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JHET INTERVIEWS: PHILIP MIROWSKI[‡]

BY
D. WADE HANDS

I have known Philip Mirowski for over three decades and although I am not entirely certain where we first met, it was clearly at a conference during the late 1980s. We attended a lot of the same conferences during the 1990s: some of the most memorable being the Duke conference on *More Heat than Light* in 1991; the Bergamo conference on "The Nature of Economics: Case Studies" in 1996, which produced Hands and Mirowski (1998); and the "Turn of the Millennium" conference in San Sebastian in 1999. We organized the "Agreement on Demand" HOPE conference in 2005 and edited the resulting volume (Mirowski and Hands 2006). I was also a Visiting Professor at Notre Dame in the fall of 1991 and a Visiting Scholar in the fall of 1993.

Phil received his BA in economics from Michigan State University in 1973 and his PhD from the University of Michigan in 1979. He was an assistant professor at the University of Santa Clara (1978 to 1981) and an assistant and associate professor at Tufts University (1981 to 1990) before becoming the Carl Koch Professor of Economics and the History and Philosophy of Science at the University of Notre Dame in 1990. He has been Professor Emeritus at Notre Dame since 2022. Phil has been and continues to be an extraordinarily productive scholar with over eighteen books and edited volumes, over seventy-five refereed journal articles, and over eighty articles in books and other collections. These many scholarly research works have spanned (and frequently blended) a wide variety of different topics and areas of inquiry: including economics; philosophy of economics and philosophy more broadly; history of economics and history of science as well as social, political, and cultural history; science studies; contemporary science and culture; and many others. He has also received numerous fellowships and grants over the years, including those from Fulbright, the National Endowment for the Humanities, the National Science Foundation, and the Institute for New Economic Thinking. Since the *Journal of the History of Economic Thought* (JHET) is the official journal of the History of Economics Society (HES), it useful to recognize his service and awards associated with HES: Executive Committee membership (1996 to 1998), *JHET* Editorial Board (1999 to 2008), HES Vice-President (1999), HES President (2011), and the HES Distinguished Fellow Award (2017). Additional information about his life and work is available on his Notre Dame web page: https://reilly.nd.edu/people/philip-mirowski/ and his academia web page: https://nd.academia.edu/PhilipMirowski. This interview was conducted via email during spring/summer 2024.

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Hands: Well, it seems reasonable to begin at the beginning. I have known you for a long time and yet I know very little about your life and thoughts prior to the late 1980s (perhaps strangely since we grew up not too many miles apart and are the same age). I suspect people would be interested in some snippets from your youth, early political and/or other ideas, your decision to go to Michigan State, and anything else you would like to share about the time before your undergraduate years.

Mirowski: I suppose the one fact from my early history that is salient for my career is the calamity that my mother died of cancer when I was pretty young, and since my father was incapable of dealing with it, my brother and sister and I were parceled out to live with various relatives for a few years. The aunt I landed with was both exceptionally religious and very censorious of what I might read and watch, which induced in my psyche a lingering skepticism towards most authorities, in combination with a nagging sense that I was never fully at home anywhere. I don't mean to make it sound cataclysmic; it just prodded me to want to escape my circumstances with a little more urgency than might have otherwise been the case. Also, growing up in Jackson, Michigan, I was clueless about what an academic milieu would look like.

Hands: Please tell us a bit about your experiences as an undergraduate at Michigan State (MSU). Perhaps discuss how your intellectual interests evolved and some of the faculty who most influenced you. Did your interest in the history of economic thought begin during those years, or did it develop even before college?

Mirowski: In retrospect, I am amazed that I was able to land in an experimental college housed within MSU called Justin Morrill College (JMC); unfortunately, it was subsequently shut down decades ago. For the first time, I truly felt at home. The place was a nest of what passed for "hippies" in East Lansing in that era. It was a collection of most remarkable individuals (both professors and students) whom I doubt I would have encountered anywhere else as an undergrad. Those were the days when rebellion could be channeled into defining your own curriculum, and even, in my case, "teaching" a course I had invented to fellow students at JMC. Consequently, my interests were all over the map, including intellectual history, philosophy, politics, anthropology, and, later in my stint there, economics. Since I had encountered none of this back in high school, it was like being a kid in a candy shop.

As I approached graduation, I remember thinking I had to "get serious" and declare a major. JMC allowed me to roll my own version of such a specialization, skipping the usual intro courses, and taking Econ courses that sounded more relevant, like economic anthropology, economics of poverty, European economic history, and, significantly, Warren Samuels's year-long graduate courses in the history of economic thought. I realize now that this bequeathed me an outlandish impression of much of what the economics profession was really about, but it had the curious effect of making me want to go to graduate school. Back then, I had almost no idea of what those schools were really like, and neither was I given much in the way of guidance in that regard by MSU. I remember my economic history prof, Jan de Vries, who learned that I was headed to grad school only after the process was finished, telling me I could have gotten into Yale, if only I had asked him for a timely recommendation. (Only later, during a stint as visiting professor at Yale, did I realize just how lucky I had been in that regard.)

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Hands: So much of your research, even relatively early on, demonstrated a deep interest and understanding of both philosophy and the natural sciences (particularly physics). Was your knowledge of these fields acquired only through your own reading and research, or did you take undergraduate coursework in these fields? If so, which areas and courses were particularly influential?

Mirowski: I had this insufferable notion when I was young that I could discern which intellectual endeavors were deep and consequential, differentiating them from the bush-league frivolous fluff. So, of course, one had to understand physics, since it was the bedrock of the sciences. I pursued this not by taking standard physics courses but instead enrolling in courses in the history and philosophy of physics, as well as general intellectual history. I continued this even into grad school at Michigan, auditing philosophy of physics with Larry Sklar. However, I was never very interested in physics as a vocation; I just wanted to understand how it was put together and how it changed over time. I would read histories of physics for fun. I remember thinking that the history of quantum mechanics was especially poignant back then. This accounts for why I was initially favorably inclined towards Thomas Kuhn, perhaps slightly ahead of when it became cool.

I suppose I had the same stance towards economics during that phase. Accepting the quasi-Marxist notion that the economy was the basis of society, I felt it was incumbent upon me to learn how scholars thought they had come to understand it. This undoubtedly was why I clicked so easily with Warren Samuels. He was always interested in the big questions, with his philosophical orientation, his Institutionalist stance that the economy was nothing natural but rather constructed by human beings to achieve widely variant purposes over time. His heroes tended toward Thorstein Veblen and Karl Polanyi. Under his tutelage, I initially thought of myself as an Institutionalist as well, but that proceeded to wear off over time.

Hands: Your undergraduate years—and mine—were times of heightened student political activity. How would you characterize your response and/or involvement in such activities? How about extracurricular but non-political activities?

Mirowski: I don't want to give the impression that I was entirely living with my head in the clouds. I took part in demonstrations against the Vietnam war that were inescapable at the time, but I must confess they left me feeling uneasy with a purely spontaneous format of political action. Also, I probably had overweening impressions of myself as a writer back then. I so much wanted to be a part of some literary scene that I started up my own literary magazine, called *The Anaesthesia Review*, in an excess of twee irony. With a circle of friends, I managed to put out a few issues before the thing sank beneath the waves of its own irrelevance.

Hands: So what were the main things that influenced you to go to the University of Michigan?

Mirowski: I hate to admit that back then I had no idea who was there, or how Michigan fit into the graduate school rankings prevalent at the time. With a level of oblivion only surpassed by my application to MSU, I accepted to go to Ann Arbor because they offered me money, whereas other places did not. I still find it inconceivable that I grew up forty miles from Ann Arbor but had never once visited the town before I moved to grad school.

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This is doubly distressing, since the atmosphere was far more cosmopolitan than East Lansing, and I wouldn't have been such a clueless hick about how academe works had I somehow managed to spend a little quality time there earlier.

Hands: As with the earlier question about your undergraduate education: Please discuss your graduate school experiences, your intellectual evolution, and the professors who most influenced you.

Mirowski: I was shocked when I moved to Ann Arbor at just how passionless and inhibited many of the economics professors at Michigan were back then, a stark contrast with the vivid teachers I had encountered at JMC and MSU. Most of the grad classes were taught by people who barely repressed their disdain for and disinterest in their students, and, in my humble opinion, were mostly mediocre intellectuals. The treatment of the subject matter in the required classes was delivered in a remote manner, with an attitude that telegraphed that it was simply catechism and that only a weakness in mathematics would explain why anyone thought otherwise. I recall even avoiding Dan Fusfeld's history of economics class, largely because of my personal reaction to his approach.

I do recall one junior member of the economics department taking me aside at a party and offering the opinion that I would never make it in economics. It must have been pretty obvious just how unhappy I was with most of the experience during my stint there. I would take wildly unrelated courses like Larry Sklar's philosophy of physics, or British history, just to remind myself what a real intellectual experience felt like. The thing the department was most known for in that era was its national forecasting macro model, and so I ended up taking way too much econometrics. But some friends who worked on it would regale me with tales of how all that sophistication went out the window once they had to produce timely forecasts subject to all sorts of kludges and slapdash alterations to make the results come out "right." And the professors never even bothered to teach the difference between Bayesian and Neyman-Pearson approaches, much less cover the eight different schools of probability theory. Their intellectual horizon was confined to the next issue of *Econometrica.*

The one person there who seemed to have some larger intellectual ambitions was Gavin Wright (who soon afterwards moved to Stanford), and he was responsible for allowing me to believe I could follow some of my immoderate inclinations in economic history and perhaps still enjoy a career in economics. I wasn't terribly interested in American economic history—his specialty—but maintained some independence by opting for European (especially British) history instead. Those were the heydays of cliometrics, so it was easy for me to relate various historical questions to quantitative exercises in data analysis. In those early days, the history kept me cognitively alive and the cliometrics got me published.

Hands: The University of Michigan was one of the founding schools for the Union for Radical Political Economy (URPE), and several faculty were involved with URPE during the time you were there. Readers might be interested in how you were, or were not, involved in and/or influenced by this aspect of your graduate school years. There

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may have been a dynamic aspect to your relationship; if that is the case, how it evolved would certainly be interesting.

Mirowski: I was pretty active in URPE in Ann Arbor, and in those days my interests in economics were primarily nurtured by my fellow grad students, as opposed to the economics faculty. At the biweekly potluck meetings, we could talk about stuff going on in economics that was actually interesting; and that was a cohort of some very smart people, most of whom did not subsequently go on to academic careers. Some of those friendships extended well after I left Ann Arbor. There were only one or two faculty who might attend from time to time, but I do not remember them being very comfortable with the kinds of stuff we were into then. There were a few convinced Marxists, but I remember discussions ranging far more widely, from papers on evolution to political disputes on campus. I probably would have dropped out of economics if it were not for the URPE crowd convincing me there was more to it than what we were being taught in grad school.

Hands: Your 1979 PhD thesis, *The Birth of the Business Cycle*, was published in 1985 (Garland) and reprinted in 2015 (Routledge). Can you say a bit about how it connects—if it connects—with your more well-known early works such as *Against Mechanism* (1988) and *More Heat than Light* (1989)? From the summaries I have read, *The Birth of the Business Cycle* seems to be relatively traditional economic history and macroeconomically oriented, and involved the construction of time series data. While your other early books are certainly historical, they seem to be historical in substantially different ways from the business cycle book. How do, or did, you see the relationship between your thesis and your work from the late 1980s?

Mirowski: That PhD thesis was my strained attempt to marry together "acceptable" concerns like the history of macroeconomics and cliometrics with the holdover conviction that one might do something mimicking theory that was not conventional (what I now consider) "bastard Keynesianism" or, worse, rational choice theory. I am still amazed that Gavin let me get away with it. It actually starts with a truncated history of economic thought concerning macro fluctuations, which reveals that I nurtured a warm spot for intellectual history; it summarizes Veblen's *Theory of Business Enterprise* (Veblen [1904] 1978)—something no one paid any attention to back then—and whips up a little "model," bending a knee toward the orthodox model imperative, and performed extensive original archival work extracting what still persist as some of a very few continuous economic time series for eighteenth-century Britain, and then conducted some cliometrics. The sum total resembles a species of Frankenstein chimera, which, however, did allow me to lop off body chunks to publish as individual journal articles.

You are right to say there is something profoundly incongruous about that book, primarily because it attempted to stitch together all sorts of things that could not happily cohere in the context of the economics profession but did scratch numerous itches I suffered back then. Primarily, I planted a flag in opposition to the cliometrics practice of treating the economy as an ahistorical phenomenon, operating in the same analytical manner over the whole course of human history. The precept that there is no fundamental

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change over the course of economic history undermined the whole *raison d'être* of pursuit of the study of history from the outset, which, I would argue, explains why economic history has subsequently been exiled from most contemporary ranked orthodox economics departments.

The proposition that economic structures, down to the level of markets, don't operate according to timeless principles is a theme that, if anything, has gotten only more pronounced in my later work. This is the origin of the thesis that economics *cannot* be governed by the same fundamental principles as physics.

Hands: It seems like some of your earliest work, your thesis in particular, was influenced by American Institutionalism, specifically Veblen. If that is correct, how did you become interested in Institutionalism and how do you see your relationship with it over time?

Mirowski: You can discern from my previous answers that Warren Samuels inculcated an appreciation for American Institutionalists when I was an undergraduate, and that I even managed to interject some Veblen into a PhD thesis at Michigan. However, it was apparent from the get-go that, strictly speaking, what Veblen did at the turn of the twentieth century would never be granted the honorific of "theory" today. Furthermore, the level of irony he could take for granted in his audience would be totally lost on most of my contemporaries, who thought that prose was just an inconvenient padding to keep the equations from running into one another.

Nevertheless, even that level of acknowledgment permitted strangers to try to lump me under the category "Institutionalist Economist," which could still be a category of economist in the 1970s. It even got me my first job. However, anyone could see that the Institutionalist School was gasping its last breath in economics in the 1980s, and, further, "American" economics had become identified with a curious amalgam of neoclassical doctrines as standard discourse in the postwar period (as you and I argued in a couple of later articles). There were clashing conceptions of what would constitute a science in the pre- and postwar periods, as anyone familiar with the Institutionalist fascination with evolution would realize. The decline of pragmatism also had implications for the fate of Institutionalism. And then there were issues of the way in which economics had become ensconced in the curriculum of the postwar university, which covers the rise of business schools, professional schools, and the firming up of disciplinary identities.

The eclipse of Institutionalism posed all sorts of fascinating questions concerning what the subject of economics was really about. Those were the sorts of things that motivated me in the 1980s.

Hands: Moving on from graduate school, your first position was at the University of Santa Clara. It looks like you started there ABD and finished your thesis during the first year. In any case, what attracted you to Santa Clara? What were you hired to teach? In general, what kind of experience was it? What did you learn about the things you liked and perhaps did not like about being an economics professor?

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Mirowski: I was hired at Santa Clara by a guy who was one of the old-school Institutionalists, out of Wisconsin I think, and who was hoping I might offset some of the numerous Stanford products out there. He did want me to teach both economic history and history of thought, which is one reason I thought it might be salubrious. I did start ABD, so I don't think anyone there had a clear idea of what I was really doing. I had also interviewed at a couple of the supposed "radical" economics departments, but they were perhaps more dubious about how to pigeonhole what I was doing. Also, this was South Bay in California in the 1970s, which was on the cusp of becoming Silicon Valley but which had yet to attain its vaunted status; there was a lot of money sloshing around there—something I had never experienced in my previous life. My overriding impression was that the locals were more engrossed with how they looked and what sports they pursued than any intellectual pursuits; this also applied to a lot of the local engineers at Intel and that ilk, whom I often taught in the MBA program.

Santa Clara was a bit sleepy, with faculty more often discussing their real estate coups than anything particularly intellectual. I have to confess I was increasingly unhappy there, teaching rich kids and business school night classes. I soon was drawn to participate in a seminar run by Don Harris (father of Kamala!) at Stanford, where I became acquainted with other grad students dissatisfied with the state of economic theory. I was not much enamored of Harris's personal fascination with the Cambridge capital controversy, but I found his students invigorating, and soon became an honorary member of their seminar. It reminded me of URPE at Michigan but perhaps even more intense and challenging. I stayed in touch with many of that cohort after I left Santa Clara, even coming to cooperate with them in editing and publishing our own journal, entitled *Social Concept.* That lasted only a few years, but it was a pretext to pursue the sort of life I had naively imagined was the lot of the academic professor, and as such kept me going.

Hands: Your remarks suggest that you did not want to spend your entire academic career at the University of Santa Clara, but what in particular drew you to Tufts? I believe you spent 1981 to 1990 at Tufts with the exception of one year as a Visiting Associate Professor at the University of Massachusetts Amherst. Please talk about your Tufts experience in general and, if you would like, your experience as visiting faculty at UMass Amherst.

Mirowski: I was pretty primed to leave Santa Clara by 1980, when I was contacted by a friend from Michigan grad school who was at Tufts, asking if I might be willing to consider moving. I was then hired by Chair of Economics Dan Ounjian, who was another example of an earlier generation of cultured and open-minded economists, which seems to have withered away in the interim. Ounjian would more or less let me teach what I wanted, to a population of really smart kids, many of whom seem to have had Tufts as their backup school, in the contingency they got rejected at Harvard. And then there was the attraction of Boston, which proved to be far more cosmopolitan than 1970s South Bay in California. I really started to enjoy teaching at Tufts.

It was at Boston that I shifted gears to specialize much more intently on intellectual history, relative to doing economic history, although I continued teaching and

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publishing in both. I attended a number of seminars at Harvard and got to know a few of the faculty and students there, although more consequential was the Boston area history and philosophy of economics colloquium, run on a loose basis by the faculty located at the profusion of universities in New England. We would meet monthly at some restaurant in the area, and one of us would lead a discussion of a paper in a draft stage, often in the Kress Library at Harvard. It recreated some of the free-form discussions I had first enjoyed at URPE at Michigan. (After I moved to Notre Dame, I tried to recreate something similar in the Chicago area; but it never really quite jelled.) Many historians would come down to Kress for access to their amazing collection of early economic pamphlets and publications; the Kress staff would often help facilitate our meetings.

All that was missing at Tufts was potential research collaborations and, in particular, a pool of grad students. I had managed to interact with one or two at Harvard during that period, but some requests to formally mentor some students had to be declined. This was one reason I accepted a visiting professorship at UMass Amherst; another was to get some flavor of what a "radical" economics department was really like back then. That economics department had gone through a series of struggles from the mid-'70s onwards; when I got there, the radicals had bifurcated into a Sam Bowles-Herb Gintis faction and a Rick Wolff-Steve Resnick faction, and they were clearly attempting to recruit further acolytes, splitting the grad population into similar camps. I had no interest in those phalanxes; curiously, I got along much better with the designated neoclassical theorists there, especially Randall Bausor and Don Katzner (who later wrote a useful history of the UMass department [Katzner 2011]). I also collaborated with Carol Heim on some topics in British economic history.

Hands: Most *JHET* readers immediately associate you with *More Heat than Light: Economics as Social Physics* (1989) and for good reason. It is clearly your most well-known book on the history of economics, it was the subject of the 1991 HOPE conference organized by Neil de Marchi published in 1993 (De Marchi 1993), and, at least according to Google Scholar, it is your most-cited work. Would you discuss the evolution of the ideas/arguments in *More Heat*? How far back had you been thinking about the main argument? Are there individuals or sets of ideas that played a significant behind-the-scenes role in the development of the work?

Mirowski: I had lived through the centenary celebrations of the Marginal Revolution in the later 1970s, and the first reaction I had to them was that there was no proposition less credible than that marginal utility theory was "discovered" jointly across language communities. All you had to do was actually read William Stanley Jevons or Léon Walras or Hermann Gossen to realize that they were not so much uncovering some tangible ontological entity that had escaped prior notice, as they were pursuing a different set of objectives: the introduction of mathematics into the discourse of political economy, or the confident assertion of the status of economics as a "science," or the elevation of something called "the market" into pride of place in analysis, plus any number of subsidiary individual projects (such as Francis Edgeworth's attempts to build bridges to psychophysics in philosophy). My long-standing interest in the history of physics prepared me to link the timing to the phenomenon of the rise of "energetics" across the European context, and then the rest of the story clicked into place. By the way,

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Thomas Kuhn's often overlooked early 1959 article on the simultaneous discovery of energy conservation was crucial in my initial appreciation of the issues involved (Kuhn 1959).

I soon realized that starting from the premise that neoclassical price theory was bowdlerized energetics ported into political economy opened up a vast array of questions about the nature of science: the role of metaphors in science; particular choices of the types of mathematics that might be resorted to in order to project the ambitions of physics onto political economy in the nineteenth century; problems in the transformation of the very notion of "value" from earlier classical traditions; questions of the extent of divergences allowed from the analytical structure of rigorous physical theories; the struggle to fit prior conceptions of "production" into the mix; the difficulties of assertions of continuity or rupture in the history of intellectual disciplines in general; the meta-problem of whether, once you started to copy physics, when and where could you plausibly abandon that train, especially in the context of the contemporary rise of thermodynamics from Clausius forwards; and a host of lesser issues. The more I thought about it, the more I came to appreciate that this was not just one simple thesis but a conceptual reset: essentially a reconfiguration of what it meant to do the history of ideas in the economics context. It was a direct illustration of a belief that had been nurtured since I was an undergraduate: if you constrain yourself to remain rigidly within the accepted boundaries of any narrow disciplinary history, you would miss all the crucial action in the evolution of human thought.

This, more than anything, explains why I had no patience with historians who have been concerned above all else to defend the virtues of their chosen heroes in any given discipline. Hagiographies were the standard approach when I began doing history of economic thought; and I fear their allure has not dimmed in the interim. Not only does that tend to presume that disciplines are impermeable, but it also restricts itself to the imagined consciousness of a single human individual. It reminds me of the dreadful practice of American political historians in endlessly recounting, celebrating, and excavating the "Lives of the Founding Fathers." I am aware this construction of "history" is wildly popular amongst the general public, but, just like the interminable run of the superhero movie genre, it is entirely empty of any real nutrition.

Finally, this was the beginning of my ongoing fascination with the maintenance of willful ignorance in academic cultures, which later led to my work on agnotology, science studies, and the history of the organization of scientific research. I cannot express the depths of sheer denial I encountered amongst economists once *More Heat* garnered some attention. I would recount the relationship to energetics, and point to the fact that Jevons, Walras, Irving Fisher, Alfred Marshall, and a host of lesser figures all *admitted in print* that this was what they were doing with price theory, and proceed to insist that this explains why so many lapsed physicists subsequently became famous economists; only to be confronted with the riposte "I simply don't believe it." Of course, this was the era when historians and philosophers of science were beginning to admit that mere abstract logical argumentation was insufficient to explain the evolution of paradigms, Lakatosian research programs, Ludwik Fleck's thought collectives, and so forth; hence, my interest in the work of people like you from the get-go.

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Hands: Your Notre Dame position as the Carl Koch Chair in Economics and the History and Philosophy of Science in 1990 provided an excellent, and in many ways a unique, opportunity to advance various projects along the intersection of science studies and economics. In particular, can you discuss the "Natural Images in Economics" conference in 1991 and the associated 1994 volume, which brought together a wide range of scholars from different fields to address a variety of questions about the evolution of economics and its relation to the natural sciences? Anything else you might want to talk about regarding the early years at Notre Dame would be interesting.

Mirowski: I am not so sure I was hired at Notre Dame to bridge science studies, philosophy, and economics; but it was an opportunity that I stumbled upon at the Riley Center and was happy to pursue during my time there. I should give a hat-tip to the late Ernan McMullin for considering the request, which might not have been favorably entertained at many other history and philosophy of science (HPS) units back then. I think rather that a certain faction of the Notre Dame administration was more or less happy to support the development of a heterodox economics department, as long as some other diverse considerations were met, such as the fact I had been educated at Catholic grade and high schools, and that I had been deemed "acceptable" at regular orthodox economics departments like Tufts. Another draw was that, back then, the department didn't seem to be riven by the factionalism I had experienced at UMass. Indeed, the orthodox economists there when I arrived were friendly with their heterodox colleagues, and seemed contented that the members were encouraged to take certain moral and critical stances with regard to the profession.

There was, of course, a certain trade-off, with me abandoning the cosmopolitan coastal power centers of academia for what might seem a place situated off the beaten track. I cannot tell you how many times I had to explain where Notre Dame sat geographically to outsiders. However, it was a place where I could imagine starting a family and have some grad students and a few colleagues with whom to have some interesting discussions. The administration funded an inaugural chair conference, which eventually produced the *Natural Images* volume (Mirowski 1994a). That was my attempt to broaden out discussion of the insights mentioned in the previous answer to other incidents and time periods in the history of economics. The Reilly Center folks were happy that I managed to recruit some well-known historians of science to the project, like I. Bernard Cohen, Camille Limoges, Sharon Kingsland, and Ted Porter; I had hoped they would interact with some characters more familiar to historians of economics like Arjo Klamer, Geoff Hodgson, Margaret Schabas, and Michael White. I confess in retrospect I don't think this colloquium resulted in a gratifying meeting of minds between historians of science and economics for the most part, although I still regard some of the papers therein as combining historiography with philosophical issues in ways that are still fresh and suggestive. My own lecture therein sought to suggest that what counted as "natural" versus "social" in some incidents from history were not given and fixed but negotiated in the process of inquiry, like some sort of toned-down Anglo Foucault. Mostly, instead, people there seemed to get hung up on the issue of whether one could insist that a scientific metaphor could be "true" or not in a different context, something I still think is not of primary significance when it comes to tracing the transfer of models, templates, and ideas between disciplines. However, this meeting occurred in the wake of Deirdre

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McCloskey's popularization of the rhetoric of economics, which may not have been the most conducive frame within which to address the kinds of issues I imagined would have been explored. In retrospect, most reactions to the volume have tended to pigeonhole the volume within that rhetoric schema, whose relevance has faded in the interim.

Hands: Could you discuss the role that various Notre Dame colleagues and students played in your research over the years, such as the *Science Bought and Sold* volume with Esther-Mirjam Sent (Mirowski and Sent 2001), and some of the papers and books you coauthored with your graduate students and former graduate students?

Mirowski: I have been fortunate over the years to encounter some students and colleagues who have shaped my research trajectory for the better, and not always consciously. It is a knotted problem to decide just what form of encouragement and what level of collaboration are beneficial for each individual student. Guidance requires a deft touch; considerations of one's own research should ideally be secondary. Nevertheless, real research is inevitably a two-way street, and I have had a few students-cum-colleagues who have been perspicacious instigators and real inspirations for my subsequent work. For instance, early interactions with Koye Somefun over problems of computability in his thesis led to some joint papers, a significant component of *Machine Dreams* (Mirowski 2002), and then, in his absence (he is now at BNP Paribas), the further development of my theoretical work on markets as computational automata. Perhaps even more consequential was my time spent with Esther-Mirjam Sent, first serving on her dissertation committee at Stanford and then having her as a colleague in the economics department. It was not so much her excellent work on rational expectations as her work on Herbert Simon done while at Notre Dame that played a critical role in the conceptualizations of certain sections of *Machine Dreams.* As you note, we also had extensive discussions on what it meant to claim that there could exist an "economics of science" of any legitimacy; she and I convened a conference at Notre Dame that eventually resulted in the *Science Bought and Sold* volume, and indirectly, downstream, in my later *Science-Mart* (Mirowski 2011). I see now that interactions with Esther-Mirjam fostered my own growing concerns with the various ways epistemology matters in economics, which in turn subsequently led to my work on the neoliberals, and, later, the treatment of knowledge in neoclassical theory. And then, more recently, I have come to realize a wide number of shared concerns with former graduate student Edward Nik-Khah: the intersections of science studies and economics; the history of game theory and mechanism design; the "performativity" literature associated with Bruno Latour and Michel Callon; the economics of the pharmaceuticals industry; and, ultimately, as coauthor of *The Knowledge We Have Lost in Information* (Mirowski and Nik-Khah 2017). An HPS student, Manuela Fernandez-Pinto, became a sounding board for the explorations of the ways economics impacted so-called social epistemology. I also have to mention Rob van Horn, who started out with me working on pharmaceuticals research and then shifted to the far more momentous work on the history of the Chicago School and the early neoliberals.

One likes to think that one is personally responsible for whatever captures your intellectual interest; but this roster shows that good research is primarily sparked by

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other interlocutors and their insights, and that I have been lucky in my students as collaborators.

Hands: *Machine Dreams* in 2002 followed *More Heat* by more than a decade, and yet, in many ways it seems to be the second part to the history of neoclassical economics you initiated with *More Heat.* Please discuss how you saw the connections between *More Heat* and *Machine Dreams* and also how you see the relationship now as compared with when you were involved in the writing. Please take this opportunity to share any other thoughts about *Machine Dreams* and to comment on any of the commentaries and/or reviews that followed its publication.

Mirowski: You are right to notice that *Machine Dreams* was originally intended as a follow-up to *More Heat*, in that physics and science had moved on dramatically into the twentieth century, but on initial impression, neoclassical price theory remained comparatively mired in stasis. However, no grand plan emerges unscathed from encounters with the historical record, and this was also where your and my research endeavors most closely intersected.

It seemed like the rise to orthodox dominance of neoclassical price theory was in the first instance largely an American story, so I embarked on an intense bout of archival research —probably the most protracted in my career—in order to get some clues as to how this happened. I am not altogether sure how we decided this, but eventually you and I did a number of articles together (never gathered together in one place), which covered the struggles with empirical price theory from Henry Moore to Harold Hotelling to the raft of characters who became famous from the 1930s through the 1950s and who settled on demand theory as the central doctrine of a scientific economics. The key paper was our joint contribution to the 1998 Mary Morgan and Malcolm Rutherford *History of Political Economy* (HOPE) volume, which argued that there were three distinctive versions of formal price theory in the 1950s, essentially identified with the three dominant schools of Chicago, Cowles, and MIT (Hands and Mirowski 1998). Our thesis expressed therein claimed that it was precisely this diversity of approaches that rendered neoclassical theory powerful enough to defeat other rivals, which contradicted the usual philosophical presumption that it would be unity of doctrine that generally explains the triumph of a research program.

While it was important to get that crux of the narrative straight, that corpus still did not adequately respond to my own concerns growing out of *More Heat.* For instance, our joint work did not confront what had been happening in physics and other sciences over the course of the twentieth century—this, by contrast, was the genesis of *Machine Dreams.* It was standard wisdom in the history of science community to assert that WW II was the inflection point, after which the US came to dominate the world in the natural sciences; so, the war must have had profound effects upon the quest for scientific status on the part of economists. I spent inordinate time with the Cowles archives, both from Yale and from RAND, and came to the conclusion that this orthodoxy learned its updated physics through interaction with the novel intellectual formation known as "operations research," which was little more than the imposition of physical and statistical models to analyze the conduct of war. Thus the influence of natural sciences

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on economic ideas persisted into the new century, albeit through this odd detour. It also confirmed our joint work, given that the major representative figures of the three American neoclassical schools of economics all pursued parallel stints as operations researchers, and enjoyed lavish military support throughout their careers.

Judging by reviews, this important argument did not seem to garner much in the way of attention on the part of readers. This was due to the fact that one major consequence of this dynamic was the initial intrusion of the computer into modern economics. I confess that computer narrative tended to swamp the previous two theses in the book, as its consequences unfolded pervasively throughout the second half of the twentieth century. I may also have gotten sidetracked in the fascinating career of John von Neumann—a ripping tale in its own right. The bottom line was that the most important consequences for twenty-first-century economics derived from the sciences of computation—again, economics was the tail being wagged by the natural sciences dog.

Back at the turn of the millennium, I found that the median response of your average neoclassical economist to the book was that I had oversold the significance of the computer for the evolution of economics. Two decades later, I must admit to feeling a bit vindicated in this respect.

Hands: It seems like somewhere toward the end of the first decade of the twenty-first century, much of your work began to focus on various topics associated with neoliberalism. I will ask a number of questions about this literature—it is extensive, much cited, and ongoing—but at this point I would like to give you the opportunity to discuss some of your research from the earlier decades that you feel has not been given sufficient attention, has received the wrong attention, you would revise if you had the opportunity, or is particularly dear to your heart …

Mirowski: Well, no one gets to control the shape of the reception of their work, and I am no different in that regard. I get a lot of grief for being a harsh critic of economists; nobody really likes a scold and a muckraker. Maybe I should have suppressed this tendency more than I ever did; but the truth is that I get much of my emotional energy to do the work from being continually provoked by much of what economists say and do. It doesn't help that I have suffered some personal attacks on my career.

Nevertheless, I have striven to do something more than kvetch. For instance, I had ambitions for my theory of markomata in the *Journal of Economic Behavior and Organization* (Mirowski 2007) as an example of a road not taken (but perfectly possible), which would avoid some of the worst aspects of orthodox price theory, as well as the pernicious influence of neoliberalism. One or two people have attempted to take up the gauntlet, but mostly it has been ignored in economics. I have also produced a number of exercises in what I think of as applied science studies. One paper of mine of which I am very proud is the 1994 *Science in Context* paper, which presents a theory of how the values of the fundamental physical constants have been stabilized by social processes that resemble those of arbitrage operations in finance (Mirowski 1994b); but as far as I can tell, that idea has fallen stillborn from the presses. There is a sequence of papers over the last two decades that scrutinize the political implications of science for

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democracy, culminating in an unpublished survey of the issue in science studies, unpublished but available on my download page at academia.edu [https://nd.academia.edu/PhilipMirowski].

Hands: Before moving on to your more recent research, I would like to ask two questions about topics that I believe readers will find very interesting but are not directly related to any specific research project. It seems like many readers would be interested in where/how you learned to write like you do. In preparation for this interview, I reread parts of your research, but I was generally trying to understand the argument, the point, of what you were writing and not how your thoughts were expressed. But there were times—in particular, the first chapter of *Machine Dreams*—where I was pulled in initially by the enchantment of the writing itself. Granted your style has its critics, but nonetheless it is a fundamental characteristic of your research work and I think it would be very interesting to hear you talk about it.

Mirowski: I started out not wanting to be an economist or historian but rather a writer. The great discovery of my undergraduate years was finding just how much I loved the work of authors like Donald Barthelme, Thomas Pynchon, and, somewhat later, David Foster Wallace. I didn't just bathe in their prose—I wanted to *be them.* Unfortunately, I eventually had to admit I didn't have the chops to attain that ambition, much less to play bass in a rock 'n' roll band. I also made two 16 mm fiction films while an undergrad, but that experience revealed that no one was going to fund my quest to become a famous director. Part of education is learning the shape of our own limitations.

Be that as it may, I cannot abide people who complain: Why don't you just write clear, straightforward, simple prose? The notion that there is something like transparency in expression is one of most despised doctrines renounced by the authors whom I really respect. Short, abrupt Hemingway sentences are just as artificial and deceptive as the pages-long prolix sentences of Proust: they both express attitudes and epistemologies that cannot be adequately conveyed by paragraphs aimed at the lowest common denominator of "See Jack run." The discovery of levels of meaning and semi-hidden structures is one of the great pleasures of reading; or at least it used to be, before electronic texts and AI have come to erode the secret lives of books.

By the way, these comments apply equally to mathematics. There is nothing transparent or neutral about resort to the calculus of rational mechanics to formalize something called "equilibrium" in exchange. It is as freighted with unspoken implications and layered metaphors as a Shakespearian sonnet.

Returning to *Machine Dreams*, from a stylistic point of view, that book is the bastard offspring of Thomas Pynchon's *Gravity's Rainbow* and Paul Edwards's *Closed World*, two books that have served as endless inspirations for the sort of writing I have wanted to do (Pynchon 1973; Edwards 1997). Of course, it is not as sublime as either, but that's not for lack of trying.

Hands: The excommunication of historical, methodological, and heterodox faculty from the Notre Dame economics department—in both the 2003 two-department

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reconfiguration (what Robert Solow famously called a "cruelly bad idea") and the final dissolution of the Department of Economics and Policy Studies in 2010—is well known, if not understood, among more senior History of Economics Society (HES) members, but that may not be the case for those newer to the field. Would you like to explain what transpired and then comment on the whole series of events now that a number of years have passed?

Mirowski: After much meditation, I have decided that this episode was way too fraught and complex to deal with in an interview. I am not ready to parse how much of it was administrative duplicity, how much was garden-variety academic politics, how much to attribute to outside economists, and to what degree it is evidence of the degeneration of discourse in the academic economics profession. However, there is one aspect relevant for the current audience. When the administration decided to attack the department, the very first salvo was a demand that we remove the history of economic thought from our graduate program requirements. When our department voted to resist that ukase from above, that set in motion a train of events to eventually dissolve our department.

Hands: It seems like at some point around 2005–06, neoliberalism became an integral part of the majority of your research; it filtered into a wide variety of topics and continues to play a significant role in your work. I suspect that readers would be interested in how this transformation took place: what initially sparked your interest in neoliberalism, whether you initially realized how important it would be in your future research, or anything else you would like to talk about regarding the early stages of your interest in neoliberalism.

Mirowski: This has been a perfect example of my not being entirely in conscious control of my own research program but, rather, being buffeted and swayed by interactions with important others. Previously, I had avoided doing any work on Chicago, because I had thought it was mostly a province of bromides and stale platitudes in the history of economics. I tended to concentrate instead on the orthodox situated on the left, as in the work on Walras, on Veblen, on Cowles, and some stuff on Paul Samuelson. Then I got a visiting position at the International Center for Advanced Studies at New York University (NYU) in 2005–06. There I met and befriended Dieter Plehwe, a political economist at the WZB Berlin. It was through Dieter that I was introduced to the notion that many historians did not correctly apprehend the real character of right-wing movements from the 1930s onwards.

Although Dieter is a historian of great repute, he is also much more of an activist than I, so I confess I was initially a bit skeptical. After all, I had been trained in economics and had spent much of my career on the history of the subject, so therefore it seemed implausible that I, not to mention most of my peers, had been misled about the character of modern right-wing political economy. Purely fortuitously, at that juncture, Rob van Horn had decided he would like to do his thesis on the rise of the law and economics movement, and consequently had begun trawling the Chicago economics department archives. Conversations with Dieter eventually brought me round to the notion that something called "neoliberalism" was a distinct movement in intellectual history, with its own singular approach to politics. I had previously been vaguely aware of some work

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by Friedrich Hayek (his *Counter-Revolution of Science*, for obvious reasons), but now I began to situate him at the center of a movement called the Mont Pelèrin Society (MPS), which encompassed a stellar international roster of famous economists, not to mention a substantial subset of the Chicago School.

Dieter introduced me to a roll call of international scholars working on the history of neoliberalism, and persuaded me that no single historian alone could ever do justice to such a cosmopolitan transnational conclave. Indeed, a conference convened at NYU devoted to the topic forced me to confront the fact that, back then, the biggest lacuna in the joint project was someone concertedly focused upon the American wing of the movement. Conveniently, Rob van Horn had accumulated a vast archive of primary material concerning the interaction of Hayek, Henry Simons, and the Chicago School; thus, he and I were well situated to contribute the Chicago chapter to the volume *The Road from Mont Pelèrin* that I jointly edited with Plehwe (Mirowski and Plehwe 2009). Our primary thesis was that the rise of the vernacular Chicago School was best approached as an artifact and outpost of the MPS (and *not vice versa*): a perspective entirely absent in the previous history literature.

This incident turned out to be the second time in my career (*More Heat* being the first) where I felt induced to reconsider my entire previous understanding of the history of economics, at least as I had heretofore been taught. In both cases, what had been portrayed as stolid continuity was in fact a significant rupture. The unsettling suspicion that nothing was actually as it had previously seemed and that I subsequently felt impelled to reconceptualize the meaning and significance of entire schools of thought have been some of the most profound and satisfying experiences I have been bequeathed as an academic researcher. Each served as a talisman of the possibility of really learning something, a phylactery proving (to me, at least) that I was getting somewhere.

Hands: Continuing on with the general topic of neoliberalism, the argument is often made that the meaning of "neoliberalism" is … well … fuzzy. That said, your use of the term seems to be relatively specific and grounded in a particular historical context and a distinct set of political economic ideas. While I recognize that neoliberalism cannot be inscribed on a 3 x 5 card—as you have said—would you discuss how you characterize neoliberalism and discuss the evolution of the term as you use it? Anything else you might want to say about the general ideas associated with neoliberalism would be appreciated by readers.

Mirowski: It is hard not to give a long-winded answer to this question of the intelligibility of neoliberalism, which is the first thing that gets brought up by people who have not followed the literature in this area. Perhaps I might begin with a quote from Hayek:

> [C]urrent political terms are ambiguous, or even that the same term often means nearly the opposite to different groups. There is the much more serious fact that the same word frequently appears to unite people who in fact believe in contradictory and irreconcilable ideals. Terms like 'liberalism' or 'democracy', 'capitalism' or 'socialism', no longer stand for coherent systems of ideas. They have come to describe aggregations of quite heterogeneous principles and facts which historical accident has associated with these words. (Hayek 1948, pp. 2–3)

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If Hayek felt that way in the 1940s, then why does everyone now feel justified in surmising there is something exceptionally dubious or imprecise when it comes to the contemporary terminology of "neoliberalism"? I have written extensively on this issue —see especially my Postface in the *Mont Pelèrin* volume, chapter 2 of *Never let a Serious Crisis Go to Waste* (Mirowski 2013), and especially my INET Working Paper n. 23 (Mirowski 2014)—so perhaps I may be allowed to be a bit abrupt in this venue. It seems the reasons fall into four rough categories:

[A] Language describing political positions and movements is persistently notoriously imprecise and freighted with emotional content. Think, for instance, about the endless ink being spilled over the term "fascism" in the contemporary context. Regrettably, it is just par for the course. Nevertheless, political doctrines and supporting political infrastructures do actually exist, and move masses.

[B] But more to the point, I think most people cannot bring themselves to admit that they understand very little of what passes for political theory and political discourse in its actual historical settings. If they did admit this, then that implies that their most impassioned and cherished political convictions might be built upon sand. Rather than concede this, they instead opt to impugn the messenger or engage in the most tortured denialism.

[C] Yet, there is a third factor that must be cited in this regard. It can be historically demonstrated that MPS-affiliated theorists have not been completely clear and above board about the contours of their doctrines. Partly, this can be attributed to the fact that the MPS was itself comprised of a number of somewhat differentiated schools of thought, and the center of gravity of their doctrines have evolved over time. But equally, we demonstrated that a number of MPS figures initially explicitly described their movement as being mobilized under the banner of "neoliberalism" but rather abruptly ceased doing so some time in the mid-1950s. Their chosen neologisms subsequently tended to suggest bland continuity with classical liberalism rather than rupture.

[D] The most common error is to treat "neoliberalism" as a poor synonym for "libertarianism" or "conservatism." The main protagonists took pains to renounce both of those political positions; yet some figures realized that a modicum of ambiguity might nevertheless serve their political interests. This became subsumed under what I have described elsewhere as adherence to a set of "double truths" (Mirowski 2013, pp. 68–83). Just as with the earlier case of the three schools of orthodox neoclassical theory, neoliberalism owes its success to its curious ideological shape, which functions thanks to, and not in spite of, its paradoxes and contradictions.

Dieter and I sought to navigate these complications by insisting on the centrality of the Mont Pelèrin Society and its attendant complement of think tanks, satellite organizations, and other external signs of political cooperation as a necessary litmus test to indicate membership in the neoliberal thought collective. This linkage of thought patterns to objective organs of political action is one of the major contributions of the volume.

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However interesting the Mont Pelèrin thought collective might be for historians, there has been a further reason why it keeps inserting itself into topics that I have written about in the intervening years. The predominant motivation is something that may elude many of my readers. The core doctrine of neoliberalism is concertedly epistemic, and therefore philosophical. It encompasses a set of propositions concerning human ignorance and the way in which political activity is relatively impotent in the face of that ignorance. (Somehow, MPS political activity deems itself immune from this doctrine.) It also preaches a belief in the market as a superior information processor, far more powerful than any human cognition, with everything that seems to subsume under metaphors of the computer and the ineffability of market wisdom. Accepting that the market is always smarter than you is an injunction to "Stop Making Sense" (*pace* David Byrne). For instance, the hostility to intellectual expertise is baked into the neoliberal world view, with all the dire consequences that have reverberated down to the present.

Much of what I have written since the MPS work has been inspired by the insight that many issues in social epistemology are clarified tremendously by starting from the premise that a neoliberal stance is the default position in the topics addressed.

Hands: Let us turn away from neoliberalism in general and focus specifically on the book *The Road from Mont Pelèrin: The Making of a Neoliberal Thought Collective* you edited with Dieter Plehwe. It is a book that garnered quite a bit of attention. In addition to editing the volume, you had a chapter with Rob van Horn on the Chicago School and a Postface on "Defining Neoliberalism." Google Scholar lists the book as your third-most cited work, while both the Postface and the chapter with Rob are also in your top ten citations. This is a nice work to discuss in this *JHET* interview since it has received a lot of attention and involves ideas and individuals who are known to many historians of economics. Please expand on how this book came together, the response it received, and any other things you want to say about it.

Mirowski: The volume grew out of the NYU conference mentioned above. I think it established a number of markers for later historians: situating the origins of the MPS in the Paris Lippmann Colloquium of 1938; identifying the initial alliance under its umbrella of the three schools of economic doctrines—the Austrian School, the Ordoliberals, and the nascent Chicago formation; describing the struggle to break away from earlier laissez-faire and Manchesterian positions and the dispiriting climate of utter political defeat they faced in the immediate postwar period. The joint work with van Horn has tended to shake up the prior stories that Chicago economics had taken to telling about itself. Rob and I especially insisted that early MPS meetings were the incubator of hostility to the idea that monopolies were detrimental to market competition. Even at the conference, there was a sense that we had merely scraped the tip of the iceberg; and, as you suggest, it seems to have precipitated a tsunami of work on the neoliberal thought collective and their political activities. There were way more actors and topics than we managed to cover in the MPS book. Some noteworthy authors who credited our joint work in sparking their own substantial contributions include Will Davies, Ola Innset, Melinda Cooper, and Quinn Slobodian, among a raft of others.

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One salient aspect of this wave was the curious fact that its impact seems to have happened almost entirely outside of the economics profession. I got the impression that the median response within the orthodox economics profession to *Road* was rather similar to that toward *More Heat than Light.* My favorite exemplar is Robert Solow, in a review of Angus Burgin's *The Great Persuasion* in 2012:

> Hayek's ambition—to rebuild a modernized Neoliberalism on ethical foundations other than simple individualism—would go nowhere in the MPS.… Outside the economics profession, it was invisible. The MPS was no more influential inside the economics profession.… Burgin thinks that the MPS had served the purpose of providing a sort of institutional home for the cultivation of neoliberal ideology. I am skeptical. (Solow 2012)

No evidence; no real history but plenty of facile generalizations; no sense of the larger intellectual landscape outside of American economics; and, finally, simple *ex cathedra* pronouncements that, if he didn't personally see it, it didn't exist. I tend to regard this trope as the boilerplate MIT dismissal of the history of economics, which began with Solow's colleague Paul Samuelson and has had as its culmination the expulsion of any capacity for historical sophistication within ranked economics departments in the US.

I admit I blow hot and cold lately on whether it is even worthwhile to address any of my work to the economics profession anymore, especially since it is obvious the preponderance of my readership has migrated elsewhere since the MPS book. My shift into science studies accounts for some of that, but like Al Pacino in *The Godfather*, something else keeps pulling me back in. For instance, I am stubbornly insistent that intellectual developments that the profession treats as having long faded into oblivion persist with a long tail down to the present, often inspiring doctrines they frequently credit with presumed excess novelty. My work with Eddie Nik-Khah on the history of information concepts in economics has been very much inspired by the observation that Hayek's epistemic doctrines live on in such 'modern' areas as game theory and market design, however much their enthusiasts may wish to deny it. *Pace* Solow, Hayek's influence upon subsequent formal economic theory has proven far greater than his own, expanding through time, whether he can recognize it or not.

Hands: Your 2013 book on the 2008–09 financial crisis—*Never Let a Serious Crisis Go to Waste* —is also a book where neoliberalism is the dominant theme and, like the *Mont Pelèrin* volume, it concerns issues and events that are familiar to most historians of economics. It is your second-most cited book; it has also been translated into Spanish and German. It seems to me, although I may be incorrect, that the financial crisis book is more about the neoliberal thought collective in action/practice than in its historical, theoretical, and contextual development. In any case, it would be nice to have you discuss the background, response, and anything else you want to talk about regarding the *Crisis* book.

Mirowski: One of my closest readers, Christian Thorne, once suggested that I be considered a non-Marxist embodiment of Antonio Gramsci, but nonetheless he felt let down that what he called my "metaeconomics" had so much to say about economists and, yet, so little to say about the economy. Perhaps readers of this journal sometimes get

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similar comments. If I had had the opportunity to respond, rather than suffer from *esprit de l'escalier*, I would have insisted that I have persistently doubted the ontology of a separate and autonomous economic machine—another reason for skepticism concerning neoclassical price theory—and have instead sought to explore how the economy is partly constituted by our theories concerning its operation.

The *Never Let* book was my contribution to this agenda. After all, I knew I had been living through a major economic conjuncture, which was getting revised and rewritten (some might say whitewashed) in real time, even before one could confidently say it was "over." To me, it was obvious that economists were partly responsible for some of the financial innovations that had caused the crash, as well as mistakes made in the responses; but it was equally obvious that the profession was busily attempting to shed any and all responsibility for the disaster by spinning elaborate alternative narratives about what had gone wrong. It seemed an opportunity custom-made for an historian of economics with substantial background in recent orthodox economics. It was rendered doubly apposite by the fact that commentators quickest off the blocks to rewrite the economy as secluded essentially above suspicion were demonstrably affiliated with the neoliberal thought collective. After all, the one thing to which neoliberals could all happily subscribe was banishment of any possibility that markets could be self-undermining. To accomplish this, they had to invent all sorts of appendages to "the economy" to render crisis events "normal." What qualified as "the economy" was getting revised in real time.

Because my potential audience was conceived as the general, albeit educated, public, rather more effort was put into exposition of neoliberal doctrines than recounting any abstract sequence of economic history, although I hope most of the high points got covered.

Hands: I have been asking questions directly related to your research, which is, of course, important, but perhaps a break from the content of books and papers would be useful. You've had many visiting positions—some as research fellow and some as visiting professor—Trento, Uruguay, NYU, Oxford, Ecole Normale Paris, Duke, and others. Maybe you could discuss some of these visiting positions: the associated research projects, of course, but it would also be nice to hear about some of your experiences and interactions that are not directly related to your research. You've had a lot of opportunities over the years to visit many institutions and exchange ideas with a wide range of scholars; perhaps there are some particular ones that would be interesting to discuss.

Mirowski: Some of my fondest memories come from experiences during the various visiting gigs that you mention. For instance, Trento is a great location to bask in Italian culture, avoiding the tourist crowds whilst enjoying the quotidian comforts of Germanic efficiency. It was there that I got to spend quality time and leisurely lunches with Axel Leijonhufvud, who was one of the most congenial human beings I have ever encountered. Likewise, Annie Cot helped elevate Paris above its already vaunted status as a center of culture. The time in Oxford was catnip to an anglophile such as myself. I had enjoyed various stints in Albion starting with the research on my dissertation, but the time spent at All Souls in Oxford was incomparable. I had gotten funding to work on a

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history of the so-called Nobel Prize in Economics jointly with Avner Offer and Gabriel Soderberg; however, differences of opinion over interpretation led me to withdraw from authorship of the book they eventually produced on the topic. Numerous notebooks of archival and historical work have unfortunately gone begging from that experience, although I did manage to publish one small article (Mirowski 2020) and one short video for INET summarizing what I regarded as the major conceptual takeaways from the history of the Bank of Sweden Prize in Honor of Alfred Nobel.

Hands: I have asked you to talk about a great number of research projects and yet it seems like there are many more that could be discussed. Two that jump out at me are 1) your work on information and knowledge in economics (with Edward Nik-Khah), and 2) your most recent book project on Open Science, although there are undoubtedly other possibilities. Rather than asking specific questions, I think I will just open it up and leave it to you to comment on these or any other parts of your research that you would like to discuss. This might also be a nice place to say a bit about your long association with HES and *JHET* and how they have contributed to your research and other scholarly activities.

Mirowski: If there has been a common denominator running through all my work, I think it involves what would be considered as philosophical questions concerning the structures of inquiry and how they play out in the cases of economics and the sciences. (This is one reason I have avidly appreciated your work over the years.) For instance, I have written a fair amount on how the mathematics characteristic of neoclassical economics has been such a poor fit, given widely accepted contours of the subject matter. I kept asking: Where are the conservation principles in economics? but have yet to hear a plausible response. I keep coming back to the various ways computational considerations always leak into how we think about computers and the economy. I am fascinated with the ways in which philosophers keep struggling to reduce the epistemology of science to a few basic principles, only to have each collapse into shambles every generation or so. As you notice, my work with Eddie Nik-Khah starts from the premise that there is no single well-defined thing as "information," and yet, it ends up transforming the culture repeatedly since the 1950s. I spent a fair amount of effort following the pharmaceuticals industry, which I regard as a poster boy for corrupted research, but, unfortunately, never managed to publish much on it. And lately, I have witnessed repeated claims that "openness" will fix whatever epistemological failures ail modern science but have yet to hear one really compelling argument that this will do something other than foam the runway for the final commercialization of every aspect of the research process.

Sometimes people complain that I operate too readily with half-buried ironies. Take, for instance, my fairly large corpus concerned with discussing the modern degradation of science. The grand irony conjured therein is that the economists, who so desperately themselves had wanted to usurp the status of physics, later went on to play a major role in the destruction of the science base, at least in the United States and Europe. By insisting that the university must be re-engineered to run like a business, higher education recast as the accumulation of "human capital," and the market redefined as the ultimate validator of all knowledge, the most advanced science infrastructure in the world has

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been corrupted and debased, possibly irreversibly. That was the message of *Science-Mart;* and, perhaps more refracted, in my recent work on Open Science.

Regarding HES, I would say that in the era of the presidencies of George Stigler and William Grampp, I couldn't even get my proposed presentations accepted at an HES meeting. That changed once Warren Samuels ascended to the presidency; and after that, I greatly benefitted from meeting major authors in the field, and got a better sense of the sorts of issues that tended to motivate their research.

Hands: Since we have moved from your early life to your most recent work, perhaps this is a good place to wrap this interview up. I think this has been great and I hope you feel the same way. I believe *JHET* readers will enjoy it and find the discussion very useful. Thanks so much for doing this.

Mirowski: The pleasure is all mine. Thanks for all the effort you put into it.

SUPPLEMENTARY MATERIAL

To view a bibliography of Philip Mirowski, please visit http://doi.org/10.1017/S1053837224000439.

COMPETING INTERESTS

The author declares no competing interests exist.

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