Work and Politics*[5], this chapter you shared with me, and you had mentioned that what you absorbed from your father was his fascination with how things work, his respect for working people's knowledge, and social inequality, if only in local contexts.

HARDING: [Yes].

KLETT: I underlined that, because I think this—

HARDING: That's what I meant.

KLETT: —you were getting at that point, right?

HARDING: [Yes]. He was a good liberal.

KLETT: [Yes].

HARDING: If you know what—

KLETT: [Yes].

HARDING: It's a limited position. It's—

KLETT: Right.

HARDING: But for his era, that was progressive. And when he worked at Fort Monmouth—so he arrived at Fort Monmouth in [1942]—so he came to Camp Evans originally when we moved East. It had been Camp Evans. It's Sandy Hook State Park now, in New Jersey. It's that hook of land up there across from Manhattan.

[5] Sandra Harding. "Philosophy as Work and Politics," in *The Philosophical I: Personal Reflections on Life in Philosophy*, ed. George Yancy. Lanham, Mass.: Rowman & Littlefield Publishers, 2002, 23-42.

KLETT: Okay.

HARDING: But it had been a big radar station in World War II. And it was an outpost of Fort Monmouth. And so, we lived on the Jersey shore, and he worked there, and then when Camp Evans shut, he [moved his work to the main site of] Fort Monmouth, [a dozen or so miles away].

And that was in the era of McCarthyism, and he—two of his friends committed suicide during the [Senator Joseph] McCarthy era—McCarthy was picking on Communist Party members, and especially at Fort Monmouth. And my father was not a Socialist. My brother was in the CP, and my brother was [the founder of] one of the [local] SDS [Students for a Democratic Society] groups. And my father loved him, but the political distance between them was humongous, because my—a kid will figure out how to really annoy their parents in the worst possible way. **<T: 15 min>**

KLETT: It was politics. So, can you name your siblings?

HARDING: I had three sisters and a brother. One sister died, and so now I have two sisters and a brother.

KLETT: She died in childbirth or later in life?

HARDING: No, she just died of—I think she had a heart attack and a stroke and died of a bunch of things when she was sixty-five.

KLETT: Okay. Could you name them in birth order?

HARDING: Yes. So, there's twelve and a half years between me and the youngest. The sister who died is the next one, Rosamond. She was born in 1937. Victoria, Vicki, who lives in New York City now, and has kids, and so forth, was born in 1942. David was born in 1945. And Constance Joy, referred to by us as Joy, was born in 1947.

KLETT: So, when did you leave Sunland for New Jersey?

HARDING: Nineteen forty-two.

KLETT: Forty-two. Okay. So, that was—

HARDING: We actually moved to Tujunga [California] for a year or two there. I forget why. Then from Tujunga, nearby. They're very close.

KLETT: In LA [Los Angeles]?

HARDING: [Yes], out in the hills there. So, I did kindergarten and half of first grade and then second grade here [in California], and then in 1942, moved East and started third grade in Bradley Beach, New Jersey. And I'm going to tell you a very funny thing that has nothing to do with this story.

KLETT: Okay.

HARDING: So, 1942, [I've arrived] in Bradley Beach, New Jersey. [I went to elementary school there]. A friend of mine [today, here in this retirement community], her name is Elinor Liebesman, the guy she's married to is in assisted living over there, [across the courtyard from me]. He has Parkinson's. [. . .] He and I were in the same classroom from second grade through eighth grade in Bradley Beach, New Jersey.

KLETT: Wow.

HARDING: Isn't that weird?

KLETT: That's wild.

HARDING: It's very weird. So, watch out. Your pasts come around in unexpected ways.

KLETT: Maybe even downstairs.

HARDING: [Yes].

KLETT: Wow. So, could you maybe just paint a picture of what your childhood was like in these elementary years for you? Obviously, your siblings are in different stages, but you move to New Jersey, your parents have another couple of kids. What's your everyday life like?

HARDING: So, we—I had to babysit them all, so they all [still] hate me in some level. It was a pretty good—it was a lovely childhood. [In New Jersey] we lived in a neighborhood. It was very much a neighborhood. We played out on the street. We played basketball in other kids' backyards, and people were running in and out of the [other houses]—you know, the parents were looking over [whichever kids were] in sight. We lived three blocks from the beach, so we spent a lot of time on the beach. I grew up body surfing and just loved the beach.

My parents believed in lessons, so I had tap dance lessons, I had ballet lessons from when I was four until I was sixteen. I had piano lessons from four until seventeen. I must have had—I was a twirler in high school. You know, twirler?

KLETT: [Yes]. Lead the band, right?

HARDING: Pardon?

KLETT: You lead the band?

HARDING: You lead—yes. Yes. It was—I think of myself as having had a very happy childhood. I had a lot of friends. And started out in—did my freshman year in one local high school, and then the family—the family had been renting houses in Bradley Beach, and then they got enough money together to buy a house and move ten miles south to Brielle, New Jersey, which is the beginning of the Inland Waterway down to Florida. It's a boat culture. You never have to go out on the ocean. You go into the Chesapeake Bay, and there are canals and what not—I mean, maybe you have to go on the ocean for a mile or something, and then you—so the whole place was a boat culture. And I had a sailboat, but by this time I'm talking high school. And had friends, and started to have boyfriends, and I'm not sure what else to say about it. We spent a lot of time on the beach.

ROBERTS: When you were back here in California, before you went East, were you helping at all with the café, with the coffee shop diner?

HARDING: I was seven when we left.

ROBERTS: So, did you—did you help out at all?

HARDING: No, I sat up at the counter and read the soup cans. I was an **<T: 20 min>** easy reader.

ROBERTS: [Yes].

HARDING: I was—no, they didn't have me—they didn't want me around dangerous kitchen appliances or anything.

ROBERTS: Sure.

HARDING: And I was too young to do any waiting or anything. I mean—

ROBERTS: [Yes]. And when you moved East, did her—your mom had been working, helping out with the diner.

HARDING: [Yes].

ROBERTS: What was she doing while your father joined the civil service?

HARDING: Having children.

ROBERTS: Every two years.

HARDING: [Yes]. Exactly. And she had a lot of miscarriages. So, Vicki was born in '42 and David in '45 and Joy in '47. So, she had five kids. And she did go back to work when Joy—when the youngest kid got into school on a full day, she started working as a substitute teacher, and eventually as a school librarian. And she did that for many years.

ROBERTS: Did she have a particular subject that she taught?

HARDING: No, I think she taught at the very low grades, kindergarten, first grade, second grade.

KLETT: So, you didn't encounter her in your own schooling?

HARDING: In my own school?

KLETT: [Yes].

HARDING: No.

KLETT: So, during this period, what were your—I'm curious about your first recognition of this thing called science, and what your early impressions were of that.

HARDING: Not good. I hated chemistry in high school. I can't—I must have taken physics—oh, [yes], I remember, and then I was in college. We had this horrific teacher. He was really crazy. I took almost no science. I had no relationship to science, except that my father was an engineer, so he was always fixing things, and I was the eldest kid, and he just longed for a son. And so, for ten years I got to be his son. So, he taught me to play baseball until my brother came along ten years later, right?

And so, I was always fixing things with him. And so, it's really engineering more than science that I had a relationship to. But I don't particularly have a relationship to science now, apart from gardening and so forth. And I, as you know, didn't come into this field through a background in science, but rather through philosophy of knowledge production, fundamentally.

KLETT: [Yes]. It sounds like your sense of science at that time was as sort of like a—a sort of—an academic recognition—

HARDING: It was a school topic.

KLETT: [Yes], a school topic. Exactly. When did you realize that engineering and science were intimate fields, or intimate activities?

HARDING: I don't think that was—I mean, I did know they were intimate. But I don't remember a moment when that thought came to me or anything.

KLETT: [Yes].

HARDING: I think I was just allergic to science. I mean, also, teachers were terrible in those days.

KLETT: [Yes].

HARDING: Around girls, in math and science. I can remember seventh and eighth grade, the math teacher would not call on the girls in the class because, she said, "You're not as smart as the boys." So, when we raised our hand, she would never—she told us what she was doing. "I'm only going to call on the boys, because the girls aren't smart enough."

I mean, you know, that whole world was—this was before feminism, right? I mean, the whole world was—just to expand this issue, when I went into philosophy, there were almost no women in philosophy. In the generation above me, there were maybe two or three who wrote. Ruth Barcan Marcus was a logician. I can't think of the names of the others. I mean, there were probably plenty in the Black colleges and in the Catholic colleges. But not in the private white colleges or the big state university systems.

So, at the American Philosophy Association, my generation of graduate students, and I'm ten years older than most of them, because I went back to graduate school late, we were mentoring each other. And, you know, when I entered, the women's movement was forming. This is the next stage, so I won't go into it. But when I entered graduate school, I entered in '68 in sociology. I was in SUNY-Albany, and I didn't want to go into my husband's [philosophy] department, and I didn't want to go into English, which I had been an English major as an undergraduate, and edited the literary magazine, and was a reporter for the **<T: 25 min>** paper, and so forth.

I didn't want to go into English, because I'd learned—that was the era of New Criticism, where there was—it's the equivalent of analytic philosophy—I don't know how to— And there's a few. History went through—it was a post-World War II isolation of the formal elements of these disciplines from any historical or cultural background. It's what all the critics of STS were trained up in, right? So, if you're looking at any cultural or historical phenomenon, that's interesting, but it has nothing to do with the content.

So, we analyzed words and sentences quite apart from the context in which they were produced, any meanings they might have that laid outside the text. So, I wasn't about to go into that, because I learned to enjoy novels again, fifteen years later. So, I picked sociology.

But then the department at SUNY-Albany was great at producing quantitative master's graduates, and I'm one of the few philosophers who's taken a statistics course, and I loved it, because meanwhile, I'd been grading my husband's logic papers from his students, right? And I'd gotten into it. And statistics was a lot of fun.

But I couldn't—I was interested in sociological theory. I wanted [Karl] Marx and [Max] Weber and [Emil] Durkheim and all the big guys. And my advisor gave me the equivalent of Cliff's Notes on each one of them. And I was outraged in graduate school to get Cliff's Notes on Max Weber and Durkheim and the others, Talcott Parsons.

So, the easiest next university for me to—I mean, Yale [University] was actually physically closer, but it was really hard to get there from Albany [New York]. So, I looked at NYU, because they had a very good at that time social theory set of faculty in sociology. And so, this is how I ended up in philosophy. The sociology graduate courses met three times a week for two hours each. I looked over in the catalogue, and the philosophy department courses—no, they met twice a week for two hours each. The philosophy department graduate courses met once a week for four hours, or three hours, I guess it was. So, I switched to philosophy as a—

[Crosstalk]

I mean, I'd already been socialized into the discipline, because I'd married a philosopher, right? And so, it was really easy. I mean, I knew what was going on.

KLETT: [Yes].

HARDING: And I was in the first generation of returning women students, which NYU had a particularly large supply of, because so many women were for family reasons attached to the area, and their husbands were teaching, or whatever they were doing, in the area, so they couldn't go elsewhere. But NYU was not as highly ranked a graduate program as it has subsequently become, but it was a good one. And it was possible for us to do the degree there. So, that's when I began taking back my maiden name, Harding.

KLETT: Where did your—where did your—your desire for social theory come from? Like you—as you say, like science wasn't the calling.

HARDING: I think partly the civil rights movement and feminism. The sixties. I was actually not involved in marching around the streets in the sixties, because I was either—had babies, or I

was in graduate school. My babies were born in ’65 and ’66—I mean—babies, they’re now 52 and 53.

But the spirit was there, and the women’s movement was very interested in social theory, obviously. And in Albany, New York, there were something like thirty colleges within an hour—

KLETT: Right.

HARDING: —of downtown Albany. So, there was all kinds of women’s organizing. And New York had a liberalized abortion law three years before Roe v. Wade, and it was the state capital, so there was a huge—and the civil rights movement was very active in Albany, and it had a big Black population. So, there was a lot of marching around in the streets, and Marx’s *Capital* study groups, and, you know, the whole apparatus of sixties intellectual social theory.

So, I think my whole career has really—my career, once I got out of grad school, **<T: 30 min>** has really ridden the crest of the social justice movements. It’s very much taken its impetus from them. And I’ve always intended to work for them, to provide the kind—I mean, strong objectivity was done not out of abstract interest in it, but because the social justice movements needed a way to argue that their work was objective, more objective than the racist and classist and sexist and eventually ableist and homophobic and so forth, [mainstream] theories.

And so, I designed those—I’ve always designed my writing projects to serve as—number one, to be taught, so they have to be in a language that you can teach at the upper-level undergraduate or graduate courses. I mean, they don’t always work that way, but that’s been the goal. And they have to be of service to the social movements. And so, I think I just was in that moment in history when that was happening. I mean, the same thing happened around this postcolonial stuff.

KLETT: [Yes].

HARDING: It just started to happen.

KLETT: So, know—so knowing where we end up in the sixties, then, I want to—because we kind of leapt over the—

HARDING: Yes, we did.

KLETT: So, the question about science and eventually social theory is really about role models, mentorship. Where were you seeing—you've cited your parents, both of them, and their—both what they lived through and what you witnessed them live through, and the sort of—their enduring attitude for this.

HARDING: [Yes].

KLETT: Were there other sources? Were there other professionals or other teachers or folks who gave you some kind of inspiration to eventually find your way to philosophy and social theory?

HARDING: Well, my writing always got praised, and in elementary school, my seventh-grade teacher wrote—you know those little books that the students take around? I forget what they call them.

KLETT: [Yes].

HARDING: Ms. McInerny wrote in it, "You will be a great writer." And I looked at that page for years, and I didn't know what it meant. And I later came—she saw something. And I loved writing, and I still love writing, much too much. I write too much. But there's something about that azure screen, and thoughts start to look true when they're up on it, right? Compared to before the screens, my manuscripts have these strips of typed paper taped—you know, the revision process was not like it is now.

KLETT: [Yes].

HARDING: So, I think my writing always got praised, and then the minute I did start writing professionally, I was writing for these social movements. And as I say, we mentored each other. I mean, there's a few faculty I could mention who were, you know, useful, but they weren't serious mentors all the way along or anything. And frequently, they were discouraging. I can remember as a graduate student getting papers back. My doctoral dissertation had one little check on page 73, no comments. I'd get straight As in everything, but no feedback from the faculty.

And I still see that happening in graduate schools. I think it's a hugely irresponsible thing. And I always made my students publish before they got their doctorate. I mean, I give them feedback all the time. And one graduate student told me once as she was leaving my

office, she said, “Sandra, we graduate students, after we meet with you, we always go out in the hallway and cry.” I said, “Oh, I’m so sorry.” She said, “But you’re the only professor who gives us honest feedback on our work.” And I always tried to be tough love, to be really clear about what they needed to do. But to give them all the support I could for doing it. But I probably didn’t do the last part as much as I should have. So, we mentored each other . . .

KLETT: Where—what sort of literacy—I’m wondering like what sort of—was it fiction, was it the great novels, was it pulp? Like—or were you reading more—

HARDING: Well, I was an English major in college in comp lit.

KLETT: What’s the prehistory of that? So, like you’re—obviously the assignments were pleasing your elementary school teachers. What were you reading to influence your writing?

HARDING: All of Shakespeare in one ten-week course. All the Shakespeare major plays in a ten-week course. **<T: 35 min>** What were we reading? I don’t know—one advisor I had, Fred Brantley, he paid attention to my work. [redacted 00:35:07 to 00:35:40]

So, I was reading everything. I mean, I had sixty English credits as an undergraduate. You’re only supposed to have thirty. But I just took every literature course. I just liked reading all of it. So, I remember I took everything. And I read novels, lots of novels.

KLETT: Did you have a favorite author?

HARDING: I loved Jane Austen. I read all my way through Jane Austen I think three times. This was later. I mean, I read them once in college, but later, I read them all, Hemingway, and, you know, whatever the—I haven’t read a novel now in probably twenty years, maybe thirty years.

ROBERTS: Is that on purpose?

HARDING: The material I read on colonialism is like—it’s like reading a novel all the time. There’s all these villains and victims and retribution and exotic locales and parts of history you never knew about. I find all my novelistic pleasures completely satisfied by this postcolonial literature. And meanwhile, I really just like analyze—I really like the analytic part of philosophy, not in the way that it was taught then, but trying to figure out what’s going on here that’s not so visible on the surface.

KLETT: I'm thinking, because, you know, you cited—to mention Jane Austen, for example, it's—you know, it's a lot of manners, it's a lot of social conventions, right? Like you kind of need to know the stakes of those stories to appreciate them.

HARDING: [Yes].

KLETT: And so, I could see how that would translate well into a social theory. And an interest in social theory, right?

HARDING: That's interesting.

KLETT: As you—especially when you've got large casts, you know, a cast of players—

HARDING: Oh, that's so interesting.

KLETT: —siblings and things.

HARDING: [Yes], I hadn't thought about that.

KLETT: All those political dynamics that are in these stories.

HARDING: No, I took everything. Greek and Roman literature. I mean, whatever they offered, I took it.

ROBERTS: I guess I'm curious, even earlier than that, so maybe scrolling back, you know, you're a child of the Depression.

HARDING: [Yes].

ROBERTS: Your parents have themselves very unstable childhoods.

HARDING: [Yes].

ROBERTS: You've said, you know, you took lessons, there was a lot of kind of idyllic family nurturing going on.

HARDING: [Yes].

ROBERTS: But what was the imagined outcome for you as a life path or a career path that—

HARDING: I—

ROBERTS: I mean, either foisted upon you or for yourself.

HARDING: Well, my parents had very different visions, but let me tell you something first. I remember, I think it was the summer I was twelve, so the little local library was a three-block walk, and the librarian would only let me take out two books a day. I couldn't take out any more. And so, I'd take them out and I'd bring them back in two days, right?

And I spend one whole summer trying to read my way through the fiction section. I started with A, and I just read everything. I just read them in order. And I sat out under the hydrangea bushes in the front yard, so my mother wouldn't find me and make me come in and take care of the kids. And I just read novels the whole summer. And, I mean, it was all trash, and good novels. I mean, whatever came up in the alphabet. I think I got up to L in three months. I mean, it wasn't a big library. But that's a lot of stuff.

So, let me see. You were asking—what was your question?

ROBERTS: Just kind of what did you imagine doing, or what did your parents imagine you doing?

HARDING: I don't think I had any imagination about it. My mother had to make a case to my father—my father thought that girls could be a nurse or a teacher or a secretary. And that's what he wanted. He didn't want me to go to college. He wanted me to go to one of those other schools. And my mother had graduated from Elmira College, which—out of the orphanage, which was the first college to give Phi Beta Kappa to women, and she'd **<T: 40 min>** been Phi

Beta Kappa. And she had to convince him that I could go to the local, you know, Douglass College of Rutgers University, which was about an hour away.

So, I don't think I had any career senses at that point. And I didn't have much sense in college. In the 1950s, there wasn't much sense given to women about any kind of career except secretary and some others.

KLETT: [Yes].

HARDING: And indeed, I think in my graduating class at Douglass in 1956—how many kids were in that class? I think there were about 150 in that class. It wasn't huge. I'd be surprised if there were four who went to graduate school at that point. I mean, others have gone back later. Four out of 150. I mean, it's—quite a few were married already by the time they graduated. A couple had kids. I didn't have any—I didn't have a sense of it, and I really didn't after college, either. I worked in New York, and then in Poughkeepsie, for twelve years or something like that. I graduated in '56, and I went back to graduate school in '67 in sociology, in '68 in philosophy at NYU. So, '67, '56 to '67, that's eleven years or something like that, just in one New York.

You know, typical English major, fancy secretary-type job, I worked for four years in the legal rights division of the American Broadcasting Company, when that was the first —I took the first television law course offered in the country at NYU. That's—so this is '56, and TV is just getting going, and the three big networks, and so forth. And after four years, my husband and I went to Europe for, I don't know, a couple of months, so I quit that job, and when I came back, I got a job working as the assistant to the president of the Keedick Lecture Bureau, which was I think maybe the second oldest lecture bureau in the country. It dated back—his father had founded it in the Chautauqua days. You know, the Chautauquas, all over the East Coast, these big summer—they were educational projects, big auditoriums, and you'd have famous novelists, and Amundsen and Scott, polar explorers, and whoever, come into—they were popular education. And he had founded the Keedick Lecture—the father had founded the Keedick Lecture Bureau. And I worked as his assistant for four years, and I wrote publicity, and I wrote letters.

And, I mean, I didn't have any—I can't help it, I was a young woman of my era, and—I mean, the only—when I came back to go to graduate school, so my kids were born when I was thirty and thirty-one, and that's 1965 and '66, and I was living in Albany, New York. And what was I doing? I was—oh, I was teaching something or other. No, I just—I was just—[yes]. One kid was born in Poughkeepsie, and the second was born in Albany, So, I—that year, with two babies, was the first year of my life since I'd been five that I hadn't been either in school or working. And nobody was speaking English to me all day long. There were just these little babies. Then my husband would come home from SUNY-Albany.

And after a year—so meanwhile, the women's movement had started up, right, and so a group of us started meeting. And they were other faculty wives, for the most part, or the first women faculty at SUNY-Albany. They were the first—they were offering the first courses in the English department on women's literature in the—and so a couple of these already had their degrees, but others with small children—well, my friend Gloria also had kids who were a few years older than mine, and she had gone back to grad school.

So, we were—we elderly women, age thirty-one and thirty-two, were starting back to grad school, which was the first time that the professors had this **<T: 45 min>** experience of older women coming back to school. And they were—I had professors who were younger than me. They were arrogant. They were really something. It was an experience. But I had my colleague group, so we were mentoring each other on how to do this.

ROBERTS: When did you think about going back to graduate school? You mentioned earlier that—

HARDING: Right then.

ROBERTS: When you were at SUNY-Albany?

HARDING: [Yes], and my friends were—oh, my God, they were teaching their kids to read when they were three. They were taking them to ballet lessons every day. It was like, I thought, no way, how could this—this is not a life for me. So, a couple of my friends were starting back to grad school at that moment, and so they—I can remember Gloria and I talking about it. She had just started, and I started back the next year.

And—[yes], so I went to grad school for five years, getting my doctorate in '73, I think it was. And that's—I just was a product of that 1950s—

KLETT: It's interesting to me, because you had a mother who went to college.

HARDING: Yes.

KLETT: And so—

HARDING: That was the 1920s.

KLETT: Right. So—

HARDING: That was the first women's movement.

KLETT: So, it's not impossible to imagine going to college, but your father was also—so you're almost playing out this sort of dichotomous thinking on what women could do.

HARDING: [Yes].

KLETT: And it's—and it feels like almost, the way you're describing it, like—like tiers are being unlocked.

HARDING: [Yes].

KLETT: But not—it's not "we can do anything," girl power.

HARDING: No.

KLETT: It's, well, women have done this, so we can do that, too, and it almost seems kind of halting in its movement.

HARDING: [Yes], because there was nobody ahead of us who was mentoring us, and pulling us up in. There just wasn't, for me.

KLETT: [Yes].

HARDING: I mean, there were people who appreciated my writing. There were people who appreciate this, that, or the other thing. But they were not—as I said, philosophy was pitiful. I mean, there were a few more women in sociology, but not a lot. I mean, you know, it was different in different disciplines.

KLETT: Right. So, you started Douglass College in '52.

HARDING: [Yes].

KLETT: Was there much—and you were seventeen. So, was there much—it doesn't seem like there was much turmoil in you going to college, if you—

HARDING: Once my mother convinced my father, it was fine. And then there was the money issue, so I worked my way through college.

KLETT: Where is Douglass College?

HARDING: In New Brunswick.

KLETT: Okay, So—

HARDING: New Jersey. It's on one side of New Brunswick, and Rutgers is on the other side.

KLETT: So, that was an hour from your home?

HARDING: And that was an hour from my home.

KLETT: And so, you major in English and comparative lit.

HARDING: [Yes]. I edited the literary magazine for two years. I was president of the English Majors Club. I got straight As in everything, had a wonderful time.

KLETT: And was that about working, or was that about just loving the experience and wanting more—

HARDING: Just loving the experience. And I of course was away from—I'd had boyfriends in high school, but Rutgers was on the other side of town, and my parents were a long ways away. So, freshman year, I think I had—I forget what it was. I counted it up. I only did blind dates. I

only dated guys once my entire freshman year. It was a whole different—to have—I mean, Rutgers was a huge men's college. And it was all—all the fraternities and the parties, and there was a—you know, a very—Douglass College was also only forty-five minutes from Princeton [University]. So, I dated Princeton guys and Rutgers guys. I had a wonderful time. I was a good girl, but I did have a good time.

KLETT: Could you say something about, you know, the fifties, and the experience of that, being Cold War, and your—what was it like going to college during that period? And in ways beyond your studies and your studies and your—

HARDING: I don't think I understood the Cold War until much later. I mean, I got a few little pieces of it from, you know, my father's friends committed suicide, but I didn't know those friends. They weren't ones he—I mean, they were friends of his that work at Fort Monmouth, but they weren't close, really close friends. And I wasn't big on reading newspapers in college. I didn't get into that until later. I don't know. News was—I can't imagine it now, but it was another world. Right? My world **<T: 50 min>** was circumscribed by the whole college activities.

I don't think I—I sensed that we were asking—we, college women, were asking for things that we had to struggle for at the university. We finally got them to let us into—we had to wear a skirt to dinner every night, so some of us just hung a skirt up on the coat rack and put it on over our jeans or whatever we had on. But, you know, it was in between the—it was the beginning of the end, in some ways. The sixties weren't there yet.

KLETT: [Yes].

HARDING: And I don't think—I didn't have a very good sense of the fifties until much later. I mean, the New Criticism, which in one form or another appeared in all of the disciplines, all of the social sciences and humanities, this refusal to look at the historical/cultural context of intellectual work, I was very excited by it, because it was this whole new thing, and it was exciting stuff. And Professor Brantley was writing about it in *The New Yorker*, and I was in his short story writing class, and, you know, I didn't have any understanding of the world, I would say. It was just really limited.

ROBERTS: Can I go one more time kind of back before college? Based on that, just thinking about what dinner conversation, what household conversation was like, because you mentioned the part about your father's friends committing suicide during the McCarthy era, that your brother was very active in the Student—

HARDING: SDS.

ROBERTS: [Yes]. And the social—Student Socialist Party work. Were there politics discussed? What was your sense of—

HARDING: At the—at our dinner table?

ROBERTS: Or just in the home, or informally, or more formally?

HARDING: Not—not much. I can't—except for my father's criticism of David, and of course, David was critical of me. I mean, as a younger brother would be of his older sister. "If you're so Left, how come you're not down at the factory working with the women in the factory? How come you're just writing this nice stuff and you're—" This was later, after college.

I don't remember politics being discussed. What would that have been, [Dwight D.] Eisenhower? I have no recollection who was President in '52 or '56. Summers I went home and worked. Where did I work? I worked in the telephone company as an operator one year, two years, actually. I did that in high school. Then I worked as a waitress, I think, for the rest of the high school years, and earned my—I paid my own—I mean, tuition was incredibly cheap in those—I think it was four hundred dollars a year or something like that, at Douglass.

And then I moved to New York when I was twenty-one, and still wasn't involved in politics. We were—I rented an apartment in [Greenwich] Village with two friends, and we had literary soirees, and invited—I mean, I had a wonderful time. But I can't remember—dinner table conversations were about the neighborhood and about people we knew in common and all my younger sisters' and brother's activities. They weren't very located in the larger world. I don't remember that. I think we were purposely protected from it in some ways.

KLETT: When you mentioned—early on you mentioned your father including minorities in—

HARDING: [Yes].

KLETT: —positions of leadership, were these things that he ever discussed when you were younger, of like, "I can't believe this guy at work believes this thing, and I"—did you get a sense of his sort of—

HARDING: No, he was very proper. He wouldn't be considered an older father now. [He] was forty when [I] was born. But he was then. That was—and he was almost thirteen years older than my mother. And he was very proper. I mean, he had been a farm boy, and then a very [elegant and sophisticated] young man in the 1920s in New York. But no, they just had a struggle to keep—we were poor. When we grew up, we had—there was always food on the table, but there wasn't a lot of money for anything else. And my mother made a lot of our clothes, sewed them on the sewing machine and so forth. **<T: 55 min>** And a lot of topics were just never discussed, like divorces, or a parent of a friend who committed suicide, or—I mean, it was a kind of constrained WASP-ish world in some ways, in retrospect, I think.

KLETT: There's a line you use in *Work and Politics*, and you were talking about your parents, and you say that you—I think you're saying you inherited the cautious but principled optimism with which they weathered their lives, and I was curious about that, of why do you think they remained optimistic, considering the struggles that they'd been through?[6]

HARDING: I think they loved each other so much, and they loved the family, and they didn't have any sense that it was their fault that they were poor. I mean, they could see what the world was. My childhood was very full of love, of being loved. And we had other relatives. We had aunts and so forth, not a lot, but we had a few. My grandmother lived with us for a while.

But I don't—so cautious but optimistic, because I think they had a lot of hopes for us, and they were making it. It was a struggle, but they kept food on the table, and they got us all into college, and so I think they—they were proud of us, and they were proud of each other, and the whole neighborhood was like that. I mean, so, you know, they had friends all over the neighborhood, and friends at work, and so forth. And I don't know what they thought about McCarthyism, or—I mean, I don't know—ever remember those as topics of conversation. They were cautious because you never knew what would happen, right? I mean, whether it was health-wise or politically or economically or—

KLETT: [Yes]. Right. But there was a sense of agency there?

HARDING: Yes.

KLETT: You could determine your fate to some degree?

HARDING: Yes. And they were—

[6] Harding. "Philosophy as Work and Politics."

KLETT: And they were doing well while doing it?

HARDING: My father had been in World War I. I didn't mention that, but he was too old to be drafted in World War II, but in World War I, he went in in the last year of the war, 1919. When did the war end, 1919? Maybe he went in in 1918 to 1920, something like that. And he was an aerial photographer who—flying these rickety little planes. They'd drop—he swore they'd drop bricks on each other. I can't believe that. That was some of their weapons. They'd fly over the German airplanes, and drop bricks.

KLETT: Oh, okay.

HARDING: He made up stories all the time, so I'm not sure about some of these. But he—

ROBERTS: It sounds like it could be true. [laughter]

HARDING: It was—

ROBERTS: They were rickety planes. They were rickety planes.

HARDING: And he would be lying on his belly in the back with a camera, right, shooting aerial shots of the Loire Valley, and, you know, he was stationed in France in World War I. So, he'd seen a lot of the world. He'd worked on a banana boat in the Caribbean, and, I mean, he was very adventuresome by nature, but also there were those opportunities. He had a—when it snowed, he had us take off our shoes and all walk barefoot in the snow. He said it was an important experience for us. And the neighbors—my mother was so embarrassed. We were all out there in our heavy winter outfits, except no shoes. [laughter] Walking around the in the front yard, and dragging the four-year-old. I think the two-year-old was relieved of this duty. But the rest of us—so he had a lot of kind of scientific spirit, I would say, a certain adventuresome educational experience.

KLETT: It's also—that depiction of him looking through a plane in the nineteen teens, right?

HARDING: [Yes].

KLETT: And seeing the Loire Valley, that's an extremely rare perspective to have had access to.

HARDING: Yes. That's true.

KLETT: And I feel like that—I mean, it sounds like he'd seen a lot.

HARDING: [Yes].

KLETT: Not—no pun intended. He's seen a lot in this life, and he wants you to see that—

HARDING: That's true. He had. Because that was the era of "Go West, young man," and so forth and so on. You know, it was that particular era in American history, where there was a lot of destabilization of families and urban development. And, I mean, from—he was born in 1895 in Illinois, through to 1935, when I was born, that forty years was a huge shift in the American economy and politics, and two world—one world war, and another one about to start. And [yes], **<T: 60 min>** they had a kind of—I think their love of each other gave them powerful energies, and of us. And that it survived through all my mother's miscarriages. I think she had about five, actually. You know, those were the days when they didn't know how to deal with that more effectively, so she had five kids, and I think she had five miscarriages, and they lived through all that.

So, they managed to keep their health, and we were all moderately healthy, and the kinds of things—bad things that befell other people didn't befall us. And they got us to college.

ROBERTS: Did all your siblings go to college as well?

HARDING: Excuse me?

ROBERTS: Did all of your siblings go to college?

HARDING: Yes. Well, David was at Johns Hopkins in engineering his freshman year. He dropped out to help found [a chapter of] SDS. And he didn't go back to college for another fifteen years, and then he went—I was so angry at him. He went to a Black college [on an affirmative action scholarship]. He took a place, a white guy, at a Black college. I knew the

Black college. I'm forgetting the name of it. It's in Baltimore [Maryland]. So, anyway, he went back and got a degree later. [redacted 01:01:19 to 01:01:38]

KLETT: Sure. So, I wanted to return to your decision after college to move to New York City. You're working for ABC.

HARDING: [Yes].

KLETT: And you're twenty-three. And you were doing these sort of literary salons—

HARDING: I was twenty-one when I moved—

KLETT: Twenty-one when you moved there, twenty-three when you meet [the man who] becomes your husband.

HARDING: Twenty-three when we get married.

KLETT: Oh, when you get married. Okay. Could you tell us about when you met your husband, what was his name—

HARDING: [Yes].

KLETT: —and all those things?

HARDING: So, his name is Harold Morick, [. . .], and he's a philosopher. And when I met him, he was a graduate student at Columbia in philosophy. And I told you I'd come back to Harry Emerson Fosdick, right?

KLETT: [Yes].

HARDING: So, first of all, my parents met at Riverside Church during the Depression. My father was head of the theatre guild. The church ran a whole bunch of social services during the

Depression for people. So, my father was head of the theatre guild, and met my mother, and they ran away to California.

So, years later, well, many years, I'm living in New York. My parents are living down in New Jersey, and there's a reunion of the theatre guild up at Riverside. And so, my parents invited me to come. So, I joined them, and there's—to me—all these old geezers wanted to dance with me, right? I'm, whatever, twenty-five, and they're sixty. And Hal ran the elevator there. He grew up right around there. His parents ran a delicatessen that was called the Columbia—it wasn't called it, but informally, it was the Columbia delicatessen, 124th Street and Broadway. And so, he was the elevator operator. We met in the elevator at Riverside Church, and started dating.

And so, where was I working at that point? Oh, so I was working for ABC. I was dating lots of other people. We had soirees and invited all the handsome young men we met to them, and did play readings. And it was the Village, right, in 1950s. It was a lot of fun.

KLETT: And what was the attraction or the common ground?

HARDING: Oh, my God, he was writing his dissertation—he'd written his master's on Albert] [Camus, and he was writing his dissertation on [Ludwig Josef Johann] Wittgenstein. And he was very tall, dark, and handsome. What's not to like?

KLETT: Had you read Camus? And did you have a sense of—

HARDING: Pardon?

KLETT: Had you read Camus yourself, and thought, well, if he's scrutinizing—

HARDING: No. But I ended up typing three times his doctoral dissertation. I could never take a course on Wittgenstein and Camus when I went to grad school. There was no way I was—that was that, and I was going somewhere else.

ROBERTS: [Yes].

HARDING: I mean, I had to finagle my career then carefully to stay out of—he was—he had quite a few books and **<T: 65 min>** collections of papers on Wittgenstein and that era of philosophy. And I needed to make sure I got out of it, so I—so—

ROBERTS: Was that for his good or for yours?

HARDING: My good.

ROBERTS: [Yes].

HARDING: Well, I mean, [. . .] I didn't want to go to SUNY-Albany because I didn't want to be in his department. And I had this—so pissed with my advisors at NYU when I got there. I'd go for an appointment in their office, the first thing they'd say is, "How's Hal?" And I would fume and say [to myself], "Call him up yourself, you jerk. I'm your mentee. Let's chat." So, that's when I took back my maiden name. It didn't do any good. They knew who he was. I could see the handwriting on the wall, and I'd better get an identity that was separate. No, he was very charming. We had a lovely time.

KLETT: Harding is your—

HARDING: Maiden name. [Yes]. My children have a hyphenated name, Harding-Morick.

KLETT: So, clearly, Harold was handsome, as you said, but what—can you remember more of like what did you imagine this young man was up to that was so attractive to you?

HARDING: Well, he was very attentive. He's was very loving. He—we played tennis every week. We did a lot of fun things around New York. He lived up with his parents at that point, like Amsterdam and—across the street from Columbia, fundamentally. And I lived in the Village. And he had some, you know, fun friends that we spent time with, and I had friends, and—I don't know. We just had a good time together. And he didn't try to control me the way various lawyers and doctors I was dating did. When push came to shove, they wanted to tell me how to fix my hair, how to dress, what to wear, what to say here, what to say there. And he had none of that. He wasn't into that. It was such a relief. I think that was a major thing.

KLETT: Was there sort of a—a downtown/uptown distinction, since you were coming from the Village, and he was up there?

HARDING: No. I mean, he had been used to socializing in the Village, and his friends lived mostly in the—his [boyhood] friends lived mostly in the Columbia area.

KLETT: Is Riverside Church—is that in the same part of the—

HARDING: Yes, it's a block away.

KLETT: Okay, so he gets his doctorate.

HARDING: We get married.

KLETT: You get married in—when you're twenty-three, so—

HARDING: Right.

KLETT: —in '58?

HARDING: And he gets his—so let me see. We got married when [we were living in] the Bronx. He started teaching—I can't exactly remember what college it was. It was some little college over in Jersey City, while he was writing his dissertation. And so, how long did we live there? I mean, he didn't get his dissertation until—he took a long time. Everybody took a long time in those days. They don't permit that any longer.

I can't even—I could figure out the dates if I sat down and did it. But we moved to Poughkeepsie in '64, so we were married in '58. Is that right? I graduated in '56. We were married in '58. We moved to Poughkeepsie in '64, and '66, he taught at Vassar for two years. And I started having babies. And then in '66 we moved to SUNY-Albany. And I think he got his PhD before we went to Vassar, or one year after. So, he got his—he's a year and a half older than me. So, he graduated in '55 from Brown University, and had started law school at Columbia, and decided he didn't like it, and so he switched to philosophy. So, '55. So, '56, he started philosophy, and he got his degree in '65, probably.

KLETT: Okay. Which is also the year your first daughter was born.

HARDING: Yes.

KLETT: And what's her name?

HARDING: Dorian. [. . .] She's a librarian at Yale.

KLETT: Oh. And then the next year, you had your second daughter?

HARDING: [Yes], **<T: 70 min>** Emily [. . .] and I and Eva, her daughter, lived together from when Eva was five months old until two years ago, when they returned—I'm in deep sorrow at having—I mean, I grew up with this kid, right? She's—you'll hear more about this later, but she's got three more weeks to be fifteen, and then she starts college on August 14th. She's going two years early to college.

KLETT: Wow.

HARDING: Yes.

KLETT: You weren't kidding about them taking a long time back in the day.

HARDING: [. . .] So, they lived with me—it was wonderful. And Emily worked for a colleague at UCLA all those years, managing grants. The education department at UCLA is the highest external grant recipient at UCLA except for the med school. I had many colleagues who had somewhere between $3 million and $15 million in grants running, because everybody invests in education.

So, Emily was a kind of middle-level employee of [one of my colleagues], managed grants there for eight years, and then she—when Eva started turning into a teenager, if you know what I mean, she decided she needed her old community back there in Northampton. She'd gone to Smith. She lived in Northampton for twenty years. She worked there after college.

So, to my great loss, they moved back, and then I sold that house and moved in here within a year. So, I've been here two years. So, she left here three years ago. So, I have an intense relationship with both of them, and a good relationship with Dorian. Just lucky. It hasn't all—you know, they're always difficult, but—

ROBERTS: Where is Eva going to college?

HARDING: Simon's Rock, Bard [College] at—you know Bard at Simon's Rock?

ROBERTS: [Yes]. And where—

HARDING: You know it?

KLETT: [Yes].

HARDING: They invented early college, is what they say. They've done it for thirty years. You know, Bard College, it's about an hour north of Manhattan. But Simon's Rock, their other campus, is up in Brattleboro, Massachusetts, I think.

ROBERTS: Something like that. [Yes].

HARDING: It's about an hour from where they live now in Northampton. So, Eva will live on campus. This little kid. She's not little. She's not a kid. But 15? To me, she's still a little kid. She's trying—she's had a formidable and—a totally disappointing task of trying to teach—teach me trigonometry. I wish her a lot of luck, but it's not working very well.

KLETT: So, if we can go back, then—

HARDING: [Yes].

KLETT: —those years, '65 and '66, when you had mentioned this was the first time since you were a young child that you weren't in school or working.

HARDING: [Yes].

KLETT: And it seems like that was different for you.

HARDING: Oh, it was awful.

KLETT: And you were aware of that.

HARDING: I hated it.

KLETT: You hated it?

HARDING: I mean, I loved my babies, and I loved my husband, but I—that was not how I wanted to spend—and looking at my friends, who were highly educated, I mean, you know, college graduates, going crazy teaching their kids to read at three, oh, my God, it just struck terror in my heart. This is not what I envisioned for myself, and I wasn't going to do it.

KLETT: And what we—today, we'd call it helicopter parent. You didn't want to be the person who's just surrogate to their children.

HARDING: Not only a helicopter parent. It's a parent with no other life. I mean, a very frustrated generation of women. I mean, we weren't too far from the fifties, right, in '65, and it was still—it was not for me.

ROBERTS: So, you said when you got into NYU, you wanted to—earlier, you said you wanted to have a separate identity—

HARDING: [Yes].

ROBERTS: —intellectually than your husband.

HARDING: [Yes].

ROBERTS: How did that guide either the courses you took or the topics you chose to take interest in?

HARDING: So, he [worked on] <**T: 75 min**> Wittgenstein, remained in analytic philosophy of that particular era, and I took a course in that, but I hated it. I mean, the minute the word Wittgenstein appeared, I noticed an allergic reaction. [laughter]

I typed that dissertation—I say three times. I don't know if it was three times. It was a lot of typing.

So, my—I loved my philosophy of science class, and I loved the logic classes. And I did my—came to do my—I was moving through graduate school very fast, because people—women like me were [returning] to college, and having been managing households with a working husband and two kids, we could manage graduate school. It was easy. I mean, we had no—we knew how to organize our weeks to have time to get our work done. It was—it was hard to do, but we knew how to do it, compared to our peers in graduate school, who were coming out of undergraduate school. They had no idea how to organize graduate work. And we just stormed ahead. We knew what we were doing. It was kind of fascinating.

When I was about to get my doctorate, one of my professors whom I liked a lot, Raziel Abelson, [redacted 01:16:19 to 01:16:24] he said to me, and I am now 38, I think, he said, "Sandra, what are you leaving so soon for? We've hardly gotten to know you." I'd been there five years, right? While commuting from Albany, New York, right? It was—that was the era. It was an era.

So, I took—I just never went into [Hal's] areas. And it wasn't—it was a conscious choice, because partly, I didn't want to be where he was. Partly, I didn't want to—those people didn't interest me. I'd read his dissertation and his books and everything, and they weren't interesting to me to pursue. I mean, he was in a way an earlier—not a completely earlier generation, but—so his dissertation was started in '57, did I say? Something like that. Fifty-six. And mind was started in '68. So, it's a different era in philosophy.

And a funny thing happened. Yes. So, NYU, how did this happen? They had for several years been advertising to hire an epistemologist in the graduate philosophy department, maybe two or three years. They hadn't hired one. So, they had all these great visitors from surrounding institutions come in and teach epistemology courses.

So, I took a terrific course with Henry Hiz, who'd been Noam Chomsky's logic teacher. And it was a—I mean, Noam Chomsky, right? I mean, who was already a political figure at this point. And the course was a comparative study. So, Henry Hiz was Polish, and he's a generation older than Chomsky.

[. . .] It was a comparative study of Quine's *Word and Object*, and I wrote my dissertation on Quine. And I forget the name of the book, but it was a Polish logician, a famous Polish positivist logician. I took a whole course subsequently on Polish logic with him, who knew? I mean, it sounds like a bad joke. But no, it was a completely—I think this is very important for my career, actually. It's a kind of theme—you know, I—which reminds me, I'm just going to give you this. This is that other autobiographical essay that I ran into. This is your

copy. This is the book it's in. It's mostly similar, but it's got some things in it that are a little different.

KLETT: Thank you.

HARDING: And I think I'm getting this from that essay. I think—I don't think I thought it up now. I had to read it—I had to pull it up again. If there could be alternative logics—I mean, it—[Hiz's Polish positivist logic]—was an alternative logic. It treated [basic logical] terms differently. If there could be alternative logics, then there could be alternative anything, right? I mean, if logic is not universal, [but historical and "local"], and if there's not just one structure, appropriate structure, to how we [should] think and organize thought, then **<T: 80 min>** any field could have another [legitimate and valuable] way of doing things. And I think that that experience, that kind of experience—I think I talk about that in this other autobiographical [piece that] I found. I had in a number of ways—I think being an older student, and having been trained in philosophy, and an area that was no—not going to be mine, and then coming into another area, and—I think that has stayed with me forever. I mean, here I am now, living in a Jewish [retirement] community, and I'm not Jewish. And I don't—I'm illiterate in Spanish and Portuguese, and I'm [helping to found] a Latin American studies journal. I mean, it's—I know how it happened, but you—it doesn't look like a logical thing to happen in this relationship.

And I think I've always been comfortable with that kind of contradiction, and I haven't tried to smooth it out. I mean, it's part of my philosophy not to smooth out those, not to get them all integrated, but to keep their separate virtues separate—I'm very clear about it, with this [new journal, *Tapuya: Latin American Science, Technology and Society*.]

KLETT: Now taking a step back, so when you're at Albany, you're—and your husband is working in the philosophy department. You don't want to become your husband's student by taking—

HARDING: Right.

KLETT: —by going into that, so you go to sociology.

HARDING: Exactly.

KLETT: What was the interest in doing sociology? And second, you had mentioned—elsewhere, you mentioned John Gunnell's tutelage.

HARDING: Oh, yes, I did.

KLETT: And that it's through him that you encounter Kuhn and Quine.

HARDING: Yes. Do you know Gunnell?

KLETT: I don't.

HARDING: Okay.

KLETT: I don't know him. No.

HARDING: He's still a significant figure in political science.

KLETT: [Yes]. Could you give us some—why—why was sociology the alternative—

HARDING: Okay, so why sociology? I chose sociology with all the wisdom that a 16-year-old would choose a major. There I was, coming from English, and I wasn't going to do English, and I didn't want to do philosophy, my husband's field. I thought, I like people. I'll go into sociology. It turned that the sociology department, they didn't like people. They liked numbers—[data].

KLETT: Right.

HARDING: And so, I ended up—I really—it was—I'm saying that, but on the other hand, it's interesting, in that my—to me, that in my whole entire teaching life, after year one, I've always had a joint appointment in a sociology department, and to—including a joint appointment—at the graduate school of education, there are only 2 philosophers out of 50 faculty. It's a completely social—I mean, it's economics of education, politics of education, ethnographies of some educational context.

And so, I think that movement between—there's a—the two sides are both part of me, and I put them together in my own way, but I don't try to smooth out the dissonances between them. I don't know if that answers your—

KLETT: You had mentioned earlier about learning that you—the statistics.

HARDING: [Yes].

KLETT: Was that—was that this period, or later on?

HARDING: That was the sociology—yes, that was a requirement for sociology. That's how they let me into sociology, because I had no sociology background. I'd had one course in undergraduate school fifteen years earlier. I said, "If you let me in, I'll take the statistics course immediately, and if I flunk, okay." [The required statistics course was the great weeder-out of potential sociology doctorates]. I did the same thing with NYU [when I wanted to get into the philosophy department], because I had no background in philosophy. I'd taken one course as an undergraduate. And again, I said to them, "If you let me in, I'll take the logic courses immediately." Most people put off the logic—I'd been grading my husband's logic papers for years. So, I—that ended—those statistics and logic courses ended up a route for me.

KLETT: [Yes].

HARDING: I enjoyed them, and they got me into places that I might not otherwise have gotten.

ROBERTS: So, were these decisions about the educational programs about curiosity or career?

HARDING: Were my decisions?

ROBERTS: [Yes]. I mean, what—

HARDING: Well, kind of a mix—

ROBERTS: —did you imagine you were going to do with a graduate degree in sociology or philosophy?

HARDING: Teach in a university department?

ROBERTS: But not in your husband's university department?

HARDING: Not in his department.

ROBERTS: So, where did you think—what did you think would happen if you had a graduate degree in philosophy, you're at Albany, and—

HARDING: Well, I taught. [. . .]

[Having achieved my doctorate], I first taught at a funky little **<T: 85 min>** leftover 1960s college that was part of SUNY-Albany. It was called the Allen Center, and it was a social science early admissions, and I taught philosophy in it. My husband was uptown in the philosophy department. This was downtown, [several miles away]. It was a small little department. It was a separate little college, but it was tiny. I think we had 17 faculty, maybe.

And there were—there were—I'm not—I don't think I had a very clear idea what—what I would do, but it didn't—it began not to be a problem, because—so I got my dissertation in '73, I think it was, and by '74—so '73, '4, I was teaching at the Allen Center half time. And '74, I got a full-time job at the Allen Center. The half time moved to a full time. So, I could support my children, and I divorced.

So, then the—so that's '74. So, I taught two more years until the Allen Center was shut down by the State University, in procedures that—last time I checked was five years ago, but it left SUNY-Albany on the AAUP Censure list for thirty-five years. They were the number one [on that list]—they had—their [closing down the institution] procedures had been so bad. They just cut us all off. And there were other colleges, [such as some of the CUNY ones], going through similar kinds of processes where they didn't—weren't so bad.

But I immediately got a job at the University of Delaware. We had divorced for two years. And it was very—he was not happy with it, but we were on friendly relations. The kids were—when we divorced [in Albany] in '74, so they were eight and nine in '74. So, he lived a couple of blocks away, and the kids were with him [on alternative weekends and summer weeks]—you know, we worked it all out. He was a little bit aggressive about it, but not excessively. And within three weeks, he was—he had moved in with a woman [down in New York City] whom he didn't know three weeks earlier, when he left our house. I mean, he couldn't live without somebody there [to manage his life]. And she became an actually wonderful second mother to my kids. She was the head of the journal, editor of the journal for the American Historical Association. [They were partnered for eight years or so]. And she and I—they split a long time ago. He's been [now remarried] for twenty years [to another woman],

and has—his youngest kid with Jeanette is three months older than his [only] grandkid, through our youngest daughter. I mean, his wife is two years younger than his youngest daughter [by me]. And we don't have to get into how—he's a very sweet guy, and he's doing fine.

ROBERTS: Was part of getting the career, getting the degree, because you wanted to have that autonomy? Or you wanted to have the autonomy possible?

HARDING: No, I don't think so. I don't think so. We—we didn't—we never fought a lot. We just outgrew each other. We—we were married for 17 years, I think it was. We—because we were married something like eight years before we had kids, something like that—I can't exactly remember. We kind of just outgrew each other in different ways, and have remained amicable to this day. I have dinner with him every year or so at the American Philosophical Association. [redacted 01:28:40 to 01:30:08] **<T: 90 min>**

KLETT: If I could ask Jody's question a different way, is what did you think you were going to—why were you doing what you were doing?

HARDING: I knew you'd keep asking that, and I keep not answering it.

KLETT: Because I remember what I thought I was doing when I was in undergraduate and then grad school, and it was always a little different, but there was some emerging image of who I thought I was going to become.

HARDING: I think that's because you're a guy.

KLETT: Is it?

HARDING: I don't know. I don't think I—I had an image of what I wasn't going be, but I don't think I had a very positive image of what I—a very understandable grasp of what I was going to do. I mean, the first time that happened was when I went back in philosophy, or sociology, I suppose, because then I was going to teach. And I knew what that would be like, and so forth. But before that, I didn't.

KLETT: Your friend Gloria, who was inspiration for going to grad school—

HARDING: [Yes].

KLETT: —so it sounds—almost seems like she was the example of the one that got away.

HARDING: Yes.

KLETT: And you wanted to get away, too.

HARDING: Yes.

KLETT: So, again, it was retreat—it was escaping, not—

HARDING: Yes.

KLETT: —going towards something? Is that—

HARDING: Yes.

KLETT: —fair?

HARDING: Yes. She was married to a very distinguished psychiatrist. He had been one of the people who did the—closing down the mental institutions. He'd been [an important] leader in that. I forget the name of the guy who really did it—[Szasz?] But he was the second to do that. He was a crazy guy; [he burned a huge cross on their lawn when she went back to grad school. He hated feminism]. And they—she was—at the point when she was going back to graduate school, she had two kids who were a little bit older than mine, crazy Dan, and his parents and her father living on the third floor [of their house] at this stage. And she was taking care of all of these exceedingly needy people.

And she went back to grad school. She was sick of this, too. But he burned a cross on her lawn when she went back to grad school. I mean, really. So, I always felt kind of [lucky]: Hal will never do anything like that, so hey—

KLETT: [Yes].

HARDING: —I thought I was getting off easy those days. [laughter]

So, I think it—it was an era when I—until I got into that familiar academic context, I couldn't really imagine a life for myself. I mean, I saw possible lives in those New York jobs, but they weren't—I didn't hate them, but they—they weren't anything I aimed for. I mean, I just tried to be a good secretary or whatever my title was, and had my own social life, and so forth, going on. I'm sorry to sound so—I know you hope there's some significant—

ROBERTS: No, no. I don't think it's a hope. I think it—I am—it takes so many turns, I wonder at what point—is there a time when you—so you're clearly maximizing each situation.

HARDING: I'm trying—no, that's good.

ROBERTS: And—and I think that that—that—I—you know, is amazing. I think that's what a lot of people do. I think we tell ourselves that we have some sort of intent. But I—I do want to—I don't want to lose the intent, if there starts to be one. I—

HARDING: I think it doesn't start until I get into a place where I can envision something.

ROBERTS: [Yes].

HARDING: I couldn't—I couldn't envision—what could I envision when I was working in the Keedick Lecture Bureau or—or ABC? I—I could envision ABC. There were people in higher ranks, and I could see how that was attractive to do, and maybe I'd end up doing that. But it wasn't where I—I needed much more skills and experience to do it. They were considerably older than me. And that could have been, but meanwhile, we took a trip to Europe, and I had to quit that job, and when we came back, I worked elsewhere.

I just think I didn't—I don't think I did it—I always had huge energy for whatever I was doing, but it was evidently not focused in any understandable way, until I got into a world that I knew very well, because I'd been socialized into it through my husband. I knew exactly what university life was—he taught in that New Jersey college, then he'd been to Columbia philosophy department, and he had taught at Vassar. He was teaching at SUNY. And I knew what those job changes had been about, and what his challenges had been, and I'd graded his papers, and I knew all his colleagues. And it was a very familiar and easy—kind of easy thing to imagine. And I—there was nothing else that—that was—I was imagining. I just **<T: 95 min>** knew I did not want to turn into those kinds of crazed mothers.

KLETT: Do you want to say something about Gunnell, and the—

HARDING: [Yes].

KLETT: —what you gained from—

HARDING: [Yes], so the year I was back in sociology—let me see. I'd—is this true, that I knew him socially already? I think I knew him socially already, through friends in political science. He was in the political science department, John Gunnell. And he was teaching a graduate seminar on Marx and Quine or something like that. I can't remember. But it was a political—there were about ten of us in it. He let me get into it. I mean, it was my first year in graduate school, and he must have been hungry for students in the class. He let me into it, because they were all political theorists in that class.

And it was very exciting. I loved the format, the writing papers and sitting around a table and really interesting ideas. And that's what led me to start—when that—sociology at SUNY-Albany fell apart, to start looking for the courses in sociological theory at NYU. And then my wise decision to make that bus trip once a week instead of twice.

KLETT: [Yes].

HARDING: Instead of twice a week.

KLETT: Because Kuhn is sort of a recurring character—

HARDING: Kuhn. I'm sorry. That's what it was. It was Quine and Kuhn.

KLETT: [Yes]. Right.

HARDING: That's what it was. It was Quine and Kuhn.

KLETT: Did you—was Kuhn—did Kuhn figure in your imagination at that time? Did you read Kuhn as well? Or was this—

HARDING: Yes.

KLETT: His—

HARDING: Wait a minute. Is that right? I'm sorry. Now I can't remember—or was Kuhn the next year, when I got to NYU? Because I remember when we brought up the topics, topics from Kuhn to Richard Martin, he—my adored logic teacher, he said that was just ephemera, it would disappear, and we weren't going to talk about it. So, maybe I'm wrong. Maybe Kuhn was the next year.

KLETT: I saw the—in the printout you gave me, he says that—

HARDING: [Yes].

KLETT: —a piece of ephemera, so you and the rest of the students read it on your own.

HARDING: Yes. We immediately formed a Kuhn study group and started talking about it. If he wasn't going to talk about it at all, there must be something important there. So, I forget exactly the focus of the Gunnell seminar. It seems—it couldn't have been Marx. What was it? I'm sorry. I can't remember. But it was—whatever it was, it was really exciting to me.

ROBERTS: It seems like in this note that we have that the students had already read Kuhn, and so you—the course was focused on Quine's *Word and Object*.[7] Does that sound about right?

HARDING: You know, that sounds about right. The two courses are getting mixed in my mind. They're a year apart, and one's at SUNY-Albany, and one's down—

KLETT: Reading Quine in sociology, that's—

HARDING: And then there's the Quine and Polish logic course the following year. So, over three years.

[7] Willard VO Quine. *Word and Object*. MIT Press, 1960.

ROBERTS: That's a lot of Quine.

HARDING: It was a lot of Quine. I think we only read him in two of those courses, so I can't exactly remember what we were reading in Gunnell. But I do remember the excitement about Kuhn. He was very exciting.

ROBERTS: When you took the faculty position, did you think that you would keep up kind of a line of research, or that you were going to—is this—did you think that the line of inquiry you were developing in the dissertation was something that you were going to make—continue to pursue? Or—

HARDING: So—

ROBERTS: —did you imagine the faculty position as much more of kind of a teaching—teaching load?

HARDING: Oh, no, I intended to write. So—so I have to look at my CV to tell you what was happening. So, when I look at the books, the oldest book there is '76, *Can Theories Be Refuted? Essays on the Duhem-Quine Thesis*.[8] That came directly out of my dissertation. It was—my dissertation was on the Duhem-Quine thesis.

ROBERTS: And that's pretty quickly after your dissertation. That's just—

HARDING: [Yes]. Yes. Right. I immediately published that. And then the next thing, I—I mean, I loved writing and loved doing research. Then there's that special issue. Then, 1983, Merrill Hintikka and **<T: 100 min>** I, when we come to the science wars, this is the beginning of the science wars story.[9] I'll give you the first piece of it here.

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[8] Sandra Harding, ed. *Can Theories Be Refuted?: Essays on the Duhem-Quine thesis*. Vol. 81. Springer Science & Business Media, 1975.

[9] "The science wars were a series of intellectual exchanges, between scientific realists and postmodernist critics, about the nature of scientific theory and intellectual inquiry. They took place principally in the United States in the 1990s in the academic and mainstream press. Scientific realists (such as Norman Levitt, Paul R. Gross, Jean Bricmont and Alan Sokal) argued that scientific knowledge is real, and accused the postmodernists of having effectively rejected scientific objectivity, the scientific method, empiricism, and scientific knowledge. Postmodernists interpreted Thomas Kuhn's ideas about scientific paradigms to mean that scientific theories are social constructs, and philosophers like Paul Feyerabend argued that other, non-realist forms of knowledge

We had decided to put together this collection of feminist epistemology, metaphysics, methodology, and philosophy of science papers, and I think I mentioned—did I mention she was married to Jaakko Hintikka, who was a very famous, powerful philosopher, analytic philosopher and logician? He was from Finland.

And he held two very powerful positions. He was chairman of the board of the journal *Synthese*, and he was the editor-in-chief of the same publisher book series called Synthese Library. So, he had promised Merrill and me a special issue of *Synthese*. And when the board heard the title, *Discovering Reality: Feminist Perspectives* and so forth, they ran amok.[10] Three of them threatened to quit if that was published, and one of them [told Jaakko], "I suppose they're going to have on the cover a photograph of Betty Friedan waving her bra." It was a crisis.

And so, he gave us—he was very annoyed. He was very feminist. He gave us a volume in the Synthese Library series, and that's what this is. It's Volume 83 in the series.

So, that was the second book. And then I started developing—I mean, *The Science Question in Feminism* was the first big monograph.[11] I was writing all the time. I can't—you know, there's a lot of—I don't even list the essays. I'm lecturing in—my work was—so when we move to this—I know we're supposed to be back in the past more, but this is the beginning of that science wars narrative that has immensely shaped everything about my life, the institutions I work in and the projects I work on.

And in—in my mind, it's central to the founding of STS that we—Donna [Haraway] and I were the—and Evelyn Fox Keller, to some extent, along with Latour and Steven Shapin—were the first big targets of criticism by physicists.[12] Of course, [the scientists] hated the idea of feminist epistemology in philosophy of science, but they hated the idea of any social studies of physics or chemistry or biology.

And so, I think the—this science wars story is a very important one for the—understanding the formation of—I mean, if you just take it as a discipline, I mean, the things we call disciplines were mostly created in the 19th century. I mean, this is one of the rare ones that—that's created later. Maybe not so rare, but it's certainly different from history or philosophy and so forth.

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production were better suited to serve people's personal and spiritual needs." https://en.wikipedia.org/wiki/Science_wars, accessed 20 August 2020.

[10] Sandra Harding and Merrill B. Hintikka, eds. *Discovering Reality: Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science*. Dordrecht, Holland; Boston : D. Reidel ; Hingham, MA: Kluwer Boston, c1983.

[11] Sandra G. Harding. *The Science Question in Feminism*. Cornell University Press, 1986.

[12] Donna Haraway, interview by Joseph Klett at Haraway's home, Santa Cruz, California, 25 March and 5 April 2019 (Philadelphia: Science History Institute, Oral History Transcript # 1098).

And it's—it's hugely contentious from the minute—critics of Kuhn—Lakatos and Musgrave's volume, Shapin, and Popper—I mean, Kuhn just—Kuhn and what's his name? A Brit.[13]

KLETT: Hacking?

HARDING: *Science and Its Problems*.

KLETT: Hacking?

HARDING: No, Hacking—

ROBERTS: Laudan?

HARDING: No. *Science and Its Social Problems*.[14] It's a couple of years before Kuhn. I can't think of his name. He's not that prominent. He didn't become that prominent. But it's a Kuhnian kind of clear focus on science and technology in culture. And I think, you know, this science wars thing streams through my whole CV in kinds of ways that were at the time challenging, but I—but I don't think my career has suffered in kinds of ways that I'm—I regret because of it. But I've had a whole series of this kind of situation I'm telling you, where the editors say they'll—they'll quit if my work is published. And I'll tell you some—I got turned—CNRS [Centre National de la Recherche Scientifique] withdrew a—[CNRS, which is the French equivalent of the National Science Foundation (NSF), withdrew an] invitation to be a visiting professor, for—I forget whether it was three weeks or six weeks, whatever. They offered it to me, and then they wrote back and apologized, they couldn't do it anymore, because [a vicious French criticism of my work] had come out.

The head of NSF, of our division—**<T: 105 min>** I'm visualizing him, and I've lost his name.

ROBERTS: Fred Kronz?

HARDING: Pardon?

[13] Imre Lakatos and Alan Musgrave, eds. *Criticism and the Growth of Knowledge.* New York: Cambridge University Press, 1970.
[14] Jerome R. Ravetz. *Scientific Knowledge and Its Social Problems*. Transaction Publishers, 1973.

ROBERTS: Fred Kronz? The current one?

HARDING: No, it'd be earlier.

ROBERTS: Before that?

HARDING: This would be in the 1970s. I had had an NSF summer fellowship, and the next year, I applied for something or other, a semester off, fellowship or something. And he called me up and told me he was so sorry, but he couldn't afford to give me any kind of funding in the future, because he was scrutinized all the time by the right wing, and if he [gave me a grant], NSF would be damaged. So, I continued reviewing for him, you know, for years, but he couldn't—I couldn't get any funding.

I mean, and so that kind of story, yes, it's about me, but I think it's mostly about STS. I don't think it was me individually. Anybody who wrote on those topics. The feminists were an easy target at first. And to me, what's interesting also—I'll just put this out there—so it was about fifteen years of attacks on me and Donna and Evelyn Keller and Latour and—and then it kind of ended. And that was when the anti-racist and the postcolonial accounts were beginning to appear. And these [critics of Western science] were senior foreign men, and these white physicists were not about to get into struggles with Indian or Latin American or African leading figures in their country. It's one thing to attack junior—and Donna and I were very junior at that point, and so was Evelyn Keller. It's one thing to attack us. I mean, Latour is French. You know the French are always open for sober people's attacks, right? [laughter]

But I—and I think that history has—has something to do with—I'm trying not to be self-serving, but I don't—I never even took it totally personally.

KLETT: [Yes].

HARDING: I always thought it was part of the attacks on feminism, and part of the discomfort with the social—with the idea that sciences and their societies co-constitute each other. It's just discomfort with that, that physicists and chemists and biologists, and eventually some of the sociologists. I got huge attacks from empirical sociologists. So, anyway, I forget how we got into that, but—

ROBERTS: I don't recall, either. What is—what do you think—I mean, well, I'm sure we'll dive more into it, but just to keep the thread for a moment, what is the source of that discomfort? [. . .]

[END OF AUDIO, FILE 2.1]

[END OF INTERVIEW]

INTERVIEWEE: **Sandra Harding**

INTERVIEWERS: **Joseph Klett**
Jody A. Roberts

LOCATION: **Harding's home**
Playa Vista, California

DATE: **24 April 2019**

HARDING: And so, we were talking about how I got into epistemology and—

KLETT: [Yes]. So, this is Joseph Klett with Jody Roberts. We're here in the house of Sandra Harding for day two of our oral history. It is April 24, 2019. And so, we're going to pick up—you have decided to go to grad school. You've tried out sociology at Albany. And you decided that a once a week meeting in philosophy is a much more desirable commute to New York City than a two day [per week commute]. Could you tell us more—let's talk here about—you had actually discovered Quine at—well, I don't know, did you read Quine in Albany before you went to NYU?

HARDING: I can't remember, as I remember—as we were closing down yesterday, there were two years that I can't remember. I was in these seminars that had—one with John Gunnell, and then there was with the issue of Kuhn and my philosophy of science teacher at NYU, and somewhere in there, and then Henry Hiz and Polish logic. And there—we were definitely reading *Word and Object* there, reading Quine.

KLETT: [Yes].

HARDING: Alongside—I forget his name, some Polish logician, famous guy. I actually knew his name, which is pretty weird. And I can't exactly—so I'm not sure I had read—

KLETT: When was Quine—like when was *Word and Object* published? I don't have a sense of what—

HARDING: I should know the answer to that, don't you think?

KLETT: Was he just—when was he active?

ROBERTS: It's an early 20th century—

HARDING: He only died recently.

KLETT: [Yes]?

HARDING: Okay. Let's see. I've got to have it in here somewhere. How about *The Science in Feminism*? It's in there. *Word and Object* in there? O, P, Q, *Two Dogmas of Empiricism*, 1953.[15] Okay. let's try something a little later. That's the only reference I'm making. I mean, I can't remember when this stuff—this is way back there.

KLETT: But it is contemporary with Kuhn?

HARDING: [Yes].

KLETT: Right? So—so—

HARDING: Oh, he's younger than Kuhn. He's a little bit younger than Kuhn.

KLETT: Okay.

HARDING: But they were both alive, of course, when I was in grad school.

KLETT: [Yes].

HARDING: Let me just see if I've got it here. *The Web of Belief* is 1978.[16] "Two Dogmas of Empiricism." I don't have *Word and Object* in here. So, '50—oh, there we go, 1960 is *Word and Object*.

[15] Willard VO Quine. "Main Trends in Recent Philosophy: Two Dogmas of Empiricism." *The Philosophical Review*, 1951: 20-43.
[16] Willard VO Quine. *The Web of Belief*. New York: Random House, 1978.

KLETT: And Kuhn is like ’62, right? Is that—

HARDING: Kuhn is ’62 and then ’70.

ROBERTS: Right.

KLETT: Right.

HARDING: Second edition is ’70.

KLETT: So, not exactly hot off the press, but you’re—

HARDING: Close.

KLETT: People are really investing time in these things at this point, and what I find interesting is that you are in sociology, looking for the alternative, and you found continuity through—at least the way that you’ve told it with Gunnell.

HARDING: [Yes].

KLETT: In sociology, you—you actually find a continuity through Quine, relative to philosophy.

HARDING: That’s true. I—because it—so I think what we’re talking about here is the beginning of the social studies of science, right?

KLETT: [Yes].

HARDING: Because these guys were logicians and philosophers, and it—Kuhn was a historian originally, but it—I mean, I think if you had to pull what his greatest—people think his greatest contributions are, I think they think it’s as a sociologist.

KLETT: [Yes].

HARDING: I mean, at least that would be a contestable vision of him. And, of course, there are two other constraining factors through this period for me. One, I have to stay away from my husband's—

KLETT: Right.

HARDING: I don't want to get into that work.

KLETT: Right.

HARDING: I don't even like it. I mean, at that point, I didn't—I didn't like it. I never quote them, his people. [laughter] Partly I'm sick of it, and partly I was sick of—I was sick of it, and partly, it just wasn't relevant to what I was doing.

And the other constraining feature—what was the other on that I had in my mind? So, well, I'm always working in service to the social justice movements. Feminism, originally, and feminism has Women of Color feminism happening very soon.

KLETT: [Yes].

HARDING: I was just checking in—I don't get onto it in my writing—I'm not—my first book is not—there's nothing by People of Color in it. And I was checking a couple of the early readers, and the first mentions start to be Patricia Hill Collins, and African-American sociologists and historians of science, who are—**<T: 05 min>** so I'm always—you could call it constrained by, but I would call it enlarged by.[17] I'm always reaching over there in my writing to make to—in the beginning, to check it out there.

KLETT: [Yes].

[17] Patricia Hill Collins. *Black Feminist Thought: Knowledge, Consciousness, and the Politics of Empowerment.* New York: Routledge, 1991.

HARDING: I mean, by now I just move into a place—I've moved into Latin America, as I tell—

KLETT: Right.

HARDING: —Leandro Rodriguez Medina, the Argentinian/Mexican editor-in-chief of *Tapuya.* all the time, and so I'm illiterate.[18] I just—my head is—that's where my head is.

KLETT: Could I ask about the sort of discourse of second and third wave feminism?

HARDING: [Yes].

KLETT: Was that—was that—did you use those—that wave terminology at this time?

HARDING: No, third wave wasn't really—this is second—definitely second wave. I mean, the second women's movement in the US, but also Europe, and—began around—I mean, they date it to sources, the resistance to *The Feminine Mystique*, right, to the containment in the households in 1950s of women who had been managing all kinds of things while their husbands were away at war, and then they're stuffed back into households after the war.[19] And so, that—there's that on the one hand.

On the other hand, the civil rights movement, because a lot of the early feminists came out of the civil rights movement, and out—and consequently, out of socialist feminism, too. And so, that—that is at earliest '68, maybe. I think the first women's group formed in maybe New York City, in '68. And by '70 or '71, I think I was mentioning already that New York State had a liberalized abortion law, and—

KLETT: Right. And then Albany was sort of a hub for—

HARDING: [Yes], Albany was the state capital, and there were all these colleges all around, and people were friends who were doing their first teaching of women in literature and women's history. Women's history had a much older legacy from the 1920s. I mean, as—there was an association, I mean, a formal association. But none of the rest of us had that kind of history. Only the historians.

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[18] Leandro Rodriguez Medina. *Centers and Peripheries in Knowledge Production.* New York: Routledge, 2014.
[19] Betty Friedan. *The Feminine Mystique*. W.W. Norton & Company, 1963.

And the third wave doesn't get into my—I have paid very little attention to it, I have to say. But it's definitely a decade or more later.

KLETT: [Yes].

HARDING: Before it gets going. But meanwhile, the Women of Color movements are coming out of the civil rights movement, are saying, hey, white girls, what are you talking about? And writing very challenging articles that—just take Patricia Hill Collins' works, and she's a standpoint theorist. Do you know her work?

KLETT: Absolutely. [Yes]. Her sociology is great.

HARDING: [Yes]. So, she's taking on sociology. And showing how its categories work—completely misunderstand the lives of Black women. She takes those basic—if you take—not the introduction to sociology that's normally offered, but the social problems intro in sociology, every one of their problems misses the important role of Black women in leadership, in education, different modes of sexuality, different forms of political organizing. She just goes through the weeks of the social problem syllabus, and takes them on.

But she's also overtly but kind of sweetly taking on white feminism that was—we were all critical of the family, and she says, "Forget it, the family is the last bastion of resistance to dominating cultures." And it's no accident that Barbara Smith and a couple of other Black feminists named their publishing house Kitchen Table Press. It was around the kitchen table that slave revolts were organized. It was—

KLETT: [Yes].

HARDING: So, that was very visible to me immediately. And I had already, with no feminist content at all, been reading—this is the only way I wandered a little bit into Wittgenstein, but immediately pulled myself out. But there were these discussions about rationality. Peter Winch started them and so forth.

KLETT: [Yes].

HARDING: Is that the 1960s, maybe? I think it's when I was in grad school, which would put it late 19—anyway, there was an attempt to begin to rethink the standard of rationality in light

of Africa—it was studies of Africa, and of different kinds of rational thinking in Africa that didn't fit the modern Western image. And so, I knew that already, and it was very—you know, it was presented as an epistemological issue.

KLETT: Right.

HARDING: It would today be **<T: 10 min>** presented, because of the ontological turn—you know the ontological turn in anthropology?

KLETT: [Yes].

HARDING: Viveiros de Castro and so forth?[20] It would be presented as an ontological issue also, metaphysical.

KLETT: And so, this is at NYU, or are you still at Albany, when you're having this—well, you said that it was immediate to you. When was this?

HARDING: It was—it would have been at Albany, because I was—no, it would have been—well, I was at NYU—I commuted from Albany to NYU.

KLETT: Right. You mentioned that Albany—the SUNY-Albany experience was very quantitative, doing—

HARDING: In sociology.

KLETT: In sociology. Right.

HARDING: Yes.

KLETT: Yet you were also getting previews of Kuhn and Quine. And so—which was not statistics at all.

[20] Eduardo Viveiros de Castro. "Exchanging Perspectives: The Transformation of Objects into Subjects in Amerindian Ontologies." In *Common Knowledge*, 10(2004): 3.

HARDING: Well, through Gunnell's political science course, but that was the year before I moved to NY—before I went back to grad school.

KLETT: Right.

HARDING: So, I was in Albany for how long? I moved there in—oh, '66, maybe. I think '64, we were in Poughkeepsie, and I started having babies. I always refer to this as an era of my life when I was just—I met a guy, a faculty guy, at a party at Vassar, the second year, just before we were about to leave, and I'd known him, and had his kid in my third-grade math class that I taught one year. And we met at a party when I was about—we were about to move to Albany. He said, "Oh, my God, haven't you had that kid yet?" [laughter]

KLETT: Because you were still—

HARDING: I was eight months pregnant for the second time. [laughter]

The kids are only—what are they, thirteen months apart? So, that—it would have been in graduate school that I was—I can't remember the context in which I was reading Peter Winch and so forth. On the other hand, it was off on the edges of my husband's work, so—

KLETT: Right.

HARDING: —there may have been discussions around the dinner table. I mean, I was giving dinner parties for all these—for [the British analytic philosopher] AJ Ayer, right, and—I mean, [my husband] was having all these famous philosophers into the philosophy department, and I'm cooking away and listening to the guys. Awful era.

KLETT: Were you able to, in that—in that context, were you able to talk freely? Did you feel like, oh, I'm going to leave him alone, let him do his—his professionalization?

HARDING: No, I—I followed his work very close—I mean, typed his dissertation a few—

KLETT: Right. [Yes].

HARDING: And his master's thesis I'd read. So, he and I had conversations—I edited—I don't know if I have them—I think I have them. I edited—I mean, he edited them but I actually did a huge bunch of the editing. Well, the spines of these are not popping out at me. I—he—his name is on it, but I did a huge amount of the editing work. I mean, his is the intellectual work. I mean, he's directing it.

One was *The Problem of Other Minds*.[21] One was something like *The Other Minds Reader*.[22] If you look up Harold Morick, you'll find these three little readers that—from before I went back to grad school. So, they were in those years, when I was grading the logic papers. And I loved doing it. It was a lot of fun. I mean, he just—it was his intellectual project.

KLETT: [Yes].

HARDING: But I learned a lot from it by talking over, okay, should we put—should I put this one in, or that one in?

KLETT: Right. It strikes me, the parallel of when you were in school, and the female teachers saying I'm not calling on the girls because the boys are smarter. There's the assumption that informally, we can understand this. You can edit it beautifully.

HARDING: [Yes].

KLETT: But there's also the assumption that institutionally there's just places you can't go.

HARDING: [Yes].

KLETT: You're ineligible for those places.

HARDING: [Yes].

[21] Harold Morick, ed. *Wittgenstein and the Problem of Other Minds*. Humanities Press, 1967.
[22] Harold Morick, ed. *Introduction to the Philosophy of Mind: Readings from Descartes to Strawson.* Harvester Press, 1970.

KLETT: And so, I'm thinking about here when like—when those informal spaces—or when you recognize sort of in-between spaces, right, that's not the institutional space, and it's not just small talk.

HARDING: Right.

KLETT: So, it is a dinner party conversation or someone comes in the kitchen to say hi, and maybe you talk intellectual shop.

HARDING: [Yes].

KLETT: And especially because you then go to NYU, which you have—as you mentioned, you know, so many of the young men get shipped off after they get their master's. And so, it's a female heavy PhD program.

HARDING: [Yes].

KLETT: And so, I'm just kind of—I'm trying to get a feel for this world—

HARDING: [Yes], well, also, the wives of **<T: 15 min>** some of Hal's colleagues were—so they were married to philosophers, too, and they didn't—I mean, I had had all this experience doing kind of technical work, if you know what I mean.

KLETT: [Yes].

HARDING: I mean, in other contexts in New York and so forth, and loved to write, and so forth. But I had a conversation—Joe Gould—Pat Gould had been his student, I think at Cambridge or—I can't remember this story now. This is from whatever, forty years ago. But she knew a lot of philosophy, too.

And so, there was this little shadow group of wives, I mean, there weren't a lot of us, but there were a few of us. We didn't tend to talk philosophy with each other, but we were comfortable listening and, you know, participating a little bit in these discussions without presuming to be the official philosophers.

KLETT: [Yes].

ROBERTS: So, just something interesting. I think the way you described your relationship to your husband's work at least introduces this idea, even though you're looking for your own intellectual terrain, there is this kind of conversation you're having out loud and maybe internally as well about things could be other.

HARDING: [Yes].

ROBERTS: This system could be otherwise.

HARDING: [Yes].

ROBERTS: You know, our systems of logic, you know, what would it—what would it mean for us to imagine that?

HARDING: Yes.

ROBERTS: And that you were—you're interested in maybe starting to wed some of those theoretical concepts with what does that actually mean in these particularly emerging social contexts as well. Does that—

HARDING: I think that's more advanced than I was at that point.

ROBERTS: [Yes].

HARDING: I think I get to that once I go back to grad school. I start—you know, I have all these kinds of weird experiences, both writing and—not weird, but, you know, I'm, whatever—how old was I? Thirty-three or something like that when I went back to grad school, so I'd been out of college for ten or twelve years. I was older than that. I forget. Thirty-eight I got my dissertation. Five years. Thirty-three. Something like that, when I started back in sociology or philosophy.

But—so we older students—I mean, this is like older students—I mean, now older students are just normal. I mean, there's all kinds of older students coming back, and they bring

all kinds of experiences that puts them way ahead of the twenty-one-year-olds, whatever—but this was just happening before that was common. And it was a little bit shameful. I mean, I wasn't shamed by it, but I can remember—poor woman. The wife of one of my husband's colleagues, he was a miserable guy, and she was a pretty miserable woman. But when I started back to grad school, one day we were having tea or something or other, and she said, "Sandra," she said, "is your home life so unsatisfactory? Is your marriage and your family so unsatisfying that you have to go back to grad school?"

KLETT: [Yes].

HARDING: And I just was dumbfounded. I was totally dumbfounded. To her, it was a shameful thing. So, this is that world. I mean, you know, there it was.

KLETT: [Yes].

HARDING: So—

ROBERTS: So, I'm curious how—how you end up writing on Quine.

HARDING: [Yes], how—

ROBERTS: And how Quine figures in—in that—in those years as this pivot point, and—[yes], just—

HARDING: There's something here that happened that I know it happened, and I can't exactly remember the details, but here's the part of it I can remember. I had some other topic in mind, and wrote up a proposal. So, I'm at NYU. I'm in year three or whatever, about to finish classes and start the dissertation. [And I'm seeking a dissertation advisor.] I took it around to various—what was it? It was criticism of something or other, and it was very original. It was probably stupid. I have no idea. I can't remember what it was now. And not one of the professors would take it. It wasn't in their fields, right? This was this crazy returning student, right? And being much more independent than they wanted.

And so, I shifted to Quine, which was a related topic, and still had the experience that nobody—not one of the seven faculty I approached would take me for a doctoral student. So, I sat down with Steve Kahn, who was the vice chair of the department at that point, and I'd had a course with him. I liked him. He said—I said, "Steve, nobody'll—**<T: 20 min>** nobody'll take

me as a doctoral student." He thought about it for a minute. He said, "Sandra, you are a student in this department. You have satisfied all the [requirements]. You are entitled to a doctoral advisor." [laughter] And he said, "Let me work on it." And so, I ended—I'm just going to let that go.

KLETT: Okay.

HARDING: So, I ended up—Bill Ruddick ended up taking me on. His wife was a very distinguished feminist scholar, Sally Ruddick. She taught at the New School. And Bill was a philosopher of medicine mostly at that time, and he was a really sweet guy. He was around my age, maybe a little—not too much older. And he was—he was just—he didn't give much help. I didn't really need much help. I mean, I knew how to do philosophy. I had this Albany bunch of people, right? And I was dead set on getting through quickly. I was not going to hang around forever. Forget it.

And so, Bill's the one—I mean, after Bill and [another guy] who was—there was a third advisor I had. I forget who it was. When I—I think I mentioned I turned in my dissertation, and when they gave it back to me and said—there's one—on page 97 there's one little pencil check in the margin. That was it. No comments [from any of the advisors]. And that had been my experience with papers. I'd get an A.

KLETT: Huh.

HARDING: With absolutely no feedback on it, right? Or else I'd get a [C]. Like once I got a [C]. And I think I had no grade lower than an A, except for this one [C], because I said—I'd asked if I could rewrite a paper from [an earlier class]—I think it was on John Austin or something. I can't remember. But I wanted to move it somewhere else. And he said yes, but then when he got the paper and I gave him the original paper, he had changed his mind, and he said, "You can't turn this in." So, I rewrote another paper.

But I was used to having a problem getting feedback, any—of any—of any sort, let alone helpful sorts. So—

KLETT: Was that the experience of other—other students, other women students?

HARDING: [Yes], I don't really know if that was. I can't remember talking—by that time, with my dissertation, I wasn't in that much contact with other students, right? Because I wasn't down at NYU. I actually wrote my dissertation in Edmonton, Alberta, my husband was visiting in that philosophy department, and I had told him—I think I told you this yesterday. He had a

sabbatical. I said, “You want to go visit—” he was thinking Germany, and I don’t know where [else]. I said, “No way. You can go anywhere you want, but they have to speak English, because we have a five-year-old and a six-year-old, and I’m not [spending time] inserting this family into some foreign language community [while I’m writing my dissertation]. Forget it.”

So, he picked University of Edmonton, and it was a wonderful year. It was actually terrific. But I was there doing it on my own. We came back probably for the holidays or something, we came back and checked into the department. But—

KLETT: I—I want to put a bookmark in this, because I think it—I’m thinking, you know, as someone who’s also graded papers, and you kind of set a curve, right?

HARDING: [Yes].

KLETT: And you imagine—so there’s sort of like a—the mystery here is was this a particular treatment that you were receiving because of your identity, or was it treatment because you just happened to excel, and they feel like, this is immaculate work, I’m not going to give feedback?

HARDING: I—no, I think it—I would make another—I don’t think that was the issue. I think—was it my identity, or was it their normal practice with doctoral students?

KLETT: Exactly. Right. Disciplinarily—

HARDING: Because I still have colleagues who are really unhelpful with their students. They just never give them any decent advice.

KLETT: [Yes].

HARDING: I mean, I told one of them, “Don’t you dare give this guy [another A.” The student] had [shown me] a bunch of As on his papers, and no comments. [But they were] terrible papers. And then [the student] comes to me [to be his advisor], and I have to deal with somebody who’s gotten straight As and doesn’t know how to write.

KLETT: Right. [Yes].

HARDING: As a—

KLETT: And you're the bad guy, because you have something to say.

HARDING: And [my colleague] is a very good friend of mine. He said, "Okay, Sandra."

KLETT: But it also—it seems like it would have something to do with the field of philosophy, where the—maybe the—the standard practice is this is not something you can quantify, this is not something that feed—like feedback is something that's not necessary, which I think when you—as you develop, you know, your own approach to mentoring, you have to ask, is that—

HARDING: It's not my view. **<T: 25 min>**

KLETT: Right. And so, that would change the way you then mentored—

HARDING: I don't know what they were doing. I think they—I don't think it was that deep an analysis. I think they just were lazy, and didn't—hadn't had an experience of being—I mean, I was mentored in other ways. I mean, good conversations in class, right, and good conversations in an office, and so it's not that I had no mentoring. But the—when it was connected to grading in any way, it—

KLETT: Right.

HARDING: —or a lot of work on their part, they just didn't do it.

KLETT: [Yes].

HARDING: And NYU had a lot of graduate students. I mean, it was a big urban university with a—and a lot of returning students at this point, for reasons I mentioned.

KLETT: [Yes].

HARDING: And, you know, [the profs] all wanted to be at—thought they belonged at Princeton and Harvard, and were very annoyed at being at NYU, and they weren't going to spend a lot of time on some elderly [female] pipsqueak like me. They were busy—you know, at certain points in their careers that—where they were busy with their own work, and probably had too much advising to do. I don't know. I really don't know the history. I think I'm just not as thoughtful as you're hoping.

KLETT: No, I—

HARDING: I'm not sure—

KLETT: What I'm trying to get at is the way that science studies people particularly—so I'm thinking of, like, the cohort of people like Leigh Star and Donna and people who—mentorship is a very serious, dead serious practice.

HARDING: [Yes].

KLETT: And that when writing is the currency, not to give feedback is a crime, right?

HARDING: [Yes].

KLETT: And is irresponsible. And that is something that maybe differentiates science studies from philosophy, or these older practices, where it is sort of this elitist thought of, well, if you're there, you're there, and you're there for a reason. We don't need to over-articulate that.

HARDING: That could be. I don't know that. Left to my own devices, I would explain it more—I would explain how I got to Quine and my teaching practices and so forth as intensely connected to my service to a social justice movement. [. . .] **<T: 30 min> [**And maybe new disciplines inspire more effective mentoring of students?]

And so, here are the ways in which my everyday writing is very managerial. I have to write very clearly, make difficult philosophical and theoretical issues comprehensible to upper level undergraduates and graduate students. My papers have to be a particular length. Six thousand words is just about right. Why do they have to be that length? Because that's the length that will get republished in anthologies and assigned in classes. If it's longer than that, it won't be.

They have to be—they have to be clear. They have to have—so they have to—my constant attention to good English is about that, in part, and short. And they have to be—and I'm always trying to insert social justice movement insights and goals into mainstream STS or philosophy or sociology or whatever. For example, the subtitle of my last book, the title is *Objectivity and Diversity: Another Logic of Scientific Research*.[23] And that's an echo of a Popper title. It was *The Logic of Scientific [Discovery]*, but for social scientists, research [is the way they think about science], and—so I never forget that when I'm writing.[24] I mean, I do. I'm thinking through a topic or something, but not—by the time I get to editing it in any way that's what I'm doing. And it's totally—it's not the image of philosophers channeling truth, beauty, and goodness and—

KLETT: Right.

HARDING: It's a very practical project, as you could see in that essay, "Work and Politics."

KLETT: [Yes]. Well, it's interesting to me, given your background in—in literature, that you would—managerial is not what I would think of, but—not the word I would use, but it's pretty apt, considering you're coming from literature through philosophy in the era of—you already mentioned like management studies, right?

HARDING: [Yes].

KLETT: Like neoliberalization is dawning—

HARDING: I didn't think of that.

KLETT: Right?

HARDING: I didn't think of that.

KLETT: To my mind as a sociologist, like what you're describing is just good writing, but that's because I've been so conditioned to think—

[23] Sandra Harding. *Objectivity and Diversity: Another Logic of Scientific Research*. University of Chicago Press, 2015.
[24] Karl Popper. *The Logic of Scientific Discovery*. Abingdon-on-Thames: Routledge, 1959.

HARDING: But that's—

KLETT: —the way that we communicate our work is exactly these parameters you've been laying out.

HARDING: [Yes], but there's good writing that doesn't insert social justice movement concerns into mainstream—

KLETT: But that's not exactly managerial, is it?

HARDING: Well, it's connected to the standards of good writing in philosophy, which were analytic philosophy.

KLETT: Okay.

HARDING: Right. So, I have to figure out how to put these really complex issues into some—so simple that stupid philosophers can actually grasp it. Forgive me. They say, "I don't know what this word means here, Sandra." Give me a break.

ROBERTS: I mean, I think what I hear you describing, and I do want to come back to Quine at some point, is you're using managerial, but I think the other—the other—it's just very strategic. You have something you want to say.

HARDING: It is. Yes.

ROBERTS: You have a pretty good sense of who you want to say it to.

HARDING: [Yes].

ROBERTS: And you know the rules.

HARDING: I do.

ROBERTS: So, it's really about—it's managing rules.

HARDING: [Yes].

ROBERTS: And that's the managerial aspect of it. So, I know—

HARDING: I know practices.

ROBERTS: —I know how long this should be. I know where the punchline has to be. I know how to frame it up front.

HARDING: Right.

ROBERTS: So, that if I do this one effort and craft these six thousand words the outcome will be these other six things.

HARDING: [Yes]. And I try to get titles that are usually questions, titles of books or articles—

ROBERTS: That you answer.

HARDING: —and I get—I try to do—what do you call them, little sound bites, like strong objectivity. I mean, I really worked over what to call that.

KLETT: [Yes].

HARDING: Right? There was the model of the strong program [in the sociology of science. Bloor, right]?[25] And I thought, let's call mainstream objectivity weak objectivity. That'll get their attention. [laughter] And the feminist and the anti-racist and anti-colonial objectivity is strong objectivity. We're succeeding at your project, mainstream philosophy and sociology. [You're not succeeding at it].

[25] David Bloor. *Knowledge and Social Imagery.* Boston: Routledge & Kegan Paul, 1976.

KLETT: [Yes].

HARDING: So, there's a kind of aggressive—strategic and managerial doesn't quite grasp the aggressiveness of **<T: 35 min>** what I very consciously set out to do. And so, [yes], so—I mean, so Quine was for me a good—I think I've said this already, but, you know, he was trying to get out of positivism, and certainly made some terrific contributions to [that task], but in the way that Kuhn wasn't a Kuhnian, Quine wasn't a Quinian, either. He didn't succeed, totally, by any means. But he certainly made some powerful moves toward it.

And that was as big as I could—that was as big an escape from positivism, without going to, for example, continental philosophy or something else forbidden and—completely forbidden in my department, right? That I didn't read anyway.

KLETT: [Yes].

HARDING: I mean, I'd read [Michel] Foucault, but I didn't—[Jacques] Derrida was beyond me, and so forth. I think—I don't think he was writing yet, then. Maybe he was. I forget. So, I was trying to work at the edges of the possible within the disciplinary frameworks.

KLETT: Right.

HARDING: And Quine seemed—not only that, he was alive. So, there wasn't a huge secondary literature, number one. And number two, I took—I saw—I'm strategic, right? I mean, it should be—people should be fired for allowing their [graduate] students to write on Plato these days. Oh, my God. How could you say anything [new and] authoritative about Plato?

And nobody thought [Quine] had an epistemology, as my advisor pointed out. He was treated as a logician and a metaphysician. And so, I had to make the case that it was an epistemology. And my dissertation was very short. I got it as short as—I got away with it as short as I could. It was 160 pages or something.

KLETT: The lack or the perceived lack of epistemology, is that why the seven faculty didn't take the project initially? Because there was no there there?

HARDING: That could be. It was probably a mix. They probably had too many graduate students. I probably—they didn't want an old lady who had her own ideas about what she should be doing, and besides, I wasn't working on their topics.

KLETT: Okay.

HARDING: I mean, that's [what] my colleagues liked. They liked students who worked on their [faculty] topics. And I do, too, so I tend to take—and I—you know, I've turned down graduate students who just were—they were in musicology or something, and maybe could bring standpoint to it, but I don't know anything about musicology. How could I advise such a paper? And so, I suggested somebody else to go to. So, I think it was probably a mix of practical and theoretical issues.

KLETT: And what was the role of [Richard] Martin?

HARDING: He was—he was my official advisor for the dissertation.

ROBERTS: Not Bill Ruddick?

HARDING: Oh, excuse me. I always mix up their names. Isn't that terrible? [. . .]

KLETT: Oh, okay. Richard Martin.

HARDING: Yes, because he once was on an APA program on nomological something or other, and all four participants were RW Martin. There were four philosophers—

KLETT: Wow.

HARDING: —that were named RM Martin. I don't know—one of them was probably a master's student somewhere. But they all got them all on a program on something about—anyway, he wasn't on my dissertation committee. His own work was way far away from—I mean, he was very much a logician. But he was a very warm and lovely informal advisor, and was always very kind to me. I can remember him—I once gave him a paper for one of his classes, and I'd worked like crazy over it, and I was very unhappy with it. And I handed it in, and I said, "I'm so unhappy with this, so I would really be happy to write it over, work on it

longer." And he said, "Mrs. Morick, I'm never happy **<T: 40 min>** with my papers." I thought, "Oh, I didn't know that, that you could publish papers that you weren't happy with."

KLETT: [Yes].

HARDING: I mean, but he—that kind of supportive—and I could be the only person in his class who really liked his teaching style. Most students didn't like it, because he didn't prepare a formal lecture. Excuse me. He assigned us readings and he started the class by saying, "Okay, so what did you find problematic?" And everybody did—they'd all sit there, and they had their pens ready to produce an outline. And I just loved those discussions, because students raised all kinds of issues, and he just kept going at it that way. So, I enjoyed him on a lot of levels.

And he also entertained graduate students. His wife was a well-known art historian, I think it was, at Columbia or something like that, and they had a huge mansion, just the two of them, up in the Catskills somewhere. And he had big parties up there for all his graduate students. It was one of these semi-castles up in there. I don't know. She's an art historian, and they bought with all the art hanging on the walls, probably, you know.

KLETT: Wow.

HARDING: So, it was a lot of—he was kind of caring for his graduate students in those ways.

KLETT: Was his admission that he's never pleased with his work, or often displeased, was that your first time hearing that from a philosopher of that status?

HARDING: I think so.

KLETT: [Yes]?

HARDING: Because it gave—he had the courage to publish even though he was unhappy with the work. And it gave me a sense that that was okay, to be—you couldn't get perfect papers always, but it was still worth putting it out there for some kind of discussion. And that was not an idea that was—philosophy had this notion of channeling perfect truth, beauty, and goodness, and it—I think I only read Kant—I mean, I got through my history of philosophy exams with, I was told, the highest grade they'd given on those exams forever. I never took a history of philosophy course, except in undergraduate college. The way I studied for all my comprehensives was that the department kept a file of questions from former exams, history and

philosophy and logic, whatever. And in each case, I took the set of them and outlined answers to what I thought were the let's say ten most likely to be asked, which ones appeared on four of the last six years, right? Strategic. [laughter]

Designed those, and then you had—these were usually in-class exams, I think, for three hours, or something like that. And sure enough, there was exactly the question on Kant I'd prepared. I mean, I read only the little parts—I have no idea what Kant says about anything. I read all the little things that had appeared on the exam. And so, I had a very selective approach to becoming a philosopher. I mean, I must have read more Kant, because he appears in introductions to philosophy, and I taught those for years, so let me see. What were you asking me? You were asking me—

ROBERTS: We were just talking about the role of Richard Martin.

HARDING: [Yes].

ROBERTS: Was he the chair at the time?

HARDING: No. [No].

ROBERTS: Just was one of your early—

HARDING: He was a very senior member of the department. And he'd been there a long time. I forget where he was before. And he wrote logic books and ontology books, and people thought he was kind of fuddy-duddy. I thought he was wonderful.

KLETT: When you were delving into Quine at this time, and the—I'm wondering about the—what science is, like as it's—as you're doing Quine, you're thinking about, say, the philosophy of science. Like, how are you imagining what science is as a practice, as an institute—

HARDING: Am I thinking about science? I don't think I'm exactly thinking about science. I'm thinking about whatever counts as the best knowledge, but not thinking particularly about science. Kuhn might be thinking about science.

KLETT: Right.

HARDING: But not Quine. Quine was coming out of logic and metaphysics. And he—I haven't read it in decades, but I don't remember any particularly close analyses of—there must have been some, but I don't recollect—

ROBERTS: So, **<T: 45 min>** you're following—I mean, so I think it's an interesting distinction. I mean, so you're not after a specific—as opposed to maybe others who we might interview in and around kind of origins of STS, where there's a science quandary at the beginning that they have to go explore. You're really coming at this from a very solid philosophical set of questions about logic and epistemology.

HARDING: Yes. That's right.

ROBERTS: And that you end up over in science—

HARDING: And how to produce the—how to justify the kind of knowledge that the social justice movements—

ROBERTS: [Yes].

HARDING: How to justify that.

ROBERTS: So, do you think—so, I mean, I don't want to ask it as like a conjecture, but I don't know how else to say it, but, I mean, in some ways, you end up over there because is this the only area where there are active debates about epistemology?

HARDING: I end up in STS?

ROBERTS: [Yes]. So, I mean, do you end up—you end up looking at science in part because the other epistemological models seem so set, that the only place where there seems to be some discussion about whether or not the model is right happens to be Quine and Kuhn and these—and these—

HARDING: That could be a good way to put the issue.

ROBERTS: —spaces where they were accidentally opening up a space—

HARDING: But, I mean, I was looking at science in some ways, because I was looking at standard accounts of women's biology, of treating—you know, *Discovering Reality* is full of Ruth Hubbard and—you know, it's full of scientists, and known scientists, writing about the problems of science for women, because they don't understand our bodies, they don't—[26]

ROBERTS: Right.

HARDING: —understand our histories, they don't understand this, that, and the other thing. So, it's not that I was avoiding science or not preoccupied with it. I mean, at times I would be. But only as a student of what the critics were saying about it, and I wasn't independently exploring biology texts or doctors' offices or doing any of those sociological things. So, it was—you're right. I was coming into it from a philosophical background that took its cues from the social justice movement.

The other thing is that it might be that you and I are using the word science in different ways. I'm thinking not here so much as something that came up yesterday. I can't imagine—oh, science versus technology, because I refuse to use it—I tried to use the term high science in here, in my last book, and they didn't let me use it, high science versus science. So, science for me includes technology and indigenous knowledge, and I apologize for that, using it to include indigenous knowledge and everyday local knowledge in my last book, because nobody uses the word science for that. Modern Western science doesn't, and indigenous people don't.

But I wanted a level playing field, is what I say in the introduction, I think. And so, I now use it exchangeably with reliable knowledge, whatever counts as reliable knowledge. And so, that's interesting. I was just rereading that other biographical piece that I found. I was re-looking at it this morning, and—why is this—oh, no, I was looking at a couple of pages of one of these criticisms, science wars criticisms. And Alan Sokal and Bricmont—you know Sokal and Bricmont? That book.[27] They're taking out after me, and they're trying to be very kind. Of course, "many of the things she says are true, but the conclusion she draws are ridiculous." I mean, that's just a—I found I had three pages of it.

And they never consider the point I'm making is not that modern physics is wrong, but that there are other questions that never get asked because of the industrial military funding of science, and science is expensive to do. So, I mean, there's a whole literature now on neglected diseases and so forth that are prevalent and easy to cure, but they have no—the pharmaceutical companies aren't going to make any money off it.

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[26] Harding and Hintikka, *Discovering Reality*.
[27] Alan Sokal and Jean Bricmont. *Fashionable Nonsense.* Picador, 1998.

KLETT: Right.

HARDING: So, they don't get funded, and—and Chagas and some of those ones in non-Western—you know, non-industrial societies.

And so, for me, the term science is a very slippery one, and I'm always trying to figure out how people are using it. So, I **<T: 50 min>** don't buy into the argument that—a conventional argument that science explains how things work, and technology applies science to the world. I don't buy that at all. I think technology—technological practices produce their own kinds of knowledge that fit or don't fit with the dominant scientific theories, and that has to be worked out. But there's just plenty of cases—I mean, so that's one reason the dominant science philosophy can dismiss non-Western knowledge. It's all—"They might have technical proficiency," they say. "They know how to build the Great Wall of China and so forth, sail across the Pacific Ocean. But they're not scientists."

ROBERTS: So, I want to put a push pin in the reference to the military industrial nature of the production of science, because I think—Joe and I were having a discussion about the fifties at breakfast, and so—

HARDING: Oh, I see.

ROBERTS: —a lot of our conversation yesterday, and what sticks out. But I want to tie that, because I think that might come back to the question that we also tabled for now, which is what feels so threatening about these alternative approaches.

HARDING: [Yes].

ROBERTS: And I think we might be slowly uncovering what we think might be a place for that.

But I want to get us maybe back on the timeline and think about where we were yesterday. So, we filled in a little bit more of the detail, but this transition to the University of Delaware, and, you know, what that meant for you personally, professionally—

HARDING: [Yes].

ROBERTS: —intellectually.

HARDING: [Yes]. I was very happy to get a job, number one. I got several offers, Lehigh, and I forget what the others were, but I needed a place where I could move my kids, and I didn't want them too far away from their father, so I wanted to stay roughly in the Northeast. And the department—the philosophy department—was very attractive to me, for two reasons. Well, for several reasons. I mean, they had had a master's program in philosophy until I think the year before. Maybe it was still existing the year I got there. And it had a specific focus on philosophy of science and on chemistry, surprise, surprise. [Because DuPont was extremely powerful in Delaware]. But it turned—that's why it failed, because it turns out chemists aren't theoretical and philosophical, so nobody was going to take a philosophy degree and focus on chemistry. But they had found a couple of philosophers who did focus on chemistry, and had them in the department.

So, there was that, but the department also had high involvement in precisely the kind of program I had come from. What did they call it? So, I'd come from this early admissions, general—maybe it was an honors program. Is that what they called it? But it was interdisciplinary, and it was working with very bright undergraduate students, and there were a lot of faculty who—the head of [this alternative] department [at Delaware] was in the philosophy department. The next head of that department also came from the philosophy department. So, they had this interesting multidisciplinary possibility. They weren't going to be as pissed as most philosophers were when I was hanging out with sociologists, right?

KLETT: [Yes].

HARDING: And historians. And it—the chair of the [philosophy] department was a great defender of me all the way through. He had a couple of serious sexually scandalous guys in the department. There was one other woman in the department. [. . .] She was so elite and so elegant, and she taught Plato and Aristotle, and students wanted to learn Greek when they took courses with her. She had them all in there learning Greek. I mean, she was the only other woman in a fifteen-person department.

The chair, Frank Dilley, he died just a few months ago, and I was thinking about him. He changed—he very much wanted me in the department. [redacted 00:54:49 to 00:55:05] **<T: 55 min>**

And he made sure the—he got the rules of the department and the rules of the college cleaned up, so that I could get through. I mean, he respected what I did a lot. And I was publishing, you know, plenty. My actual tenure vote in the department, my tenure vote in the department was 100 percent yes. But when you read the tenure letter, you would never know that. The tenure letter picked out all the things they didn't like about what I did. But it was 100

percent vote for me. So, he managed to maneuver, "Okay, you can say what you want about her, but you got to vote right, right? Look at this record." He did that kind of thing.

And one of the guys in the department [. . .] was the philosopher of science in the department. And he tried to publish a collection of papers called Medusa—or something, *Medusa's Philosophy*. It was an anti-feminist critique, right? He was going to put a picture of Medusa with the snakes on her head on the cover. I mean, I found out about this not from him, but because Ian Hacking, who was at Princeton at that time, I think, sent me a letter, a copy of a letter. And I didn't know him, though he'd been selected by the department to be on my tenure committee, I'm pretty sure. I'm not supposed to know that, but I'm pretty sure that he had written a tenure letter for me. And I didn't really know him well, and he was writing a lot of tenure letters for women philosophers. [. . .]

And he sent me, without any notice to anybody, a copy of the letter he'd written to [the young philosopher of science, who] had asked Ian for a paper for this [Medusa] collection. And it was this—Ian Hacking was at Princeton at the time, and [the Delaware philosopher] was a Princeton graduate, I think. Maybe [he] had been a student of [Hacking's], I can't remember. [. . .]

And Ian said he was ashamed to discover that one of his students was involved in such an anti-intellectual and nefarious project. I mean, I can't remember the wording. I read that, and I thought, "Oh, my God."

You know, and there were a couple of guys out there who were doing that stuff. Richard Rorty was, regularly. I mean, not—I distantly knew him. But he was writing letters for tenure for women philosophers in this era. So, I got tenure in—excuse me—when did I move there? Sixty—no.

ROBERTS: Nineteen seventy-six?

HARDING: Thank you. Seventy-six. Okay. So, '79, I think I got tenure. I'm not sure. It sounds right. Is that right? It sounds right. So, Rorty and Ian Hacking, I mean, they were big deals already, and there are probably a couple more. I can't remember exactly who. But it was an interesting era. You know, I was in the Society for Women in Philosophy [SWIP]. It had already established the Committee on the Status of Women in the APA, [which was the official, formal APA committee, as opposed to the informal SWIP]. I mean, so we were active in the APA. [The APA] knew who we were. We were publishing, and we knew each other. I mean, we had these networks. They were extremely important. We were mentoring each other and everything.

But there were some of these guys who just stepped up to the plate.

KLETT: Just to rewind the tape a bit, because you were also—your first job was at the Allen Center—

HARDING: Yes.

KLETT: —at SUNY-Albany, and that's—you had actually—is it right that you got the job before you passed the dissertation?

HARDING: Let me see. Is that right? It—

ROBERTS: Chronologically—

HARDING: I was there three years, and I moved from there in '76. I think I got the—no, I had my—I got my dissertation in '73. Right? I'm sorry to ask you—

KLETT: No, you're right.

HARDING: Seventy-three. And I took the job half time. I had—

KLETT: Okay. That's right. [Yes.]

HARDING: —other possibilities, but I had two small kids, and I didn't want to commute an hour each way over to somewhere that had a job I could have taken. And so, I—there was a half time opening at the Allen Center, which was brand new. I think it maybe had—maybe that was its first year. Was I there for it—[yes], I think maybe I was there for its first year of existence. I think it only existed for three years. I'm not sure. It was shut down the **<T: 60 min>** year I left, I know. Maybe it had been open the year before, but I'm not sure.

KLETT: I ask, because it feels, the way that you narrate it in the "Work and Politics" piece is that this is where the sort of—the sort of tires hit the ground, so to speak. That this is when you encounter Christine Pierce.

HARDING: [Yes].

KLETT: —people who are actively working—as you say, "we had to invent feminist philosophy."

HARDING: Yes.

KLETT: And it sounds like that—you brought that sort of cottage industry with you to Delaware.

HARDING: Yes. But I met there immediately wonderful feminists. Gloria Hull was her name at that point. She has an African name now, [Akasha Gloria Hull], and she's [retired from UC] Santa Cruz [University of California at Santa Cruz]. But she was—all of the men are white. All the Blacks—all the—all the women are white, but some of us are brave.[28] She was involved in that early African-American feminist collection, [The Combahee River Collective], and had—she was in literature, and had done a lot of work writing on Black women, and was herself African-American. So, she was one of my [valuable] friends.

And Maggie Andersen, who is a sociologist who edited two or three big readers over thirty years—I mean, they named the rooms of their house for some of these readers.[29] Her specialty was race theory, and she'd grown up in Atlanta. And so, there was a current director of women's studies whom we all hated. And then Maggie and then Gloria and I directed women's studies there for, I don't know, fifteen years, between the three of us.

And so, they already had—I mean, I brought a very distinctive experience. It was—Albany women's studies wasn't quite as well-developed yet as Delaware was. Moreover, the race issues were really significantly different. Race issues were so central in Delaware, right? It's a border state, Mason-Dixon line, and this—the university be—the state being party to the busing decision in the Supreme Court, and a couple of those early men and women, African-Americans who'd been active in those civil rights struggles in Delaware were around the university and around the women's studies community.

And so, the feminism was deeply connected with the race issues in Delaware in ways that they weren't at—Albany was a—the Young Lords, there was a powerful civil rights movement in Albany, too, but it didn't get into the Allen Center, or into the—I didn't, at any rate, get into it. I don't think I knew any Black faculty at SUNY-Albany in those days, [apart from the Dean of the Allen Center]. And there were only a few at Delaware, but I knew them immediately, and started organizing with Black women, and had an appointment in African-American studies at Delaware, and taught a course on African and African-American philosophy in I think I would say '80—in '81 or '81 and '82.

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[28] Gloria Hull, Patricia Bell Scott, and Barbara Smith, eds. *All the Women are White, All the Men are Black, but Some of Us Are Brave: Black Women's Studies.* Old Westbury, Conn.: Feminist Press, 1982.
[29] Margaret Andersen. *Thinking About Women.* New York: Macmillan, 1983.

And I was trying to get the philosophy department to hire somebody who actually knew about [African-American philosophy]. I mean, I taught Frederick Douglass, and there was a book on African—one book on African philosophy or something. And we did hire somebody, and I—Bill Lawson, and then I didn't teach it anymore, but I was very involved with the Black community, Black intellectual community in Delaware. And Delaware is a tiny state, you know, so the politics are real up close in the university.

KLETT: [Yes]. Was there any kind of shock or surprise when you went from the Allen Center to Delaware, where race all of a sudden—you know, we talked earlier about the sort of—the dawning of the third wave, and the influence of Black scholars—

HARDING: Third wave isn't anywhere—I don't know what you're thinking about as third wave feminism.

KLETT: I guess I mean the dawning that Women of Color have a different perspective that—

HARDING: [Yes], but that's not the third wave. That's part of the second wave.

KLETT: Okay.

HARDING: The third wave comes a decade **<T: 65 min>** later, and it's a really younger generation. Third wave as I've heard it used is more about the nineties, maybe, and I'm back in '80—the eighties. I mean, so the end of the seventies is the beginning of—I mean, the end of the sixties, '69 or so, is the date—the date of the beginning of the second wave. So, it's about twenty years in there, where we're in the second wave. And so, that Women of Color feminism is part of that second—you're right, it's a very specific thing that happens, and it's different from how it started out with Betty Friedan and *The Second Sex*, right?[30] But it's not yet what I've heard called—

KLETT: Okay.

HARDING: But I could be—I'm so out of touch. You're probably more in touch—

[30] Simone de Beauvoir. *The Second Sex*. New York, NY: Alfred A. Knopf, 1971.

KLETT: Well, no, I think you're describing the—more of a historical activity, and I'm thinking more of the ideas that are kind of—

HARDING: Oh—

KLETT: —starting to gather, and folks like yourself are the first to take them up. Anyway, you encountered this new—

HARDING: [Yes], I was trying to look and see where I first started raising race issues. And I think there's actually—does *Discovering Reality* have a couple of essays in it already?[31] Excuse me. I can't remember what I saw here. No. I think *Discovering Reality* doesn't. That's interesting. Okay. Because this is—this is the 19—what is it? What's the date here? This is the second edition, 2003, so twenty years earlier. This is '83.

And so—but by the time I get into feminism and methodology, which date is—what did I say, '83?[32] Okay, so '87, we're four years later, and I have several—I have Joyce Ladner, "Introduction to Tomorrow's World: The Black Woman."[33] I have—I think I have another race paper in here. Where is it? Is that the only one? Oh, Bonnie Thornton Dill, "The Dialectics of Black Womanhood."[34]

So, I was—so that Black women's movement was already very active by then, the early parts of it. And I was starting to pick it up. But it wasn't really shaping my thought. I treated it I think kind of as an add-on, in a certain way. Of course, it changed. It meant you couldn't—you had to think about whose lives were being talked about. But it—and it certainly was—it's not that it didn't affect the epistemology. It just—in retrospect, I think it extended the epistemology to new groups, start from women's lives—start from Black women's lives, and start from Asian women's lives. And there was a Jewish women's feminist movement emerging.

And so, it certainly changed what white women were saying, but it was only beginning to form a real network, and it wasn't yet doing what's happened in the last twenty years, this whole pluriverse. It didn't get into the ontology. It—actually, it did in certain ways. But Viveiros de Castro always talks about, you know, indigenes in the Andes Mountains. I mean, he's not looking at women's movements.

So, I opened one of my recent essays with—I can't—it's one of the Latin American ones—with the argument that, actually, the arguments of science and technology studies were

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[31] Harding and Hintikka, *Discovering Reality*.
[32] Sandra G. Harding, ed. *Feminism and Methodology: Social Science Issues*. Indiana University Press, 1987.
[33] J.A. Ladner. "Introduction to Tomorrow's Tomorrow. The Black Woman," in *Feminism & Methodology*. Sandra Harding. 1987.
[34] Bonnie Thornton Dill. "The dialectics of black womanhood." *Signs: Journal of Women in Culture and Society* 4, no. 3 (1979): 543-555.

already made by the feminists and the new social movements of the sixties, and there's slight gestures toward that in the early STS, non-feminist STS, but they're very slight. I mean, they're using the same kind of—I mean, Kuhn opened the door in *Scientific Revolutions*, right? I mean, revolution, right? I mean, he was connecting it to a different historical revolution, of course, but—

KLETT: Right.

HARDING: So, there were things in the air at the time that were floating around, and only partially grabbed by different groups, and they don't really gel in significant ways. But I was interested to see where I'd **<T: 70 min>** started talking about these issues, but I'm not into postcolonial work yet.

ROBERTS: [Yes], so can we—can we dive into what you just mentioned? I mean, I think there's two—two different paths we could follow. One would be, you know, what did you think Kuhn opened up accidentally? You know, and I think—I say accidentally just because I think there's so much post-Kuhn interpretation of Kuhn's work, as you suggested earlier.

HARDING: [Yes].

ROBERTS: And the other is when do you think you became conscious of the fact that the science studies folks were just re-asking the questions that the feminist theorists were—had already been trying to sort out.

HARDING: Okay. Could you hold that for a minute?

ROBERTS: [Yes].

HARDING: And I'm going to get some more water.

[Crosstalk]

HARDING: I'm sorry.

ROBERTS: So, I said—oh, that's okay. I—

HARDING: Just the last thing, and I'll remember.

ROBERTS: I had two—we can go two different directions.

HARDING: Yes.

ROBERTS: One would be what do you think is the space that Kuhn opened up.

HARDING: Oh, okay.

ROBERTS: And the other was at what point do you realize that the science studies folks are just re-asking questions that the feminist theorists had already been grappling with?

HARDING: Well, I think Kuhn opened up the space that Jasanoff characterized as science—sciences and their societies [co-constitute] and co-produce each other. And I think he opened that up in the way that he looked at the history of science. And Feyerabend—I was just looking at Feyerabend recently for a variety of reasons, and of course, he opened that up even more. And when I taught at the Allen Center, I forget if I mentioned this, several of—three of my [men] colleagues [. . .]. One was—two were physicists, and one was a mathematician, and they'd all come from [University of California at] Berkeley. And Paul had had a serious crisis. Should he continue being a [folk and rock] guitarist, or should he get a job in physics? [laughter] He came to the Allen Center. He was a lot of fun.

But he had been in Feyerabend's class, and had a purple mimeograph copy of *Against Method*.[35] And he knew Dorothy Smith's work, which I didn't know. And so, those three guys sat in at the Allen Center on my first philosophy of science classes, and then they took me out to lunch, and they said, "Sandra, you're not going to pollute these young minds with that positivist stuff, are you?" That I'd just learned—I mean, I'd just gotten my doctorate.

KLETT: [Yes]. Right.

HARDING: And they were a pleasure and a pain in the neck. But it was very—it was very interesting.

[35] Paul Feyerabend. *Against Method.* London: New Left Books, 1975.

So, I think—I mean, I don't think I have a lot more to say about it than for me, Kuhn opened—it was a really different kind of history of science than I had encountered, and it opened up the sociological—deep sociological questions in kinds of ways that I didn't know anybody else was opening them up, except that guy whose name I still can't remember. *Science and Its*—well, I can find it if I check—I put it in the bibliographies.

KLETT: Where was Dorothy Smith at the time? Is she at BC [University of British Columbia]?

HARDING: No. [Yes], she could have been at BC. Or was she already at Toronto? I can't remember. She was either at [University of Toronto] or BC.

KLETT: But that was your introduction to—

HARDING: Yes, it was that early paper of hers. She had given it at the American Sociological Association, I think in 1972.[36] But the next papers to—she had a couple of papers out in the late seventies. But then around '79, I—well, first of all, I was a peer reviewer for *Signs* on Hilary Rose's first paper, and I complained that this paper was not citing the work of Hilary and Steven Rose,[37] and it needed to. Well of course, I couldn't have been blinder. [laughter]

I met her shortly thereafter, and Nancy Hartsock and I had been meeting for a few years in—at—she was at [Johns Hopkins in] Baltimore, and [Donna Haraway] had been at [Hopkins]. And they had taught a course that refused to die. And so, they were meeting with a couple of graduate students two years later still and I joined that [group]. And year one, Donna was in it, and then Donna went to Santa Cruz.

So, the next—and then Nancy and I started writing standpoint papers to each other, in effect. So, at any rate, there were a whole bunch of people who were doing that. But let's go back to Kuhn sociology—

ROBERTS: I just wondered if you were talking about Ravetz.

HARDING: Yes. Jerome **<T: 75 min>** Ravetz. *Science and Its Social Problems*.[38]

[36] Dorothy Smith. "Women's Perspective as a Radical Critique of Sociology." *Sociological Inquiry*. 44, 1974.
[37] Hilary Rose and Steven Rose, eds. *Ideology of/in the Natural Sciences.* Cambridge, MA: Schenkman, 1976.
[38] Ravetz, *Scientific Knowledge.*

ROBERTS: Right.

HARDING: Isn't that the name of it? And it's a little earlier than Kuhn. It's a couple of years earlier. I didn't discover it until after Kuhn, but then when I read it, I realized he was really grabbing some of the issues.

ROBERTS: [Yes].

HARDING: [Yes]. But the other direction you asked was how did I—

ROBERTS: What point did you—do you think you started to realize that science studies was asking those questions? And it might be another way of thinking about how your intellectual work evolved at Delaware, to put you then in contact with what was emerging as science studies.

HARDING: You know, I'm not—I wish I could remember when I first went to an STS meeting. I think they date 4S to what, '76?

ROBERTS: Roughly, [yes].

HARDING: It wouldn't have been that early. That would have been just a couple—that I would have just been moving to Delaware. I wasn't in it yet. And I can't remember anybody at Delaware who was in 4S. But I don't think it was late as when I moved to UCLA. So, somewhere in there I must have gotten hip to STS. My recollection is that at the beginning, I would have been—I was, I think, probably co-organizing feminist work with Ruth Hubbard who was maybe attending. Who else was attending? Donna was. [She] was in 4S early. So, maybe it was through her that I got there. I can't remember.

But I remember it was very hard to get audiences for feminist sessions [at 4S]. There were plenty of people who wanted to talk about women in science, right? Policy about women. But there weren't people into the epistemological issues. And I've [been able] to [organize projects with] Wenda Bauchspies now. You know who she is?

ROBERTS: [Yes].

HARDING: [She was the Director of our STS division at NSF]. And she was organizing those sessions, and she was way back a little closer to that side than she is now, and she's now much more deeply into exactly what I'm into, but in her own ways.

And I can remember going to a session of hers, and she had five panelists, and I think there were three other people there. And that's what would happen at our sessions, too. Occasionally, some guys would lurk along the back of the walls. They didn't want to actually sit down, but they wanted to see who was up there. It was—I mean, the same thing had happened at the APA earlier with philosophy.

KLETT: Right.

HARDING: So, I think that 4S was a little slow to come to those issues, and maybe that's also because in Europe, intellectual feminism, if I can put it that way, was slower. There, they were ahead of us on quotas for women in parliament and, you know, some of those practical things. But Hilary—I mean, so Hilary Rose was doing standpoint theory and, of course, the Marxian tradition was much more powerful in Europe than here.[39] I mean, not that it as consciously being practiced, but it was part of their intellectual history in a way that was not the case here.

And so, I can't remember when—when I first started going to 4S or what I first started doing, but I got aware of that kind of resistance to what I was doing. Not that it was overt that I can remember.

So, let me see. The science wars. I have to look at that, because I want to see what the overlap is. When did the science wars start? I outlined this for my own purposes. There we go. No, this isn't it. Excuse me. I've got too many papers here. I'll find it. It's sitting right here somewhere. I think it had—it had a twenty-year—here we go. So, it really gets going in the 1980s. I think I told you, the *Synthese* board threatened to resign [rather than publishing the papers that became *Discovering Reality*].[40] That's 1983. And Jaakko gave us *Discovering Reality* in the *Synthese* book series.

And then also in 1983, **<T: 80 min>** yes, it was after *Discovering Reality* came out—I think I mentioned this, too. The guy who was head of our division at NSF—I can't think of his name—I had had a summer NSF grant, and was getting ready to apply, and I was chatting with him about what it should look like for a term off, term-long NSF grant.

And he called me and said he was really sorry—I mean, he did it on the telephone. He didn't want to write it. But he couldn't give me any grants, because he was being so inspected by right-wing politicos that it would damage NSF if NSF gave me a grant. Can you imagine?

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[39] Hilary Rose. "Hand, Brain and Heart: A Feminist Epistemology for the Natural Sciences." *Signs: Journal of Women in Culture and Society.* 9: 1, 1983.
[40] Harding and Hintikka, *Discovering Reality*.

And then—so that's '83. Then the—then it really gets going in '97, because that's when *Higher Superstition* appears, [though significant parts of it had appeared earlier].[41] Levitt and Gross. You know *Higher Superstition*?

ROBERTS: [Yes].

HARDING: Which has a whole chapter on me and a whole chapter on Donna. And there's a terrific analysis in one of these books, in Ross, '96, by Roger Hart, whom I think I mentioned, of a chapter, on me—I think it's in *After the Science Wars*.[42] Is that right? Is that where Roger Hart is? That's where Steve Fuller's terrific bibliography is.[43] No, it's not in there. It must be in the science wars. So, the science wars are very connected to me with this issue about STS.

ROBERTS: In what ways?

HARDING: Because I took the issues—I took the science wars to be about STS.

ROBERTS: And so, you suddenly find yourself lumped in with a group that you didn't see yourself with originally?

HARDING: No, that wasn't the issue. The issue was that in attacking me, they were attacking STS.

KLETT: [Yes].

HARDING: It's not that I didn't identify with STS. As I say, I just can't remember when I was first going there.

ROBERTS: I was thinking, too, that you had referenced earlier that you drew on the language of the strong program as you were formatting strong objectivity. So, is that one pathway in for—

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[41] Paul R. Gross and Norman Levitt. *Higher Superstition: The Academic Left and its Quarrels with Science*. JHU Press, 1997.
[42] Hart, Roger. "The Flight from Reason: Higher Superstition and the Refutation of Science Studies." In *The Science Wars,* Andrew Ross, ed. Durham, N.C.: Duke University Press, 1996.
[43] Steve Fuller. "The re-enchantment of science." *After the Science Wars: Science and the Study of Science* (2000): 181.

HARDING: Yes.

ROBERTS: —what was your—

HARDING: Bloor and [Barry] Barnes and—[yes], I was reading—

ROBERTS: And so, when did that happen?

HARDING: That's a good question. I'd have to check the bibliographies. Pretty soon after they published it. When was that, in the eighties?

ROBERTS: [Yes].

HARDING: So, probably in the eighties. So, probably about a decade—so I was reading Bloor and Barnes. I knew Karin Knorr Cetina, because her husband was in the chemistry department at the University of Delaware, and they were commuting between Bielefeld and Delaware. And I had lunch with her a couple of times in Delaware, whenever that was. But she wasn't really into feminism, and she was a little leery around me. But I knew her work. *The Manufacture of Knowledge*.[44] Is that the title of it?

ROBERTS: [Yes].

HARDING: So, whatever that date is, I was already into that work, and I was into Bloor and Barnes shortly after they were produced. And met—and also into two Americans who'd gone to London in the 1960s to escape the draft. Why are their names—Les Levidow, who I'm back in touch with now. Do you know him?

ROBERTS: I know the name. [Yes].

[44] Karin D. Knorr-Cetina. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*. New York: Pergamon Press, 1981.

HARDING: [Yes]. He's the [founder and editor] of *Science as Culture*. And I called him up and had a good discussion with him when we were putting *Tapuya* together. We called up editors of other journals, and he—it was really fun to talk with him again, because I'd met him in London decades before. And Robert [Young]. I've got his reader in the other room.[45]

But Les Levidow and [. . .] Robert Young [. . .] were by now firmly Brits, but they'd both been Americans who to escape the draft had in the sixties gone to England. And they were both working kind of on the borders of where I was working. I mean, I found their work very sympathetic—they were very critical of the dominant standards. They weren't into feminism in particular, or into—Robert Young was into the race issues, because race was a very big intellectual issue in England in that era. So, I don't remember being at 4S then, but I do remember meeting these guys, and they knew my work, I knew their work, and so I was making connections with these people.[46]

ROBERTS: And Young, if I recall, worked on philosophy of biology. Is that right?

HARDING: [Yes], I think he did originally.

ROBERTS: So, it's interesting to think about kind of **<T: 85 min>** theoretical boundary objects of sorts.

HARDING: [Yes].

ROBERTS: You know, so you coming in from race, but coming from a race and social justice perspective, encountering science studies, but because there were folks like Young and others looking at science that suddenly got them to those social concepts, and whether or not there were these kind of emerging topics where—where these groups could meet, and whether or not that's one potential place where these different theoretical frames maybe began to be in conversation.

HARDING: [Yes], because he—they were both very philosophical. Their kinds of criticisms of mainstream science were deeply philosophical—and I was also—I think it's around that time, but I'd have to look at—up in some bibliographies that I've got here, that I started reading the critics of modernity. There were a couple of modernity collections that came out by historians that were talking about the violence that was done by assuming modern Western modernity was the only form of—how it participated in capitalism, and—okay, so also, I was working on the

[45] Robert J. C. Young, ed. *Postcolonialism: An Historical Introduction.* Oxford: Blackwell, 2001.
[46] Les Levidow and Robert Young, eds. *Science, Technology and the Labour Process: Marxist Studies.* London: CSE Books; Atlantic Highlands, NJ : Humanities Press, 1981.

development issues already. I was probably starting to teach a course on women, gender, and development. [Nalini] Visvanathan. I taught that course for a long time. I loved it. And I met a lot of the people from India and everywhere who were publishing in those realms.

[Yes], so I have to mention—just tag this other issue, too. I was traveling a lot. I spent three weeks in South Africa, sponsored by HSRC [Human Sciences Research Council of South Africa], the social science division, three months after [Nelson] Mandela became president. Would that be '93? And I was at my—where was I based? In Pretoria, I think.

But I traveled—I was two weeks in Pretoria. Yes. That's where HSRC was. And then I was around the country, Cape Town and Durban and Port Elizabeth. Who's that South African guy? He was in Port Elizabeth. He was such a terrific theorist. I can't think of his name. But I was—so I was there.

And so, just before '95—'95 was the Year of the Woman, I think, in the UN.[47] And so, in ['93 and] '94 and [after], there's about a three-year period here where I was doing a lot of UN work. And I co-edited this chapter of [*The World Science Report 1996* entitled: "The Gender Dimension of Science and Technology" with Betsy McGregor].[48] So, this would have been ['96], designed for the Year of the Woman.

We wrote that in two months, and it's I don't know how many thousands of words. I mean, Elizabeth—I'd met Elizabeth—PAHO, the Pan-American Health Organization, had hired me to teach a course in Costa Rica on epistemology, and then give lectures sponsored by the health ministries in the next [three] countries north —what is it? Nicaragua, Honduras, I think Guatemala, maybe. Three countries to the north. [In the early 1990's]. So, I was meeting all these Latin Americans and [in my lecture tours] South Africans, and their philosophic issues were not the philosophy of science that I was used to. I mean, so I had—they—their issues were about the problems with modernity and the enlightenment and the way development was imposed on them, and how could they—they needed health services that were not provided by the federal government, and [in Costa Rica I think it was] they had all these strategies for designating peasant men from the mountains as health chief for that village, and it was—but it was feminist work that they were training them into, because women were the health providers in the communities, but they had to make the guy [recognized by] the federal [government as the chief in order to legitimate the health program]—I mean, it was fascinating.

So, it was a mix of feminist and anti-racist and anti-colonial and development and anti-modernity issues that I was just exposed to. And they had no place in any philosophy of science department. They just had no place in it. So, I was working in this other world that was happening and exciting, and they loved my work. I mean, in the—**<T: 90 min>** a set of sessions that UNESCO and UNIFEM organized, preparatory—I think it was the year before that [trip to Central America].

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[47] The United Nations organized an International Conference on Women in Beijing in 1995.
[48] Sandra Harding and Elizabeth McGregor. "The Gender Dimension of Science and Technology," in: *World Science Report 1996*, Paris: UNESCO, 1996.

So, UNIFEM had had complaints about science and technology since the League of Nations. They—in the [nineteen] twenties. They would produce lists of bad health policies for women. But they had had no epistemology. So, when they saw standpoint epistemology, they grabbed it.

So, I was so popular. It was both rewarding and unnerving [for me], because I hadn't thought it in their context, and I just was walking around with my ears open all the time, and smiling a lot, hoping all went well. I was giving lectures to an entirely non-English speaking audience, and I came back sounding like [Fidel] Castro, because [in Central America] I always had a translator standing next to me. And so, I'd speak [short half-sentences], "duh duh, duh duh, duh duh," pause. The translator would do that phrase. "Duh duh, duh duh, duh duh." And I came back talking like that, and my friends said, "Sandra, what happened to you? [laughter]

But it was immensely exciting. And so, that is all going on at the same time that the questions we were talking about my relationship to STS are happening. And STS was more open to [my thinking] than philosophy of science, even though [STS] wasn't doing [international work] yet. I mean, it—they weren't doing their international projects. And I never went to the—I never even—well, maybe—I did go to a couple [4S conferences] in Europe, so maybe they were—I went to one in Bielefeld, I think. I can't remember when that was. But maybe they were cosponsored by the European STS committee.

But I don't remember any of the postcolonial work in those early STS meetings.

ROBERTS: And I just want to be clear for the—for the recording, that the document you were referencing is "The Gender Dimension of Science and Technology," which was done as part of the World Science Report.

HARDING: That's right.

ROBERTS: That's what you gave us to look at.

HARDING: And—yes. And it—it's an extract from the '96 World Science Report.

ROBERTS: Right.

HARDING: And it, not the whole report—and you know what the World Science Reports are. They sit on coffee tables all over the world, right? Engineering firms' coffee tables [feature them] to show they're aligned with international good policies.

This particular chapter, they ran a special—they ran special discussion—presentation sessions on in Jakarta. UNESCO did, Jakarta and somewhere in Latin America. They invited me to come, but I was too busy to go. And they'd get all the local journalists there [in Jakarta and elsewhere], because this was the first-time feminist issues had been presented in this epistemological way that gave [policy-makers] a handle on it. And they publish this in—I don't—thousands of copy in seven languages, and distributed them at the Year of the Woman that was meeting sometime around then, or whatever it was.

KLETT: Since we're on this topic, you had mentioned previously that you—in speaking of your managerial writing style—

HARDING: [Yes].

KLETT: —that producing this kind of report, not only in a very short turnaround, but knowing you have a very different audience in mind.

HARDING: Exactly.

KLETT: Can you say a little bit more about how you went about approaching that strategically and—

HARDING: Okay, so we took—so Elizabeth McGregor is a Canadian [. . .] veterinarian. And I met her somehow through some UN thing, I forget what, and loved her. She regaled [me with] these stories of how all the [feminist] veterinarians delivered each other's babies. I mean, you know, large animal, farm veterinarians, they knew how to deliver calves, they could deliver babies, and they all delivered each [other]—you know, these wonderful tales.

She was at the time the assistant to the head of Canadian development, whatever that agency was [IDRC: International Development Research Canada]. And she was organizing feminists around the world, veterinarians around the world. She organized a woman's conference [in Cairo]. I mean, it was veterinarians she was organizing. She could get away with. I didn't—but then they were doing feminist science and technology.

So, I asked—<**T: 95 min**> when I got invited to do this [World Science Report chapter], they had asked a number of other people who were fully qualified to do it, and they were sociologists, they'd all turned them down, and I knew why. It was much too big a project, and we needed data, and who could do it? And of course, by the time they got to me, it was December, and this thing was due the end of January. It was—no, it was just before

Thanksgiving. So, that's not when they got to me. That's when I said yes. They came back a second and third time, and said, "We can't get anybody? Could you do it?"

And I realized, this was an opportunity I should grab. It's going to be hell on wheels, but it's—to have this opportunity to get it in print and—I mean, the big thing about the UN is their [huge] distribution [networks], and, you know, they have all of these distribution strategies. So, I decided if Elizabeth would help me I'd do it, so I asked her, and so we did it together.

So, I'll just tell you one thing as a start. So—let me see. How did this happen? Yes, okay, so the first thing we did is commission a historical essay by [Pnina Abir-Am].[49] Do you know her? So, it was a history of [women in] science, and we put it first in [the draft version if this UNESCO document]. She's a well-[known historian—Pnina] had written a lot of histories of science.

And I turned [. . .] the draft of it in to the [UNESCO director] I was working for here. I can't remember who it was. And he immediately shot back at me, "Sandra, that's an essay." He was accusing me. ["An essay" was not an acceptable form of writing for a UNESCO document.] We were going to put it first in the volume. And so, then I had some discussions with Elizabeth, and we realized we had to write with little bullets in front of the paragraphs. We had to pull off these little topics, and—you're not supposed to write with little bullets in philosophy. You had to say these things very simply. He said, "Nobody has time to read an essay. Nobody's going to look up that bibliography." He said, "You have to write in a kind of way that can reach this audience." And it was a completely different phenomenon for me. [. . .]

<T: 100 min> [So, we recruited lots of] empirical studies. My task, all of that—my [main self-assigned] task was to make sure I could read the [charts and graphs]—seeing what it is now, when we're publishing data, I [sometimes] don't understand exactly what's happening, but I have to be able to read these [charts and graphs]. And the captions frequently don't permit me to understand what's going on. So, I insist, you have to speak to me if you want to present this material. I'm doing it with *Tapuya* all the time now. It's fascinating. I've learned how to do it.

So, in those years, there were all of these different things going on that were STS projects in some ways, but they were not within 4S's—I was kind of circling around 4S, I guess is the way I would put it in these other contexts.

KLETT: Before 4S, before STS has these institutional gathering places, something that seems like a constant, at least through your time at Delaware, is the role of reading groups.

HARDING: [Yes].

[49] Pnina G. Abir-Am. "Women in Modern Scientific Research: A Historical Overview," in *The Gender Dimension of Science and Technology*, Sandra Harding and Elizabeth McGregor (eds.). Paris: UNESCO, 1995.

KLETT: And I know, you know, feminist reading groups, like most activist reading groups, they're very informal. They come from people not getting what they need elsewhere. And so, I was wondering if you could talk about maybe lessons that you took from early reading groups, but particularly how you used them—because it sounds like even up until this, '96, you're still not finding institutional interdisciplinary spaces that are allowing you to have the conversations that you need.

HARDING: Yes. Well, two reading groups that were indeed—one was directly on science and technology studies, and they were both at Hopkins. And one was Liz Fee—Elizabeth Fee is a historian of—is she at Hopkins? No, she was at one of the federal—she was at NSF or some—she was somewhere in Washington, one of those big agencies, I forget which one. But she was in public policy at Hopkins I think also. I forget exactly. The University of Maryland, whatever. So, she and I and her graduate students met for—I don't know, we would meet every three weeks for maybe a couple of years, until that fell apart. But that came after that reading group I got into that was the course that wouldn't die, that Donna Haraway and Nancy Hartsock had taught. And of course, Donna was into science and technology studies. We didn't always address science and technology so much as epistemology.

So, I mean, I don't know what to say, what I took from one to another. I mean, a reading group is a reading group, and you figure out how to use it and make it work. But they have always been very important to me.

KLETT: I guess I'm thinking in terms of the reading groups that happen amongst say, well, we're all political sociologists, for example, and there's this new thing here. Let's bring it in. And Thomas Piketty writes his book. Let's all read it and see what does this mean for our field, as opposed to, you know, these institutional walls are preventing us from seeing these matters as—you know, the social justice dimension, for example, that a reading group—an institutional reading group or—happens in the purview of the department, doesn't allow us to be as flexible or nimble in the applications of what we're doing.

HARDING: [Yes].

KLETT: Or—I'm just wondering—because definitely when Donna talks about the—this—she may be referring to the same reading group, the role of fiction and writing fiction, and, you know, dealing with poets and artists and people that are part of that—

HARDING: She did that more than I did.

KLETT: —network. [Yes].

HARDING: Well, I wasn't trying to reform philosophy. I've more recently been trying to reform philosophy of science without having to suffer being anywhere near them, you know, but clearly, they're my target. And—and the Philosophy of Science Association [PSA] has radically changed in the last few years, but they were so awful to me. I haven't been back for twenty years. **<T: 105 min>** But I think Alison Wylie has been a president, and also Helen Longino. I mean, they've been taking [significant feminists as their leaders].

So, Nancy was trying to reform political science. Patricia Hill Collins was trying to reform sociology. But I wasn't trying to reform a particular discipline. I would be happy if I could have, but it never—I wasn't going to work with them, and I wasn't teaching them, so to speak. I was in these other departments. I mean, I had a joint appointment at Delaware. I had a joint appointment the whole time to the sociology department and taught a course on methodology. [This was not a course on] methods, but methodology and philosophy of science that was required of all the sociology graduate students. And so, it was a joint course between philosophy and sociology. So, I had had more sociology graduate students in my courses than anybody else in the sociology department, which is why they finally—I was writing all these reference letters, and so they finally, you know, said, "Sandra, we've got to give you an appointment in this department."

So, my tasks didn't get defined in exactly that way, but of course, I was focused on changing—on providing philosophies of science that were useful for the groups I was interested in. I mean, I think I mentioned this. Sharon Traweek arrived at UCLA the same time I did, and we went to each other's projects, and I knew her ahead of time from when she was at MIT.

And she said to me, after a year of following me around, she said, "Sandra, I've figured out what you do. You plant yourself on the edge of an institution and you refuse to go away." And that is true. That's what I was doing. But I think for Dorothy Smith and for the sociologists and political theorists, political scientists, they were much more embedded—they were in those departments, and I wasn't. I had a philosophy department appointment at Delaware, it was my main appointment, but that's the last time I've had an appointment in a philosophy department. I didn't—never did at UCLA [after I was permanently appointed to the university].

KLETT: [Yes].

HARDING: They didn't want me and I didn't want them. But they were very kindly to me, and I taught the feminist philosophy course for twenty years, and when I was leaving UCLA, got sweet letters from the guys in the department, and so forth. I mean, they were very happy to have me teach a feminist philosophy course. But they're so backwards. I mean, they're so into

the epistemology and metaphysics curriculum of the Cold War. They have almost no branches out of that.

KLETT: This might be a good time to ask this, because you had also in "Work and Politics" mentioned—[yes], you mentioned—it was some book that you were referring to, and you were saying that the effects of the Cold War on American philosophy were pretty crippling, whereas literary studies was influenced by, but worked its way out.

HARDING: [Yes].

KLETT: Could you expand on what you meant by that?

HARDING: So, let's take philosophy of science. So, the Vienna Circle, okay? They were almost entirely Jews. They were all socialists. They began moving to the US in the late 1930s. [Hans] Reichenbach I think came to UCLA around '37 or '38. I had dinner with his [widow] when I first arrived at UCLA. [. . .] He'd died some time before. And he hired Carnap [as a visiting professor]. And my office at UCLA was the one had been Carnap's.

So, the compromise that the Vienna Circle made when it confronted—I tell this story in the beginning of my last book as a way of talking about how the philosophy of science we have is the one that was fundamentally formed in this particular circumstance. Most of the Vienna Circle ended up in the US, and a good number of them at UCLA, at some point. They went to Paris or somewhere first. Some of them didn't come to the US, but most of them did, where they encountered McCarthyism, which was not—they were all socialists. McCarthyism was not happy with them. And McCarthyism was anti-Semitic. It was deeply **<T: 110 min>** anti-Semitic, and these were all Jews.

And so, the way—the best book here is [George] Reisch's book that came out a decade or so ago. Do you know the book? *How the Cold War Transformed Philosophy to the Icy Slopes of Logic*.[50]

KLETT: No. Is that the—

HARDING: It's fabulous.

[50] George A. Reisch. *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic*. Cambridge University Press, 2005.

KLETT: Do you know, is that the one you were referring to in that article?

HARDING: Yes, it is the one I was referring to.

KLETT: [Yes].

HARDING: *How the Cold War Transformed Science to the Icy Slopes of Logic*, Cambridge. You want to see it? It's a terrific history. I mean, it—Nancy Cartwright had been doing some work on the other member—not Reisch. Oh, what's his name? The—another member of the Vienna Circle, for some time. But this is a comprehensive study of the effects of the Cold War on philosophy of science. So, let me see, what was the question?

KLETT: So, this actually comes after—because "Work and Politics" is 2002.

HARDING: [Yes].

KLETT: And this is 2005.

HARDING: Yes.

KLETT: I think the book you're referring to in that statement is actually from the early nineties, and maybe it was about literary studies. Maybe that's the distinction.

HARDING: I can't remember what it is. I'm so sorry.

KLETT: No, I want to read this stuff myself. So, we were talking about the sort of Cold War effects.

HARDING: Oh, okay.

KLETT: Like how did you see the Cold War effects on philosophy?

HARDING: Oh, okay. So, the Vienna Circle had to make a hard choice, and what they decided to do is go with Carnap rather than Reichenbach and the other guy whose name I can't remember who Nancy Cartwright writes on. I can look it up, because I must—I think I cited her in here. Did I? Excuse me for just a second. Cartwright. Are you going to interview her?

ROBERTS: I don't think she had come up, but I've got quite a list going just from our conversation the last two days.

HARDING: Neurath. Otto Neurath. *Philosophy Between Science and Politics*, 2009.[51] But she'd been writing a number of papers earlier. But I don't know that—I'm sorry, I don't remember.

So—so the choice they made was to take out the politics from their Vienna Circle logical positivism. And it—this was the same time that ordinary language philosophy—it's a little before ord—John—John Austin, Wittgenstein and then John Austin, ordinary language philosophy comes in. And the two fit together perfectly, because neither of them was interested in history. Right? They're both [not] interested in the larger political environments within which [sciences and their philosophies occur].

And so, Reisch talks about—that's what he means by the icy slopes of logic, that the philosophy of science becomes fundamentally logic. And it loses—and the Vienna Circle's agenda, the unity of science thesis that Neurath I think was—was Neurath the author of that? I think—

ROBERTS: I believe so.

HARDING: [Yes]. Was intended—it was a call to the sciences in the early 1930s, when Nazism was forming, to address the social issues that were about to be solved in horrific ways. And they could see, [yes], that was happening. It was a clear political call. By the time I heard about the unity of science thesis, it was a reductionist theory about how physics is the basic science and everything else is reduced to physics. I mean, it was a completely different meaning to it. And so, Reisch is just so good about capturing this deeply socially responsible political agenda that the Vienna Circle had, how they all had to escape the Nazism and what happened to their philosophy when they got to the US. So, they cut it back to save it, in effect—cut it back to Carnap's program, because they could see that philosophy of science, and indeed science, would be endangered in the US if the full political program that [the Vienna Circle] had called for in Europe were maintained in the US. So, it's a very sophisticated argument about politics and philosophy.

[51] Cartwright, Nancy, Jordi Cat, Thomas E. Uebel, and Lola Fleck. *Otto Neurath: Philosophy between Science and Politics*. Vol. 38. Cambridge University Press, 2008.

And so, my **<T: 115 min>** beloved philosophy of science teacher had been a student of Carnap's, right? And we celebrated Carnap's birthday in his class each day. I thought, wow. I mean, each year. I'm in some kind of special club here.

KLETT: [Yes]. No, what you're saying about the value placed on studying real people—living people, that this is sort of an access, right? Like the sort of genealogies of intellectuals who work together, and that you were kind of connected to Carnap through his students.

HARDING: [Yes], so I—[yes]. And that's long before Nancy Cartwright started writing about Neurath or, you know, Reisch's book came out.

ROBERTS: Another pin. I mean, I wonder if this is something to come back to in terms of seeding what becomes science war places of debate. Because there are again these—the way you just discussed this, I mean, there are large stakes in this debate.

HARDING: Yes. There are.

ROBERTS: This is not a philosophical debate.

HARDING: Nope.

ROBERTS: This is a deeply political debate that has ramifications for all of the fields involved. And how much, again, you know, these kind of choices that are made—

HARDING: That's right. Yes.

ROBERTS: —to be able to persist, what arguments had to become to look durable but whether or not they also contained within them some tensions that really could not be resolved.

HARDING: I mean, I think for me, it was an incredibly fruitful experience to have.

ROBERTS: [Yes].

HARDING: To get to live through this moment when the Cold War philosophy was prevalent, and then the shift was happening, but people didn't quite know how to do it, and—because at the same—you know, this is all happening—it's such an—pick any single year to talk about, it's a very complex scene of different groups, half wandering around, and half intent on something that may or may not be useful, and was or wasn't successful.

ROBERTS: [Yes].

HARDING: It can't be presented as a wonderful singular line history of some sort.

ROBERTS: But I think you're also putting these boundaries up of time on something that is sometimes, like other controversies, rather, other wars, debated as something that has been in existence for a long period of time, and saying, well, no, this is actually part of a very recent phenomenon.

HARDING: Oh, I see.

ROBERTS: And dated to, you know, these particular decisions that we made.

HARDING: Oh, I see.

ROBERTS: And so, I'm finding that very helpful, kind of bookended by, on the more recent side, that in 1994 we have *Higher Superstition*, but in 1996, you publish your report for the—report on science to great fanfare.[52]

HARDING: Yes.

ROBERTS: And so, this is a very—

HARDING: It's all coming out at the same time.

[52] Gross and Levitt, *Higher Superstition*.

ROBERTS: —this is a very limited amount of time, where there both seems to be—

HARDING: They're both going on—

ROBERTS: —great anxiety and debate, and also—

HARDING: [Yes]. Great thrills about the work I'm doing.

ROBERTS: Right. That this is actually right on, and that—and so—

HARDING: [Yes]. It's so—it wasn't confusing to me at the time. I understood there are people who like your work and there are people who hate your work, and you know who you should be working with.

ROBERTS: [Yes].

HARDING: But I always had to think about who was reading it, and how it would be used.

ROBERTS: Right.

HARDING: And so— as I was explaining to Leandro [the editor-in-chief of *Tapuya*] this morning, my work has just always been very strategic and managerial, because I'm always aware that all of these different people are reading it and doing different things with it. And, you know, people ask me, so how come you aren't arguing with somebody rather who came out with some criticism or different reading of what I'm doing? And I say, you know, it's like your children. They go out and they do whatever they're going to do, and you can try to pull them back, and I do every once in a while with my work, but fundamentally, people are going to do what they're going to do with it.

And my efforts have to be focused on how to make the work useful in the kinds of ways I care about, and kind of—I mean, I've had enough job stability. I've never had a problem—I was never threatened with being fired. I mean, I had jerk Medusa around me, and, you know, some jerks around me sometimes, but I've never worried that my job would be in danger, though that tenure moment was a little bit too exciting, **<T: 120 min>** when I said, "Great, I've got it," and then I read the letter. I said, "Wait a minute, this is the letter supporting my tenure?" [laughter]

Gloria Hall had the same thing. She—it happened to her the next year, I think. I said, "Gloria, just take it and run."

KLETT: [Yes].

HARDING: And so, we were always living in the middle of these cross-currents of—[yes], it's funny to think how involved academic—you know, the Delaware dean I loved, because when I was directing women's studies there, she had a monthly meeting of all the department chairs, and so there'd be, you know, thirty—in the college of arts and sciences, so there'd be thirty people in the room and twenty-eight of them were guys, right? [. . .]

And she frequently pointed out to us, when we were getting into big arguments, that university departments are very distinctive. They have the shortest career ladder of any professional field you look at. There's like three steps and that's it. And there's nothing to do but make trouble after that third step. She said, "So, you have to find something else to do." I mean, of course, a lot of them were finding other things to do. They were growing roses, and not doing any physics.

KLETT: [Yes]. Right.

HARDING: [redacted 02:01:26 to 02:01:28] They were—whatever. But she had a really good institutional view of the peculiarities that show up in university work, and the intensity of people's arguments. I mean, it's like we're more identified with our thoughts and writing than in other fields. I mean, doctors are identified with what they do. You do it or you don't.

KLETT: It's interesting, the way you point that out, because like as soon as I learned what tenure was, when I was maybe in high school, my understanding was always your goal is to get tenure so you can make trouble. It insulates you. That's the whole point. And so, like it's interesting, the sort of—the maneuvering through those three steps that we do to appear, you know, sort of—to heel a certain line, to stay within the edges of the institution, and to veer close to the edge, maybe for our integrity, but to not fall off the edge, because if you fall off the edge, you'll never get to make trouble.

HARDING: [Yes].

KLETT: You'll just kind of be irrelevant.

HARDING: But I've had to choose where I locate myself very carefully. I mean, I located myself in a graduate school of education. When I first got a joint appointment to sociology, my philosophy colleagues said, "Commiserations, Sandra." They thought sociology was a terrible field for a philosopher to—they would never think of wanting what I—it gave me a whole freedom. When I came to UCLA, there was a debate about in what department my line should be. I didn't know about this until later. It was a big battle that evidently occurred over me. So, I was being hired—women's studies had gotten a half line from their dean, and they were looking for another department to put me in. And so, they'd get a half line from some other department. That's the way women's studies hired in those days.

And they very much wanted me, and I'd been—there were two battles over me before I got there that I didn't know about. There were two women in physics and engineering who were very critical of feminist work. They both were active feminists in terms of supporting women in their fields, but the kind of stuff that Sharon Traweek was doing and I was doing was quite another matter. And they were both involved in women's studies. They raised a stink.

And then there was—I forget exactly how this goes, but what I know is there was one famous meeting evidently of the women's studies program and the dean and provost, and the dean and provost were told to get—the [eight full professors from UCLA departments] told them, "Appoint her," right? And they had some conversations ahead of time with these guys.

And so, by the time—so meanwhile, I was being [offered to other departments]—I didn't want [my other half-time line] to go into philosophy. History wasn't really appropriate, and history wouldn't want me there. I mean, I couldn't teach the basic history courses. But the graduate school of education—so it's a graduate school, right? They were not training teachers—had been recruiting me, because they had a [cross-listed] philosophy of education line open for four years, because they could never get a candidate that satisfied both the philosophy department and the education school. So, they finally pulled the line out of philosophy, had it solely in education. **<T: 125 min>**

And I'm not a philosopher of education, so that wouldn't have been a good fit for me. But what happened was they hired Doug Kellner. Do you know him?

KLETT: I know him by name.

HARDING: Okay. He's politically great. I mean, he writes on all these—he's very left and writes all this media stuff.[53]

KLETT: Right.

[53] Douglas Kellner. *Media Culture: Cultural Studies, Identity and Politics Between the Modern and the Post-Modern.* Oxford: Taylor and Francis, 1995.

HARDING: They hired him for the philosophy, the main philosophy of ed—it's an endowed chair he holds. But there was a second position attached to it, so they hired me on it. But what they hired me to teach is exactly what I want to teach. I never have taught philosophy of education. I don't know anything about education. And so, I—you know, they wanted me to teach standpoint theory, the methodology courses, and feminist theory.

So, first of all, I had to consider whether I wanted tenure. I was tenured at Delaware, of course, and I was a full professor, but if I—I was commuting for five years between Delaware and UCLA, and poor me, I couldn't be on any major committee on either campus, because I was only there half the year.

ROBERTS: Aw.

HARDING: It gave me all this extra writing time. And I thought, if I get tenured, I'm going to have to go on those big university killer committees, which I hate. I've never—I've managed to escape all the big worst ones. I'm a good citizen in departments, but not—I mean, because I've had twenty years of administrative positions, so I have enough meetings without, you know, has been my argument.

Anyway, so I went into this very unusual school for me, for a philosopher to go into, to do—[since I retired five years ago] they have not replaced me in that school in terms of what I teach. I mean, they should be ashamed of themselves. But I think there aren't too many Sandra Hardings around who are willing to locate in a school of education. It's too low status a school for the arts and science people, [who are ignorant of how high-status schools such as my GSEIS should be regarded]. But I didn't care at all, because I had my own status, and was quite independent of whatever department I was in. And if I could teach what I wanted to teach, they all wanted me, and the students loved me, and so forth.

KLETT: [Yes].

HARDING: So, I've had to think carefully—[yes], I did consider not getting tenure, because they have a—what's it called? They have a visiting line that can be ten years long, renewable, auxiliary, not lecturer. What's the next one? Not assistant. This is the other kind of line at UCLA. [Adjunct Professor]. And there are people who teach on it for decades, and they're never tenured, but they're full professors with all the privilege of full professors. And I thought, that sounds good.

KLETT: [Yes].

HARDING: But then at the last minute, I thought, I don't know, I don't really know how that's going to work for me. Maybe I shouldn't do it. The people I know who do it don't do controversial work, and so it's easier to get rid of that line because it's not tenured. I thought, I shouldn't be stupid. But I was move—how old was I, sixty? When I was sixty, when I was moving to UCLA. I mean, I figured I'd better move from Delaware now, because I'm already almost too old to be hired at the—what it's going to cost them for a full professor, if I'm going to retire in five years. And I had been offered a position at [SUNY] Stonybrook in the philosophy department. Stonybrook was very attractive for a lot of reasons. But I decided that University of California was a much—UCLA was a much better choice for me, so I did it. So, I've had these kinds of career considerations a lot.

ROBERTS: I think we're at a good place, because we're—

[END OF AUDIO, FILE 3.1]

KLETT: So, what did the—maybe just to talk about the transition from Delaware to UCLA—

HARDING: Okay.

KLETT: —can you just discuss the sort of recruitment process? How did that idea of moving even come up? What did that transition look like?

HARDING: [redacted 00:00:23 to 00:01:09] UCLA had been recruiting me for several years, and so had Berkeley, because they both had—it wasn't just their women's studies program. There's a separate kind of program that the big universities like these two use. The [UCLA] Center for the Study of Women was not the women's studies program. It had an eighty thousand dollar a year budget. Yes. And it was a home for grants from faculty who hated their departments and ran their grants through the Center for the Study of Women.

And we put—and it had a bunch of other activities we put at the Center for the Study of Women, which it recruited me, and I took it, eventually. We put on seventy or eighty lectures and conferences a year, many joint with other departments, but probably thirty of them that we initiated. So, it was a big creator of networks on campus for people interested in women's issues from the med school and the law school. And the professional schools, the undergraduate schools, it was a huge operation. And I loved those huge operations.

And they wanted me because Delaware wasn't in that league in terms of the women's studies program, but it was a huge program. I mean, at the time I left, it had a hundred courses.

KLETT: Wow.

HARDING: And when I arrived at UCLA, they had a hundred courses. Those weren't all paid for out of the women's studies program. They were joint—they were ones we negotiated with or other people had negotiated with history and biology and what not. But it was a huge program. And we also ran a couple of big lecture series. And so, I'd been in that job, I don't know, eight or nine years, maybe more, ten years [at Delaware]. So, I knew how to do that, though UCLA had its own challenges.

So, I moved to UCLA. [. . .] Because if I was ever going to leave Delaware, I had to do it soon, or nobody would take me. I'd be too expensive. So, I moved. [redacted 00:03:29 to 00:06:12] **<T: 05 min>**

KLETT: So, when you're commuting, were you commuting to work at UCLA, or you're based here while you're working there?

HARDING: So, let's see? What happened? There's something like five years between when I was full time at Delaware and when I was full time at UCLA. [. . .] I had a sabbatical, and so I had spent it out here. And I got a visiting appointment at UCLA, spent it out here. [. . .]

So, it was a bunch of visiting appointments, and then, when I decided I wanted to move here, I think it was—I think it was two years I was employed here full time with full pay, without the kind of contract—I mean, I had tenure at two universities, and it was one big administrative mess, but it wasn't my mess. And I immediately notified the top personnel officer on both campuses. I said, "Look, this is what's happening. If you can figure it out, go for it, but it's not my fault, and I don't want to see my name on the front page of *The New York Times*, illegally holding tenure at two universities."

KLETT: Right.

HARDING: But there was some commuting in there, and I think I told you, I was debating whether I should actually be tenured, because I didn't want to have to deal with committees, but I finally decided it was stupid to put myself at risk that way. So, that's about why I moved here.

ROBERTS: So, is there—was there an intellectual element to the timing of the transition as well? I mean, I think we spoke this morning about the report with the UN.

HARDING: No, not really.

ROBERTS: There seems to be—

HARDING: Not really.

ROBERTS: No?

HARDING: No. I mean, in some ways, Delaware was more enjoyable intellectually. I mean, UCLA has been wonderful, but Delaware is half the size, very close to Washington, DC, and Baltimore, and New York, and I was—all those UN organizations are over there, right? And they're in New York and Washington. And it didn't—it wasn't a loss. It was a really different scene, to move into a graduate school of education from appointments in philosophy and sociology departments. I mean, [at Delaware] I had graduate students, but not a lot of my own. And I didn't mind that. I don't—you know, it's a lot of work with graduate students. And here, I had to figure out how to fit into this graduate school of education. They knew they wanted me, but why I wanted them was much more strategic than intellectual. I mean, it was a good setup, and they let me do whatever I wanted to do, and the school was [excellent in quality], and I didn't have to do much teaching, and—you know.

So, it was—I was thinking about it, but probably not too articulately. I mean, I enjoyed it when I got here. I, you know, made wonderful, interesting friends. But it was mostly [at first] because I was directing the Center for the Study of Women. So, I had sixty stellar stars coming through [every year] that I was having dinner and lunch with. I mean, all the restaurants in town knew me. They saw me coming, and they'd **<T: 10 min>** make everything good, because I was always bringing dinner tables of eight and twelve people who I would get [to have] dinner with, these national and international women's studies stars.

And I got to meet people in other departments that way, too, and had developed some wonderful intellectual friends. So, it's been very successful, but I didn't move for that, because at Delaware, I was in the middle of the larger Philadelphia intellectual community. I had friends at Temple [University], and—colleagues at Temple and University of Pennsylvania, in particular. I mean, we had dinner all the time, and were in various groups together, and so forth. And so, I had a very rich—and it was an hour from [Johns] Hopkins, and I lived in Baltimore sometimes, and—once my kids left home, I moved to Baltimore a couple of years, and then I moved to Philly a couple of years, and [often visited] down in Washington, and in New York. So, they were very—they're very different intellectual communities I was in.

The thing I missed most here, that I was conscious of missing, was [that] at Delaware, I'd had a very rewarding presence in the local Black intellectual community, and that wasn't the case here. I mean, I had acquaintances here who were Black and in the intellectual communities, but nothing like the East Coast Black intellectual community. They all take holidays out on Block Island or whatever it is. You go out there, and the entire community is Black intellectuals and professionals from the East Coast and everywhere. And the Delaware Black community, Philadelphia, Delaware, was a very self-conscious community, and it's a small enough area, and they'd been through hell together, and they were very happy to have people like me involved with them, and, you know, coming—wasn't doing anything organizational—[except that I did start up both Black feminist and African feminist lecture series at the Center], but I was present and appreciating them and going out to dinner with some of them, and that was harder to do here. So, I missed that.

KLETT: This was—so you'd mentioned that the period where you were overlapping Delaware and UCLA was really prolific, because you have a lot of free time.

HARDING: [Yes].

KLETT: Because you weren't doing a lot of service stuff on either campus.

HARDING: [Yes].

KLETT: When you transitioned to UCLA and were doing the sort of, you know, sixty scholars, taking to dinner did you find that you shifted more into an administrative role, or were you still able to keep up your—

HARDING: I found on both campuses that administration versus teaching has a very different annual chronology, if I can put it that way. So, at the beginning and end of each term, I would never travel when I had these administrative positions, and I'd always be there, because that's when everything happens, at the beginning and the end. You're scheduling this, and dealing with a dean about that, and what have—new students. Teaching—and then—but then in the middle of the semester, I'd have like two months of almost free time. I mean, stuff was going on. [Usually, I did all of my half-time classroom teaching in one term, leaving the other terms free of classroom teaching.] But I could travel while all this was going on here, because I always had an administrator who managed the office. I mean, I never—I got involved in establishing some of these programs, but never from scratch. I was lucky or strategic enough never to take a position where I'd have to kill myself trying to get the thing established. This was very well-

established already. It was secure. The administration didn't dare go anywhere near it [when they were looking for how to save money].

And so, in September, I'd have to be here, but October and November, I could be anywhere I wanted, as long as I was back by December, because then all kinds of [administrative fires] would flare, flame up, and I'd have to start getting ready for the beginning of the next term.

So, I did a lot of writing on that kind of schedule. The summers in both cases were pretty much free times for writing. And I wrote most of my books in these administrative—while I was holding these administrative positions at both Delaware and UCLA. [When doing full] time teaching, you can't get that kind of time, and you can't get the—I couldn't get the intellectual separation from a full-time teaching position to do it.

When I ended—when the journal *Signs* left—*Signs* was a little different, when I was editing it for five years. It's a twelve-month job. Those manuscripts arrive, four hundred a year. And we only accepted I think eight percent. So, my co-editor and I were doing a lot of reading of manuscripts, and negotiating with **<T: 15 min>** authors, and had a big staff. And it's a twelve month a year job. So, that was a little different. But we did have each other, and we forced each other to take in effect sabbaticals, quarters off, because we had enough staff that we could manage it. And we got the [number of] staff raised—it was a whole line higher when the journal left us than it had been when it arrived. And so—and UCLA does not permit people who take these editing jobs to have their research suffer, [so we could make sure there was sufficient staff to handle the flow of work without damaging our own individual research projects].

So, for almost twenty years, I only taught half time, [. . .] and was teaching two courses a year, which was peanuts, you know, after a while, so I could do a lot of writing on these administrative positions, even the *Signs* one.

And when *Signs* left in 2005—what is that, thirteen years ago? I have suffered ever since, because I adored the secretaries. I paid for their babies to go to nursery school. They adored me. They totally spoiled me. I didn't even know how to look up a faculty address online. When I was thrown into my own resources, it was hell. And right now you can hear my technological limitations. And that was created because I was so pampered by the staff. It was interesting.

KLETT: [Yes], it seems like it has—so you directed the Center for Study of Women, '96 to 2000, and then were the editor, co-editor of *Signs*, 2000, 2005. So, did you need something to replace it, or did you take over *Signs* because you were free all of a sudden? Like what was—

HARDING: There was a huge—it was a huge achievement to get it to move to UCLA. [It took a lot of work on my part. I had been invited to apply to take on the editorship at an earlier point, but had not wanted to do it then. So, they approached me again. I thought about it. And I

decided that if I could get a good co-editor, it would be fun. I knew Kate would make a fabulous co-editor, because she had been the Director of the Center before me and had reorganized it beautifully. So, we invited about twenty associate editors from UCLA, and about six from surrounding Los Angeles campuses so that we could make it a regional project. With their assistance, we wrote up our application to edit it, and won out against four competing schools. Recruiting *Signs*] was a huge benefit to UCLA, because a lot of faculty were involved with it in a very wonderful way. And Kate and I decided to shift the direction of the journal significantly. It was a grand old lady at that point. It was twenty-six years old or something like that, when we got it. So, you know, it was the first big interdisciplinary and international women's studies journal. I mean, you could find it on every coffee table in the engineering department in a [university in] Yugoslavia. I mean, they were trying to show they were feminists. They'd have *Signs* on the coffee table.

And we shifted it to an international focus from a fundamentally US focus, and that took a lot of recruitment of articles, recruitment of reviewers. I mean, you know, I'm in that again now in another context [with the founding of *Tapuya*]. But it—no, it was fun to do. It was a—stretched me in kind of good ways, and, I mean, I was—had administered pretty [big] things, but this—and, you know, done all kinds of editing, but not [as big and complex as] this.

And when you [only] publish in journals, you don't really understand how they work. I mean, you understand them from what you need to understand from your perspective, peer reviews, and they say no, and you say, what's happening, and this is—you know, whatever. But there's a lot of infrastructure that has to be maintained and a lot more when you're founding a journal. It's driven me crazy.

KLETT: [Yes].

ROBERTS: I'd like to come back to the strong objectivity generation.

HARDING: Okay.

ROBERTS: And think about the—I just don't want to lose track of it in terms of the chronology.

HARDING: Okay.

ROBERTS: And I think we've touched on a few little pieces here and there, its relationship to the strong program, its relationship to the science wars, but if we could just maybe do a short—I'm not prescribing that, but just—

HARDING: The history of something—

ROBERTS: —but, you know, kind of a—you know, a bit of an intellectual history of the emergence of strong objectivity for you, and what was driving that.

HARDING: [Yes].

ROBERTS: What you wanted it—you're so intentional. You've described yourself as being very intentional in terms of making these strategic choices, you know.

HARDING: [Yes].

ROBERTS: Where does strong objectivity fit into that for you?

HARDING: Okay. So, I was trying to look up, and I can't quite—I'd have to **<T: 20 min>** do more research in my own history to come up with that. But in founding standpoint methodology, standpoint epistemology, the issue was objectivity, from the beginning. And we were—in biology, Ruth Hubbard was saying biology is not objective in the way it understands women's bodies, and in sociology Patricia Hill Collins [was making a parallel argument about how sociology treats Black women's lives]. And, I mean, we were always saying that they think they're objective, but they're actually deep—their thinking is deeply—their very categories, their episteme, their whole way of thinking about the issue, is deeply shaped by the fact that it's they who are doing it. It's their [elite male] lives [that their research reflects, and they take that to be objective].

Dorothy Smith provided some really useful sound bites on this. She talked about the conceptual practices of power, and she talked about how sociology works up the messy, everyday chaos of life into the kinds of categories and the relationship between them that permits ruling, that permits the education department or the welfare department or the World Bank to manage things. Because sociology, she's saying—well, there were other disciplines involved, but she's a sociologist—has worked up these categories and causal relations between them in ways that make [the] management [of groups of people] visible, how to manage them visible. Because otherwise, how would you know what to do when you want to stop a rebellion from happening in this education department, or whatever? I mean, you could kill them, in the old mode, but in the modern society, she argues, rule [is importantly conceptual]. It's their conceptual organization and practices that rule [and, thus, that have to be changed].

So, she was writing this in 1972. I mean, I didn't encounter her work until probably '70—no, wait a minute. Am I a decade off? Eighty-two, maybe, '89? I think '82. When's Patricia Hill Collins? Is she '89? I'll get a bibliography. I think that's right. Eighty-nine. [Yes]. So, [Dorothy] was already—had made a presentation at the Pacific Coast American Sociological Association, and I think it's '82. Excuse me. I've got the essays right here, so I'm just going to look at them, because I put them in this one volume on purpose, so that everybody could figure out what was happening, and now I need to figure out what was happening, because I can't remember what's in here. And I think I've got them in chronological order.

Women's Perspective as a Radical Critique of Sociology.[54] Where's the acknowledgements? Okay, let's see. IX. Dorothy Smith, 1974. Okay. So, it is in *Sociological Inquiry*, in 1974. So, I think she gave that paper in '72, I seem to remember. But the rest of us didn't get to it until '79. So, let's take Hilary Rose. Nineteen eighty-three. Okay. Let's take Harding. When did my thing come out? I've got in here—Hartsock is '90—no, Hartsock is '83. Okay. So, there we go. Rose is '83, Hartsock is '83, Harding is—the one in here is '93, but that was into the strong—"Rethinking Standpoint Epistemology: What is 'Strong Objectivity'?"[55] I was rethinking—that's a later essay.

Collins is '97. So, wait a minute. I want the other one. Collins is '86. So, it gives you a sense of—the objectivity issues were there from the beginning, but I wanted to grab them and get a sound bite, get a little something that nobody could resist reading. Right? And so, I—I don't know, I fussed around, thinking about it, and finally decided strong objectivity would be the strategically [useful sound bite]—who could resist that?

So, I pulled that out of all of our work, and it's—I'm embarrassed **<T: 25 min>** to say that these days, people usually cite only me. They often don't cite all these other people. And we were working together, not initially, but eventually, we all met each other and were writing to each other, and putting on the same panels at the ASA or the PSA or whatever, 4S. And so, it was really a team of us that were doing this, coming from different disciplines, which made some very big distinctions in how we formulated standpoint theory.

Dorothy Smith hated the language of feminist standpoint. She said, "That's high—you high theory, girls. I want women's standpoint. It's the standpoint of women in their everyday lives. How come they have a double day of work? How come the courts say they asked for it when they got raped?" And so forth and so on. She said, "Feminist standpoint is after you guys—" this wasn't exactly her language, but it's that idea—"got finished with working this up into some fancy philosophical thing, and you've taken the agency away from the women."

So, saying this just to say that there were very big distinctions. And Hilary Rose had come from the sociology of science. She and Steven Rose had written—they were Marxists, and they were both Marxist organizers in London. Do you know them at all?

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[54] Dorothy E. Smith. "Women's Perspective as a Radical Critique of Sociology." *Sociological Inquiry* 44, no. 1 (1974): 7-13.

[55] Sandra Harding. "Rethinking standpoint epistemology: What is 'strong objectivity'?" *The Centennial Review* 36, no. 3 (1992): 437-470.

KLETT: [No].

HARDING: No? You can look them up. They're very interesting. He's a biologist, and—where had he—he was at London School of Economics or something like that, and she was somewhere north, I forget. But she had been—was partnered with a scientist, and been working in the sociology of science, so she brought a somewhat different perspective. And of course, Patricia Hill Collins had started off know—because she was a sociologist, she knew Dorothy Smith's work, but she was speaking from a Black US perspective, but she was very much not a student of Smith's, but in Smith's way of—line of thinking, as opposed to mine or Hilary Rose's or Nancy Hartsock and political theory. I was probably closer to political theory with Nancy, because philosophy—the courses in political science and in philosophy that are called political [philosophy]—are fundamentally the same course. I mean, they had—they're taught differently, but they start with Plato, and with Hobbes, and, you know, you work your way up to John Rawls, and it's the same syllabus in both courses. And I didn't take those courses, but I knew what they looked like. So, let me see, your question—what was your question again? Sorry. I lose track of—

ROBERTS: No, no, just kind of coming out and thinking about what was the trajectory of strong objectivity —

HARDING: Yes. But I've had—we'll get to this at some point. I'm having—let me put it that way—I've had it for a while, some—I've got to get back in there and rethink some of these things, because the early ways we put standpoint theory, all of us, and certainly the way I put strong objectivity, was not about a pluriverse. It was about one true story, and we were going to tell it. And I don't subscribe to that anymore. And so, I have to—I've been kind of nudging the theory around the edges in that direction, but I haven't directly confronted the fact that this is a big change that has to be recognized in this theory There's no one right story of the world, and I don't care what you think, you physicists. Yes, there's a world out there. There's lots of worlds out there. And your world isn't the only one.

Indigenous people have a very different world out there, as its categories and ways of being organized are just different. And there can't be any one true way. Different ways are useful for different kinds of projects. And if you want to get to the moon, it's best not to use indigenous understandings of the world, or at least at present, it's best. But if you want to improve environmental issues, it would be much better if you used indigenous understandings of the world than NSFs.

So, I've been, you know, talking that talk for probably a decade, without directly confronting the whole large issue, **<T: 30 min>** that standpoint theory tends still to be understood by—for example, in classrooms, as wow, now we've got the one true story. And that

is the way I was thinking of it, I know, in the beginning. I mean, Marxism thought it had the one true story. Modernity—the enlightenment thinks it has the one true story. And I—there I was, a child of the enlightenment and Marxism, and—not my fault.

KLETT: Did you get any pushback from your colleagues, the authors you named, around the language of strong and weak? I know that was meant to be a provocation.

HARDING: No. No, I didn't. No.

KLETT: It wasn't seen as really aggressive or belligerent to anyone?

HARDING: They didn't mind aggression. And I didn't. I mean, what did we think we were doing? You're taking on objectivity? We're not being aggressive? I mean, that—you know, the critics' language, they just couldn't believe that girls would have the nerve to think that they could redefine objectivity. It just shows you what's wrong with girls. [laughter] You know, we—we were so used to that in each of our disciplines that—I didn't get any response to it. [Or, perhaps, I should in retrospect say that I just expected and ignored negative responses to it. I chose not to spend time responding to such responses, because it just gave them more attention.]

There's a certain point when all of us stopped writing on it except me. I can't think when—I mean, that's probably not the right way to put it. I mean, Nancy Hartsock died some years ago. [And she was a major formulator of it.]

ROBERTS: So, it's interesting. So, I think a lot of the coauthors and also just colleagues, some of your fellow travelers during this period, you've mentioned several times that they—they had these reform agendas for their disciplines.

HARDING: Yes.

ROBERTS: Yours doesn't seem to have been focused on philosophy whatsoever.

HARDING: No. That's right.

ROBERTS: You were using those tools, but not necessarily to transform philosophy.

HARDING: No. Even though they and I had the same end goal—

ROBERTS: Which was what?

HARDING: To serve the social justice movement. But they—they did it through focusing on the conceptual practices of power in their particular disciplines, that the way political science characterizes women gives—I mean, that kind of argument I gave—replicating Dorothy Smith's [analysis], that those disciplines are so powerful that we have to change the way they conceptualize everyday reality.

And I was doing that, too, but I didn't care about philosophy of science. I mean, I've only gotten back into philosophy recently. I'm not in it, but I've only—I mean, this last book is the first book in decades where I've even probably mentioned the word, or—clearly focused it on philosophy.

ROBERTS: But you were still attending annual meetings. Is that right?

HARDING: Of the APA?

ROBERTS: Of the APA, of—

HARDING: I—well—

ROBERTS: —Philosophy of Science Association—

HARDING: No, I haven't gone to the Philosophy of Science Association for thirty years. They were so rude to me one year. It was so bad. I mean, I'm used to bad. This was so bad that I thought, I don't need to do this to myself. This is terrible. I mean, you—

ROBERTS: When was this?

HARDING: Can I remember? It must have been in the eighties, or nineties, maybe, maybe late eighties.

ROBERTS: And what happened?

HARDING: So, we—there was an invited panel, me, Helen Longino, and Alison Wylie. Is that right? I think that's right. And we each gave our papers. And so, Philosophy of Science Association was probably 85 percent men. It was at Pittsburgh or somewhere, huge room.

And afterwards, first Noretta Koertge—you know her? She's an eminent—she's an ex-student of Popper's, and an eminent philosopher of science, and counts herself as a feminist. And in terms of doing things for women in the discipline, she does it. [redacted 00:34:43 to 00:34:54] I forget, there were skylights or something. She's a redhead. And this kind of golden **<T: 35 min>** light—she's sitting in the second row or something—came down and lit up her hair. And this red golden-haired woman stood up, and she just launched a whole diatribe against me in particular. Alison and Helen she didn't attack so much.

And then the guys started. I was the focus of it. Alison and [Helen] weren't, and I was probably talking about strong objectivity. I can't remember. But even my dear friend Joe Rouse—do you know Joe Rouse?

ROBERTS: [No].

HARDING: I mean, he and I were buddies. [. . .] He took me aside later and he said, "You know, Sandra, that was really a bit too much, what you were doing." I thought, "Whoa."

KLETT: So, it wasn't a reaction to you. It was a reaction to what you had presented?

HARDING: [Yes], of course. I mean, it always was a reaction to what I'm presenting.

KLETT: Well, sometimes. Sometimes that's just an excuse to raise your hand so you can dig into someone who you've got a bone to pick with.

HARDING: I never thought there were any personal bones that people were picking with me, except that for philosophers, what you think is a personal bone—I never—I mean, and people were—you know, some guys were—and women were criticizing feminism, and I'm saying they were largely attacking STS. But I never—I've always been very kindly and sweet, and go out to tea with whoever's criticizing me, and try to quiet everybody down, and have my own assessments I'm not revealing about the stupidity they—you know, they're exhibiting. And, you know, I mean, I'm pretty strategic in these ways, and I don't feel guilty about doing that. So, I don't mount attacks on other people, either.

ROBERTS: And so, that was the end of your time at PSA?

HARDING: That's the end of my time at PSA.

ROBERTS: So, did—

HARDING: Because at that point, I mean, I can't—I wish I could remember when I started going to 4S, but let me add to it that—so APAs I go to if they invite me on the program. If I know it's going to be in LA, sometimes I propose a panel and it gets accepted. But otherwise, I don't go to it, and I haven't—when I haven't been on the program, I can't think—but that's true for—there are no conferences I go to if I'm not on the program. I can't think when I've gone to one when I wasn't—I mean, if I know it's coming and want to go, I get on the program, because it's more fun that way.

ROBERTS: So, do you think that the—some of the things that were found challenging at PSA opened new opportunities for conversation with—and we don't have to use 4S synonymous with science studies. I mean, I think I want to keep that space open for you.

HARDING: That's interesting. Well, that's interesting.

ROBERTS: To think, you know, was there a larger intellectual community, a different intellectual community, you find yourself in conversation with because of this kind of movement from logic and epistemology into the epistemology as it related to science, which now is becoming a more centralized focus of your work. And you did reference earlier just it—at least a linguistic relationship to the strong program, but was there also—were there other intellectual collaborations that were opening up, or was it still—

HARDING: Well, [when I first arrived at UCLA] I was talking to Peg Jacob a lot. She was at this point —I'd met [her intellectually]—you know Peg Jacob, Margaret Jacob—on the cultural meanings of the scientific revolution.[56] She's a historian of the scientific revolution fundamentally, and has written quite a few books on it. And she had initially been at—in New York City, down on 14th Street, the New School. And then she moved to—when I knew her, she was [in Philadelphia], at Penn, maybe. [. . .] **<T: 40 min>**

[56] Margaret Jacob. *The Cultural Meanings of the Scientific Revolution.* New York: Knopf, 1988.

I haven't seen her in some time, because my work got too crazy for her too. Unable to tolerate. [She, Joyce Appleby and Lynn Hunt] wrote a [book] together on truth or something like that at a point when I was not a fan of truth.[57] [laughter]

But I was reading historians, and I was reading Bloor and Barnes, and I was—you know, with—I can't think what took me to England a couple of times for a couple of weeks. I got invited to something or other. So, I got to meet a lot of those Brits. And I remember that is where I met Bloor, there. I don't—I think I've met Barnes way back then, too, but he stopped being so central in science studies, where Bloor continued a little more. I don't know. And I met, you know, Bob Young, and Les Levidow, and some of the other Brits.

And so, I was kind of developing my own—I didn't think of it that way, but, I mean, I had made a point of going to their talks, and if they were coming into my area, wherever it was, I'd go and introduce myself to them. And I got to know Trevor Pinch.[58] Well, that's—this is later. [Yes], so I was—that's interesting. I was actually developing I think—it made me think—get a better description for what I was doing. I was developing my own science studies friendship network.

ROBERTS: [Yes].

HARDING: I had almost no one at Delaware. The program that Delaware had that was great, but it didn't—I kind of interacted with it, but it was never what I was doing. They had a fabulous history of technology program, because they had the whole DuPont set up. What was that? They had museum studies, and they had something about the history of technology and materials, something or other. I forget what it was. They had a bunch of these programs that drew off the DuPont archives. And—archives and museums and what not, Winterthur [museum, garden and library], and—

ROBERTS: [Yes], the collaboration with the Hagley?

HARDING: [Yes], the Hagley. Yes. [. . .] In Philadelphia, there weren't that many science studies people. [No].

ROBERTS: So, we were kind of joking that especially on the UK side in the strong program and its relationship that so much of that work is grounded philosophically in Wittgenstein.

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[57] Joyce Appleby, Margaret Jacob, and Lynn Hunt. *Telling the Truth About History*. New York: W.W. Norton, 1994.

[58] Trevor Pinch, interview by Joseph Klett and Jody Roberts at Cornell University, Ithaca, New York, 14 and 15 November 2018 (Philadelphia: Science History Institute, Oral History Transcript # 1092).

HARDING: Oh, that's interesting. It was.

ROBERTS: And whether or not your many years of trying to steer clear of Wittgenstein, how that—what that experience, that encounter was like especially for the early work by Bloor and Barnes and others—

HARDING: I ignored it. I ignored it. I would see it, and the text—my eyes would glaze over, and I'd go on to the next paragraph. [laughter] And I knew what they were drawing on, and I—okay, go for it, but I wasn't going to get myself into it.

ROBERTS: [Yes].

KLETT: It wasn't a disqualifier, though, for Bloor to—

HARDING: No, I don't—I'm not negative about the importance of Wittgenstein.

ROBERTS: No.

HARDING: I just had my own very personal reasons for—

KLETT: [Yes].

HARDING: —I've done Wittgenstein. It's—I'm out of that.

KLETT: [Yes].

HARDING: I mean, for several—as I indicated, for several reasons, I wanted to stay out of [my former husband] Hal's way, and—

KLETT: [Yes].

HARDING: Then I got really pissed. He came over into my area. He wrote a—did he write a paper on Quine? I think he wrote a paper on Quine. I was enraged. We had been separated for fifteen years by this time, or whatever. I'm like,"Yyou have your [philosophic field and I have mine," but] he got excited by it and started to get into it. I'm not clear—I'm not sure he got into it through my work. Maybe he did, maybe not. Quine was a big figure at that point.

KLETT: It's interesting to hear you talk about philosophers, because, as a sociologist in social theory, particularly through mentorship, particularly through mentors who are known for defining a field, it is very much like you need to pick your team.

HARDING: I know.

KLETT: And so, you find—for example, you find out so and so is a Bourdieusian, and it—the youth are okay with that.

HARDING: [Yes].

KLETT: Like these are just names with ideas. But when you reach a certain level, it's like to identify with a thinker is to assume their baggage as well, and their enemies.

HARDING: [Yes].

KLETT: And all that. And it's—it's curious to hear you talk about it, too, because it's—

HARDING: I think that's more about me.

KLETT: It's a personal thing?

HARDING: [Yes], because, I mean, I wrote on Quine and I admired him, but I did—I was very critical of him. My dissertation is critical of him, because he—**<T: 45 min>** it's not his fault, but he couldn't in fact separate himself when push came to shove from positivism. And I didn't—I did a little of that in my dissertation, not a lot. But subsequently, I've, you know, almost never related to—you know, Quine gets a tiny footnote on page 274. I mean, I haven't been in the Quine circle if there is such a thing.

KLETT: I guess I'm just tickled that you—that philosophers are not immune to the players themselves.

HARDING: Are you kidding? [laughter]

ROBERTS: Well, but I think you're also highlighting that there's this—I'm not quite sure how to phrase it yet, and so maybe—I know if I should wait, but I think that there's this critical mass bubbling up of—there's a lot breaking down—

HARDING: Yes.

ROBERTS: —in and around epistemology. So, no matter which horse you picked—

HARDING: [Yes].

ROBERTS: —whether it was Quine, whether it was Wittgenstein, whether it was, you know, kind of thinking about what the consequences of the—and the future of the logical positivists were going to be, something was amiss, but everybody's kind of writing their own.

HARDING: [Yes].

ROBERTS: And at the same time, there's—I think just listening for the time that we've been together, that it's unclear in the philosophical circles whether or not they really want to have that debate.

HARDING: That's right.

ROBERTS: And it's also unclear on the science studies side whether or not they really want the philosophers hang out with them. And that they—

HARDING: That's always been the case. I was—

ROBERTS: And the philosopher such as a Bloor has to basically repaint himself as a sociologist—

HARDING: Oh, that's interesting.

ROBERTS: —to find himself welcomed into there. And I don't know if he would accept that characterization, but I think, you know, you look at his—he's a philosopher by training.

HARDING: I'm the first—

ROBERTS: But he is painted by philosophers as a sociologist and social theorist—

HARDING: I always think of him as a sociologist, and I actually know he's a philosopher. I'm the first philosopher who got the Bernal Prize.

ROBERTS: Right.

HARDING: I mean, because 4S was defined against philosophy of science.

ROBERTS: Right.

HARDING: And I rarely meet philosophers there.

ROBERTS: Right.

HARDING: And so, [when Paul Edwards telephoned me and told me I was going to be awarded the Bernal Prize at 4S in 2013], I asked Trevor and Paul and people to check back, because I looked at the list of Bernals, and there were a few names there I didn't recognize from past years. I said, "Are any of those philosophers?" They said, "We have no idea. We'll check around." And when they came back, they said, "We can't—we can't find any who are." [I didn't know what to make of the fact that I would be the first philosopher to receive a Bernal Prize when STS had at its origins defined itself against the prevailing philosophy of science].

ROBERTS: And yet I think—I think what you've shown is that the philosophers had a lot to do with troubling the foundation that allowed some of this to happen, right? By—by—things were—things were put on rocky standing because of opening up questions about the privileging of knowledge.

HARDING: So, who are you thinking of that I'm talking about who is—who are these philosophers?

ROBERTS: Well, I think just the discussion in and around like how we reinterpret—how we might now think about the place of Quine in this lineage, reinterpretations of Kuhn—

HARDING: Oh.

ROBERTS: —thinking that he's really just coming in looking for historical case studies, and looking for a better pedagogical set of tools, but accidentally opens the door to, well, if we're not making decisions about the failure of theory based on actual evidence, we're doing it for community reasons—

HARDING: [Yes].

ROBERTS: —that's a social reason. You have just—you've just opened up the door for others to come in.

HARDING: But I don't see philosophers doing that.

ROBERTS: [Yes]. No, what I—

HARDING: I think it is historians and sociologists that are doing that, with Kuhn and with this whole—

ROBERTS: But I think philosopher—I guess what I'm—and maybe I'm reading too much into your experiences—the kinds of questions you were asking about what Quine was up to—

HARDING: [Yes].

ROBERTS: —showed the disruption that the sociologists and the others kind of flooded into and said—

HARDING: [Yes], that's true.

ROBERTS: —thank you for opening the door for us. Like you—

HARDING: [Yes], but they knew they were doing that.

ROBERTS: [Yes].

HARDING: I mean, you guys knew what sociology was doing. I mean, 4S was really clear about its founding.

ROBERTS: [Yes].

HARDING: Those early people were against the standard philosophy of science. And Kuhn and—got his name up this morning, *Problems of Science* and its—[59]

ROBERTS: Ravetz?

HARDING: Jerome Ravetz, and certainly Feyerabend. I recently had occasion [to do a project on Feyerabend]—unfortunately, it fell apart, but there it is—to go back and read some Feyerabend, who'd been very important for me initially, Feyerabend and Hanson, that Lakatos volume on Kuhn. Lak—Lakatos and Musgrave, maybe that collection of papers on Kuhn, was—I mean, that would—those were really important to me. They—so anyway, I think you compliment philosophy too much. I don't—I don't see it that way.

But I do notice an ornery **<T: 50 min>** streak in me that may—might not be entirely intellectual, and just plain ornery, that I refuse to be captured by any of these. You know, I'm

[59] Ravetz, *Scientific Knowledge*.

always somehow positioning myself—I'm friendly and try it learn from them, but I'm not your servant. Forget it. I know I have that tendency.

ROBERTS: First child.

HARDING: [Yes], first child. [laughter] Excuse me for a minute.

KLETT: [Yes].

HARDING: Are we ready for a glass of water, or no?

KLETT: Sure.

HARDING: Okay. [background noise]

HARDING: [Recently there emerged an] occasion that led me to spend a couple of weeks—I have three or four books of [Feyerabend's] that I bought, that I, you know, read forty years ago or whatever, and I [started to reread] them, because I'd—there's a collection of Feyerabend coming out—I can't think of the editors right now, but if you want to know, I can give them to you. And they invited me to contribute to their volume. And this is an attempt to—they think Feyerabend's been unjustly neglected historically, and they're trying to resuscitate him for today. And I didn't want to do—you know, I've mentioned—I don't know why they got to me, but they did.

And I thought about it, and then I thought, great, but I'm going to do what I want to do. And I didn't want to do a traditional philosophy essay about Feyerabend. So, I—and I had been thinking about—so this is about working with *Tapuya*, and I'd been thinking about some of the challenges that I faced. [Challenges that] I from the North faced, in working with them, because—in working with Latin American STS, because it has all kinds of [intellectual, political, and material] conditions that are unexpected—[at least to a non-Latin American like me]. And I educate myself, I try to be a good ethnographer the minute I get involved in these situations, and try to figure out how they're thinking about things.

But—so I thought, well, I'll tell—there's a particular story—so there's a—so I thought I'd talk about book reviews. I'd talk about something very concrete, and talk about how the very different realities of Latin America made huge problems for [getting book reviews] for *Tapuya*, and how I tended initially in each case to conceptualize it as a problem with Latin Americans,

and then quickly realized that that was a very colonial position to take. The reason they had those problems is because of the residues of colonialism. So, that was the theme of the paper.

And how did I connect this with Feyerabend? Well, I argued that I was doing what Feyerabend had done. He'd gone back and told a very detailed history of certain moments in the history of physics to make his point, and I was going into the nitty-gritty everyday stuff of book reviews. It is my job to organize these for *Tapuya*. It was one headache after another. [redacted 00:53:02 to 01:02:56] **<T: 55 min> <T: 60 min>**

I mean, I'm in a Latin-American studies reading group here that's been going for, I don't know, three or four, five years, maybe, and we're precisely reading all this pluriverse literature now, and it's really incredible. And three of the—I call them Latin American studies, because that's how it started. Three of them work in Latin America, and one of them is Honduran, and is—she's here in a Los Angeles university, but that's her history.

And then we added one of the founders of Sinophone [studies—Chinese imperialism—to our reading group].

KLETT: Okay.

HARDING: A Chinese postcolonialism—China's colonial practices up through the present day, even. And [a scholar] who's originally Italian studies, Dante scholar, but works on European colonialism in ways I've never—she knows the details—I mean, there was a moment, I forget, 1700s, maybe, I forget, which she revealed to us, none of us knew it, when England almost became a colony of Spain. There was a marriage planned between a British princess and a Spanish prince, and at the last minute, it got called off. But if it had gone through, England would have been a colony of Spain. Isn't that something? Who knew?

So, we have a very good time, and we're reading all this pluriverse literature now. And I just ordered a new book that I can't wait to read that's exactly on the issue I was talking about. It's called *Decolonizing Solidarity*.[60] And it's by—it's about a—I think Anglo-Australian, who's very involved in the Australian indigenous movement, and is very critical of how white Northerners tend to get involved in these literatures and these projects. But that's exactly the issue. **<T: 65 min>**

KLETT: Hmm. Great cover.

[60] Clare Land. *Decolonizing Solidarity: Dilemmas and Directions for Supporters of Indigenous Struggles*. London: Zed Books, 2015.

HARDING: So, this is just so far away from even—from philosophy of science. It's just—I mean, standard US philosophy of science. It's just out in some other stratosphere than they're in. But that's their problem, not mine.

KLETT: Sorry, I was out of the room. What was the prompt to this story?

HARDING: It was—there was no prompt. We were talking about that history of philosophy of science and responded to Kuhn, and Feyerabend was in there, and I wanted to mention that he was the only one of that group that was very clearly positioned—positioning himself in light of the social movements of the sixties. And he taught at Berkeley, and he was married to a Latin American.

ROBERTS: [Yes].

KLETT: [. . .] We can pretty much go anywhere we want. I was wondering about the Society for Women in Philosophy.

HARDING: [Yes].

KLETT: And how that—how—what we—what your involvement with them was like, and, you know, what you took from them and where you diverged.

HARDING: [Yes]. I don't think I diverged at all. Within philosophy, there are these two groups, the Committee on the Status of Women, which is an official part of the APA, and which SWIP insisted on. SWIP got it in place.

KLETT: [Yes].

HARDING: And which has—does surveys of colleges, and is a site for complaints about bad practices, and, I mean, it's—it's an official body, and people get elected to it, and they're appointed by the APA. But SWIP was the grassroots organization, and they exist in every [regional philosophy group] that we ourselves put together. [This started] back in—let me see, the first SWIP—my friend Ann Garry here formed the first Pacific SWIP, and that was the year I was in Canada, writing my dissertation, so that would have been—oh, I'm so sorry, I keep forgetting when I got my dissertation. Seventy-three, maybe, '72, '3. But I think she formed it in '71. I was on the East Coast at that point. And it was '70—when I got back from my dissertation, it was already founded.

And so, we—we had meetings at the [divisional] APAs. As you may or may not know, the APA doesn't meet as a whole. It has a divisional structure.

KLETT: Okay.

HARDING: The East Coast, the Midwest, and the Pacific Coast. The East Coast in effect functions as the national, because they—you know, the—it occurs at the right time of the year for the job interviews, so it always [is scheduled]—it used to be between the Christmas and New Year's, and I think they've moved into—a week into early January, and the other two are like at the end of March and the end of April. [. . .]

The East Coast one is just humongous, and is huge, and job applicants, and everything. And so, we formed in [these three] divisions, and [our meetings were] partly consciousness raising and networking, and partly developing agendas. So, for example, we trained the young—we brought in our graduate students, and we trained ourselves and the graduate students. We'd hold practice sessions of various sorts, to sit in the front row at a lecture, at a presentation, and always ask a question. [And how to do a job interview.]

So, women started showing up in the front row of all these, asking significant questions. We trained—we practiced getting a question and not a diatribe going on, or a thesis going on forever. We held practice job interviews, and helped people deal with unusual—you know, difficult circumstances. I can remember being interviewed by—I forget what university it was, but it was like twelve guys, and the interview was in a bedroom. And I was sitting on the bed with [twelve guys standing around]—

KLETT: Jeez.

HARDING: —whatever kind of skirt. It wasn't a mini-skirt, but—**<T: 70 min>** and they're standing, and chairs around. It was pathetic. That's just a pathetic—

KLETT: [Yes].

HARDING: It's like a caricature. You can't imagine. It's like a caricature of patriarchy. I mean, and everybody there was just all [into patriarchal projects]—you know. So, we got principles put in at the APA. All interviews [were to] occur at tables [in the hiring center]. They should not occur with the interviewee sitting on a double bed in the middle of the room with a bunch of guys standing around. Oh dear . . .

And so, we did all these—I mean, they're very funny in retrospect, but we did all of these kinds of training ourselves to be more powerful presences in philosophy, and to make demands on philosophy in a lot of ways. Let me see. We got the journals doing—the journals were not doing blind reviewing in philosophy. And we got them doing it. We insisted on it. It turned out they never—still don't do it in physics. You know why? Can you guess why they don't do blind reviewing? Because everybody knows who the—where they are, anyway. The minute they describe what their research project is, with some multibillion-dollar nuclear reactor or something, everybody knows—

ROBERTS: There's only one guy with that.

HARDING: Well, there's a hundred and fifty of them, according to the—

KLETT: Right. [Yes].

HARDING: — [author line] of the paper, but [that was not the situation in philosophy]. So, we did that kind of research, and we—we got—we got the APA—we got APA policies changed about hiring, about publishing, appropriate behavior on platforms or whatever else came up. And we got the Commission on the Status of Women established to be an official APA—APA committee, and with a site for complaints and so forth.

And we networked. So, we met at the APAs, but we also met twice a year at people's houses, or campuses. It was so fun. I mean, we—it would be Friday night through Sunday morning. So, I can remember driving—I met wonderful people, because we'd drive on the East Coast—I don't know—excuse me—from Delaware up to Massachusetts or somewhere. So, we'd have the long drives together. And then we'd arrive, and 30 feminist philosophers would be making spaghetti in the kitchen. We'd all be eating. We'd be sleeping in people's houses, and have sessions sitting around the living room, and spend, you know, a day or two, day and two evenings, or something like that.

So, it was incredibly bonding. And I organized the East Coast one initially, and kept organizing it. And I went to those SWIP meetings, every one of them, for probably ten years, something like that. Let me see. I know when I stopped. It would have been '85, because then I became director of women's studies at [the University of Delaware], and had so many meetings I just couldn't go to all of them. But the SWIP ones were so important, and they created just wonderful networks of us knowing each other and helping each other out and mentoring our—we'd bring our graduate students to these.

And we had men—men were—the Midwest one, central one, didn't permit men. They were very radical. We did—but on the East Coast and West Coast, we welcomed men, and we did have a few men who came to these meetings. [These SWIP groups] still meet, and I get

the—we have a Southern California SWIP that meets once or twice a year. Now it meets [in people's living rooms], and Ann Garry and I co-organize it in different ways.

And it has a somewhat different format. We turned it into an evening—kind of afternoon, evening salon, with two brief presentations by people from Southern California. But we can get up to forty people. I can't have it—I mean, this living room is about the size of my last living room, and, I mean, I could handle fifteen people here, but I can't get beyond that. So—but Ann Garry [has enough room and Kayley Vernallis] lives out near—next to Caltech [California Institute of Technology], the next town over, and she has a big place. And so, they're great.

KLETT: Do you see your earlier organizing efforts with SWIP being regenerated in younger philosophers?

HARDING: It—all those groups still run.

KLETT: But are they **<T: 75 min>** being run by thirty-year-old philosophers?

HARDING: [Yes]. It's a continual influx of them. I haven't been to them, apart from our—the way we do our one here [in Los Angeles], and various structures of them have come into existence, and gone out of existence, so there's been a Boston one and a New York City one, and I think there was an Austin, Texas one, that lasted—the Texas one lasted for a while. The New York City one has lasted forever. And, you know, there's a big population of people around there. And I don't know what formats they're using these days. But in general, it's just a great way to network women graduate students, and these days, men want the feminist input, and they want to identify that way. I mean, not a lot of them, but we get some brave ones.

KLETT: Are other organizations—I'm thinking here like [. . .] 4S. Did you go regularly to 4S after that? Or—

HARDING: [Yes].

KLETT: Okay.

HARDING: Not always—I mean, when they went to Tokyo, I didn't—you know, the foreign ones are—I've gotten very sick of travel. But I have gone to some foreign ones. I went to European ones. Have I been to ones anywhere else? I don't think so. Leandro is hosting 2022.

KLETT: Wow.

HARDING: In Puebla. I have told him innumerable times he's crazy and out of his mind. He just smiles and does it. And it was in the last 4S newsletters, that 2022 will be in Puebla.

KLETT: It's official.

HARDING: I mean, Puebla is not that big. It's a beautiful place to go. The [. . .] university is gorgeous, and it's all wonderful. And he's a terrific organizer. But it's my duty to tell him how crazy he is to host that. But he knows what to do. He's going to get 4S to provide all kinds of assistance. He's [done research projects with Wes Shrum]. Do you know Wes Shrum?

KLETT: I do know Wes.

HARDING: They're on research teams together. [. . .]

KLETT: [Yes]. Have you been involved with 4S more, like beyond an attendee or presenter, have you had any role in leadership?

HARDING: [No. Well, I did get the Bernal Prize. But] I've turned—I've been invited [to leadership positions] and turned [them] down. And I get all kinds of organizing and attractive stuff, but I just can't do it anymore. I figure people are lucky I'm alive. They [should] just appreciate that I'm alive. I'm not going to go at 84 organizing anything. Forget it. Unless it's something I want to do. I turned down—oh, yes, EASTS [East Asian Science Technology and Society], just invited me onto their top editorial board. I've been on the background board. And it's the other journal that is most similar to *Tapuya*. It's actually very different from *Tapuya* financially and in other ways, but it's similar [in that it is stretching 4S way out globally]. And we have joint projects together. *EASTS* and *Tapuya* has a project this year and a project next year that'll happen, and they're great.

And I turned it down, and I left it there for two days, in case I weakened, but by the end of two days, I felt—I have so much work going on, and it—I don't—I'm not going to go to meetings. I'm not—I don't review papers from journals, except under extraordinary circumstances. I just—because I'm [pre-]reviewing them [when they arrive, should they be sent out for peer review] for *Tapuya* all the time, and it's a lot of—*Tapuya* is a lot of work.

KLETT: [Yes].

HARDING: And that's where I want to put my energies. They're useful there. I have a—it's hard to find somebody like me [for that particular kind of *Tapuya* project]. I keep—I'm working on working my way out of these jobs for *Tapuya*, but it's not easy to find people who are familiar enough with Latin American STS to be involved with that, want to do it, and have the editing and copy editing skills, and organizing skills, that I have, [and have the time to do it], for—I mean, Leandro does it, and other people do it, too. I don't mean to take all the credit for it. But I can—I [am positioned to be able to] do kinds of infrastructural things that they can't do.

KLETT: Right.

HARDING: A, [the Latin Americans often] can't do [that kind of work], and B, sometimes they [don't have the time to do it, or they even won't do it]. They're too [polite and] shy. I can negotiate with Taylor & Francis in my tough girl mode, which they would never do. They're so polite and deferential, [probably because they have had manuscripts] rejected by Taylor & Francis journals, right? [As I have too, of course. But] they don't—**<T: 80 min>** they're thirty-eight to forty-one. I mean, they're not my generation. But [many scholars in] my generation there [in Latin America historically have had] very little to do with those journals. And they've done humongous work [in Latin America] to get STS courses, departments, journals, Spanish language journals, and so forth. They've done huge—huge institutional work in Latin America under extremely difficult conditions. I mean, not only the difficulty of doing these jobs, but in the political context there in Argentina and in Venezuela, I mean, it's—it's a whole other story there.

And so, there are things I can do and know how to do and enjoy doing. I mean, I enjoy working with Taylor & Francis, and we bring those guys into our Skypes, and we take them to dinner, and if they want to take—we let them take us to dinner once, and then they take us to dinner at 4S. And they love being part of it. I mean, they—it's—for them—for Taylor & Francis, it's a total plus that they got to do this, because this is an incredible wedge for them into Latin American journal publishing of any sort. [Plus, I think they like the youthful energy of the journal and all of the rest of the editors.]

So, every time, I try to—right now, I'm trying to work out indexing, getting index—you know indexing, right? Getting indexed by Latin American journals. [Taylor & Francis] did a terrific indexing job on all the main European [indexers]. And we had Justin and Paul, our editors at Taylor & Francis, into our Skype two weeks ago, I think it was, and the topic was indexing, because Leandro and I wanted to make sure that all our editorial team understood what the indexing was. Of course, they know it better than we would know it in some ways, because their careers are so much more dependent on getting indexed by these, by Scopus and

World of Science and all these big international [indexers]—but in other ways, they don't exactly know what goes into this.

And Paul and Justin prepared a wonderful list. I mean, there was, I don't know, 25 indexers that they had already indexed us. We only knew about three of them, because those were the—the big ones. But there turned out to be all these other little indexers. However, [their journals have] never been indexed by a Latin American indexer, and there are four of those, five of those.

And so, they—so we want *Tapuya* indexed by these Latin American [indexers]. They want an ongoing relationship with these [Latin American] indexers for any subsequent journals they may publish that they want index—indexed there. Okay. But one of those—one of those indexers, we had—we had to—they couldn't get into—we gave them a list, and they were—they have a whole indexing division and so forth, abstracting and indexing, or whatever. They couldn't get into the website. They couldn't get any contact. So, we put Luis on the job at our—one of our ed team.

And he explained what it was. It's at UNAM [National Autonomous University of Mexico], the biggest Mexico—humongous, high quality, Northern type—I mean, it's not Northern, but it's got all the resources of a Northern university, in Mexico City. It's located there. [Its index turned out to be only] for journals edited in Latin America. And it would better if we corresponded in Spanish, Luis said. [A Latin American indexer refusing to correspond in English!]

And so, I—so what I've been doing the last couple of days is to try to get Taylor & Francis—Taylor & Francis, I said that all to them. I sent Luis's email to them and said, "So, maybe you want to let Leandro and Luis make this contact in this case." And the letter [Taylor & Francis sent] back was completely non-comprehending about what the issue was, because they do have people who can speak Spanish and Portuguese at Taylor & Francis. They have 1,000 employees. Some of those people are going to—at least two or three are going to speak—but they were interested in getting the—the Taylor & Francis right to be indexed, and they're not—in case they have another edited in Latin America, journal. [They didn't get that the Latin American indexer had no desire to even talk to them, let alone index an English language journal published in the UK—regardless of its topic.]

KLETT: [Yes].

HARDING: So, I just wrote back and said, "Hey, I have a better plan. How about we do *Tapuya* first, and Luis and Leandro do it? And then if we can get that, you guys can <**T: 85 min**> expand it to include all the possibility of all your other journals." But that's not how they're thinking. And I'm afraid when [the Latin American indexers] see Taylor & Francis wants rights to get indexed—they have 2,500 journals. I mean, at Taylor & Francis, these guys get that we're—it would have to be a—anyway, they can't do that. I can do it.

KLETT: Right. That negotiation.

HARDING: [Yes].

KLETT: [Yes]. The—2013, you got the Bernal Prize from 4S.

HARDING: [Yes].

KLETT: What was your—what did you decide to present on when you received the award?

HARDING: You know what? That was the last year they didn't have presentations.

KLETT: Oh.

HARDING: The next year, they started presentations.

KLETT: Oh.

HARDING: And I can see why. I mean, I—I just brought my—the fact that it was me brought to—you know, have you been at those presentations?

KLETT: I have.

HARDING: This was 2013?

KLETT: Yes.

HARDING: It's like five minutes. You go up and get [the award, say thank you, and that's it]—in those days.

KLETT: [Yes].

HARDING: Paul Edwards said a bunch of nice things about me. Half the audience knew who I was, and the other half had—were clueless. And—

KLETT: Pretty good—that's a pretty good ratio.

HARDING: Pardon?

KLETT: That's pretty—50 percent, that's pretty good.

HARDING: I guess so. [For me in particular. But the other winners were widely known by everyone]. And I had—Paul told me, "You have five minutes to say something." So, I talked about who was being betrayed. Was it them, because I was a philosopher getting this, and 4S had been situated—founded in the first place against philosophy? So, were they being betrayed? Or was I being betrayed, because they didn't understand what I did anyway? And I did—I joked around about that for five minutes, and that was it. And then you sit down, and you go on to the rest of the program.

[Background noise]

HARDING: And that was the last time they didn't have a presentation. They'd never had presentations attached to that. And that was '13. So, we're '14, '15, '16, '17, '18, [and now] '19. So, seven years now they've had presentations.

KLETT: I'm trying to think. Fourteen—do you know where you were when you received it?

HARDING: San Diego.

KLETT: In San Diego. Okay. Great.

HARDING: It was '13.

KLETT: That makes sense. So, I actually hadn't been—I was at—I helped to make Trevor's—with Lucy—Lucy and I made the video for Trevor's, in lieu of him being able to be there, but actually, I haven't been—

HARDING: Is Trevor this year or last year?

KLETT: He was last year, 2018. [Yes].

HARDING: Last year. That's right. He was. Yes. Yes. I heard it.

KLETT: But actually, I haven't seen a presentation.

HARDING: Is he in any better health than he was?

KLETT: When we saw him, he was doing pretty well, but I think he's just trying to keep things manageable.

HARDING: Keeps things manageable. [Yes].

KLETT: [Yes].

HARDING: I love him. I'm so sad.

KLETT: [Yes], he seemed optimistic, but he's also that kind of person.

HARDING: Yes, he is.

KLETT: [Yes]. Other—any other professional groups that you would have regular affiliation with in the last—

HARDING: So, STS, APA. I don't go to the National Women Studies Association groups anymore, though I helped to organize one of them thirty years ago or whatever it was, on the

East Coast, and went regularly. They were fantastic. They were—they were huge and exciting and all kinds of crazy stuff going on. I took my kids to them a couple of times, and they loved it.

For many years—how long? Maybe fifteen years, I was very active in a group you've probably never heard of, the Society for Values in Higher Education. You know it?

KLETT: No.

HARDING: It started out as the Society for Religion in Education, and it was the alumni group for the Danforth Fellowships. So, you got a Danforth Fellowship and when you graduated from graduate school, you got invited to join. And it was a wonderful group. I was—I had had a Danforth Fellowship for Women. Danforth had cut out women from their normal fellowships for a ten-year period, because women didn't finish their degrees, they said. I can't exactly date these years when it didn't happen. Fifties, maybe. Something like that. Didn't finish in—at the same rate that men did in four years or five years, or whatever they were doing for a Danforth Fellowship. Ten years later, they came back and reevaluated, and discovered if you went to five or six years, they finished at a much faster rate than men. They **<T: 90 min>** had children or whatever, they had domestic duties [during the fellowship years].

So, Danforth instituted for ten years the Fellow—they were fellowships for returning women students. Actually, you had to have had a three-year gap in your education at some point, whether it was undergraduate or graduate. And I had had a—see me, like Jesus Christ, I was out in the wilderness for twelve years or whatever. So, I had one for graduate school.

And it was a wonderful group. I mean, they were from many different disciplines. It was a week-long—on a college campus. I took my kids every year. They had all kinds of activities for kids. And there would be, I don't know, two hundred people there, maybe three hundred. And we had—it had self-organized sessions, every morning, every afternoon, all through the week, parties, playing around, swimming, whatever. And I became at some point the director of it, I think two years before I stopped going. Okay, so that ended—that, again, 1985, when I took on that women's studies directorship, I cut out going to a lot of these other—because I was at meetings all—I just couldn't do it. And so, that was very important to me, and created wonderful networks. They had a lot of—they were very progressive around Black faculty, too. So, I got to meet and work with up close a number of distinguished Black intellectuals, which was a new experience for me, and just wonderful. And I would run into these guys and gals later, and it was really nice.

Other organizations. So, I've mentioned the Latin American studies group. I've always had these reading groups of one sort or another. And I get invited—I don't have regular relations with or memberships in, but for many decades, I was giving papers and plenaries at American Sociological Association, PSA, and so forth. I do almost none of that now. I've just backed off.

[END OF AUDIO, FILE 3.2]

[END OF INTERVIEW]

INTERVIEWEE: **Sandra Harding**

INTERVIEWERS: **Joseph Klett**
Jody A. Roberts

LOCATION: **Harding's home**
Playa Vista, California

DATE: **25 April 2019**

KLETT: Okay. So, this is Joseph Klett and Jody Roberts here at the home of Sandra Harding. This is the day three and final session of our oral history. It is April 25, 2019. And so, we're going to begin with the question of what were the earliest role models you recall of women philosophers, and how did—how did they help influence the trajectory of your career?

HARDING: Okay, so I'm going to mention three issues here. I think I already mentioned that the first women philosopher I ever laid eyes on was Philippa Foot, who was an eminent British philosopher [who worked in ethics, which I did not]. And I saw her in—it would be—was I actually introduced to her? I think not at that point. I later had lunch with her, but that's another issue. Okay, so I first laid my eyes on her in—when I was writing my dissertation in Edmonton, Canada, so that would be 1972, '73, because she—and my husband was [a visiting prof] in the University of Alberta philosophy department. And Philippa Foot was making the typical visitor [professor] trip through Canada. I mean, Canada is population-wise four thousand by fifty miles. Once you get fifty miles across the border—

KLETT: Right. Right.

HARDING: —there's nothing much up there, except the University of Alberta, then you're up in the Arctic territories. I mean, all the major cities are within fifty miles of the border with the US. I mean, Edmonton is the only major city of Canada that does not appear on the map of the US, because if you look at a map of the US Edmonton is just—Calgary appears, which is two hours to the south, so 140 miles to the south of Edmonton. But Edmonton is the only major city [not on the standard US maps].

So, the typical tour, you start in Newfoundland or something, and sweep across—you don't just come across. So, Philippa Foot was for a week at the University of Alberta, which was a good size philosophy department, I don't know, fifteen, seventeen faculty or something, a lot of graduate students. And she had a British style. And she was very commanding. And I was so—I was totally fixated on her. I mean, she projected aristocratic horseback riding, you know,

and kind of stereotypes of British aristocracy, right? I mean, in my mind. I—it just was—you know, the wool suit, and the beautiful sense of self, and . . .

But in a small seminar that I went to—I don't know, there were twenty-five people there—the discussion—they were asked—faculty and graduate students were having a discussion with her, and after about fifteen minutes, she said, "Wait a minute. Come on, come on, I want to do some philosophy, guys." I thought, "Whoa." [laughter] I mean, she commanded them—she told them that their conversations with her were not at the level she expected. I was astounded. I mean, I didn't do anything with it right away, but I certainly have historically. I was doing it this morning with these guys at Taylor & Francis.

I offered to—I know. We'll get back to it. But I offered to put out a call for a new hire at Taylor & Francis who was Spanish and Portuguese fluent, through the ed team. Of course, I'm joking. [laughter] I am not hiring into Taylor & Francis, right? But I—you know, I was telling them, this is what has to happen. I said, "How about I do this?" And Paul knows I'm joking. Justin didn't. So, I had to put just kidding, so Justin knew I was joking.

KLETT: And that kind of—that kind of assertiveness was unfamiliar at the time—

HARDING: Yes, that kind of—[yes]. It's a certain kind of academic posture, but it's rare for women to occupy that posture. It's rare to hear it expressed as clearly and maybe offensively as Philippa Foot was doing, but she was totally charming and everything. She just—she said, "I want to have a real philosophy discussion. Come on, guys." Whoa.

ROBERTS: How did the room respond?

HARDING: They talked up. I mean, you know, it was a relatively young department. At the time we were there, it was full of Americans, because the exodus **<T: 05 min>** in the Vietnam War filled these Canadian universities with US people, many of whom stayed forever.

ROBERTS: Right.

HARDING: And there were a couple of older guys in the department, but they were of a far earlier generation, weren't the liveliest intellects in the department. So, there was that.

And let me see. I'll continue the Philippa Foot story now, and then go back to—so this was 1973. So, I'm going to jump decades ahead. I arrive at UCLA, and my office for the first three years on some kind of [adjunct prof] position or whatever, was in the philosophy

department, which I enjoyed a lot. I was half time at Delaware and half time at UCLA, more or less.

And UCLA has visiting lecturers, and every year they had—this has been the case for years. But Philippa Foot had a—she taught half time every year. Half of every year she taught in the department. So, she was in the department when I arrived. [And there was only one other woman in this department.]

And I was, you know, totally feminist at this point. She had exhibited no feminist tendencies. And so, I don't know, I didn't do it immediately, but I forget how long into my arrival in the department this happened, a couple of months, maybe. I invited her to lunch at the faculty center, and she accepted.

And we had the lunch, and I mentioned I'd seen her in Edmonton decades before. And, you know, we were chatting a long, and then she stopped the conversation, and she said, "You just invited me to lunch because I'm a woman, right?" And I had to negotiate my way around her anxieties around feminism, and around—I don't know whether that was entirely it, or what it was, whether my style of talking with her was—I mean, I don't know what it was. But it was a kind of funny end to that.

So, now I'm going to—and she—shortly after that, I left that—I no longer had an office in that department, because I was over in the [Graduate] School of Education, and, you know, tenured in and everything, and she died not too long after that, maybe five years or something like that.

KLETT: Do you think that she felt you were patronizing her only because she was a woman, and not because you were in the same intellectual trajectory?

HARDING: That's the way it came across, but I don't know if that's what she thought. I didn't work—she works in ethics, and I didn't work there at all, in that field.

KLETT: Okay.

HARDING: I was well-established, had a lot of books by that point, and everything. She'd never read any of them, never heard of me. So, I don't really know.

KLETT: Interesting.

HARDING: But it was—but I have to say, it was of a piece with her sense of self that I had first encountered. And I have to say, as somebody—a friend told me that when she lectured, she always had a little angel on her shoulder who was telling her how wonderful she was, something like that. I forget what she said. And I always had Philippa Foot on my shoulder. She wasn't telling me how wonderful I was. But I would try to walk onto the stage and command—I mean, I still do this. I have—I feel I have to do it. I have—you know, and very politely and friendly and warm, but making clear who's in control of this program.

KLETT: [Yes]. That—

HARDING: I mean, guys do that all the time, and they don't need to. They don't need to. But a woman in my field does.

KLETT: Right. I think you said on the first day that you with a smile will assert there's expectations to be met.

HARDING: [Yes].

KLETT: And I think that's a—[yes], that's—

HARDING: And I go out of my way to be helpful, and, I mean, I have a reputation for being very helpful in the discipline, and so forth and so on, but I'm really clear about what I expect from people.

KLETT: [Yes].

HARDING: And so, that's one thing—that's the only role model in another generation that I can think of that I ever had. I mean, apart from men. I mean, I really studied men, and tried to adopt their styles of teaching. So, I notice—you know, my professors say—they stretch out on a [desk] or whatever there was in front of the room while they're teaching. There's no way I could do that. You know, we'd be sitting around the grass at Haverford or somewhere. They'd be stretched out [lying on the grass and leaning on their elbow], or—they'd be totally relaxed. And we women tended to stay stuck behind the **<T: 10 min>** podium, right? [And uncomfortable sitting on the grass with heels on.]

And so, I always try to break that. I—there's no—and many sit with their legs crossed like this. [Men] sit in the male aggressive posture, [as the anthropologists say, knees spread out], like this. I mean, there's no way a woman could sit like that.

And so, I very consciously tried to negotiate my—and women tend to have a lot more hands in front of their faces, which is a kind of defense against the relationship between the observer and—to me it is. So, I made very—I gave myself exercises of—

KLETT: Don't use your hands?

HARDING: Speaking—I had to hold my hands down, or put something in them to prevent myself from doing some of the hand gestures that women tend to do. So, I studied how men behave, and tried to adopt a posture that was not a man's posture, but it wasn't a woman's posture, either. It was my adaptation of it. And I always walk out from behind the podium, and in the—usually at the beginning, and then again at the end. Certainly during the question period I get out from behind the podium and walk up to whoever's talking, or the audience of—

KLETT: Did you adopt a certain way of dressing to accommodate—because a lot of the body stuff you're talking about is clothing dependent, right?

HARDING: Yes. And at the beginning—[yes], when did I last wear a skirt? Probably thirty years ago. I had one in my closet for a long time in case I needed it, but certainly at the beginning, a skirt was the norm, though the Allen Center was pretty relaxed. I wore some slacks there. But it's—you know, that change in dress style came during that period. And how did I otherwise—I mean, I tend to dress in a uniform that's kind of a manly uniform. I have a friend here who's an artist, and she commented on it. She always dresses in beautiful colors and textures and styles. And I forget how we got to this, but she said, "Well, you have a uniform." I said, "I do?"

Then I remembered, I once saw an article on academic women's dress style. I never thought about how academic women did have a dress style. I thought everybody else did. In business, they had high heels, and so forth. But not—we didn't have a dress style. But it—as it was described, exactly. It's kind of a modification of a man's style. But to wear jeans to class, I rarely did that, and most of my [male] colleagues do. Did I do it—I would only do it if I had everything else dressed up, you know, a lot of earrings and, you know, whatever. But most of my male colleagues wear jeans to class all the time. I don't know why I don't do it. It just is I need to claim all the dignity I can get from people, and—

KLETT: [Yes]. But you are wearing pants?

HARDING: [Yes].

KLETT: It's just not—

HARDING: Thirty years.

KLETT: [Yes].

HARDING: [Yes]. So, that's Philippa Foot and that issue. But then the—so she's the only woman I can recollect in a generation ahead of me who was a role model. Alison Jaggar is younger than me, a few years younger, and we've been buddies forever, and she's part of this larger SWIP group, and, you know, we've visited each other's campus, we know each other's kids, and so forth.

But it's true. I saw her give a paper, I must have been a graduate student, or shortly after graduate—after Philippa Foot. And she had on a bright—she's very beautiful, and she's a Brit, very beautiful with long blonde hair. And she had on a bright green mini-skirt at a huge lecture. And I was astonished. I didn't know that a woman could get up there like that. And she's a very powerful speaker, has published many books, and knows how to handle an audience, and so forth.[61]

And I've told her this story many times, and she's a little bit embarrassed by it, because she hasn't worn a bright green mini-skirt in three decades. But that was interesting to me, that she could do that. It didn't give me—it was a model in the sense that I can be who I want to be, and do a powerful philosophic whatever, and just insist on the analysis. But I wasn't sure about the—

KLETT: That—was that your first encounter with her, was seeing her in the green mini-skirt?

HARDING: Was that my first encounter with her? I don't think so. I think I'd seen her at SWIPs in jeans. But I'd never seen her perform. This was two hundred people in the audience or something like that. It was a huge public lecture. **<T: 15 min>** No, I had seen her. We had interacted before.

[61] Alison Jaggar. *Feminist Politics and Human Nature.* Totowa, N.J.: Rowman & Allenheld, 1983.

KLETT: And did you see any correspondence between the outward appearance of these women philosophers and the ideas that you were interested in exploring? Like did you have to make certain concessions in your outward appearance in order to accomplish other kinds of intellectual tasks?

HARDING: I don't think I thought of it that way. I might have unconsciously been affected by it. But I didn't conceptualize it that way. I was clear that we all needed to speak what we thought should be said, so that—regardless of what we looked like, and so forth. But I, both in—especially in teaching, you know, that—very difficult relationships with both women students and men students. Some guys would get—I had two incidents I can remember where an undergraduate guy came up for an office hour with me in the philosophy department at Delaware, two different guys, different years, and was so pissed at me that I was terrified. I thought he was going to get violent.

And in both cases, my department chair had been wonderful with the first one. I was sitting there at my desk, and he was over there. It turned out his father was a doctor, and he took my critiques of science to be a critique of his father. And the door to the office was beyond him, and it was an old house we were in, right? And I remember sitting there and calculating how I could get to that door.

KLETT: Wow.

HARDING: And I did. I stood up. I said, "Excuse me for a minute." And I just went right past him and down to Frank's office, our [department] chair on the floor below. I said, "Frank, this one's for you." And he had no problem. And so, he said, "Just tell him if he has complaints, to come down to me."

KLETT: [Yes].

HARDING: And that's exactly what I did. And I had the same thing happen later.

KLETT: So, this wasn't—he didn't present this as an intellectual challenge to you. He was saying, "I have a problem with you individually, and—"

HARDING: I don't think it was for him that separate-able. And I don't think for me it was that separate-able. But I think what the case is, is he would never have done that with a guy, with a male teacher. He would never have done that. And at any rate, I felt physically threatened. I mean, he was, you know, some big guy, right?

The other guy was pissed because I wouldn't write a recommendation letter for him. Let me say, he was older. I mean, he was later in his career. And he was going to—he wanted—he was applying to law school, and he had a D average, D. No, he had a C minus average. He had to have a C to graduate, so he must have had a semester ahead of him when he could improve. But I had given him a D in my course.

And I said, "You know, I'd really like to be able to write—" I mean, what I did when I would turn down an inappropriate request—I mean, the idea of asking me for a letter of recommendation when he got a D in my course? We want this guy as a lawyer? [laughter]

It's pathetic. I said, "I'm so sorry, I can't write a letter for you. You should go to some of your other faculty." And maybe he didn't have any. Maybe he didn't—nobody'd give him anything higher than a C minus or a D. I don't know. But he got really mad at me. But I'd already had this other case. I said, "Excuse me a minute. Frank, got another one."

So, [yes], he told me—he said, "Oh, everybody applies to law school with a D average." I said, "Oh, I didn't know that." [laughter]

KLETT: What about between—what about decorum as a philosopher encountering sociologists, or encountering other disciplines? Was there a distinction like that that made you a philosopher versus your colleagues?

HARDING: No, not in the schools I was in. You know, I had constant interaction with women and men in other departments in my university, as we all do. At Delaware, the women's studies office was next door to sociology, and I had an appointment in sociology, you'll remember, for most of the years I was—and taught a course in there, and had **<T: 20 min>** a lot of graduate students in there. And that was wonderful, because my best friend was in the department, eventually chair of the department, and had been director of women's studies before I was there. And she was a race theorist; white, but a race theorist.

And there were also two African-American guys in that department whom I knew through sociology, but also because I secured an appointment for myself in African and African-American studies, those few years I was teaching that course, and getting Bill Lawson hired in philosophy. So, I had a lot of—I mean, they were there. Political science was on the other—down the hallway the other way.

KLETT: [Yes].

HARDING: I knew a lot of people in literature. And where were there differences?

KLETT: Did you—do you think the sociologists comprehended a philosopher as continuous with their work, or were you kind of the—were you see as the one who's off on a hill dreaming at the clouds, whereas they're—

HARDING: No, they wanted me in the department, because I had all—because—no, they saw the rest of philosophy as sitting on a cloud there, but they didn't see me as doing that.

KLETT: Okay.

HARDING: What I wanted to say was that at Delaware, as at UCLA, women and men in the professional schools can tend to dress very differently, in education, the law school, and so forth, because they're out in the public all the time. They're interacting with the public. And so, they walk around with ties and, you know, dressed up the way lawyers or doctors would be dressed up, and so forth.

KLETT: Right.

HARDING: So, that's the distinction, more than—otherwise, there's this academic culture that we all have striped shirts on.

ROBERTS: So, tell me if I'm jumping too far ahead, and I'll reserve it, but I always—I wanted to take your observations from your own kind of sense of being mentored, even if very informally, through these experiences, and how you communicated those in the mentoring of your own students. How did you share these stories, if at all? How did you prepare them for—

HARDING: I didn't share those stories. Okay, so I do remember there was an era where all the young girls, undergraduates who were going out for job interviews, had their belly buttons showing. I don't know what happened to women's fashions, but suddenly, all these twenty-year-olds or whatever they were, out of college—they'd be, [yes], twenty-year-olds. I mean, they were adorable kids, but they were walking around with their clothes, with their [bra straps showing]—do you remember that era?

KLETT: The midriff?

HARDING: [Yes].

KLETT: [Yes].

HARDING: They were going out for job interviews with their belly buttons showing, so we—all us women faculty talked about how we had to have conversations with those students about how they couldn't go out with their belly button showing. No skin showing between your neck and your hands and your ankles and whatever. No, I did all the appropriate mentoring with my students. I didn't cite that stuff. But—

ROBERTS: But what about with graduate students? I mean, I think you—you've kind of suggested or hinted at so many things that are not gone.

HARDING: [Yes].

ROBERTS: But that only slowly become visible to you. You know, whether it's presentation style, it's presentation mannerisms, it's how to command a room.

HARDING: [Yes].

ROBERTS: And I admit, I mean, I think it's—you know, so my own ignorant background, it wasn't until I was sitting next to a colleague several years ago at a conference and we saw two very different presentations happen, one by a young man and one by a young woman, and the young woman stood there, postured, and delivered the most artfully crafted, perfectly worded twenty-minute presentation, and it was great. It was clearly very well-thought-out and rehearsed. And the gentleman either just before or just after got up and looked disheveled, and, you know, grappled and tousled his hair, and you were witnessing his struggle with the material rather than his command of the material.

HARDING: Yes. Got it.

ROBERTS: And I made a comment to her afterwards of like, "Is this the way it always happens?" "Yes." Like, kind of what took you so long to wake up to this? And I think it wasn't until seeing them juxtaposed—

HARDING: Oh, I see.

ROBERTS: —but also sitting there with a, you know, a female colleague that I respected, and being older, and watching this play out, and thinking she's been coached.

HARDING: [Yes]. Absolutely.

ROBERTS: She's knows what is to be expected. And I don't think that that—but she wasn't indicative of every young person, and—

HARDING: [Yes].

ROBERTS: —nor was he indicative of every young male. But I'm just curious how you—how some of that coaching and mentoring happened.

HARDING: Okay, so first of all, for—you know, since whatever, since I arrived at **<T: 25 min>** UCLA, after the first three years or whatever it was, five years, I've been in a professional school. And so, they have—their models—the faculty are very—you know, are often about to go out and talk to the principals, or, you know, they're engaged with the larger community in certain ways. And these graduate students are not—almost none of them are twenty-two years old. They've all been out teaching, and want to become either us or—or principals and superintendents and policy people in Haiti and in South Africa and Washington and so forth. So, they have very professional images of who they're going to be. So, they're not your typical undergraduate liberal arts or science graduate students.

I would be on committees with—in other departments, but the ones in my own department were entirely this professional class. They were older. And they were extremely respectful, because they were all terrified of graduate school. You know, they're been out teaching, and now they were coming back into the best education graduate school in the country, right, with the highest standards. Hardest to get in in the country. It's got ranked number one of—in their—it's on the front page of the *LA Times* a couple of weeks ago. They—I don't know, whatever the rating system is. These rating systems are not totally trustable, but there they are.

And it's always been ranked second or third, after Harvard, Berkeley, and Stanford. But it got first. And what UCLA had cared about was not so much that it was first. Well, it did care about that. But that it beat Berkeley.

KLETT: Right.

HARDING: Because Berkeley always looks down its nose at UCLA.

KLETT: [Yes].

HARDING: So, it's a little different situation. But I do a lot of mentoring, constantly. And not only my students, but in conference contexts, where I have other graduate students. [. . .]

[I worked closely with several graduate students in Philosophy at Michigan State University. They had] hired me to work with the graduate students and junior faculty, because they were trying to upgrade their graduate program. [. . .] They adopted a lot of my—what I insisted on. And one thing I insisted on is that graduate students be taught to present from notes. They wouldn't do it at the beginning. They said, "No, we do—" you know, I—"No, we train them to read a paper." I said, "There's no way that when they're in the job market, that's going to work. They want—employers want to see what you're going to do in philosophy 101. You're not going to read papers at those students." And they heard me, but they didn't do anything about it for a while. [. . .]

[And I've heard recently, long after I left that visiting position, that they have revised their graduate practices to take a number of my suggestions.]

So, I always do that mentoring as kindly **<T: 30 min>** as I can manage to do it. [. . .] So, that—and I worked—I mean, I was really happy to work with that department on that—the department chair wanted me to do that. But they would—if they had just had a different standard. You know, Michigan State was not—the University of Michigan is the famous graduate school and so forth, but Michigan State wasn't really known for that, I guess. I mean, I don't really know a lot about its history. They had wonderful people in it, but they weren't famous people, the way they have at University of Michigan.

And so, I had graduate seminars there the whole time. I mean, I would only be there for a couple of weeks in—I'd be there for two weeks in October, usually, and a week in March, and then a lot of email work. I was paid on a—paid for one month a year, and I definitely spend another week on email, and I had three doctoral students whose dissertations I directed. I think I directed them.

KLETT: Were they all philosophy?

HARDING: [Yes], all philosophy.

KLETT: So, I was—the question I had about you mentoring, because you mentioned yesterday, I think, that people would approach you to be an advisor.

HARDING: [Yes].

KLETT: And, for example, you mentioned say a musicologist who had come and ask you for—to be an advisor, and you said, you know, I don't know musicology. I couldn't really do that. So, I was wondering what your criteria is, given the interdisciplinary nature of STS and science studies.

HARDING: [Yes].

KLETT: That, you know, there are various criteria through which we can relate to each other and understand how we might advise each other. So, what were you looking for and what were some of the red flags?

HARDING: It's not mysterious at all. I was perfectly clear about it, to them. The main thing was—so I was in a social science college, right? Fifty-two regular faculty, and another fifty auxiliary faculty, okay? None of the latter were supervising dissertations, but the first fifty-two were. And except for Doug and—Doug, who's a philosopher, and me, they were all empirical researchers. Fabulous quantitative studies, qualitative studies. So, that's who my graduate students were. Very few of them were purely theoretical students.

So, my first requirement was if they were an empirical student, I would be willing in many cases to co-direct their dissertation with an empirical researcher in the department, but I wouldn't be the—I refused to be the head person, because I'm not going to be trusted. I mean, I have a pretty good sense of what's reasonable quantitative arguments, but I'm dealing with the same issue with *Tapuya* now. We just got into heavily quantitative studies, and I—one of them I barely understand the quantitative claims being made about the data. Fortunately, I'm not the main reviewer of that one. The other one, I was the main reviewer, and the peer reviewers made a lot of—they loved the paper, but they made a lot of suggestions for improving the way the author was talking about the data.

He took almost every—I could see him taking the suggestions. I understood what the arguments were and what the issues were. So, I had felt pretty comfortable about okaying the paper. And then when I read the second paper, which is on a related topic, I got anxious. And so, I just set up with Leandro that I can in no way be—give the final okay to a quantitative paper. It's—even if I'm right that it's okay, I shouldn't be doing that. So, I said—we have a whole email going on that the subject line is, your statistical eyes.

KLETT: Right.

HARDING: So, he found—he doesn't—he can't do that for every paper. I mean, it takes a lot of work to look at thirteen charts and see if they're labeled right, and what they're saying about them is—but he found an economist on his campus who's happy to do it. So, that's the first thing, if they're doing—it's not just quantitative. It's qualitative, too. I should not be the—it doesn't matter that I have a very good sense of it. I've never collected a piece of data—

KLETT: The theoretical side of things. [Yes].

HARDING: At all. The second thing is if I don't know anything about the topic, I'm not a good advisor, and that was the case with this musicology stuff. And those are my two—otherwise, do I think the project is reasonable? I mean, **<T: 35 min>** I think those are the two main reasons I turn it down.

KLETT: So, it sounds like it needn't be strictly a theoretical exercise, but you do—you do look for a theoretical hook that you can speak to and make sense of their project with?

HARDING: If they're using data—if they're collecting data, if that's part of the evidence, then I need a co-director.

KLETT: Okay. Right.

HARDING: A third requirement is this—I think this is true. They [usually] have to have taken a course with me. Because if they've taken a course with me, they know my standards for good writing, they know what I know and what I don't know, and just somebody wandering in who's looking for a—I used to do that when I first arrived there, and I ended up reading many—much too many—you know, somebody needed a third advisor. Oh, Sandra's new, ask her. I said yes to some of them, and I'm reading all of this stuff that I don't care about, I don't know about. So, I stopped doing that. And so, the way I do it is they have to have taken a course with me. So, that's the three.

KLETT: Have you had any experiences mentoring where you felt confident that you could be the final authority on something, yet you found a drastic disagreement? Like a philosophical disagreement that you had with the person?

HARDING: That's a good question.

KLETT: Because there's certainly—in social theory, you know, you get—you may—

HARDING: I've been stretched. Let me put it that way.

KLETT: [Yes].

HARDING: I'll just give you a couple of examples. I stuck with them, in both cases. I let them lead the way here, and I'm not quite sure about what the results were, but it looked to me like a useful exercise.

One wanted to use standpoint theory to talk about mentally disabled people. Can you use this—the standpoint of mentally disabled people? Now standpoint theory was an organizing tool originally, right? It was about a group—it's not about an individual having a standpoint. It's about a group having a standpoint. I mean, it's not about what I as a particular woman with particular experiences think, but what us women academics experience, what we all testify to.

And I wasn't sure how that worked with mentally disabled people. But then the kid pointed out that there were in fact activists in the mentally disabled community, so I decided to go with it.

Now the other one, I really was quite sure it was wrong, but I thought, I don't know, let's see what happens. Animals, start off thinking from animals' experiences. Well, animals tell us a lot. We [think we] know what our dogs think in a lot of ways. We [think we] know what our cats think. We have no idea what elephants think. Right? We have a pretty good idea what predatory animals think. I mean, we've been—we can read enough to understand that [for them] we are only prey. That's who we are. And so, you'd better deal with it. [laughter]

But I—standpoint? I mean, not only [did this have] the problem of the first one, but I thought it was a stretch. But on the other hand, as I thought about it further, I thought, okay, so what he wants to do is actually get somewhere between standpoint theory and between conventional ways of—progressive ways of thinking about animal rights and so forth. So, I thought, okay, let's let him go with it and see what happens. And it was—you know, it was definitely between.

KLETT: [Yes].

HARDING: So, I just let it go.

KLETT: This is—sorry.

ROBERTS: Oh, no, no. I was going to say, I mean, on a similar line, and if it's a distraction, just go on with your question, how you police those boundaries outside of your own students. Or did you feel the need to engage with researchers—so as standpoint theory, as strong objectivity, are becoming concepts in the kind of—you know, in the vernacular of science studies, did you feel concerned about places where you felt like they were being used or deployed inappropriately, or used in ways where the person had an empirical product that was really the basis of the thing. They needed to bring in some kind of theoretical concept. They could mention a footnote, standpoint theory or strong objectivity, move on. And say like, I don't think that's strong objectivity. That's just—

HARDING: Okay, so—

ROBERTS: —you know, fill in the blank. And whether or not that's something that you just have to accept, it's out in the world, and—

HARDING: That's number one. It's not my—you know, standpoint theory is going to **<T: 40 min>** do what it's going to do. It's like your kids.

ROBERTS: [Yes].

HARDING: You know, every once in a while I grab them by the neck and pull them back, but [that usually doesn't really work]; forget it. On the other hand—and it's done a lot of strange things out there. I mean, the critics of it, for one thing. The science wars, for one thing. I mean, totally missed the point of it.

On the other hand, if I'm in an audience and someone's presenting it, or in that kind of a context, I'll raise a question, and I won't tell them they're wrong, but I'll say, "Well, a conventional way of looking at it is so and so," or something like that, or "How would you get around the criticism that so and so?" And frequently, they'll say, "Thank you. I didn't think about that." So—but I don't go out and seek them. I mean, it's—it's out there.

ROBERTS: [Yes].

HARDING: It's just out there. So—and I'm not the only author of it, either, and even among us, as I pointed out, there are big differences. I mean, Dorothy Smith, as I mentioned yesterday, I think always wanted the standpoint of women, not the feminist standpoint. I mentioned yesterday, because she didn't want elite theorists' experiences, or interpretations of others' experiences, to be the standard. She wanted the everyday lives of women, actually.

And she—so her data studies are wonderful. One of them was she was very poor as a single mother, and her kids would get criticized at school because their clothes were dirty or falling apart or something or other. And—by teachers. And so, how to present the standpoint of these poor kids to teachers and faculty in these schools they were being discriminated against because of their poverty, and she didn't have the time to do the whatever, PTA or whatever. She had a job, and—so she took very everyday experiences and developed them, and had herself occupied one of those positions.

KLETT: Can you say a little more about that forging—the period where standpoint is being forged in conversation with people like Dorothy Smith, and were there like—that's a great sort of counter-perspective, and it sounds like in her mind, feminist was like an intellectual—

HARDING: It was—it was an elite. It—well, as I—as standpoint theory was articulating it.

KLETT: [Yes].

HARDING: I mean, she thought there was an everyday feminism, but they didn't necessarily—that needed—she thought we were overtaking their experiences.

KLETT: Were there other perspectives that you encountered and had to grapple with over—

HARDING: Sure. Women of Color was probably the most powerful. We were not—we did not at the beginning know—[race issues] didn't even occur to us at the beginning. It very quickly did, because Angela Davis was writing. Nineteen seventy-two is I think one of her first very important, big essays.[62] So, 1972, I was still a graduate student [and I heard her give a spectacular lecture at SUNY-Albany]. And there were other Women of Color who came out immediately. I noticed and started quoting them in my own writings. [Meanwhile, all of us standpoint theory founders had been political activists in other parts of our lives in public

[62] Angela Davis. "Reflections on the Black Woman's Role in the Community of Slaves." *The Black Scholar* 12, no. 6 (1981): 2-15.

politics in the 1960's and after. So, we were immediately able to hear the issue of Davis and other Women of Color. Even though not with the depth and scope that they developed.]

And then by the time we get to the *Feminist Standpoint Theory Reader*, as I pointed out, I'm using Patricia Hill Collins as very important.[63] [I had already published the edited collection *The 'Racial' Economy of Science: Toward a Democratic Future* in 1993.[64] Yet] I would say that colonialism was nowhere in my consciousness. I mean, I had read [Edward] Said, but I don't know, that was far away, and I didn't talk about the Orient, and it didn't get into my consciousness in the kinds of ways that later critiques did, [such as those by Mignolo, Santos, and the Third World Network (which I had just discovered in a pre-print that one of the network participants had sent me in 1992].[65] And I think that the Latin American authors are moving that discussion way forward in kinds of ways that, as I said, the postcolonial [scholarship] didn't, powerful as it was in many ways.

KLETT: Can you say more about that? Because there—you know, that's sort of like the strain of poststructuralist literary studies mostly that took up Said, and—

HARDING: Yes, that's right.

KLETT: —Foucault and Spivak.

HARDING: Yes, that's right. **<T: 45 min>** [Yes].

KLETT: And I'm wondering, because I'm—you know, from my perspective, anthropology has a place in STS.

HARDING: Yes.

KLETT: But—and those are the folks who would import those kinds of poststructuralist works. From your—you know, from your position as a philosopher and kind of at the margins,

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[63] Sandra Harding, ed. *The Feminist Standpoint Theory Reader: Intellectual and Political Controversies*. Psychology Press, 2004.
[64] Sandra Harding, ed. *The "Racial" Economy of Science: Toward a Democratic Future*. Indiana University Press, 1993.
[65] Edward Said. *Orientalism.* New York: Pantheon Books, 1978; Third World Network. "Modern Science in Crisis: A Third World Response." In *The "Racial" Economy of Science,* S. Harding, ed. Bloomington: Indiana University Press, 1993.

what—what's your relationship—because you mentioned that poststructuralism sort of missed you.

HARDING: Yes.

KLETT: Or you missed it. But I'm wondering, how did you—how did that like—that relationship or non-relationship develop over time?

HARDING: [. . .] So, the first thing I'd say is that there's a deep affinity—I experienced a deep affinity between poststructuralism and feminist standpoint theory, for example, because both were criticizing the universal status of Western—the Western epistemes. I mean, they were doing it very differently, and coming from a quite different—partially different background, because—is it true that 100 percent of us came from—[yes], 100 percent of the early standpoint theorists were socialist feminists. And Dorothy Smith was a scholar—both Dorothy Smith and Nancy Hartsock had written papers on [Max] Horkheimer and [Theodor W.] Adorno, and, I mean, [Jürgen] Habermas was, you know, a later child, gone liberal, as far as they were concerned.

KLETT: [Yes].

HARDING: Me too. But Horkheimer—so they were very steeped in Marxian ways of thinking, as was poststructuralism. It's not always at the surface, but it's part of the European intellectual inheritance in a way that's not the case in the US.

I mean, most of us—I mean, I actually was taught *The Communist Manifesto* in my civics class in high school in the early 1950's. You know why? It was presented as a bad example of governance. [laughter]

That's how it was taught. But they don't even teach it anymore as a bad example, right? So, that's the first thing, that there was a kind of deep—I didn't feel threatened by it. It was coming from a different philosophic tradition, it was—I don't know, it was coming from [Georg W. F.] Hegel and whatever. And that wasn't mine, and I was busy with my own projects. And feminism was appearing in it. I read the early works by [Luce] Irigaray and—Irigaray and who was the other woman? [Hélène] Cixous.[66] I'm probably mangling the French. I apologize.

[66] Luce Irigaray. *This Sex Which Is Not One.* Catherine Porter, tr. Ithaca: Cornell University Press, 1985; Hélène Cixous. *The Newly Born Woman.* Betsy Wing, tr. Minneapolis: University of Minnesota Press, 1986.

And it was very interesting. It was difficult to read and kind of mysterious, artistic or something. It wasn't the straight-ahead style that I used. But it—we all felt affinities with it, but it wasn't ours.

So, that was the first thing to say. And I began reading Foucault. [. . .] And I kind of got what he was doing. Did I try to read Derrida? I think I tried to read something or other of his, and who else was around in those days from the poststructuralist? [Gayatri] Spivak I read a lot of.[67]

So, I learned from them in a certain way, but I always saw it as kind of complementary to what we were already doing. On the other hand, I had an anxiety that we weren't doing enough. You know, missing some really important parts of it. And I think now that this—I haven't gone back and rethought the relationship of poststructuralism to this pluriverse discourse. I don't know what it would be. None of the Latin Americans seem to be coming out of it. They're coming out of a lot of European philosophy. Most of the Latin American philosophers went to Louvain. They had Catholic educations. Where's Louvain? In Brussels?

ROBERTS: Well, just outside—it's in Belgium.

HARDING: [Yes]. Most of them have—I mean, it's a Catholic continent, in effect. A couple are from Notre Dame. But if you're going to go abroad to a Catholic university, that's where they go. **<T: 50 min>** In the case of Leandro and the rest of the editorial team, [they are in a later generation], so that's the earlier generation I'm talking about. Probably a good amount of that still occurs, but the people I know who had—who I'm familiar with who had that education are in—they're either my generation or a little bit younger than me, from Latin America.

[Leandro's] generation, I mean, at least the *Tapuya* people, none of them had that education. He went to—he has his doctorate from Cambridge. Luis's is from Cardiff. Somebody's is from Exeter. Another one's from Berlin. Somebody's from Toronto. So, they're doing European degrees, mostly, but not Catholic degrees. [Of course, these are social scientists, though a few have philosophy training also.]

KLETT: Right.

HARDING: And his family, he and his family—I try to have discussions with him about living here [in this retirement community has a kosher kitchen]. I was complaining to him and Luis the other day on email that Passover is the first time I've really—I've been annoyed with the food here. It's a whole week with no grains and no leavened breads, right? So, I'm cheating. I'm

[67] Gayatri Spivak. "Can the Subaltern Speak?" *Marxism and the Interpretation of Culture.* Urbana, Il: University of Illinois Press, 1988.

sneaking into the dining room. A piece of bread with cream cheese, and I hide it under a napkin, and then I eat it with my coffee. I mean, they have plenty of good food. They have eggs and fruit and cheese and so forth. But I really miss—at breakfast.

So, I was complaining to them, and otherwise—I mean, it's a kosher kitchen, and I don't—it's fine.

KLETT: Sure.

HARDING: And there's none of their other practices that are—I mean, I'm really happy to—I go to [services] occasionally on Friday night. I'm learning how to do them, and—I'm not trying to become Jewish. I'm trying to be with this community's values.

KLETT: Sure. What—what's the connection with the Catholic? You were talking—

HARDING: So, I'm talking to—I'm telling Leandro and Luis, and we were on a Skype last week, I guess it was, the three of us, so we can see each other's faces, they say, "Oh, we don't pay any attention to any of that religious stuff." And actually, I'd also just been looking up—I was about to have a conversation with them but decided not to. I decided to hold off. Somebody here had asked me, are there any Jews in Latin America? And I said, "I'm sure there are, especially in Argentina and Mexico," because I know what those—how urban those populations are, and how World War II created a huge influx, especially into Argentina, and also into Peru.

But I didn't—I hadn't actually encountered—I've been there a couple of times, and I don't remember synagogues or meeting people who were obviously—in some obvious way Jewish. And I never—they're not obviously Catholic to me, either. I mean, they're Latin American. That's all I know. Or not. They're blonde and whatever. But—so I looked up Latin American Jews in Wikipedia. It goes on for pages, tells you how many there are in every country, what time they—what year they arrived, how many synagogues they have. I mean, it's a—so I was all prepared to start this conversation, because I want to hear from Leandro and Luis what—if they had Jews in their communities. But I decided if they didn't do any of that religious stuff, I'd wait for another time to have the conversation.

KLETT: It's interesting, the way you describe it, because to my mind, the—like Catholic, and particularly in the European context, the Catholic is basically a stand-in for the French—French philosophy or the French theory, right? So, like you've got Paul Virilio, you've got—I don't know if Lyotard was Catholic. But—

HARDING: [Yes], I was reading Lyotard.

KLETT: But the sort of like cosmology—the Catholic cosmology and the sort of moral basis that comes with that gets refracted through a lot of—even [Georges] Bataille, right, like is—is grappling with Catholicism in a way that the rest of Europe really wasn't. I'm—

HARDING: That's interesting. I didn't know that.

KLETT: [Yes], the Spanish side of things—you know, I'm sure there's a Catholic strand there, and through the Italian stuff, but in terms of the impact of poststructuralism in particular, postmodern theories in the US, there is I think a distinction of what's continental is often really, we mean French, right?

HARDING: Yes. That's right. [Though I think also German and German influenced. Hegel, Marx, Sören Kierkegaard, etc.]

KLETT: Maybe [Emmanuel] Levinas—

HARDING: That's right. And—plus Marx.

KLETT: Plus Marx. [Yes].

HARDING: I mean—

KLETT: But the British took up Marx pretty thoroughly in their own way.

HARDING: That's right. That's right.

KLETT: But they—I feel like there's kind of a distinction between doing philosophy through this French theoretical **<T: 55 min>** way, and maybe the more British analytical way, or epistemological way.

HARDING: [Yes].

KLETT: That is still reverberating through a lot of this, and what you're pointing to with the Latin Americans is they're sort of like—

HARDING: They have a different route.

KLETT: They a third party.

HARDING: [Yes].

KLETT: And they're trying to find a place to land.

HARDING: And they also—I have this chapter—maybe I'll give you the paper, this section in a paper, about the history of positivism. Do you know about Latin American positivism? I didn't until I ran into this wonderful collection of essays that—I can get the citation for you. I've got it somewhere right here, because I've been working with this paper on the—

KLETT: Is this related to [Milton] Friedman at all, and the Chicago School?

HARDING: No. No. So—well, let me just get the citation, because if I don't have the citation in front of me, I can't think. It's a terrific set of essays. I highly recommend it. Let's see. Gilson and Levinson, *Latin American Positivism: New Historical and Philosophic Essays*, 2013.[68] Gilson. Gilson.

So, from the perspective of Latin Americans, the Vienna Circle positivism is the fourth positivism, or is it the fifth? It's the fourth. The first for them is utilitarianism. The second is [Auguste] Comte, and that French positivism of the—when is Comte, the 18th century?

KLETT: [Yes].

HARDING: The third is Latin American positivism, and it emerged out of Comte. There were Latin American intellectuals who went to study with Comte, and also some French who came to Latin America. And so, they developed a positivism, a very—what they—they brought

[68] Gilson, Greg, and Irving Levinson, eds. *Latin American Positivism: New Historical and Philosophic Essays.* Lexington Books, 2012.

positivism to Latin America, and they lied intentionally about one aspect of it, of Comte. Namely, they dropped the part—I haven't read Comte in, I don't know, fifty years, so I can't remember where this is and so forth, but they dropped the part about how technocrat—how positivism always ends up in a kind of technocracy.

So, meanwhile, meanwhile—and the person who tells this story best is Mary Louise Pratt. You know her, anthropologist? [. . .] *Imperial Eyes*.[69]

KLETT: Oh, sure.

HARDING: [. . .] And so, she has a whole chapter there on the end of colonialism in Latin America. What did independence mean? So, Latin America was formally completely independent by about 1810. So, think of India and so forth. I mean, it doesn't even start up.

KLETT: Right.

HARDING: There may have been complaints before and so forth, but the extrusion of England from India and other places doesn't even begin until after World War II. But Mary Louise Pratt raises the issue, what did independence mean? There was no model of it in Latin America. It's true, 1776, right, the British had been—you know, had—gotten rid of in the US. But the British colonies had a very different structure from the Latin American colonialism.

And so, there were a whole set of questions. Does this mean that all the priests have to go? Because Latin American colonialism was fundamentally run by the Vatican, or the Catholic Church, or whatever.

And so, there's a whole—so there's almost a century of huge turmoil, and at this point, the essay I chose to focus on was about Mexico, and it's about the election of [Porfirio] Diaz as President in 1830, I think, maybe, something like that. **<T: 60 min>** I forget when he was—no, he's at the end of it. I forget when he's elected. He's thrown out in 1910. That's the Mexican Revolution, right? I think that's the date.

So, about a hundred years of political turmoil, and Pratt cites—there's a great paragraph I quote here somewhere, that Mexico by whatever date, by whatever—Diaz got elected, had had fifty presidencies, thirty-four peasant revolts, and so it just had this huge history of disorder.

So, Diaz adopted positivism. He was a progressive positivist. And he got elected by the warring factions of the big land owners, the peasants, and the—and the peasants, the landowners, and the urban classes who had control—who had been the—were mostly mestizos,

[69] Mary Louise Pratt. *Imperial Eyes: Travel Writing and Transculturation*. Routledge, 2007.

and had, under the particularly Spanish rule, had been trained into the upper levels of the church, for one thing. They managed the church, but also the government. They weren't the wealthy, aristocratic rulers, but they'd been trained to run the country, and they expected to run the country after independence.

And so, though the peasants—so the—there were these warring needs for the end of colonialism. The peasant groups and indigenes expected democracy. So, the mestizo classes expected to be able to not have their bosses on them. And the landed gentry expected not to be bothered by anybody. And so, that century of warring there, everybody adopted positivism and elected Diaz, because, thanks to the failure to complete—accurately report Comte, it provided something for everybody. It provided order. It seemed to promise a kind of social order. They were going to provide a—more democracy, a kind of leveling of inequality. But that didn't happen.

And so, for Diaz to remain in control, he had to move—more and more move towards the technocratic control side, which seriously annoyed the landed gentry, and didn't make the peasants and indigenes happy, either.

So, to get to the end of the story, he was thrown out by 1910, and that was the end of Latin American positivism. And the paper—what I'm arguing here—and the Vienna Circle knew nothing of this. This was invisible. I mean, it's been invisible to you. It was invisible to me. But I argue in this paper here that it haunts Latin America, and makes STS very—have to be approached in a very different way, because of this powerful history that Latin American positivism had, both the hopes and the disasters and the end of it.

And it creates a very different understanding of democracy and science, the progressive uses of science and technology. It puts another layer of moral and ethical goals and anxieties into that issue. And that's fueling this decolonial theory in kinds of ways that didn't happen in terms of the postcolonial. I mean, one can go back and reconstruct some of the postcolonial stuff, but they didn't have this history.

KLETT: [Yes]. Can we use that, then, to talk about the American context in—let's go to 1980, [Ronald W.] Reagan gets elected, right? We're seeing the culture wars starting to heat up, right? And we could maybe see the science wars on the horizon. How—that sort of positivistic infusion in Latin America, don't we have something similar in the States, and aren't we grappling with that at the highest level of government?

HARDING: I put it a different way here, and would put it a different way, that those positivist elements here came specifically from the history of the Vienna Circle. Right? Which they're in the 1938—and maybe there's somebody **<T: 65 min>** a little earlier, through after the war, migrating to the United States, they're all socialists, they—they're almost all Jews. They encounter anti-Semitism and the Cold War and McCarthyism. And so, it was a—you know, it was a retraction of the political goals of—I had to look for his name yesterday, and it's out of

my mind, starts with an F. Carnap and—Nancy Cartwright writes about him. Anyway, I could pull it out of here. He most clearly articulated the other element—Cartwright. Otto Neurath. Otto Neurath.

KLETT: Okay. Right.

HARDING: He most clearly articulated the political goals of the Vienna Circle. But in order to save both philosophy of science and science and technology themselves, they retracted the—

KLETT: Right.

HARDING: —goals of the—or set aside the political issues. So, I think it has a different history.

KLETT: It's got a different history, but doesn't—are the stakes or the terms of the conflict not—like Latin America aside—we don't have to necessarily use these comparatively, but I'm just wondering, then, like in your own very personal struggles here at UCLA, or at Delaware and then at UCLA, how did you see the stakes of—you talk about the social justice end. [. . .] We didn't really talk about the terms of this—of this at a sort of high level.

HARDING: [Yes]. I can go back a little bit first and say that—friends of mine, [when] they read this [recent essay], they say, "Sandra, are you resuscitating positivism?" And in a certain way I do. At the end, I say, if you—this account of the four positivisms draws attention—I mean, from the enlightenment through the Vienna Circle—draws attention to an enduring hope that science and technology can be used for progressive ends, which the—how one understands progressive ends has been the issue. Does that mean taking it away from the political disputes that are inevitable in human social life? Or does it mean putting it into service for some particular political hope, which is not going to be the political hope of other people.

And I—so I said—and I think—end up by saying, why should we care about this Latin American positivism? I think apart from the intrinsic historical interest and so forth, it provides an additional rich context for thinking about the many attempts to get post-positivist today. We're still trying to get post-positivist. In our case, Vienna Circle post-positivist. But it reveals a rich, deeper strain of modernity, connected with modernity, this—for me, because I'm coming at it from the Latin American angle, that we haven't dealt with in general.

I mean, we—and most people think to be modern is good. There's the savages and the primitives and the uneducated and so forth, and then there's us moderns, who are—and, you know, the science wars critiques of me, oh, my God. Alan Sokal says, "I presume Sandra

Harding wants to use a computer. I again presume she's going to want a modern doctor. So, it just shows how incoherent her philosophy is." You know, he doesn't get it. I mean, he's nowhere near—you can't criticize science, he's saying.

KLETT: Our colleague Nick yesterday referred to this as the desk pounders.

HARDING: The what?

KLETT: The desk pounders who say, I'm banging on a desk, aren't I? Like—

HARDING: [Yes], that's what—well, that was ordinary language philosophy, right? Right.

KLETT: But the sort of like facile demonstrations of, you know, if I yell—

ROBERTS: Reality, [yes]?

KLETT: [Yes].

HARDING: [Yes].

ROBERTS: This is solid, right?

HARDING: [Yes], that—that's [John] Austin.[70]

KLETT: And therefore you are wrong.

HARDING: That's Austin. **<T: 70 min>**

KLETT: [Yes].

[70] John Austin. *How to Do Things with Words.* Oxford: Clarendon Press, 1962.

HARDING: Thumping on the table, right? Never mind the molecules in there or anything else.

KLETT: [Yes].

ROBERTS: But I hear—I think what I hear is this—and maybe it's a way to kind of get into modernity and kind of multiculturalism, postcolonialism, I think—I'd like to hear more about your engagement with the modern, in part because I think, if I understand what you and Joe are kind of going back and forth on, is that there's this sense in which if we take your description now of the four kinds of positivism that Latin America has experienced, there's a certain ethical and moral framework built into the exercise of what science and technology will do for them.

HARDING: Yes.

ROBERTS: Which yesterday we talked about how Neurath and Carnap and others coming to the United States very purposefully subdued that political project—

HARDING: Yes, because it was—

ROBERTS: —put aside the ethical and moral framework for survival.

HARDING: Yes.

ROBERTS: And so, you have this bifurcation of positivism existing in two very different means, one—

HARDING: [Yes].

ROBERTS: —one which has a long legacy of the ethical entailments of science and technology and the development project. That continues, as part of that legacy. And another that has—is kind of building this experience of there is no ethics tied to this, except the good. Let's not worry about it. Let's just get on board. And that at this moment—so while that it was short-lived, or maybe it was short-lived, kind of that science war experience, it did create this rift in trying to figure out how to put the development project back together. Both as something that happens at home, but also that happens abroad.

HARDING: Right.

ROBERTS: Because the branches separated for too long, and now trying to put them back together is complicated. And it sounds like you're—so if I'm reading that conversation correctly, part of your interest in going back to the project of modernity is looking for the roots of where some of this fractiousness came from. Is that—

HARDING: That's one way to put it. I mean, I can't believe—I admire Jasanoff's work for a lot of reasons, but that little sound bite about sciences and their societies co-[constitute] and co-produce each other is just central to my thinking all the way along.[71]

And so, what I—the resources I also draw on are an analysis I did, I think in my last book, on how deeply Protestant is modern science. And I'm drawing on historians, right? Margaret Jacob. I mean, there's a whole bunch of them who talk about the specifically Protestant notions of is it—and Foucault does this, too, doesn't he? What does it take to create a reliable observer of an experiment? Is that Foucault? Who was that, writing about constructing the scientific experiment?

ROBERTS: Well, but, I mean, that gets in—[Steven] Shapin and Schaffer's looking at—[72]

HARDING: Yes, that's right. Shapin and Schaffer—

ROBERTS: —*Leviathan and the Air Pump,* you know, so as we transition to the public audience.[73]

HARDING: That's right.

ROBERTS: The public has to be built first.

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[71] Sheila Jasanoff. *Designs on Nature: Science and Democracy in Europe and the United States.* Princeton: Princeton University Press, 2005.
[72] Steven Shapin, interview by David J. Caruso and Jody A. Roberts at Science History Institute and Harvard University, Philadelphia, Pennsylvania and Cambridge, Massachusetts, 9-10 April and 15 May 2019 (Philadelphia: Science History Institute, Oral History Transcript # 1100).
[73] Steven Shapin, and Simon Schaffer. *Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life.* Princeton University Press, 1985

HARDING: Yes, that's right. It's countering our assumption, well, if we just open our eyes, we can see the way the world really is. I mean, I'm trying to get behind that by trying to—so for me, I try to pull—I'm using history of science for philosophic purposes, and pulling together these various kinds of evidence of the distinctively cultural way we think about science and technology, and modernity, and that there are other ways to think about it that most of the [rest of the] world is into.

And our ways of thinking about it are always constructed in opposition to those, whether they're overt or not. Those are pre-modern, they're uneducated, they're religious, they're superstitions, and so forth and so on. And yet, you know, for like fifty years or so, it's—quite a—I mean, *The Cultural Meanings of the Scientific Revolution*, it's not fifty years old, Margaret Jacob.[74] It's forty years old, maybe, thirty years old? It's not new. And she's talking about the cultural **<T: 75 min>** meanings of the scientific revolution. And it occurred in the context of the Protestant revolution, right?

And the particular—I point to particular features of Protestantism that one—that one can find in modern life, that's—the modernity discussions, the Latin Americans—excuse me—point to—who is this that I'm thinking of in particular? Mignolo maybe?[75] I forget who—Santos?[76] How modern democratic governments build in Protestant assumptions. They're not Catholic and they're not Jewish. You can—never mind Muslim. But they're about individuals—individual beliefs, individual votes. They're about beliefs. They're—they're not about rituals and [ceremonies and] dietary practices.

And so, you can see Protestantism shaping liberal democratic governments in kinds of ways that are invisible to us. We just grow up in a Protestant world, whatever our religion may be. Catholicism and Islam and Judaism appear as religions in a kind of way that Protestantism doesn't, unless you're in church, right? So, it's—I try to pull that all together, because, of course, I'm interested in appreciating the value of indigenous and everyday knowledge in many ways, not that it's perfect, but it's—our particular self-serving ways of thinking about what's valuable knowledge production, what isn't, are laughable to the rest of the world, particularly in an era where we're very clearly identifying how modern scientific technological product—practices produce humongously bad environmental domination of nature, the improvement of land [only in terms of] commercial purposes, and so forth and so on.

And so, I think it's—I think everybody should be interested in that, and I don't know how else to get that interest than by using these histories of science to draw attention to things we never were brought up to know.

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[74] Margaret C. Jacob. *The Cultural Meaning of the Scientific Revolution*. Temple University Press, 1988.
[75] Walter D. Mignolo. *Local Histories/Global Designs: Coloniality, Subaltern Knowledges, and Border Thinking.* Princeton: Princeton University Press, 2000.
[76] B de S. Santos. *Epistemologies of the South: Justice against Epistemicide.* Boulder, CO: Paradigm Publishers, 2014.

KLETT: Did you—were you willing to adopt the language of technoscience? I know that's a—

HARDING: I use it.

KLETT: [Yes]?

HARDING: I use it. But sometimes it seems too . . . [I think about Donna Haraway's writing]. I have a long, difficult relationship with [it]. She knows. [laughter] She's busted through—she forces people to think differently by the way she writes. [. . .] And I—she has several times sent me a draft manuscript, and I can't read it. She sent me a draft manuscript of—what was that big book of hers?

KLETT: *When Species Meet*?

HARDING: Hmm?

KLETT: *When Species Meet*?

HARDING: No. Twenty years earlier.

KLETT: *Cyborgs and Women*?

HARDING: *Cyborg Manifesto*.[77] She sent that to me. And I couldn't read it. And I knew it was my fault, that she was doing something really wonderful with language. I mean, I could read parts of it and so forth. But it seemed to me a flashy literary style that was in defense of itself, kind of. I wanted her to just say what you're saying. You can say it at the front and back and do your thing in the middle, but say it.

KLETT: [Yes].

[77] Donna Haraway. "A Cyborg Manifesto: Science, Technology, and Socialist-Feminism in the Late Twentieth Century." *Simians, Cyborgs and Women: The Reinvention of Nature*. Routledge, 1991.

HARDING: And it didn't do it. And of course, I've always—a huge focus of my work is to say in perfectly understandable English that can be understood by a senior in a science and technology studies class, let alone graduate students and anybody else. [redacted 01:19:27 to 01:19:57]

ROBERTS: So, twenty years ago, is this "Cyborg Manifesto," or **<T: 80 min>** was it the—

HARDING: Thirty years?

ROBERTS: —*Primate Visions*, one of the other—

HARDING: No, not *Primate Vision*. I know "The Cyborg Manifesto" came out in the next book.

ROBERTS: Right. The collection of essays?

HARDING: [Yes]. It was in that book. And she has been a very important mentor for me, I have to say. I'm always intensely grateful to her. She got me—she's the one who got me to read Barnes and Bloor. She was a reviewer of the first draft of the—for whoever the publisher was at Cornell, *The Science Question in Feminism*. And she said, "You have to look at Barnes and Bloor, because they're doing something close to what you're doing, but they're doing it differently. You need to look at it."

And I did, and that's how I got into it. And we've had, you know, wonderful conversations, and we have lunch at 4S every year, and so forth. But we just come at things from—are working with different audiences and have different goals.

KLETT: I hope it's not distracting, but I'm noticing there's a set of binaries here, that there's the Catholic/Protestant distinction. Donna's very much a Catholic.

HARDING: That's right.

KLETT: Right? There is the managerial versus artistic writing style, how you've characterized it.

HARDING: Yes.

KLETT: [. . .] And that's something that—going way back to our first session, when we were—when we were sort of foreshadowing the science wars, and the way that we are talking about epistemology was very dichotomous. It was either, you know, you either believe in a standpoint or you don't, right? You either believe in objectivity or you—a strong objectivity, or you don't.

HARDING: [Yes].

KLETT: And—but I think there's a certain spirit there that keeps you and Donna in each other's orbits, right?

HARDING: Yes.

KLETT: And working together, and ideally—

HARDING: And having shared a whole history in so many ways.

KLETT: Having shared a history, so that you—your purpose is, you know, we can imagine maybe the same ends, whereas when we're thinking combatants in the sort of culture war sense, we know that the goals are very, very different.

HARDING: You know, I'm not—

KLETT: And the means are different as well.

HARDING: —completely against binaries. They're a very useful way to organize the world. And they're very developmental. We come to think us and them in [our birth] household. [Mamma and not-Mamma] is the first binary that shows up, and before it, a kid doesn't know where—I'm drawing on feminist psychoanalytic theory, right? Where it ends and mama begins, right? It's all one—you can see that with babies when you have them around. Its foot floats by [its vision], and they have no idea what it is. How fascinating, right? It doesn't belong to them yet.

So, I think binaries can be very useful. The particular set of—I mean, they're a way of organizing the world that has its uses. Of course, they always distort the whole field. The whole field is much more complex. And they overly homogenize each group. So, I always have a footnote at the beginning that says something about that, because, for example, I'm, you know, talking about Latin America versus the rest of the world. I mean, really. You know, and using Northern terms to describe Latin American supposed commonalities, and the Latin Americans don't accept those commonalities at all. Even the term Latin America is not a Latin American term. It comes from the North. I mean, they're Brazilian first. They're not Latin American. You know, they might be Catholic. Maybe they're Catholic second. It's a long ways until they get to be Latin American. But we talk about, Latin American, the same time—in the same breath as Argentina or Mexico or Honduras, you know.

So, it's not that—so for me, the question is when is a binary useful, and when is it obscuring? And we always have to notice that it's [always] obscuring in some ways or other. The question is, are those more important than the issue we're trying to focus on? The rich versus the poor, right? The third world—I mean, I always have something at the beginning that says—it's usually footnote number two. Is it footnote number two here? It might be. [Yes], it's **<T: 85 min>** number two. I think it's number two, or number one. Well, here it is. "Readers may find it confusing that I will sometimes refer to the West versus Latin America, at other times the North versus the South, and at other times, the first versus the third world, or industrialized versus unindustrialized societies. Wouldn't it be better to use just one set of terms throughout? Alas, no. Each of these contrasts came into use in a specific era of local politics. While none have subsequently completely disappeared, the use can be misleading or irrelevant with reference to other eras. I shall tend to use the contrast most appropriate for the particular historical context at issue. Of course, use of any such contrast can be problematic, as it tends to homogenize both sides, and obscure the often widespread hybrid or mixed context. Moreover, it can tend to distract attention from other, more important issues. Yet we need to be able to refer to powerful and widespread social forces and their resistors, and without further empowering the former power. We are stuck with only contradictory options."

And I have that footnote in some form or other—it's footnote one or two in everything I write recently. So—

KLETT: I'm wondering about that in the sense that sort of resistance to binaries being definitive versus the—you know, in the sort of assaults that you experienced during the science wars, it's very much, you know, at least the sort of right-wing politics generally love binaries, right? And they do binaries as being a way to understand—

HARDING: So, [too] Marxism. Bourgeoisie versus the proletariat. Excuse me. I interrupted you.

KLETT: [Yes], no, I'm just—I'm trying to get you to speak more about the science wars in detail through that—maybe you get—in many ways, you get interpolated into binaristic—

HARDING: But I think that's the normal for most conflicts, that something like that happens. For me, the more interesting thing about the science wars is [not] why did they come into existence, as I indicated the other day, [but rather] why did they disappear when they disappeared? To me, that's a really fascinating question, because they only lasted—in my understanding, they only lasted about fifteen years. They began with—I mean, I started to go through list yesterday, but let me skip that.

ROBERTS: No, you had them running—when we spoke yesterday, from like '83 to '94.

HARDING: [Yes], that's right.

ROBERTS: As kind of rough bookends.

HARDING: Eighty-three to—wait a minute. *Social Text*—'83 to maybe '98.

ROBERTS: Because we had—

HARDING: Two thousand one. Let me—let me name them '83 to 2001, and add to that list I was providing.

ROBERTS: [Yes].

HARDING: So—well, wait a minute. Maybe I have to take off '83 and start with '94. Eighty-three was that *Synthese* board, but that wasn't a big public outcry. That was a private thing going on between the board and Jaakko and us. It didn't hit any public discussion that I'm aware of. I never saw any memos from it, or any—report of it was totally from Jaakko. And so, I can't really date that big science wars to then. That was my experience. And I had had—so I had had that experience.

And then I, in the same year, ’83, I had the experience with the NSF guy. But, again, that isn’t a public thing. That was my experience. But then the big starter is 1994, *Higher Superstition*, Levitt and Gross.[78] You know that book?

ROBERTS: [Yes].

HARDING: Because there’s a whole chapter on me, and there’s a whole one on Donna, in that, and that’s interesting in itself. And I suggested that the best—I suggested two analyses that are useful for understanding it. One is in Ross ’96, and that’s—here you go. That’s in here. I think the other one’s in here. So, first one, Roger Hart, who’s a historian of Chinese medicine, did I mention, who had been a UCLA graduate student? **<T: 90 min>** I had met him, but I didn’t really know him. But I think he was a student of Ted Porter’s in the history department here.

And he—okay, this is the second one. He wrote this article on the way those guys Levitt and Gross treated me. Where is he? “The Flight from Reason: *Higher Superstition* and the Reputation of Science Studies.”[79] But it’s entirely on me. It’s on how they used two sentences—their whole forty-page article on me used two sentences, took them out of context in numerous ways, and twisted them around again and again. And he keeps putting them back into the context in which they’re taken. It’s fascinating.

I looked, and I thought, wow. Thank you. And it—but the best easy set of bibliographic references is Steve Fuller’s “The Re-enchantment of Science: A Fit End to the Science Wars.”[80] Do you know Steve Fuller?

KLETT: [Yes].

HARDING: [Yes]? Okay. So, that’s why I’m going to start with *Higher Superstition*. Then I had the *CNRS* rescinded invitation, and then in 1996 came *Social Text*, the Sokal affair, and Sokal’s involved in these two books, very much.

KLETT: Can I suggest that—so like this is almost like battles in the Civil War, right?

HARDING: Yes.

[78] Gross and Levitt, *Higher Superstition*.
[79] Hart, “Flight from Reason.”
[80] Fuller, “The reenchantment of science.”

KLETT: This is a hot war. The science wars is a hot war.

HARDING: Yes.

KLETT: But it's in the context of the culture wars, which is kind of a cold war, right?

HARDING: Oh, that's interesting.

KLETT: Like to my mind—so making the—like I think it's really helpful that you're making the distinction as someone who was, willing or otherwise, a combatant in this. But there's also a larger context in which you do your work every day. Right?

HARDING: So, how do you think about that culture wars is having an effect on my work?

KLETT: Well, you know, my example yesterday of [Rudolph] Giuliani in 2015 saying—criticizing the standing President Obama for being "anticolonial."

HARDING: But he doesn't have any effect on my work.

KLETT: Well, he is part of the political apparatus which scrutinizes the NSF. He is, you know, a representative of a major political party in a time of bipartisan politics.

HARDING: [Yes], no, that's happening, and I know it's important, and you guys are going to put together how it's important. But my experience is that my career is fine. I mean, it could have been different. Could it have been bigger? I don't see how it could be bigger. I mean, I got the Bernal Prize, for Christ's sake. I mean, you know?

KLETT: Right.

HARDING: Which was a shock to me, but there it goes. So, I don't—that's out there, and I know it's in the background, but I'm not writing—my work is so widely used, as you probably know. I mean, it's just taught in methodology courses and the social sciences all the time.

KLETT: [Yes].

HARDING: I mean—

ROBERTS: So, you—oh, I'm sorry.

HARDING: You know, the sales of the book, and the sales of the articles, oh, my God, I mean, royalties are usually tiny. And so, on my first two books, the science wars—and I don't even look at those online counts of my work, right? They're still downloading *The Science Question in Feminism*, I mean, forty years later, or whatever. Why don't you read something new? [laughter] Quote me from that.

But the royalties from those first two books—so they're '86 and '91 or something like that, you know, they're pretty high, I don't know, maybe three hundred dollars a year. Well, they're both Cornell books, I think. I can't remember. I think they are. For a few years, and then they drop off. And then on my CV, as you noticed, up until '97 or something like that, I give citations of reprints of my articles, because they came to me for—they might go to the publisher, but they'd come to me for approval. But then they no longer come, because the Copyright Clearance Center, and the European one, I forget the name of it, take over. You know this, don't you? You know Copyright Clearance Center and—

KLETT: [No].

HARDING: Oh, you've got to know this if you're talking about science studies and these—I should get my income tax out so you can look at the receipts from them. Maybe I'll send it to you on email or something.

KLETT: What is their effect on your work?

HARDING: So, when I put together a syllabus now for my **<T: 95 min>** courses now for twenty years, I can just Xerox copies of articles, and—do I ask for it? How does this happen now? No, the university Xerox machines, if you're xeroxing an article, you have to pay a little something for it. I forget what it is. So, my whole syllabus is xeroxed articles, and maybe—no, that's right. The students pay. That's how it goes. Okay. So, the university makes up a course—

ROBERTS: Course booklet?

HARDING: —booklet. That's how it goes. And the students pay something for it, three dollars, five dollars. I mean, it's cheap, compared to buying books and so forth. I forget. It may be more expensive. I never buy them, so I don't know.

But that's all managed—there are two big ones. A—okay, so Copyright Clearance Center is the US one, and I forget—the ALCS [Authors' Licensing and Collecting Society] is the European one that's taking it from all over the world. And those fees go to the Copyright Clearance Center, which takes its ten percent cut, and sends me a check once or twice a year.

[My shares of those reprint fees] have now gone up to five hundred and six hundred dollars on those old books, so people are taking chapters out of those books for their course syllabi.

KLETT: Right.

HARDING: So, it's had a huge effect on the availability of any kind of written sources. I mean, it's anything copyrighted, it goes through that—you can of course use non-copyrighted work, so my grandfather's book that came out is not copyrighted. I could just distribute that to the class. So, it's a big deal. So, put in your notes to look up Copyright Clearance Center, and I'll get you the other one. It's ALCS. I can't remember. They—

KLETT: So, this is sort of how you've been tracking your impact over time, to see how active your work's been used in—

HARDING: It's a way to do that.

KLETT: —texts? [Yes]. So, I don't—you know, I don't think anyone's going to dispute the impact you've had.

HARDING: [Yes].

KLETT: But nevertheless, I'm really thinking in terms of you—

HARDING: I don't get any pushback from that larger cultural wars now.

ROBERTS: Well, so I think that's one of the—so maybe the right way to ask that question is that—the one that we hinted at a couple of times, which is what were they so afraid of? And maybe what we see is that that dissipated in part because of the larger context of the culture wars, that there was a flare-up of who you thought your enemy might be, and then you realized that you weren't?

And I think what would be fun about that is you could come up with a lot of different start and end points. I mean, so you've moved kind of from 1983 to 2001. What if you played with the idea that the start is '94 with *Higher Superstition*, where it's imagined you're the enemy.

HARDING: [Yes].

ROBERTS: And maybe I suggest that Steve Fuller's participation in the Dover, Pennsylvania, trial on whether or not intelligent design should be included in the curriculum of a local high school, is actually the end, because nobody on the science studies side comes to his defense.[81] And you suddenly realize, if you're the scientists in the room, "Oh, you're not my enemy after all." Actually, we're in—we're—you know, we have different perspectives on—on how we're thinking about the place of science in the world, but nobody's asking us to take intelligent design seriously, except this person over here, and you guys didn't defend him.

HARDING: [Yes].

ROBERTS: And maybe that means we're actually okay, and we can—so in the broader context of the culture war, we're probably actually on the same side. I thought you were on the other side, but maybe actually we're not.

HARDING: I think that's one part of it.

ROBERTS: And I—you know, I don't want to over-simplify, but what I'm saying is I think seeing the science war as something—part of something larger, might be a way of reframing.

HARDING: Okay, but I don't know—there's that, and I always have to put a paragraph on that into my articles, because that's a relativism, and it looks like, if I'm supporting indigenous knowledge, why—I must be supporting that. But in fact, for me, the more serious challenge that does not put them on the same side is already visible in this book. So, this is 2001, and Ziauddin

[81] Kitzmiller v. Dover Area School District, 400 F. Supp. 2d 707 (M.D. Pa. 2005).

Sardar is in here. He's a guy I worked with on the postcolonial stuff way back then, and by—before that, and had a—I **<T: 100 min>** told you I was going to mention this, I had—he was the founder of the Third—one of the main founders of the Third World Network, which in my earlier works you can find me referring to, and citing and quoting from.

And that was the set of East Asian intellectuals. Sardar—one of them was—became—I secured a postdoc for him at UCLA. I'm losing his name right now. [Susantha Goonatilake.] But I worked with him very closely, and he was from where they just had this huge disaster. What is it, Myanmar. He was—Burma—what's Burma's current name?

ROBERTS: Myanmar.

HARDING: Myanmar. No, it's not Myanmar. Which—where did they—the—

KLETT: Sri Lanka?

HARDING: —Tamil Tiger—

KLETT: [Yes], Sri Lanka.

HARDING: Sri Lanka. He was from Sri Lanka, this guy I'm talking about. And I think Sardar's from India. I'm not sure. He might be Sri Lankan. And Third World Network, there were six people who co-authored a little pamphlet that came out, I don't know, '95 maybe, or something. I'm not sure. I don't know how I picked it up, but it got widely distributed. And I reprinted it. Didn't I reprint it? In—did I reprint it in *The Racial Economy of Science*, or—here, I think I—I'm pretty sure I reprinted it.[82] I won't look it up now. I'll put this here.

And so, these guys, [and Vandana Shiva], were mounting very much an epistemological and science and technology attack, kind of parallel to postcolonial studies. They weren't exactly part of it, because they were focused exactly on science and technology. And so, for me, when those—the guy I had over was not a senior scholar, but he was a very stabilized one, and Sardar was senior. And they invited me to conferences in Jakarta and other places. I never went, because it was too much for me to deal with.

But I think when the science wars guys saw that the universality of Western science was being attacked, not just by crazy little hormone-crazed girls, their girls, but by senior male scholars from the third world, they were not going to get into—I mean, maybe one of them tried

[82] Harding, *"Racial" Economy of Science*.

somewhere, but they were not going to get into that, because the first response to it would be that they're racist. And that—I think they just—Alan Sokal wasn't interested in that.

KLETT: That's interesting, that being called a racist would be a stopping point, right?

HARDING: That's right.

KLETT: In the culture wars, you can—

HARDING: Being called patriarchal doesn't bother them, or sexist, hey, it's in the eye of the beholder, but racist, from—when it's senior male intellectuals at prestigious universities in the third world—they didn't attack Said. Of course, he was at Columbia.

So, that's—I've never seen anybody say that, so I'm just making this up. But it's just amazing to me that it ended. I mean, through this all is of course the normal criticisms, analyses and criticisms of my work that one expects. You know, they're by scholars in the field. They're thinking through. They look at this paragraph. This is good, that isn't. I'm not talking—I'm never talking about that. That's always there, and that's interesting. But I'm talking about these wild public performances in the science wars.

And I know the whole—you guys can put together that background of the larger culture wars. I know they're out there, and I know they affect how kids in classrooms think. But university faculty aren't—the right wing is right. The university faculty tend to be kind of left. They're not much bought into that stance. Those—that side of those cultural wars. Rudy—he'll get a job on a university faculty somewhere, but—Giuliani. So, I just don't think about the culture wars much, I'm sorry to say.

KLETT: That's okay. Could I ask you then about your—you mentioned early on your antagonisms with your brother.

HARDING: [Yes].

KLETT: And I'm wondering how you've sort of carved out your political position relative to **<T: 105 min>** his, or—

HARDING: I didn't. I was ten years older than him. I figured it was his problem.

KLETT: Uh-huh. What is his problem, in your eyes?

HARDING: It was—this is thirty or forty years ago. It was that he—I mean, then, the antagonism came because [redacted 01:45:19 to 01:45:21] he was always out of money, and I was lending him money, and he was never paying it back, and was just a younger brother, irritating kind of thing. He is a good guy in a lot of ways, and I really admire his work. I mean, he was a labor organizer. As I said, he dropped out of Hopkins, and was one of the early founders of [an important chapter of] SDS. He founded a counter-union union within the Steel Workers. When he dropped out of Hopkins, he joined—he became a steel worker on the ships in Baltimore Harbor, and joined the union, and found it so autocratic that he founded *The Spark*. You know what *The Spark* is?

KLETT: [No].

HARDING: It's the translation of Trotsky's newspaper title, right? And he became the editor of it, and he wrote it under a pseudonym. I think it was Richard—I'm trying to remember now. [. . .] And he was always attacking the union, and he was in danger. He was actually in physical danger.

Now this is the sixties, and he—I don't know, he spent a couple of weeks in prison for something or other, some kind of marching around thing. And there was a sign outside the little—on the edge of the—when you entered Brielle, New Jersey, where my parents lived, it said, "[Convicted criminals] must report to the police." And my father said that David had to report to the police before he could come over to his home. He didn't, but my father was a good liberal, and put the first Jew in the first—

KLETT: "Them's the rules."

HARDING: Well, he just kind of—he'd done—he didn't bend the rules. [He ended up having to] just kind of ignore them. We just told my father, "That's ridiculous. Don't you even think of that." And he—[laughter] it was a little low-level family dispute.

So, David—and I—so, you know, that whole political difference, I mean, I admired him. I know what he did and so forth. He couldn't understand what I was doing, because he was not—he was so committed to working with the steel workers, and women in the factory, and, you know, on the ground labor organizing. He spent his whole early life, professional life, as a labor organizer. [. . .]

What I did take up with him is how come he kept borrowing money from me and not paying any back. [laughter] Paying back [elder sister for borrowed funds] was not part of his leftie organizing strategy. And I was just—you know, he was ten years—I had babysat him, and, you know, we had this—when I went off to college, he was seven. I don't know if—at seventeen, he was seven. You know, so we had that family history.

KLETT: Well, so we only have about ten more minutes, and I thought maybe a good concluding thought would be returning to your initial suggestion that there's a regeneration of STS possible through this global perspective, and I'm wondering if you have anything you might be able to talk about in terms of this idea of ethical STEM, because this is a theme that's going through the NSF, particularly through the STS stream, as something that, you know, if we're meant to be influencing scientific practices at all, that has been sort of floated as the current—that's what we call it. We call it ethical STEM. Whether or not you and I agree with that phraseology, that is the idea that's been put out there. And at least there seems to be some sort of momentum heading towards—in STS.

HARDING: [Yes].

KLETT: As driving towards this idea of ethical STEM. So, whether or not you like that terminology, I'm wondering if you could talk about this regeneration in the context of a world where we do think of STEM as this noble pursuit, this high achievement of the sciences.

HARDING: So, of course, I don't like that language. It focuses on the **<T: 110 min>** individual in our particular world, not on the political, and not on the scientific. And so, my—the thing I like about the oldest postcolonial, anti-colonial—one—and feminists, out of all the social movements, is that they're combining an ethical political claim, outrage at the way women or Blacks or ex-colonial lives are being treated, with some factual claims. "You guys who run the world have a distorted understanding of the world, because you never look at it—you only look at it from your perspective, which is a distinctive historical one, and is not a universal one."

And so, what I like is that refusal to separate the ethical political from the fundamental driving force of modern Western science itself. It's also what makes me anxious about it. Right? because it's part of modernity, science drive.

So, I think this is a very powerful moment in STS, and I think that Lucy Suchman and Kim Fortun in particular—plenty of other people involved—have already begun to change STS—4S in very significant ways. If you look over their committees now, you'll see a number of Latin Americans on those committees, and they don't come through *Tapuya*. They're getting them on their own. They're going out and getting them.

They now report [through the newsletter] how there are 4S groups forming in Chile, in Peru. There's a meeting in Brazil. There's—I mean, I'm particularly sensitive to the Latin America, but they're doing the same thing with Africa. It's harder to do it with Africa, but it's coming.

KLETT: [Yes].

HARDING: And I'll tag that, say something about it in a minute. And of course, it's already in East Asia, because of the EASTS, for one thing, and the whole history of Indian—particularly India postcolonialism, right? Which got into a lot of those science and technology issues.

I think that they're starting off a powerful, powerful attempt to [. . .] bring 4S up into the twenty-first century, instead of leaving it back there in 1954 or '75 or whenever it was founded. So, *Tapuya* is a very powerful force in this move.

EASTS is, too, but *Tapuya*, I think *Tapuya* is [at least an equally] powerful one. [. . .] We're publishing abstracts in Spanish and Portuguese already, right? They're there. We did them retroactively for every article at the end of volume I. It's the Spanish and Portuguese. I mean, we're learning as we go, right? But that means we have to get translators, so forth and so on.

We're now offering the possibility of publishing, as I mentioned, what we're calling—I'm referring to as drafts. They're Spanish or Portuguese translations of the English articles, which allow the journal to be taught in classrooms in Latin America in kinds of ways it couldn't be before, where students maybe have enough English to read an abstract, but not to read a whole article.

And not only that, but you can say things in Spanish or Portuguese that you can't say in any other language, and that's true, of course, of any language, that there are whole ontological worlds and historical meanings. I was just pointing to the very different ways that **<T: 115 min>** positivism is kind of in the background of Latin America in a way that it isn't for the US, and that—I'm not quite sure what effects it has, but I know that they're haunted by it in kinds of ways we're not haunted by positivism. [This can become invisible in translations.]

KLETT: What are the criteria for inclusion in *Tapuya* to make sure that you're getting representation? Like—because I can imagine you could—like do you have to have a particular institution in Latin America that you're coming from? Like—so how do you make sure that you're not being flooded by Latin Americanists from American universities, for example?

HARDING: We welcome such articles. We have a very complex way of dealing this, and prob—maybe it'll change in the future. But it's the one we've arrived at now, so let me tell you why, because it's not obvious why.

We have—first of all, it's an international journal, and it—if you look at the mission statement, it doesn't say anything about how you have to be Latin American to be in it, at all. It does say all the editorial decisions are made in Latin America, which means I don't make any, and that's true. But it doesn't limit who can be an author in any way at all. And we have many authors who are not Latin American, who are English or German or—I don't know if we have any Japanese yet—who are publishing in it. And we're cultivating more. [. . .] A project we've started that EASTS is the first one we're doing—I might have mentioned—has an article by John Law and his graduate student. Ex-graduate student. [. . .] You know John Law—who John Law is?

KLETT: [Yes], sure.

HARDING: And he has a series of articles about mostly Chinese medicine.

KLETT: Right, right, right.

HARDING: And [about the value of] using Chinese medical concepts in Western medicine, rather than the reverse way that scientific knowledge is [presumed always] to travel, and they're very interesting. So, *EASTS* is about to publish, and it might be out already—I think it was coming out the end of March, but they're a little late—a third or fourth article by them. I forget what number it is. And we will have very soon responses to that article by three distinguished Latin Americans, responses to that article.

KLETT: [Yes].

HARDING: And in 2020, it'll go the other way. There'll be distinguished Latin Americans writing an article for *Tapuya*, and we'll have the links that are set up—they're set up now for *Tapuya* to *EASTS*, but we didn't get it together enough to get it the other way first, so they'll take care of that. So, we'll publish the [target] article and the comments will come from *EASTS*. So, if you—and each—*East Asian* will publish an abstract of the article in—they publish in English, as we have published an abstract—we'll publish an abstract of the article.

So, let me go back to your question. So, the three missions were that—a commitment to Latin American thinking about science and technology, anywhere that the former or the latter

occur. A lot of distinguished Latin Americans have posts in the US. You know why they have them?

KLETT: That's where the jobs are?

HARDING: Because Latin American institutions have been so politically volatile, and lack the kind of support for research infrastructure, so many of those that do—Santos teaches in Portugal, and I think he's teaching at University of Wisconsin, and so forth. [Arturo] Escobar has long been at—where is he, at Vanderbilt University?

ROBERTS: UNC.

HARDING: I think —[yes], University of North Carolina, though he does all his empirical—not all, but he does empirical research in the Colombian lowlands, and he's from Colombia.

So, that's the first criteria. The second is to look at scientific and technological practices anywhere in the world in terms of how do they affect Latin America. So, we could look at how NSF policy affects chemistry in Bolivia, or something.

KLETT: Right.

HARDING: And third, to be a center for periphery studies. And so, this *EASTS* arrangement, we're not going through the North. It's Latin American to East Asia and vice versa. We're not going through the center.

So, none of those specify **<T: 120 min>** who the author can be. Now there are two other considerations, though, that Leandro [and I] very much have [. . .] . We want a real representation of Latin Americans from Latin America, so we're doing everything we can to encourage those submissions, and that—that's on the one hand.

On the other hand, somebody has to pay for this journal, and it's not Latin Americans who are paying, because they get fee waivers. I talked about that yesterday, right? We owe fifteen thousand dollars a year to Taylor & Francis. They give back to *Tapuya* five thousand dollars to support the journal's Puebla office. But we're now already spending ten thousand dollars a year in our Puebla office, and we're about to add another five thousand dollars, so we'll be ten thousand dollars short. Where are we going to get it? We're going to get it from the APCs, open access—you know how open access works? From the APCs. It will have to be primarily from the North.

KLETT: [Yes].

HARDING: [Obviously we will sometimes have to] charge full price, which is [now up to eight hundred dollars]. But if the authors have funded research in Latin America or anywhere else any author lives or works, they can do what all the Northerners do, and put it on the bill for the funded research. No unfunded research will ever be charged for publication.

So, our criteria—it's peer reviewed. Usually, we try to have both a Latin American and a non-Latin American reviewer, but we don't always succeed, but that's our goal. So, there's a first review by fundamentally [somebody] on the editorial team. As the papers come in, they're assigned to somebody to give a first evaluation. And I insist that we give them good copy edits at this point, if we are about to recommend them for peer review. Get them a good copy edit first, if they need it.

And then they go out for peer review, and then we follow the usual procedures for whatever the peer reviewers say. So, our criteria are—you know, there's a set of questions we ask the peer reviewers. They're what any good review does. What are the strengths of this article? What are the limitations of it? What—is the article fair to those it criticizes? Is the author—is the paper engaged in any particular disputes, preexisting disputes or debates? What are new directions of research that could be pursued? And I don't know, there's a few other questions, but you get the idea.

KLETT: [Yes]. Well, what I like and why I like ending with this is because it's—the thing that's really durable here is the insistence that the editorial body be located in Latin America, because it is—in many ways, it's a summation of both your career as an editor—

HARDING: [Yes].

KLETT: —and that your approach to scholarship through editorializing or being an editor.

HARDING: [Yes].

KLETT: And standpoint, right?

HARDING: Yes. Exactly.

KLETT: You're positioning this editorial—

HARDING: You get it.

KLETT: Right. And so, there's—

HARDING: You've got it.

KLETT: —there is no technocracy. You've got onus on you—

HARDING: It's my dream—it was a dream project, when I—it first occurred to me on the airplane returning from that conference where Leandro had given me his paper. I mean, he [later] said, "Did you plan to do this, and you came down looking for somebody to do it?" I said, "It never occurred to me," until I read that introduction and conclusion of his book, and that—my own experiences had confirmed what he was saying. Then I just asked the question as a—you know, as a [request for information]. For all I knew, there was a journal already. I mean, I didn't know what Latin American journals existed [in STS. Though, on reflection, I probably would have heard of one if it did exist in English].

But the minute he said, "That's an interesting idea," I thought, "Oh, my God, there's something here that could happen, but I've got to be pretty careful about how we do it."

KLETT: I would say for a philosopher, and I know you have a reluctant relationship to that discipline, but for a philosopher to instantiate or to materialize—

HARDING: Yes.

KLETT: —that—a project—

HARDING: Isn't that terrific?

KLETT: It's pretty rare, right? Like where else do you get that opportunity to do that?

HARDING: Well, any journal being founded, they're doing something or other with it [that they think important], right? They're—the analytic ones are fighting off the continental beasts, or vice versa, and so forth.

KLETT: [Yes], it just—it feels like the journal is like—it's just a bunch of paper. Like the—it—

HARDING: [Yes], this has—

KLETT: —the stress is placed somewhere else in the journal.

HARDING: Right. No, this—this is full of contradictions. And we keep [visible] the contradictions that we like exactly—I mean, it's a Latin American journal published by Taylor & Francis. And I'm doing a lot of the infrastructure. But I'm not **<T: 125 min>** making editorial decisions about what gets published, what's important. I mean, Leandro and I talk, and, you know, but it's always his word. It has got to be, right? Because I know I don't get what's going on [in Latin American thinking in many respects].

And—what was I going to say? [Yes], it's just—oh, the contradictions. You know, and we've been—not I personally, but the journal and some of the editors have been attacked, A, for publishing in English.[Luis Reyes-Galindo, one of the associate editors], got called a traitor to Latin America at a conference on Latin American studies somewhere on the Tex-Mex border, some school that had a lot of Latin American studies going on into it. Luis, because he—because the journal was publishing in English and was published by Taylor & Francis.

KLETT: Right.

HARDING: You know, so it's hard for Latin Americans—and the fact that it's funded primarily by UCLA, a little bit from Leandro's university, but mostly by UCLA, they are not used to a funder having no say in what goes into the journal. They're—it's totally outside their experience. And none of these units at UCLA have the slightest interest in having anything to say about it.

KLETT: That's good. That's nice.

HARDING: Well, I mean, I don't know how many journals are funded out of UCLA. I don't know, fifty, sixty, eighty, [two hundred]? I don't—you know? You've got a lot of journals coming out [of all of the departments and professional schools].

KLETT: Well, unfortunately, we're over time. Do you have anything lingering, or should we give Sandra the last word?

ROBERTS: No. Last word is great?

HARDING: It just—it's been wonderful. You've made me think in really different ways, not only about my own work, but the kinds of questions you're asking that I can't answer, and don't even know how to begin answering. You guys are going to have to—I just, again, I'll put in my—I know you want to—I know you have your particular formats you like, but the shelf life of print work is going to be great, always. So, I hope some of this wonderful work gets to—

KLETT: Thanks for accepting it.

[END OF AUDIO, FILE 4.1]

[END OF INTERVIEW]

PUBLICATION LIST

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Translated into

1986 *The Science Question in Feminism.* Ithaca, N.Y.: Cornell University Press; London: Open University Press. Tr. German, Spanish, Korean, Bahasa.

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Essays: More than 100 in diverse journals and anthologies (1973-2019). Many republished multiple times in anthologies edited by others. Translated into Spanish, Korean, Bahasa (Indonesian), German, Chinese, Macedonian, Arabic, Japanese. Plus more than a dozen book reviews.