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JHET INTERVIEWS: E. ROY WEINTRAUB

BY
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The following is a transcription of a 2019 conversation with Duke historian E. Roy Weintraub about his intellectual development over the 1980s from mathematician to economist to historian. The conversation also explored Weintraub's early and continuing attempts to forge new ways to study the history of contemporary economics, and the role of science studies in providing a natural language for such explorations. A French translation has already been published in the journal Zilsel: Science, technique, société.

"I HAD ALWAYS OPERATED ON THE OUTSIDE": A CONVERSATION WITH E. ROY WEINTRAUB ON THE HISTORY OF ECONOMICS, SCIENCE STUDIES, AND ACADEMIC MORALS

History of economic thought (HET) is still, in the eyes of science historians and Science and Technology Studies (STS) researchers, a relatively estranged field. One of the reasons for this is that, as Roger Backhouse and Philippe Fontaine (2014) have shown, the field itself suffers from a long-standing identity problem. Formerly practiced by economists themselves as an integral part of their attempt at theorizing—one thinks, for example, of the work of Jacob Viner on the theory of international trade—this method of investigation has gradually become marginalized in economics. It is in response to this marginalization that in the second half of the 1960s the first professional conferences in HET were organized in Europe and the United States. The field's increasing professionalization was consecrated in 1969 with the creation at Duke University of the journal *History of Political Economy* (*HOPE* hereafter). What was missing, however, was a form of methodological unity that would confer scientific legitimacy to it. At that time, scholars such as A. W. "Bob" Coats, Donald Winch, Warren Samuels, Mark Blaug, and Craufurd Goodwin—the latter serving as *HOPE* editor for its first forty years of

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existence—sought to convince historians of thought that it was necessary to create knowledge that is relatively independent of knowledge in economics, while affirming, at least for some of them, that HET should continue to communicate with practicing economists and should not separate itself from its mother discipline.[1] If, to do this, some, like Coats, chose a sociological path inspired by Robert K. Merton, or others, like Winch, proposed an orientation of research closer to intellectual history, the most favored strategy consisted in making appeal to the epistemology of science, in particular Karl Popper, Imre Lakatos, and, to a lesser extent, Thomas Kuhn, as a method of evaluating a particular field or tradition in economics.[2] Meanwhile, particularly in Europe, Australia, and Japan, economic traditions marginalized by the rise of mainstream economics found refuge in HET. By analyzing contemporary theory in the light of Marxist, Austrian, or post-Keynesian theories, for example, it was hoped to highlight what modern economics had "missed."

It is these two traditions, both distinct and objectively associated in their desire to contribute critically to economic theory, that E. Roy Weintraub, a mathematician and economist turned historian of economic thought, harshly criticized. For Weintraub, an evaluative history of economic analysis can only be futile, and moreover not conducive to arousing the interest of neighboring social scientists and humanity scholars. The role of HET can therefore only be to explain or understand the evolution of economic knowledge by placing it in its social, cultural, or political context, not to justify or condemn it. In the late 1980s, Weintraub, who had already been at Duke for nearly two decades, became associate editor of *HOPE*, a position he still holds today. It was as an editor that he was to intervene the most in the community of historians of thought, seeking to promote the history of science in the pages of this journal—more particularly that which is situated in the perspective of science studies—as a way of conceiving a history of economic thought that would be less internalist and closer to what is published in journals such as *Isis* or *Science in Context.* Through symposia or special issues, he brought terms such as "constructivism," "interpretive communities," "gender," "material history," or "performativity" into a field that still somewhat resents this allegedly "postmodern" jargon.

Because he did all of this with a pronounced taste for controversy and a style that could be at times considered as incisive to those whose approaches he considered absurd or obsolete, Weintraub has earned himself a reputation as a tenacious and brash slayer of traditional HET and economic heterodoxy. However, those who know him well can testify to his great enthusiasm and support for the sort of works that stick more readily to the vision he has set for the history and sociology of science, particularly those of the younger researchers who have been visiting the Center for the History of Political Economy at Duke University for the past fifteen years or so. It is in this very place, more particularly in his office filled with books on economics, postmodern theory, and science

[1] For a historical appraisal of the founding of *HOPE* and of Goodwin's editorship, see the several articles in the special issue of *HOPE* in his memory (volume 51, issue 1, 2019). Goodwin (1998) offered some reflections on his editorship of *HOPE.* See also Giraud (2019) and Edwards (2020) for an analysis of the history of *HOPE* since its creation.

[2] For a good externalist account of the changes in economic methodology, see Breslau (2005). For an overview of the evolution of the history of economic thought from within economics, see Goodwin (2008), and from outside our community, see Fontaine (2016).

studies, among other things, that we met with him, on April 8, 2019, at 2:15 p.m. for a two-hour interview.

FROM IMRE LAKATOS TO RICHARD RORTY: THE JOURNEY OF A HISTORIAN OF ECONOMIC THOUGHT

Giraud: I am at the Duke University Center for the History of Political Economy. It is the 8th of April, 2019, at 2:15 p.m., and I am with Roy Weintraub in his office.

I want to talk mostly about historiography and your role in changing or at least trying to change the historiography of economics. I might be wrong and you will correct me, but my feeling is that something important changed in your writing and that this turn takes place between 1985 and 1989. In 1985 you have this general equilibrium book [Weintraub 1985] and the title says it all, it's *Studies in Appraisal.* And by 1989 you write the "Methodology doesn't Matter …" paper [Weintraub 1989]. So there is clearly a dramatic change in focus in-between those two pieces. So what happened?

Weintraub: The 1985 book was based on—developed out of—the long-term project of appraisal of general equilibrium analysis, which I had been doing more or less since my doctoral dissertation in general equilibrium theory in 1969. But I started thinking more seriously about appraisal after I had been involved with methodology questions as part of a Lakatos study group at the University of Bristol where I was visiting for a year in 1971–72—that group had recently gotten the *Criticism and the Growth of Knowledge* book by [Imre] Lakatos and [Alan] Musgrave [1970] and we worked through that—that was based on [Thomas] Kuhn; we all had read Kuhn; we knew Kuhn—but this was a new kind of question. So we began going through that book, becoming fascinated by this way of thinking about the development of economic theories and economic analysis. The question for me was whether general equilibrium theory was associated with knowledge claims and with anything progressive in the Lakatosian sense or was it simply a foolish endeavor, as a number of people were arguing at that time.

Giraud: So this was before you came to Duke?

Weintraub: Yes and no. I came to Duke in 1970. In 1971 when I went to Bristol, Neil DeMarchi came to Duke and he sublet my Durham house, so we became friends. And when I came back in 1972, Neil was here. He had by then written his piece on Lakatosian analysis of the Heckscher–Ohlin theorem and the Leontief Paradox for the Nafplion Conference organized by Spiro Latsis. So we were on the same wavelength. After Neil's arrival at Duke, Craufurd Goodwin as *HOPE* editor became fascinated by the Lakatosian ideas, and certainly Mark Blaug did as well since he had participated in that conference, as did Bob Coats. And Coats and Goodwin and DeMarchi were all fairly close. So Goodwin was fascinated by these ideas and very quickly he began supervising the graduate students he had, asking them to do history of economics from an analytical perspective. He wanted to have the students adopt a theory of the growth of knowledge and look at some theory or area in economics and have them appraise it using Kuhn or Lakatos or something like that. Goodwin supervised most of the history of thought PhD students—I wasn't supervising any in history at that time—and Craufurd was kind of influential, though of course I was on every doctoral committee so I was seeing how Goodwin was operating with this. I was only somewhat comfortable with it, but I didn't have any reason for being uncomfortable.

Giraud: This was around this time that there was increasing interest in methodology, right?
Weintraub: Blaug's book on the methodology of economics [Blaug 1980] came out about that time. I don't know the exact date—let's see [takes the book off his shelf and turns pages]—1980. So that was sort of toward the end of that period, but it was all part of it. Blaug used Lakatos to describe—as a framework for considering, almost—a large number of subfields in economics, trying to answer the question: Were they progressive? Were they degenerative or what? I thought what Blaug did in general equilibrium theory was stupid. He didn't seem to understand what he was doing. In that period, I was still writing technical papers and submitting them to economics journals and getting them published. But as a sideline, I was talking with DeMarchi and trying to think about how one talks appropriately about general equilibrium theory. About that time, I was also asked by Bob Clower to review the [Kenneth] Arrow and [Frank] Hahn *General Competitive Analysis* book for the journal he was then editing, [Weintraub 1974] *Economic Inquiry.* So I re-engaged with the discussions about what is general equilibrium theory, does it work? What are the problems?

I started writing using Lakatos's *Proofs and Refutations* [1976]. It seemed like an interesting kind of exercise, no one had done it before. I wasn't thinking about publication, and started to do it as dialogues, conversations between a professor and students, using the kind of dialogue method that Lakatos used in *Proofs and Refutations.* I was producing some of these for my amusement. I didn't have any idea about publication at that point. At some point in that process, I realized that people were making these sorts of historical claims about general equilibrium analysis—What do we know about the history of this Arrow–Debreu model that everybody was complaining about? It was wrapped up with heterodox economics, with post-Keynesian attacks on neoclassical economics and all that stuff.

Giraud: It seems that the only historical discussions of general equilibrium theory took place in the beginning sections of theory papers, like reviews of the literature.
Weintraub: Exactly. Well, what do we really know about Arrow–Debreu? I started doing some reading about it and quickly realized that there was a lot that could be learned. So I started mostly out of curiosity. I started writing letters to people, asking what they knew, because I had some accounts and these accounts didn't seem to make a lot of … they weren't very coherent. There weren't a lot of them. So I started writing detailed letters to all sorts of people who were involved in it, many of them through Cowles. I was quite shocked when they started writing back to me with lengthy letters describing what they knew, how they knew it, who was doing what and when, and this was about the same time that Arjo Klamer was a graduate student with us. Neil and I were jointly supervising him. And Klamer—this would have been early 1980s—about that time Klamer wanted to do a dissertation on what is the new classical economics, how is it different.

And I said to Arjo, "You know these people you are writing about are still alive. Why don't you go talk to them?" He got a grant of some sort from our graduate school and he went around talking with [Robert] Lucas and [Thomas] Sargent and [Neil] Wallace and all of those people. And that ended up as his book *Conversations with Economists*

(I think that was the title of it).[3] We were doing, sort of stumbling toward, a kind of oral history way of thinking about writing histories of current economics. So I got involved in writing the kind of history of general equilibrium theory using what people told me, using what sources there were, using the primary materials as they were written and what commentaries I could find around them.

Giraud: Writing history seems to have been rather a new challenge for you. Was it a difficult transition?
Weintraub: It didn't seem so at the time. It was a time specifically in 1981–82 when I'd been elected head of the university faculty at Duke. So for the first time in my life I had a second office and I had my own secretary sitting outside that office. I could write something in longhand in the morning and it would come back to me typed that afternoon. And I could spread out all my materials on this large conference table because I only used that for meetings like twice a week. I kind of felt I could get all of the evidence and materials out in front of me and I could start writing. And getting it back and editing, I didn't have to use scissors and Scotch tape and yellow line paper. It went much quicker than I thought, and I was able eventually within a fairly short period of time to do a history. I had earlier done a survey piece for Mark Perlman of the *Journal of Economic Literature* [*JEL*] on "Microfoundations of Macroeconomics" in the mid- to late 1970s [Weintraub 1977]. Mark was no longer editor at the *JEL* but Mo Abramowitz was, so it was about that time I proposed to Abramowitz that since this is a literature and it hasn't been talked about, how about if I shape what I'm doing into a survey piece for the *JEL,* and he encouraged me. We had about three or four iterations and it eventually came out in the spring of 1983 [Weintraub 1983].

Giraud: Did you ever present this material in a seminar, say, or a conference?
Weintraub: Not exactly, but right before I had a final draft, in fall of 1982, Bob Clower and Axel Leijonhufvud invited me out to UCLA [University of California, Los Angeles] to teach a half course with Clower on general equilibrium theory and monetary economics. So I went out there and I had a very good audience. I finally had a lot of really smart people who knew something about the theory. They were very supportive of what I was doing. So I felt encouraged by that and that it could actually inform some kind of current discussions.

Giraud: [Deirdre] McCloskey began writing about rhetoric in the *JEL* around then, correct?
Weintraub: I don't know if my paper was exactly in the same issue of the *JEL,* but it was approximately the same issue where McCloskey's piece on economics and rhetoric ["The Rhetoric of Economics"] came out. Might have even been the same issue of 1983. I was fascinated by that.

Giraud: Yeah.
Weintraub: Arjo's book on conversations had fascinated McCloskey and it ended up—because McCloskey was doing rhetoric, Arjo was talking about conversation and so on. So Arjo ended up connecting with McCloskey and either he went off for a post-doc year at

[3] See Klamer (1984).

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Iowa when McCloskey was there or something like that. Then Arjo ended up at Wellesley. In that period of time I had been doing very standard straight, even Whiggish, history.

Giraud: Yeah.
Weintraub: Not any kind of interpretation as much as trying to bring the pieces together. About that time—it was after my *JEL* piece had appeared, after McCloskey's piece had appeared—Arjo came back to Duke to visit—he had friends here from graduate school and DeMarchi and me or maybe it was just me. It must have been just me because DeMarchi was in Amsterdam around that time—he had this half-time appointment or maybe full time or whatever appointment. I had a bit more time because I became chair of the Economics Department in July of 1983. Arjo came back and we were having lunch and he pulls out this large bag. We're talking history or whatever and he comes with this bag—he hands it to me and says, "Don't talk to me about history until you've read these." I said, "What is this?" And he hands me a bag consisting of [Richard] [Rorty's 1979] *Philosophy and the Mirror of Nature.*

Giraud: Oooh.
Weintraub: And [Latour's 1987] *Science in Action*, and what was the Bloor book, I forget which Bloor book, but it was on Wittgenstein and mathematics—it might not have been Bloor. Certainly [Nicholas] [Rescher's 1979] book *Peirce's Philosophy of Science.* Oh yeah, [David Bloor's 1983] *Wittgenstein: A Social Theory of Knowledge.*

Giraud: Okay.
Weintraub: And so he says, "Read all of these and then we'll talk. We can talk then."

Giraud: [Laughs]
Weintraub: I mean, this is not a graduate student faculty exchange, but this was Arjo, this was how he operated. And I said okay. So I started reading and I was fascinated. It was connected to some additional things. Since I was being named chair of the Economics Department there was also a new chair in the English Department—Stanley Fish. Stanley and I got to know one another through chairman's meetings.

Giraud: That was what I was going to ask.
Weintraub: That's exactly that period. I started reading Stanley as well. Stanley had this phrase he used: a "Kuhn–Rorty Fish." That this was the turn in sort of critical studies—Kuhn, Rorty, and Fish. So, of course, I started reading Fish as well. Like his book *Is There a Text in This Class?* [Fish 1981] and so on and so on.

I was doing all this reading—Barbara Herrnstein Smith came to Duke about that time. And Stanley, after she arrived, would tell me—he and I would be talking—"I don't have as much to say to you but the person you really should be talking to is Barbara. You should let me introduce you to her." She had finished the book *Contingencies of Value*, she knew some economics and so on [Smith 1988]. So I began talking to Barbara. I don't know what year she was out at the Humanities Center. I don't think that came until later. At any rate, I was getting into that network and there were people here at Duke because of Stanley. It was a quite extensive collection of people at Duke University. There was Fred Jamison; there was a guy I became good friends with in the School of Divinity—Stanley Hauerwas—who were all doing this kind of work. They all had read these canonical texts and could speak really well about these issues, and I found it all fascinating. And as department chair, I was able to function in this … in a larger … outside the Economics Department environment.

Giraud: Was DeMarchi moving in the same direction?
Weintraub: No, DeMarchi was not doing this at that time. He was going back and forth to the Netherlands. At that point he had resigned at Duke, in fact. He gave up his tenure to go back to the Netherlands to become research director of the ABN Bank. While I was chair I persuaded him to come back to Duke. He was involved with his very complicated family situation—his first wife was Dutch. He came back here but he hadn't had those few years of this kind of reading and so … but he was moving in different directions anyway at that point, writing on the history of econometrics and beginning to write in art history.

Giraud: But DeMarchi retained his interest in history and methodology, did he not? And you and he continued to talk about these ideas.
Weintraub: Of course. The next thing that happened was that the person in Amsterdam who had the methodology chair was retiring: Joop Klant, J. J. Klant. DeMarchi was going to take over that chair on a half-time basis. Earlier he had brought Klamer over to the US and taught Marcel Boumans and Harro Maas, and all of these people, and a bit later Esther-Mirjam Sent—all of them came to do PhD work really through DeMarchi's influence. So he was in Amsterdam then on a more or less part-time basis. He was in charge, together with the local person whose name I forget, of putting together a conference honoring Joop Klant on his retirement. That conference was in December of 1985.

[Movement] You've seen this but I don't think you know it. [ERW pulls down from the wall a photograph titled *Economic Methodology: A Symposium in Honor of J. J. Klant.*] Pictured are Wade Hands, Dan Hausman, Bruce Caldwell, Arjo Klamer, Jan Kramer, Mary Morgan, Bart Nooteboom, Bert Hamminga, Blaug, McCloskey, Jack Birner, Klant, DeMarchi, Jan Pen, Weintraub, and Terence Hutchison.

Giraud: Okay.
Weintraub: This was the first time we had ever met one another.
Giraud: Yeah? Okay.
Weintraub: I met Mary for the first time there. I met McCloskey for the first time there. I met Caldwell for the first time there, Wade Hands and Blaug. This really was the nexus of the economic methodology movement. We had the philosopher Dan Hausman and so on. This was the start of it. It was quite formative. Now, I had done the paper then chapter in the 1985 book where general equilibrium … I appraised it using the hardening of the hard core and so on. And I had questions about … well, what about excess content in the Lakatosian sense and blah blah blah and what about empirical work? So I set myself for that conference to give a paper on the empirical content of general equilibrium analysis and it was … I think I called the paper "General Equilibrium Analysis Is Empirically Progressive" or something like that.

Giraud: So at this point you are still doing history but through methodology; you are still doing Lakatosian appraisals, right?
Weintraub: Yes. I gave this paper to try to establish progressivity, empirical progress as opposed to theoretical progress. I gave the paper and in the comments McCloskey came

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at me with, "Okay, so what? You've established that according to this framework, which defines x, y, and z, that general equilibrium analysis is x, y, and z. Now what? Don't you think that it's …" and then he used the phrase, "a procrustean bed on which you're trying to—on which you're fitting some work in economics? You're going to cut it and shape it and make it fit that."

I thought about that and was bothered by it.… Yeah, that's what I was doing. I was using this philosopher's framework to organize thinking about the growth of economic knowledge, but there was nothing intrinsic about it and I was both puzzled and I felt that McCloskey really had a very good point. Is there another—are there other ways to start thinking about this? So I started thinking about this, using all of these other new … this new literature that I had been reading, and I was bothered and wanted to see, well, could I employ that to talk about some work in general equilibrium without imposing a new framework on it? But use it … because it didn't seem like a framework to me as much as a vocabulary and a grammar for organizing things. It wasn't a philosophical system.

Giraud: Yeah.
Weintraub: And right at that time McCloskey and Klamer decided to put to the test the economics of rhetoric ideas by inviting a large number of economists to a conference to talk about the consequences for economics of the rhetoric move that McCloskey had engineered. McCloskey, of course, believed that attention to rhetoric could change the way economists were doing economics. The conference included people like Bob Solow as one of the co-organizers. He came to Wellesley. Frank Hahn was there, Wade was there, Bruce Caldwell was there, I was there, [Philip] Mirowski was there and this I think was the first time I'd ever met him. McCloskey had been hanging out with literary folk, and had invited Stanley Fish to talk. I was the only person up there whom Stanley Fish knew. So he and I were hanging out together. I remember we went out drinking one night. Stanley was at that point in his own career beginning to take on the major players in a number of the new moves in literary theory—Marxists, feminists, Freudians, and so on—regarding these as metanarratives that shape how we thought about these texts and those texts and so on. And Stanley had this powerful point that there is no position outside the texts that can modify … that have consequences for, interpretations of the texts. It was part of his argument against interpretation, but it was part of a dialogue/controversy he had with Walter Benn Michaels at Johns Hopkins about the consequences of theory.

Giraud: Okay.
Weintraub: For those whom Stanley was attacking, those folks believed that theory had consequences. That looking at Jane Austen from the perspective of feminist theory—there were consequences to doing that. Stanley's argument was that all the theory was drawn from the local ground … from the texts themselves. So that all you could do would be to use those texts to create a metastructure, which then you took back down and it was totally self-referential. It was circular reasoning and his basic argument was there could, as a result, never be consequences. Theory has no consequences in that sense.

Giraud: So the conference really had a lot of inconsistent papers, lots of disagreements, it seems.
Weintraub: Yes, well, that blew the conference apart. Here McCloskey and Klamer thought they had invited Fish to provide support to their view that rhetoric had

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consequences and here Stanley Fish, who they couldn't argue was a lightweight, was coming into their conference and saying, "No, rhetoric can't have consequences. Nothing can have consequences like you're thinking of them. Of course rhetoric is interesting. It may be important if you can get a lot of people in a community to say they like working in this way and thinking in that way but there's no necessary connection. It doesn't have consequences in that fashion."

Hahn and Solow didn't know what the hell was going on. Others were sort of just confused. I mean, Bruce had never read any of these literatures. I don't know what he was thinking. Mirowski had come over from Tufts and gave a paper in which he used a series of rhetorical devices to attack neoclassical economics. The conference was a mess and it took a number of years for that book to get put together [Klamer, McCloskey, and Solow 1988]. And I think I was the only one there for whom that conference had consequences!

Giraud: What do you mean?
Weintraub: I gave a paper there, which was my attempt to see how the rhetorical structure of— rhetorical devices associated with—the idea of equilibrium might work. The point of the paper was that there are two different ways of talking about equilibrium and each of those two ways had distinct metaphors associated with them and so on and so forth. I think I called that paper "On the Brittleness of the Orange Equilibrium" to make sure that nobody understood what I was talking about. What do you mean, "orange equilibrium"? Well, it's a description of an equilibrium and … So anyway, at that point I was beginning to play with these kinds of ideas and as I was doing that, I was getting farther away from being happy with methodologists and their thinking, and using philosophy—good old standard methodological arguments—like when Blaug said we'll look at Kuhn, we'll look at [Paul] Feyerabend, we'll look at Popper, we'll look at Lakatos and we'll use their ideas to talk about economics. That kind of work seemed to be used at that point increasingly, as I saw it, to provide critiques of doing economics. Those people were doing methodology in order to say that in a normative sense this stuff—general equilibrium—wasn't good economics. This wasn't good economics. That wasn't good economics. They were saying that methodology had real consequences for the doing of economics. I thought that those kinds of arguments, coming from heterodox economists mostly, were historically uninformed and they were engaging with a literature they thought could be used critically.

Giraud: Yes.
Weintraub: And I didn't think that the history of economics could be used in that fashion. It was not making any sense. I had come through Lakatos and I had seen how that was used and how I was trying to use it in the early '80s. The Wellesley conference was in spring of 1986, okay? So this was in that period of time.

Giraud: Does Fish have a paper in that volume?
Weintraub: Yes. Yes.
Giraud: So it did make the cut.
Weintraub: Eventually. I think he did. Let's see … Fish. The philosopher Christina Bicchieri, Resnick, Wolff, Coats, "Comments from Outside Economics" by Stanley Fish. This was 1986.

Giraud: It seems to me that you were sort of in-between in your thinking at that time. You were moving away from methodology, but you were not attracted finally to McCloskey's moves to rhetoric. You had been reading the new materials in science studies, but they had not really found their way into any of your writings. And you were chairing your department. Do I have this time sequence right?
Weintaub: Exactly. But in spring/summer of 1986 I separated from my wife. This is not unconnected to the larger story. In fall of 1986 I got invited to go back to the University of Bristol for a month, May of 1987, as a visiting scholar. Now, during the year-long period of marital separation, I wasn't able to write anything since I had too many personal issues being dealt with and engaged with. So I couldn't go out of town to conferences or anything because of issues of child custody and so on and so on. It all ended—got settled—two days before I left for England in the beginning of May of 1987 and I took up this position as a visiting professor at the University of Bristol for a month.

It overlapped with the conference organized by the Scandinavian Economics Society and the *Scandinavian Journal of Economics* celebrating the seventy-fifth anniversary of the journal. That was to be held in Helsinki, and I was invited.

Okay. Since I was in too much personal distress to be able to write anything in advance, I used that period at Bristol to write a paper, which was to reflect what I'd been thinking about and where I was trying to move intellectually in that period. And that paper that I gave in Helsinki I called "Methodology Doesn't Matter but the History of Thought Might," which reflected exactly my thinking at that time. Now, the main argument I will defend to this day. The details of it, you can legitimately quarrel with a number of different pieces of it. It was my attempt using Fish and some of the things I had been reading to justify, as it were, to ground my view that methodology doesn't matter in the sense that methodological arguments can't have consequences for the way we do economics. That was the substance of the argument. I was sure I was right, but it was a period when I didn't have access to a lot of the sources, sitting in an office at the University of Bristol as a visitor, to get access to the library system, all this was pre-Internet, access to all of these things. I wrote the piece mostly so I could go to the conference and get away from my personal turmoil—Finland was a long way from North Carolina—and it was fascinating. The conference was all economists—Solow was there; Seppo Honkapohja, Richard Layard, Jean-Pascal Benassy were there—well-known economists. It was the celebration of the Scandinavian so in the evening the Swedes were trying to outdrink the Danes who were trying to outdrink the Finns, you know. It was all a lot of fun. I duly gave my paper and nobody knew what to make of it, but it was okay. They agreed to publish it and I had a great time.

Giraud: Yes, it made it to the *Scandinavian Journal of Economics.*
Weintraub: Exactly. Exactly. I wish—it seemed like as good an outcome to get it published since I had no idea where else those kinds of ideas could ever find a home. And it seemed to turn into a *cause célèbre* among methodologists who got very irate—methodology doesn't matter? Uskali Mäki, whom I had good relations with before, then wrote a paper that had a title "Methodology Might Matter, but Weintraub's Meta-Methodology Shouldn't" and so on [Mäki 1994]. That takes it up to …

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BRINGING TOGETHER THE HISTORY OF ECONOMIC THOUGHT AND SCIENCE STUDIES: THE ROLE OF THE ASSOCIATE EDITOR

Giraud: That could bring hundreds of questions but what's kind of interesting—so, to use your term, that you should find "adequate"—your turn to a more constructivist kind of history became overdetermined because you had this influence of Klamer bringing you the science studies literature, Stanley Fish being here, and all these various events, but the way that you are describing that in terms of belief and resistance, to use Barbara Herrnstein Smith's [1997] phrase, seems to imply that you are not very resistant to new beliefs. It seems that you were convinced pretty easily by this constructivist stance, which may seem to be surprising, knowing that you were a hard scientist economist, right? You did some very hard science stuff. What convinced you exactly in this literature that the way to approach historically this topic that you had worked on as a theorist had to be completely different? That as a theorist you could be a complete reductionist and as a historian—studying economics—you had to become like someone else?

Weintraub: Don't underestimate the impact of my not having been trained as an economist. I was not socialized as an economist. I didn't know graduate students in economics. I hung out with mathematics students. I was in the mathematics graduate lounge hanging out, not with economic students. My first job, the assistant professor of economics at Rutgers, we'd go and have lunch together—I didn't have any idea what my colleagues were talking about. It was like they were from outer space. I didn't have any notion of what they were—I mean, they were nice people, but I couldn't understand why they were thinking what they were thinking, why they were defending positions that they were defending, why they assumed this or that. My connection to economic analysis as something I was socialized to was weak.

The second component of that was my family. My father was adamantly opposed to mainstream economics so I had that double loading of that. It was easier to say to myself … it wasn't unnatural to say, well, I need to think things through but for myself—I wasn't wedded to anything. I didn't have to overcome a lot of resistance from my past—the socialization. That's the basic answer.

The other component to it is that compared with most of my college classmates, of the twenty-one graduating mathematics majors, I was the only one to graduate in the humanities division. The others were all majoring in mathematics and minoring in physics and biology and chemistry. Unlike them, I was taking courses in philosophy, English literature, history, as well as mathematics. I wasn't so wedded to that particular path, either. It was kind of an outsider's perspective and I felt that as well in economics. The philosophy part of my work in the '70s had linked up with DeMarchi and the little group at Bristol and so on, but that was what I found fascinating about economics. It wasn't the technical problems. I could do them and I kind of understood what those people were doing—I could do it, too. But it wasn't a matter of having a transcendent desire to solve such and such problem in economics. It was quite loose. I'd always operated on the outside. I look at the mailboxes here in the Duke Economics Department. I mean, the secretaries find it amusing. I subscribe to the *New Yorker*, the *London Review of Books*, the *New York Review of Books*, the *Times Literary Supplement* … hardly anybody in the Economics Department even reads books. So there wasn't as much resistance to my thinking in different kinds of ways. That's the closest I can get to it.

Giraud: Okay, so it's 1989 and that's when you became associate editor of *History of Political Economy*, right? Did you immediately have the idea of using this position as an opportunity to draw *HOPE* readers' attention towards the type of literature that interested you? Was it a way to shake up the community or did you just want to broaden the horizon of the history of economics?
Weintraub: It's a combination of both. I wouldn't, though, frame it strongly as either. Right at that time, and it was in October of 1989, Mark Blaug and Neil DeMarchi decided to hold a conference in Capri to reassess Lakatos … 1989 … that would have been approximately twenty years after the original Nafplion conference and of course I was invited to it. I get there and I'm beginning to write in this new fashion. And through my connection with Barbara and Stanley and my own reading, I persuade DeMarchi to invite Harry Collins and Karin Knorr Cetina to the conference to talk about these kinds of ideas. And Nancy Cartwright. So all of the methodology people were there. There was Kevin Hoover and Wade and Mirowski and Nancy Wulwick … it goes on and on—you can find out who those people were from the book *Appraising Economic Theories.* The book appeared in 1991 [DeMarchi and Blaug 1991]. October 1989 was when the conference was in Capri. Bruce Caldwell, Collins, Chris Gilbert, [Bert] Hamminga, Maarten Jansen, Jan Kregel, Marjorie McElroy, Uskali Mäki, Vernon Smith. So I give my paper. It's pretty clear that there is an undertone at the conference of a strong minority group who don't think Lakatos is worth talking about. There are two major results of that. First, there were several people there who had begun doing Lakatosian analysis based on my attempting to define the hard core of neoclassical analysis and following that almost as programmatic—looking to do what I did but in other areas. Wade Hands and Roger Backhouse were chief among them. Then, there were those who were doing science studies effectively.

Giraud: It sounds like the conferees did not all share the same agenda.
Weintraub: Wade and Roger were apoplectic. They were very angry at me. It was like I had led them down the garden path with Lakatos. And they were angry that I had sort of abandoned them or abandoned this work that they had begun investing in. So there was a lot of criticism of that. Blaug was so angry at the whole conference that he refused to do a joint introductory paper with DeMarchi as co-editors. So Blaug did the introduction and DeMarchi did an epilogue. Moreover, Blaug refused to allow the papers by Harry Collins and Karin Knorr Cetina to appear in the conference volume. So we get back home and at that point and as you just said, I am an associate editor of *HOPE.* So I created a mini symposium and got the Collins and Knorr Cetina papers committed to *HOPE* and I think I wrote a little introduction of some sort. I was both critical of the standard ways of operating and very enthusiastic that there were a lot of interesting questions that could not have been asked before that now could be asked. There's a vocabulary. We're able to be talking about some of these things. And so coming to the journal … or having more connection to the journal, it's never … you know, I'm here … it doesn't hurt. It's like Craufurd at some point scratches his head and says, "Yeah, why doesn't Roy hunt down …, " that kind of thing. I felt from that point on that there were interesting conversations to be had. There were some people in the history of economics who were talking in ways that I found much more interesting, engaging, than going to the standard history of economics meetings

and listening to yet another paper on [Léon] Walras's monetary theory or this particular obscure institutionalist author or [John Maynard] Keynes precursor at such and such a time. I hadn't found those interesting before and now I was finding an actual alternative so that I could find history of economics and doing history of economics interesting because it was real history.

Giraud: So you are beginning to think that most history of economics was not what you call "real history"?
Weintraub: I was reading all kinds of other things because right at that time in 1988–89, I'd been accepted to go as a Fellow for the year to the National Humanities Center [NHC]. National Humanities Center, okay? This is right out here in the Research Triangle Park and there are historians there. And there are people in English literature and history of science and feminist studies and so on and I'm with them every day, every lunch, talking with them, learning. Learning about different kinds of ways of thinking about history. A good friend likewise visiting at the NHS, much older than me, was John Higham, a very distinguished Americanist, and when Nell and I got married, his wife, Eileen, whom Nell worked with twenty years earlier, Eileen was the matron of honor at our wedding, which we actually held in fall 1989 at the Humanities Center. So you know I was very connected with John and I asked John, "Can you point me to some stuff to read on historiography and how historians think?" So I was getting all that stuff. There were a number of feminist folks out there and in English literature.

And so I was reading Joan Scott. I was part of a study group on—what's his name? At that point he was doing … Stephen Greenblatt on the new historicism. This is what I was doing on a daily basis. And being excited by these ideas and wanting to be able to engage with those ideas in the history of economics. We're writing history so why shouldn't we engage in these kinds of conversations? I started reading Hayden White. That was the final piece really. It was there I wrote the full set of chapters of *Stabilizing Dynamics.* After that, the story is sort of a piece pretty much.

But I think you are absolutely right to identify the period—it's up … up through that, up through 1990 from about 1980, '81, '82.

Giraud: So, it's kind of interesting you talk about this. What I notice, looking at the whole picture, is that at precisely that moment you see more of STS-like pieces—whatever that means in the very general sense—coming to history of political economy. But you also see some change in the demographics, and I think that kind of struck me at a point: to be seeing a number of women writing in history of political economy, not only histories of economics per se but people coming from the new other side of it. Did you have an input in that?
Weintraub: Yeah.

Giraud: I'm thinking precisely about the symposium on gender.
Weintraub: I'd just come from the National Humanities Center where gender issues as fit subjects for the humanities were everyday normal. This time was almost the first peak of the race/class gender moves in the humanities. Political history was going away. Intellectual history was going away. Cultural history was coming in big time and Duke

especially was one of the leaders in moving toward cultural history, and so these were the people I was seeing and whose ideas I was using. Yeah.

Giraud: Okay. Going back to the start, you were writing the *Stabilizing Dynamics* book [1991]. It comes out in 1991 if I'm not wrong.
Weintraub: Right.
Giraud: And even though it's a bit later than Phil Mirowski's *More Heat Than Light* [1989], the books appear as relatively similar in the sense that they are constructing a new big narrative about the development …
Weintraub: And the questions of what constitutes evidence … seem to be shifting.

Giraud: Yes. So we now know that your opinion about methodology and historiography may have changed over the past two decades also, but at the time you were aware of Mirowski's book, that's for sure, but were you supportive of it? Did you see that book as a kind of companion piece to yours, I mean in the sense of …
Weintraub: You've opened up something …

Giraud: Yeah.
Weintraub: We can talk about it. I was the first reader for Cambridge University Press of Mirowski's book. Okay. Colin Day as editor didn't know what to do with it. He asked *me* as the first reader. At that point Mirowski's book was heading to a two-volume book, one on the physics of rational mechanics and the other on economics. Cambridge Press was very anguished about that. They did not want to do that. The question was how to reconstruct Mirowski's book. All of my letters and notes on that, I think, are at Cambridge University Press. But I was heavily involved in its reconstruction. So, yes, I knew what Mirowski was doing. You know, it was the exact same time I was doing *Stabilizing Dynamics.* He was doing stuff from physics; I was doing stuff from mathematics. It was contextualizing in different kinds of ways, but at that point I saw this as similar kinds … generally similar kinds of projects. I was full of admiration at his ambition and his research. I thought he had really done remarkable things.

As a little sidelight, I had two philosophy of science courses at Swarthmore. Two of them, one was philosophy of social sciences and one was a seminar—a double course in the philosophy of science. They were both taught by Lawrence Sklar. Sklar left Swarthmore and he went to the University of Michigan. Who did Phil Mirowski take philosophy of science from? Larry Sklar! [Laughs] As a graduate student. So there were these kinds of connections that we didn't know about. So, yes, at that time, I understood what Mirowski was doing, I liked it a lot. I persuaded DeMarchi that this was important and we had to take it seriously. And so he constructed—we constructed—the conference on *More Heat than Light,* which brought together historians of science, not so much philosophers of science, I don't think, and historians of economics. I don't have the dates of that.

Giraud: The volume [DeMarchi 1993] is published in 1993 if I'm not wrong so I'd guess the conference was 1992.
Weintraub: Yeah. Right at that time.

Giraud: It's interesting because in that volume you have a relatively short piece but it's very clear that you are trying to construct an argument using Mirowski's book. That *that* book is a kind of turning point for historiography that normally after Mirowski, we shouldn't …
Weintraub: Wasn't that the title: "After Mirowski …"?[4]

Giraud: That's exactly … so you say, okay, it's a turning point for historiography and we have to change the way we write.
Weintraub: I was seeing it as contextualizing the history of economics in deeper and richer ways.

Giraud: Exactly. So my feeling was sort of by … in writing this piece contrary to most or a lot of other pieces in that volume, you are not really appraising Phil's story in itself …
Weintraub: Yes.

Giraud: … or his relatively judgmental normative conclusions in that book; you are appraising the method. Did you have divergences with Phil concerning the way he was using historical arguments to criticize economics?
Weintraub: At that stage, I felt that some of his arguments were really strong. Some of his arguments were fairly weak. Some of his arguments seemed self-contradictory that it could be criticized da da, but the important thing was that it be read and therefore criticized. That people deal with it. That was—historiographically, I thought, this was important. We have to take this seriously. You should do appraisals. You should do evaluations. I am not doing that here, but this is an important work. What are its strengths? What are its weaknesses? And that has to be what the conversation is about. And to do that kind of appraisal, you have to engage with the way he is doing history. And so you are *exactly* right. That is what I was saying in that piece. I didn't want to get involved. I don't write like Phil. I don't like a lot of the ways he writes. I'm very provocative, but I'm not provocative on the same order of magnitude as Phil and I'm not a critic. Phil is primarily a critic. He is coming out of a critical tradition of Warren Samuels. That's not where I am. It's not what I'm doing, but I can certainly appreciate what it is that Phil's doing. I don't have to agree with it, but I can appreciate it. This is something that has to be taken seriously. That was my point. That is why I wanted the conference to take Phil's book seriously. We don't do that for normal books. We haven't done that for any other book, I think.

ACADEMIC MORALS, HETERODOXY, AND MAINSTREAM ECONOMICS: AN AUTOBIOGRAPHICAL RELATIONSHIP

Giraud: This brings me to what is undoubtedly a very big subject, that of the relationship you have on the one hand with economists, and on the other hand with critics of economic scholarship. Mirowski's example proves that you support work whose critical conclusions you don't necessarily approve of, but at the same time you are very unhappy with the kind of internalist criticism that heterodox people make of mainstream

[4] See Weintraub (1993).

economic analysis. I may be wrong, but I'm under the impression that this is something you had in common with Craufurd Goodwin then, that the latter also had a deep aversion to heterodox economics.

Weintraub: I think you're right. Craufurd throughout that entire period was teaching principles of economics. He was teaching in the Economics Department. He was respected by the economists. I mean, he was teaching a fairly standard basic course in economics. At that point, it was before Duke was fully on the workshop kind of system so we attended general kinds of talks. Craufurd was supervising students, undergraduate students, honors papers in standard economics. DeMarchi and I were both teaching. I mean, at that point, I was still teaching micro and macro—I don't know when I stopped macro by that time—and DeMarchi was teaching international economics and micro or macro and he was even teaching monetary and beginning to teach some of this—so you know we were doing—so the idea that what we were doing ourselves was patent nonsense would have slightly offended us [laughs], and we didn't think of economics in that kind of critical fashion. We all had opinions on different kinds of elements of it, but it wasn't a program of critique.

There's a piece I did—I can't remember when it was—it was in response to Sheila Dow, I think she was using something by Kuhn criticizing something—me and something, and my response, I think, it was something like—"Substantive Mountains and Methodological Molehills" or it might have been the reverse [chuckles], but it was that this was really an attempt to say "Stop it" [Weintraub 1982]. Don't keep retreating to methodology and history of economics when what you're trying to do is do criticism of mainstream economics. I mean, if that's what you want to do, do it, but do it to convince economists. You're writing economics and you're critical of economics—well, get the economists to take it seriously. Don't come onto my turf and say that methodology shows that they're wrong. They're not going to be convinced by that.

From the beginning, I was always appalled by that stuff. It's one thing for Joan Robinson to critique capital theory, but it's another thing to say that mainstream economics is methodologically flawed or using history to provide that critique. Go argue with Solow yourself and don't do it from this outside position.

Giraud: You also have this volume on game theory [Weintraub 1992], and like many volumes of that period, it has a mix of historical articles as well as pieces written by economists themselves that can be seen as some kind of autobiographical memories. At the same time, at least it seems to me, you are creating the economists' papers project at Duke, trying to convince economists to deposit their archives. Not seeking to criticize economics, removing the history of thought from heterodoxy—isn't this also a way of reassuring these economists and ensuring their participation as "primary material" for historical research in some way?

Weintraub: Yes. That's quite fair. The project in some sense began when my father died in 1983 and I brought his papers here. And as I started moving to history and started writing to all of these people to construct the general equilibrium story, I was aware that people writing history go to archives, right—they go to archives. As chair of the department, I couldn't go around and leave, and the marriage was problematic and dot dot da.

I kind of got this in the back of my head, "Well, wouldn't it be great if had Arrow's papers here instead of their being out there somewhere." So I began talking and Bob Coats was enthusiastic because he had done the thing with [Paul] Sturges about UK archives, locating where they were rather than gathering them in one place. Some of it was for my convenience [laughs]: Let's see if I can get Arrow's papers. Let's see if I can get [Lionel] McKenzie's papers and … it was slow and, for me, the turning point was when Martin Shubik called Craufurd Goodwin and me—I'd known Shubik and Shubik knew Goodwin because there was a period around 1970–71 that Shubik was being considered for the chair here—the chair that [Martin] Bronfenbrenner eventually got. Shubik had some connections with our department. So I knew Martin. Shubik called Goodwin and said, "I just had this horrible call from Dorothy Morgenstern Thomas," that she had approached Princeton library with Oskar's papers, which were sitting in a closet in her house and she asked the librarian if he would take them. The librarian came over and said to her —this was just that morning, "Well, we'll be happy to look through it and see if there's anything worth taking."

She was apoplectic and she called Martin. Martin knew we had some papers here and called us. Craufurd and I went "Wow!" So literally the next morning—Craufurd that afternoon called Dorothy. invited us for tea or coffee the next morning. We fly up there the next day and have coffee with her. She shows us the closet and she casually mentioned that Oskar had a diary. Craufurd and I are on the phone after we leave the house to Bob Byrd at Duke, the head of special collections. He comes up the next *day* with a pick-up truck.

Giraud: [Laughs]
Weintraub: That was really the beginning of that stuff. With those papers and the fact that I was upset since the last stuff I was doing in economics was game theory stuff and I had written a book on game theory in the '70s [1975].

Well, what about doing the history of game theory? I had been writing about history of general equilibrium and touched tangentially on some game theoretic stuff through Nash equilibrium., I thought we really should get some of these people while they're still alive. And so it was during the year, I got kind of permission from Craufurd and Neil, and during the year that I was at the Humanities Center was the year I organized the conference. I met with Jim Friedman, who was at University of North Carolina, to get his input on who might be worth getting engaged and I remember being in touch with Al Roth and I had met Vernon Smith at some point—certainly in Capri—and Martin. And so I put together a conference. I had been reading and teaching some of the stuff in political theory—positive political theory, so I was fully aware of [William] Riker and Steve Brams and so on in political science. And realizing Riker was of the Shubik kind of generation and that Brams had been a Morgenstern student. Rob Leonard was a graduate student here and he would do a paper. So I put together this conference and we had it and we featured a big exhibit of Oskar Morgenstern's papers and Dorothy came and a nephew or niece came da da da. That was all in that time and that was really—you're right—a major push because out of that we got the Shubik papers as well, we got the Vernon Smith papers, I don't know what else.

[After a short break, the interview resumes.]
Giraud: We were talking about criticism of economics and the role of history in that. So it is pretty clear that you have a view of history and I think it's not just history, it might be deeper in your world view that we shouldn't moralize too much about things that happen when we consider them kind of historically. I see in your work a possible exception to that—if we were to talk about academic morals.

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Weintraub: Academic morals?

Giraud: Yes, in the sense of stealing work, taking credit and so on. And that appears in the *How Economics Became a Mathematical Science* book [Weintraub 2002], a little bit in the story about the publication of the Arrow and [Gérard] Debreu piece, and it appears much more in *Finding Equilibrium* [Düppe and Weintraub 2014]. I would say it appeared even more in the draft papers that you had prior to that when it wasn't clear that you would be co-writing with Till [Düppe] and you had this paper where you wanted to kind of put an end to that Arrow–Debreu–McKenzie thing that you've been doing for decades.
Weintraub: Yeah. Yeah.

Giraud: So how do you position morality in the kind of job that the historian can do? I'm not speaking about the criticism of economics but the criticism of the behavior of economists.
Weintraub: You said you didn't particularly want me to go to my family history.

Giraud: Well, I've been trying to avoid that so far.
Weintraub: But here we are.

Giraud: Okay.
Weintraub: And that's really what it's about. I grew up [sigh] with a father who was an economist who saw the world, after middle age, after his first set of failures as an economist in the mid-/late 1950s, the world divided into "us" and "them." And "they" would do everything possible to stop "us." It was a Manichean view of the world. It was a paranoid view of the world in which connections with other economists would become —he would explode in anger and he would do what the psychologists called—he would split—have no more to do with them. It was also at that point that my mother was instructed not to have anything to do with the person's wife and so on and so on. In psychology, splitting by borderline personalities is associated with splitting off parts of one's self that one cannot accept or dislikes and projecting them onto the other person. Therefore getting rid of them. I saw a great deal of that. And that was how my father conducted his academic life.

It was very real and it was very ugly and it affected me because I was one of those he split from. In some unpublished autobiographical writings, I've reproduced two of the letters he sent to me along those lines. Both involved disinheriting me and making accusations that are scatological and violent and therefore really unpleasant. That was what I saw. That is what I had to grow up with and that's what, as well, I faced with him as an adult. That's the origin of it and so I have a deep repugnance for people who see the world in that kind of way. And a reluctance to go anywhere near that kind of way of operating and feeling that people who play along those lines are untrustworthy and are behaving unethically in terms of a Habermasian *Sprachethik,* I think. It's quite real.

Giraud: So, if I understand correctly, this way of clinging to the idea of academic morals, it is a way of distancing yourself from the behavior that your father had towards you and others.

Weintraub: There is a strong part of me that has faith in very few institutions, and the academy is one of those I have faith in—at its best. This is an institution I've given my life to. I've attempted to improve it in all kinds of ways. I've administered at the department level, at the university level. At its best, its values are values I can fully support, and subversion of those values offends me very deeply. This is part of it. I'm not religious—not in any way or sense. But the institution, I believe, is better than the United States, it's better than France, it's better than the Church. It's the university at its best.

Giraud: My feeling is that what people don't understand, when you highlight morally questionable historical events, or when you do that through others, like when you got [Melvin] Reder's paper on anti-Semitism [Reder 2000] and then wrote a historiographical reflection on the subject [Weintraub 2012], is that you don't do it just because it affects you, personally, because in this precise case, anti-Semitism concerns you as a person. The idea is not to pass judgment on Keynes through his anti-Semitism but to see his anti-Semitism as a trait of his personality that has an impact on his work, as [James] Joyce's anti-Semitism would have an impact on *Ulysses.* Do you have the impression that since this work was published, the community understands this approach better or do you believe, on the contrary, that most people still think that when you highlight the worst aspects of an economist, you just want to trash his work?

Weintraub: [Sigh] The paper on Keynes was designed specifically to counter that view. That it was a critique not of Keynes but of historians for not doing honest history. One needs to talk about this, just as [Don] Patinkin, in some of his correspondence I quoted in that paper, pointed out how [Roy] Harrod had—from our perspective now—written a dishonest history about Keynes's sexuality. That it mattered and to leave it out of the biography is to leave out something that matters, when you know that it matters and the reader does not understand the way it operates as you yourself know how it operates or have an idea how it operates.

I think the anti-Semitism issue, bringing it up, dealing with how someone has written about an episode, leaving that out, I think we have a much bigger problem now that it's become clear that gender discrimination and sexual abuse are a part of the economist's toolkit and they have been for a long time and we never talked about it. They are regarded as not important or we don't want to talk about that—I would like to see historians of economics ask themselves those kinds of questions. Ask themselves in interviews about those things. I think it's honest, it's a way to proceed.

I mean, I have—I think it's impossible to write about my father and some of his colleagues at the University of Pennsylvania without addressing that issue. That's something that historians, individuals writing essays on Sidney Weintraub, have not and apparently are not going to go near. They should. Because it's part … I see this in a little way but it's … and this is much more common, it's much more public now than it ever was. We see this now. And we see historians of economics talking about—women historians of economics talking about it but talking about it in terms of their own lives as members of the profession. But I haven't seen anything really written about major figures in economics.

Giraud: Hmm.

Weintraub: I'm curious why there is the silence. Will that silence be maintained? I hope not.

Giraud: But for you, what would be the purpose of this kind of talk—talk about abuses perpetrated by prominent members of the profession? Would it be a question of updating these acts for the simple reason of doing so or of doing it because that would lead at the same time to highlighting a perspective on economic knowledge that would have remained hidden? Let me be more specific, let's admit that archives or interviews clearly prove that …
Weintraub: … Nobel Laureate "X" …

Giraud: That's exactly what I mean. That Nobel Laureate "X" was a rapist. Is *this* the point of our study or is the point in how it has affected his work?
Weintraub: It's a question of not only how it affected his work but of how the community operated in which he operated. What's the gendered nature of that community? How did that operate in the lives of various individuals—the knowledge of it and so on? From even down to the selection of individuals to be research assistants and graduate students and so on. Not that a person was a rapist. I mean that's a fact or it's not a fact or it's an allegation or it's not an allegation, but how does it operate? How does it work? It's not a "gotcha" game.

Giraud: Yeah. Exactly.
Weintraub: And neither is it the grounding of a political campaign to modify the behavior of economists or the economics profession going forward. For me, it's not in the service of that.

Giraud: Hmm. Yeah, so it's in the service of better understanding of historical fact …
Weintraub: Yes. *Yes!*

Giraud: … and better historic understanding of the economics profession.
Weintraub: Yes. Yes. That's it.

Giraud: That's the same way that we are interested in Keynes's anti-Semitism only in the sense that it was there, not that "We got him!"
Weintraub: Exactly. It's fully consistent with that.

Giraud: Okay. That makes sense. There are many things I'd like to talk about but let's focus on that point, which seems like a very important one. In the first draft of what would end up being the *Finding Equilibrium* book, it was pretty clear that having evidence of Debreu's relatively immoral behavior towards …
Weintraub: Insufficiently disinterested behavior.

Giraud: That's perfect. Okay. My feeling is that even though you're not interested in Debreu's morals—you just seem to be saying it's immoral or seemingly immoral or I'm not as interested in that issue or some such. In the first version of that you seem to be relatively, personally affected by this. Then in the final version that now benefits from Till's research, this thing has been turning into something else. It is now a story about how the economics profession attributes credit and the moral question behind that has kind of disappeared. I was asking myself, even though you don't want to be moralizing, how do you react when it is pretty clear during the inner history that you see someone who, wow … where … if he were to be a colleague of yours, you would probably be kind of angry at him. What kind of reaction that sparks in you, because let me just give another example. I remember Phil, Phil Mirowski, ten years ago here at the Center, saying something like "The more I write on someone, the more I hate him." He said something like that. It seemed to be a condition that he needed to have to write. He needed to think that person matters, but he needs to hate him, to hate that person. Could you write a full book on someone you kind of emotionally hate?

Weintraub: No. No. No. I was always fascinated … there's that two-volume biography of Bertrand Russell by Ray Monk in … by the time Monk is doing volume two, he detests Bertrand Russell and he wrestles with this. This is the problem for a biographer. It's different from writing history, he's a biographer. I'm fascinated by that question because I read a lot of biography and the relation between the biographer and the figure is, I think, a fascinating topic. I mean, you have [Robert] Skidelsky who goes as far as to buy Keynes's country house and to refurbish it and to refurbish Keynes's study and that's where he writes his biography of Keynes. Okay. There's been a lot of fiction written about this particular topic. Now, I couldn't write in length about someone I really dislike, which, in a number of ways, makes it clear that I can't. I can write about my father but only in terms of writing about me.

Giraud: Hmm.

Weintraub: I can't do that out there. The issue about Debreu—the major difference between that early draft and the final one was that the earlier one was about McKenzie. It wasn't about finding equilibrium done with Till. This was the McKenzie survey piece for the *JEL* [Weintraub 2011] and then the earlier versions didn't have any of that knowledge about Debreu. But that came up—once Till and I were working together, Till had this massive amount of very privileged information about Debreu, which to some degree reshaped how I thought about Debreu as a human being. It didn't make him more likeable to me, it made him stranger in many ways, but it made him more coherent and Debreu's behavior became coherent and not immoral and not evil. It was simply how he operated and how he was thinking about the things at that time. McKenzie didn't count for him because he wasn't a mathematician. Debreu's behavior in terms of Debreu's own self-understanding became coherent to me and then my wanting to write about it more as Till and I were moving to—thinking through the project at the end. How did this operate? What was the effect? How did it work? Which is the same thing as the anti-Semitism argument. How did it work? What do we learn in the history … how do we … and so with the question of credit is how the stuff operated. In other words, why did it matter? Well, it mattered in this way: it mattered because of credit for the whole history, of the question of credit for scientific work in a very Mertonian kind of … numbers of ways. You have this with the [Francis] Crick and [James] Watson—the DNA structure discovery and the question of Rosalind Franklin's contribution and their ignoring her more or less. Her x-ray crystallography as being important for their discovery of the double helix … from the way Watson especially, him humanly and Crick institutionally, saw the world. This is coherent. Now, in retrospect, it's quite hard and the question of credit is fully wrapped up in that and that's where it was taken up. And curiously it was taken up not by psychologists, but it was taken up by feminists. And that's how Rosalind Franklin came into being as the third Nobel Laureate who didn't get the Nobel Prize.

Someone wrote that … maybe it was in a *JEL* referee report … that I had this passion for McKenzie to get his proper due. But I only met McKenzie once and that's when I was a graduate … had just finished being a graduate student. I didn't know anything about Lionel McKenzie when I was doing that writing. I was fascinated at how these institutions operated and how certain people were on the outside versus on the inside. You know, that comes from my own personal experience. I was not in any networks in theory. I had no connections anywhere. My thesis advisor wasn't a theorist. In my entire life I was never invited to a single conference in economic theory. I was always outside. So. Here I am at Duke. I could identify with McKenzie *in that way,* but it wasn't a question of trying to get justice for Lionel McKenzie. It's kind of that Paul Valéry quote that I once used: "All theory is autobiography."

Giraud: That was great. We could stop here but I think I have one last question and then I'll let you go.
Weintraub: Whatever.

Giraud: It's pretty clear, and I really appreciate the conversation we just had, that you're appalled by any kind of abusive behavior, not just in the outside world but inside the university. When I think of these historiographical debates of the 1990s, I have the impression reading them that they were sometimes quite heated and that to express your point of view at a given moment, you had to write quite harsh things yourself. To hit others a little bit. How comfortable were you with that?
Weintraub: I don't think that I have ever behaved that way to someone with less power than I have. I've had graduate students or post-docs or young faculty write really stupid reviews of things I've written but I've never responded to them. But for Roger Backhouse [laughs], Roger is a serious figure. Phil Mirowski is a serious figure. I can write some hostile stuff. About the work, some of these things are … some of it is simply amusing. That with Phil—it's never amusing. Phil has no sense of humor about himself. Roger didn't behave in those kinds of ways. Roger just argued and produced evidence and so on and so on. And then I went back at him. As a result we've become very close and supportive of one another over the years.

I haven't had those kinds of hostile controversies where there was a power differential, I don't think. I never responded to Uskali Mäki, though I recall I wrote strong pieces defending my views against Andrea Salanti and against Tony Lawson. I mean, what's the point? I think Alex Rosenberg wrote a thing against my Lakatosian piece and I think I wrote a comment on it. But Alex is one of the world's most aggressive intellectuals—he's very, you know, unbudgeable. I fight him here at Duke. We've been on committees together, it's just wonderful fun. We agree on some stuff and don't agree on some other things, but I don't think I've ever bullied anyone in that sense of a power differential. I have very good relations with people who've been students of mine and who've been post-docs. I can disagree with them in a quite normal kind of fashion.

Giraud: I think we're okay.
Weintraub: Yeah, I think so.

ERW Postscript Message to YG: I recently came across something by Karen Wulf in the *Washington Post* (June 12, 2019) that left me saying "Yes": "To know that Thomas

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Jefferson and George Washington enslaved men, women and children does not occlude their role in generating the founding documents and practices of the United States, but must sit beside it and be explained. Pointing this out is not serving an agenda: It is composing a more complete picture."

SUPPLEMENTARY MATERIAL

To view a bibliography of E. Roy Weintraub's publications, please visit https://doi.org/10.1017/S1053837222000414

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