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SCIENCE STUDIES AND ECONOMICS: AN INFORMAL HISTORY

BY E. ROY WEINTRAUB

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Abstract

Duke's Center for Interdisciplinary Studies in Science and Cultural Theory (CISSCT) hosted a two-year program on "Science Studies and Economics" from 2018-2020. This is a draft of a talk that was to be given in that program in March 2020, but was cancelled with Duke's coronavirus closure. Written for a general audience, the talk was not directed to economists, philosophers of economics, or historians of economics, but rather toward attendees affiliated with CISSCT. The talk historicizes the difficult relationship between economics and science studies. The absence of detailed references is a result of the talk's informal nature.

JEL Codes: A1, B2, B4, Y8

Keywords: science and technology studies, sociology of scientific knowledge, science wars, neoliberalism

Duke's Center for Interdisciplinary Studies in Science and Cultural Theory (CISSCT) hosted a two-year program on "Science Studies and Economics" from 2018-2020. This is a draft of a talk that was to be given in that program in March 2020, but was cancelled with Duke's coronavirus closure. Written for a general audience, the talk was not directed to economists, philosophers of economics, or historians of economics, but rather toward attendees affiliated with CISSCT. The absence of detailed references is a result of the talk's informal nature

Science Studies and Economics: An Informal History

By E. Roy Weintraub¹

Prologue

I've been asked by Rob Mitchell to talk about science studies and economics. My dithering initial reluctance to accept Rob's invitation was that to do the job respectfully, as a scholarly project detailing the interconnection of two research traditions, I'd have to spend a lot of time working with several archives and related autobiographical and biographical accounts. But as I told Rob, "I'm getting too old for that." Instead what I'm going to do today is talk through an informal history of the relation between science studies and economics. The few economists among you here will realize that this informal history is both too cavalier and too interested in many details. But in outline, and with respect to the larger issues, I hope it will allow you to understand a bit more clearly the currently fraught relationship between science studies and economics.

Section 1: Introduction

People whom we describe as economists have always had a degree of self-consciousness about the activities they pursue and the arguments they make. But all scholars, in all disciplines, engage both

¹ Emeritus Professor of Economics, Duke University. I have incorporated several useful comments on earlier drafts from Bruce Caldwell, Yann Giraud, Steven Medema, and Barbara Herrnstein Smith.

the knowledge produced and the images of that knowledge as they are developed, discussed, and employed to transform ways of thinking about the knowledge itself. In the 19th and early 20th Century these meta inquiries were pretty much incorporated into the same works of scholarship as the knowledge production itself.

Before the mid- 1930s, economics was not heavily theorized and individual scholars went about their business of doing economics without a great deal of self-consciousness about the activity itself. Two events changed how economists reflected on their craft. The first was the economic calamity that resulted from World War I. Economists had studied the business cycle earlier. Indeed, the creation of the Federal Reserve Bank was a result of concerns to tame the cycle. But with the Depression's onset, the need to understand the business cycle began to take over the intellectual lives of mainstream economists. The newly created Rockefeller Foundation created and funded business cycle research institutes in Kiel, Rotterdam, Cambridge, Oslo, Vienna and so on. In the US, the Cowles Commission and the National Bureau of Economic Research were similarly engaged. These institutes were committed to the modern scientific, meaning analytical and statistical, analysis of business cycles. They were staffed and directed by individuals familiar with and enthusiastic about mathematical and quantitative theorizations. It is thus not accidental that the founding members of the Econometric Society, the Society created in 1930 to foster economic analysis using mathematical and statistical techniques, numbered more Europeans than Americans.

The second contingency was World War II. Economists, specialized in thinking about the allocation of resources, were enthusiastic participants in the war effort. In the UK they ran the war economy. In the US, they immediately worked on traditional activities of controlling prices, wages, rents, profits, and interallied war loans and debts. But they quickly engaged in projects like scheduling air frame production and designing efficient shipping routes. They worked on antisubmarine warfare, antiaircraft firing control, radar, optimal military and civilian diets, and so on. On these projects

economists worked side by side with mathematicians, physicists, engineers, and psychologists. That work developed into a new field later called, in the U.S., operations research. Thus, over the 1940s economics began to look different. If we open economics journals from the 1950s and compare them to journals from the 1930s, the difference is clear. Gone were philosophical explorations in political and moral philosophy. Newly present were statistical and econometric arguments and theorizations of a developing intellectual framework that became modern neoclassical economics. By the end of the 1940s economists could argue successfully that the scholarship and research they were producing entitled them to receive research grants from the newly created National Science Foundation. By the end of the 1940s, economists at the emerging major research institutions no longer argued about whether economics was a science. The interesting question was “what kind of science?”.

Section 2: Economics and the History and Philosophy of Science

For economists outside the (then-emerging) mainstream research centers, for business economists, newspaper columnists, and political hacks claiming economic authority, there was little interest in or understanding of these matters. They continued discussing the rather silly question of whether economics was or was not a science. Most introductory, and even intermediate and advanced, economics textbooks of the time spent their first couple of pages presenting confusing answers to that question. The textbook itself simply presented standard economic analysis. Even though most mainstream economists described their work as “neoclassical”, there were some intellectual differences among them. In the U.S., economists at the University of Chicago claimed to be heirs to a tradition of economic analysis and economic policy somewhat different from the emergent Keynesian view that approved of government interventions to stabilize and regulate economic activity. In the U.K, those at Cambridge and environs who considered themselves to be the only true followers of Keynes began

arguing that American Keynesians had it all wrong. In Joan Robinson's famous phrase, the Americans were "bastard Keynesians", or sometimes simply "bastards". At the same time, Great Britain and the United States welcomed refugees from central Europe who had considered themselves subjectivist followers of Carl Menger, Ludwig von Mises, and Friedrich Hayek. Their anti-communitarian views were quite opposed to any government interventions in free markets. For them, and for many business economists, Keynesians were socialists, or even communists, at heart. There were to be sure a few Marxists still out there. But the imprecations of the McCarthy period had removed all left-wing training grounds in schools set up by labor unions and radical organizations. The few remaining Marxist economists were pushed into schools and universities with no graduate programs or much funding of any sort.

Thomas Kuhn's 1962 *The Structure of Scientific Revolutions* exploded in economics as elsewhere. Paradigms appeared everywhere. Kuhn's own arguments were read by different kinds of economists as "It's OK that we differ. We just have different paradigms." This response seemed to emerge first in the U.K. in the writings of some of Joan Robinson's students. For them, U.K. Keynesians and American Keynesians simply had different paradigms. This kind of argument fit well with the preconceptions of many economists. The label "Marginalist Revolution" had been used to describe the period in the latter part of the 19th Century. That was when the Classical Economics of Smith, Ricardo, Malthus, Mill, and Marx seemed to answer fewer and fewer important questions. The differential calculus of rational mechanics, what came to be called marginalism, made its appearance in the works of W. S. Jevons, Alfred Marshall, Leon Walras, and Irving Fisher. Some decades later, Lawrence Klein's 1944 doctoral dissertation at MIT under Paul Samuelson had appeared in 1947 as a book titled *The Keynesian Revolution*. For many economists it seemed to describe a Kuhnian revolution. Kuhn though was always suspicious of the social sciences. He was unconvinced by its claim to be a science like physics or

chemistry but he could not control how others read his book. But how did these ideas help economists to understand the history and methodology of economics?

And here, for those who are neither economists nor historians of economics, I've got to share some dirty little secrets about the economics community. First, the movement of the economics profession in the post-war years toward modeling, econometric analysis, formal theorization, and rigorous argumentation was developing alongside a rearguard action that denied all scientific claims of economists. Those individuals never abandoned the idea that economics was a branch of moral and social philosophy. Those who detested the new economics were as varied as the *Wall Street Journal*, Gold bugs, Maoists, and the Pope. But Neils Bohr's observation that physics makes progress funeral by funeral did its good work in economics. The GI Bill produced hordes of veterans studying economics in order to find good jobs. The huge growth of American business schools occurred in precisely that period and precisely for those reasons. Those schools trained a new generation of managerial economists who gravitated to operations research. Pedagogically, supply and demand curves were teachable, and upwardly scalable to large lecture classes, in ways that prewar economics as applied social theory and moral philosophy was not.

Prior to the scientific transformation of economics, most economists had a familiarity with the history of their discipline and occasionally felt inclined to write about one or another historical figure or episode. Most economists had read Adam Smith, David Ricardo, John Stuart Mill, and yes, even Karl Marx. John Maynard Keynes, Paul Samuelson, Piero Sraffa, Lionel Robbins, and George Stigler all had written some magnificent history without ever having self-identified as historians. But by the mid-1960s, the structure of the economics profession and the way it educated its students had quite changed. In that period, very few individuals would claim, or would wish to claim, to be historians of economics. Why had this happened?

Around 1960 it was usual for a first-year graduate student in economics to take four courses: microeconomics, macroeconomics or monetary economics, economic history, and the history of economic thought. As statistics and mathematics became more useful as tools, micro and macro remained in the first year as year-long courses. But a two-semester sequence in “statistics for economists” and “mathematics for economists” began appearing in programs around the world. Economic history and the history of economics were each reduced to a one-semester course. By around 1970 the importance of econometrics, data-based analysis, and modeling could no longer be put off until the students’ second year since most applied fields in economics assumed that students were able to use those tools. As a result, courses in economic history and the history of economics began disappearing from the first-year program of graduate students in economics.

As the major Ph.D. research universities were abandoned graduate training in the history of economics, fewer individuals were able to teach the subject. And if the history of economics was not worth teaching to graduate students, then certainly research in the history of economics was not something that could be encouraged in doctoral work. Put baldly, research in the history of economics was no longer considered to be quite up to the standard of work in economics more generally. Around 1970, those with an abiding interest in the history of economics found themselves cast adrift from the economics profession, much as historians of mathematics, chemistry, biology, and physics had earlier been cast out from departments of mathematics, chemistry, biology, and physics.

Section 3: Reorganizing the History of Economics

Around 1970 historians of economics thus found themselves excluded from professional journals of economics and sessions at national meetings. This was not true in all countries but in North America the fact was undeniable. The predictable result was that historians of economics began

creating their own journals and societies. Duke University was the leader of these activities. The first and still premiere journal of the field, *History of Political Economy*, acronym *HOPE*, was created at Duke in 1969 as a Duke University Press publication. The History of Economic Society was organized in a couple of meetings at Duke University and began enrolling members around 1974. Other journals were created over the next decade. By around 1980 the history of economics was developing its own subdisciplinary structures and publication outlets, much as the history of science had done generations earlier.

The history of science's relation to a scientific field provided one model for historians of economics looking to find their own place among economists. Think about the history of physics and the community of physicists say. Some historians of physics had described the development of ideas, theories, and models in Kuhnian terms. Could that framework do any useful work for historians of economics? Karl Popper's writings had done work for Terence Hutchison in the 1930s. Would reading Kuhn, and Imre Lakatos, Popper's student, do useful work for the new community of methodologists and historians of economics? But how to engage these ideas?

We're academics here in this room, so you won't be surprised to hear that the solution was to hold a conference. The first of what would be four important conferences was titled *Method and Appraisal in Economics* and was held in Nafplion, Greece in September 1974. It was hosted by Spiro Latsis, an LSE philosophy Ph.D. whose supervisor was Imre Lakatos. The conferees were tasked to address the applicability of Lakatos's methodology of scientific research programs to theory appraisal in economics. The arguments put forward were associated with retrospective examinations, appraisals, of what made a theory a good theory. Conferees A.W. Coats, Sir John Hicks, Axel Leijonhufvud, Neil De Marchi, Mark Blaug, and T.W. Hutchison represented the history and philosophy of economics, as did Latsis.

The next conference was a turning point for the history and methodology of economics community. Organized by my colleague Neil De Marchi and held in Amsterdam in December 1985, the event honored the retirement of J.J. Klant, professor of history and methodology of economics at the University of Amsterdam. It brought together for the first time individuals who would create the subdiscipline called the methodology of economics. It was at that conference that we all met one another. Daniel Hausman, Wade Hands, Mary Morgan, Bruce Caldwell, and I were joined by Deirdre McCloskey and Arjo Klamer representing McCloskey's new work on the Rhetoric of Economics. Hutchison and Blaug represented the older traditions in Popperian methodology. The title of the conference was *The Popperian Legacy in Economics* and the book of that title was published in 1988. What emerged was a set of challenges to previously settled ideas. Several conferees had been jolted by Deirdre McCloskey's 1983 *Rhetoric of Economics* paper. But most answered the question of "What is the Popperian legacy?" with "Not very much". As the philosopher Daniel Hausman told the conferees: "My thesis is that Popper's philosophy of science is a mess, and that Popper is a very poor authority for economists interested in the philosophy of science to look to." The discomfort of Blaug and Hutchison with this anti-Popperian material was palpable. Both of them were even more angered by McCloskey's work. This resulted in McCloskey's and Klamer's decision to hold a conference on *The Uses of Economic Rhetoric* at Wellesley College in the Spring of 1986. The conference volume, *The Consequences of Economic Rhetoric*, appeared in 1988.

That conference was an all hands on deck melee. Writing for economists, not historians of economics, McCloskey had argued that the usual positivist understanding of a good economic argument was unhelpful. She claimed that attention to the rhetoric of economics would improve economic arguments and thus increase knowledge itself. Attending were radical economists like Robert Heilbroner, Stephen Resnick, and Richard Wolff. They were joined by feminist economists Nancy Folbre and Heidi Hartmann. James Galbraith represented Post Keynesians. Robert Keohane and David Warsh

came from political science and journalism. Adding to the confusion were philosopher Cristina Bicchieri, sociologist/historian Coats, economics Nobel Laureate Robert Solow and literary scholar par excellence Stanley Fish. Philip Mirowski made his grand entrance into these communities with a paper that pretty much offended everyone. The conference ended with no consensus on any matter of substance. Indeed, conference co-director Robert Solow cautioned that “while there is much merit in what McCloskey argues, there is a real danger of going too far.” It’s a delicious irony that this exact charge and wording is a motif of the “Jeopardy Thesis” argument in Albert Hirschman’s magnificent book, *The Rhetoric of Reaction*.

Then in October 1989, Blaug was able to get funds from the Spiro Latsis Foundation to do a follow up of the 1974 Nafplion conference. His idea was to assess recent work in economics that used, or could use, Lakatos’ methodology of scientific research program ideas. The conference, in Capri, was as raucous as the setting was lovely. Historians of economics, philosophers of economics, straight economists, philosophers of science, and heterodox economists gathered to address Blaug’s call. With De Marchi’s assistance, I managed to secure invitations for sociologists Harry Collins and Karin Knorr Cetina.

The conference erupted as a majority of the participants wanted nothing whatsoever to do with Lakatos and his methodology of scientific research programs, arguing it was a dead end unsuited to historical research. The papers went all over the place, with the final straw for Blaug being the papers by Knorr Cetina and Collins. Blaug became so irate that he refused to allow those two papers to appear in the 1991 conference volume titled *Appraising Economic Theories*. He even managed to leave out Knorr Cetina’s name from the list of participants. He and De Marchi were so at odds over it all that Blaug refused to write a joint introduction, so De Marchi alone wrote it. Blaug’s “Afterword” complained that “No one could possibly have predicted how the mixture of people collected at Capri would react to our instructions but I was personally taken aback by what could be generally be described as a generally

dismissive, if not hostile, reaction to Lakatos's MSRP. Of the 37 participants, I estimate that only 12 were willing to give Lakatos a further run for his money and of the 17 papers delivered at the conference not more than five were unambiguously positive about the value of MSRP." One consequence of it all was that, when I got back to Duke, I immediately secured the Collins and Knorr Cetina papers for a mini-symposium in our journal *The History of Political Economy* (29, 4) that I titled "SSK and the History of Economics". Thus over the 1990s, SSK began to find its way into the history of economics.

Section 4: SSK and the History of Science

The history of SSK is well known and need not detain us here. The earliest forays from Edinburgh led a larger group of sociologists of science to begin to rethink what a sociology of scientific knowledge might look like. These moves engaged historians of science and scientists somewhat differently. Many working scientists were outraged. The idea that scientific knowledge was not simply found but was instead created, and stabilized, through complex interrelationships among investigators, the natural world, instrumentation, and so on led to the intellectually clumsy science wars.

Historians of science, faced with new ways of thinking about and describing the activities of members of the scientific community, were both intrigued by, and uncomfortable with, the new ideas. Questions of whether SSK informed accounts really did good work in the history of science got intertwined with arguments about the relevance of the philosophy of science in the post-Kuhn period. What appeared to be a naturalistic turn in the philosophy of science required adjustments in the thinking of historians of science. A number of historians of science saw that this new vocabulary, and strategies of interpretation of scientific activity, opened interesting new perspectives on the activity of doing science.

By the late 1990s matters were stabilized sufficiently that Jan Golinski could write the book *Making Natural Knowledge: Constructivism and the History of Science* (1998). He asked historians of science the question “How might our project be different were we to take these ideas seriously?” Among historians of science there was not abhorrence of these ideas as was expressed by hard-headed natural scientists like Nobelist Steven Weinberg. Those folk worried that these constructivist ideas delegitimized science. Even more agitated were many philosophers of science like Alexander Rosenberg who thought that they alone were protecting The Truth from barbarians like sociologists and literary critics. Nevertheless, science studies continued to be respectful of the work of both traditional and now constructivist historians of science. As the distinguished historian of science Robert Kohler wrote (1999), in reviewing Golinski’s book, “Constructivism is less a foundation ... than a grab bag of useful tools from which historians will select those that are useful for their own purposes... New histories will, I suspect, resemble the old in narrative structure ... but with a new leaven of insight into how large-scale structures and changes depend on everyday, local practices and events.” In contrast, the eminent Lorraine Daston was more cautious. Writing in *Critical Inquiry* in 2009 on “Science Studies and the History of Science”, she concluded that the very interdisciplinarity of science studies makes it an unlikely candidate for synthesizing “the rich but scattered and fragmented materials gathered by some twenty years of historicized history of science.”

Section 5: SSK and The History of Economics

More concretely and to the point for today’s talk, there were few attempts to employ STS in understanding and interpreting economic analysis. Probably the most important were the interventions by Donald MacKenzie in his 2006 book *An Engine, Not a Camera* and then in the 2007 collection he co-

edited titled *Do Economists Make Markets? On the Performativity of Economics*. The reception of these works in the economics community is particularly telling. There was precisely one review of each of the McKenzie books in a mainstream economics journal. David Colander reviewed the 2008 volume on *Performativity* in the *Journal of Economic Literature* (46, 3), one of the then three official journals of the American Economic Association. He began his review by saying “I had no idea what performativity was...so what would I have to say in review? But my curiosity got the best of me, and I said yes, I would look at it. I’m still not precisely sure what performativity is, but I am sure that it is not my cup of tea. In fact, reading the book reaffirmed in my mind my decision to become an economist rather than a sociologist. While I am critic of economics, I suspect that I would be a much more adamant critic of sociology and science studies. For all its faults, economics avoids some of the linguistic knots that tend to characterize theoretical discussions in sociology and science studies.” This was from a distinguished professor at Middlebury, a past president of the History of Economics Society, who continuously bragged about his catholicity of taste and wide-ranging reading habits. Similarly, economics Nobel Laureate Eric Maskin, writing (2004) a review in that same journal of another STS shaped history, commented that “The book’s organization is rather unusual, at least for a history of economic thought ...[but] perhaps books in the Science Studies tradition ... follow unorthodox formats.”. And in that same important journal this year, economic theorist M. Ali Khan (2020) reviewed a prize-winning history of the solution of a famous problem in economic theory. He wrote: “My reading ... contest[s] whether a history of economic analysis, much less a history of economic thought, can be written by taking refuge in the vernacular of ancillary discourses orthogonal to the subject matter whose history is being written ...”. This is of course the science-warrior battle shout that “only real and important scientists are equipped to speak about science at all.”

The fact that working mainstream economists like Colander and Maskin and Khan were not and are not interested in science studies didn’t mean however that science studies was invisible. It was

taken up rather within the history of economics subcommunity. I've already described how McCloskey's forays into the rhetoric of economics linked to some emergent work in science studies. Two other individuals also began employing science studies sources and arguments. One was Phil Mirowski whose historical work linked the history of economics with the history of physics and used the latter to contextualize the former. Mirowski's work was widely read and greatly praised by some but not all historians of economics as opening up different kinds of ways of thinking about the development of economic ideas. Mirowski forced attention to cross-disciplinary connections that historians of economics had usually shunned.

The second historian who began taking up these sorts of issues was me. Using my sabbatical funds, I was able to secure a fellowship year at the National Humanities Center in 1989-90. In conversations there with historians and literary theorists, and free from any obligation to limit my reading, I conceived and wrote a book published in 1991 titled *Stabilizing Dynamics: Constructing Economic Knowledge*. The book's arguments engaged with non-economists like David Bloor, Stanley Fish, Bruno Latour, and Hayden White to reconfigure the then canonical history of how ideas about equilibrium, statics and dynamics developed in economics from the 1930s to the 1970s. Mirowski's book, and mine, and McCloskey's set up what came to be understood as the intervention of the gang of three into the history of economics subcommunity. We each had our own Ph.D. students, and their work as young scholars brought these STS ideas in the history of economics to broader attention in that community. There was one difference among the three of us however: McCloskey was not a critic of economic analysis but was rather a critic of how economists thought and wrote about what it is they were doing. She was a critic of the philosophy of economics, known as the methodology of economics. Mirowski was a critic of both economics itself and particular mainstream economists whom he regarded as complicit in a flawed, even immoral, enterprise. Me, I wasn't a critic of anything since unlike both McCloskey and Mirowski I was not trained as an economist and didn't have any dog in a fight about

whether that community was up to no good. My concern was to find ways of talking and writing about the history of economics very broadly in terms of the sociology of economics, the rhetoric of economics, the anthropology of economics and so forth. In short, I was trying to find ways to write rich and compelling narratives in the history of economics.

Naturally, I began to catch hell from all sides. Such work was not much imitated in North America. The reason is fairly obvious. Individuals who were trained, indeed narrowly trained, as economists, and who became historians of economics, continued to think of themselves as economists. Yet, the “S” in “SSK” was “Sociology”, and what economist in their right mind wanted to be seen talking to them? And as for consorting with literary theorists, the less contact the better with such evil canon-trashers and politically correct deconstructionists. It was not just a kind of right wing in the mainstream of the economics and history of economics communities that reacted this way. The left wing was even more hostile. Recall that Alan Sokol justified his *Social Text* hoax as call for a return to socialist values in literary studies. Similarly, righteous leftists believed that the true role of the history of economics was to provide an informed critique of mainstream economics in order to clear away the intellectual rubbish that supported a contemptible capitalist economic system. Sraffians and neo-Ricardians were especially sympathetic to such rabble-rousing.

That was twenty-five years ago. The response in more recent years has been muted. Although science studies is not a distinct discourse in the history of economics community in North America, it has influenced many papers and books coming out of Western Europe. In England, Mary Morgan’s important position at LSE, and her early engagement with the work of Lorraine Daston, Norton Wise, Ted Porter, and Nancy Cartwright, shaped her unique historical sensibility that she has passed on to her doctoral students. Moreover, her half-time chair at the University of Amsterdam invigorated Dutch and European scholarship in the history of economics and facilitated collaborations across national borders. In France the remarkable Philippe Fontaine, at the École Normale Supérieure de Cachan, helped his

many Ph.D. students escape the straight-jacket of historical work that hardly ventured beyond *l'explication de texte*. Often collaborating with the University of Birmingham's Roger Backhouse, Fontaine and his students have reshaped scholarship in the history of economics in France and Western Europe. Harro Maas, originally at Amsterdam and Utrecht, now at the Walras Center at the University of Lausanne, has likewise fostered exceptional cross-disciplinary collaborations, for instance with Marcel Boumans and Andrej Svorníček. Work done at these centers showcase the usefulness of the SSK vocabulary and a willingness to employ ideas from science studies to construct serious histories of the creation and dissemination of economic knowledge. Indeed, in our 2019 edited book titled *The Historiography of Contemporary Economics*, Till Düppe and I engaged eleven younger European historians of economics to exhibit the usefulness of a variety of STS approaches to historical work. This was precisely the kind of the material that Lorraine Daston had earlier said did not exist. Now it does.

Section 6: Economics and Science Studies Today

But what of today's science studies community? How does it view its connection to economics? Here I'm afraid matters are simultaneously less clear and more contentious. Some individuals within the SSK/STS community like Donald McKenzie, Michel Callon, and Emmanuel Didier have done superb and intellectually engaging work. Yet such work remains scarce. For instance, the 1995 edition of the *Handbook of Science and Technology Studies* had dozens of subject index entries for economics, economic theory, and so on. The 2016 4th edition however has not a single such index entry. Not one. It would appear that the larger STS community has bought into Philip Mirowski's continuing claim that the scholarly discipline of economics is a self-serving apologia of the neoliberal, capitalist establishment that removed him from the Economics Department at the University of Notre Dame. For Mirowski and his followers, STS must become a critique to unmask the nefarious interests of economists in preserving the

neoliberal world order. You should not be surprised to hear that those of us who work in the history of economics and are trained in the analysis of economic arguments are appalled at the polemical intensity of these writings. Almost none of them, and I have read most of them, engage honestly with the history of economics. One consequence is that many individuals in the STS community think “Mirowski” when they think of “Economics”. Alas. Perhaps even worse is that many members of the economics community also think “Mirowski” when they think of STS.

I’ll end now by asking you to rethink that vision of economics as an evil empire of neoliberal tools of capitalism. The term “Neoliberalism” masks a complex set of ideas, contingent both in time and place. It was not birthed by either the Mont Pelerin Society or the Koch brothers, nor is it a synonym for “capitalism”, “economic injustice”, “markets *uber alles*”, or the “commodification of the human”. Michel Foucault thought deeply about neoliberalism’s cultural history in the 1970s. More recently intellectual historian Daniel Rodgers has written brilliantly on the subject, and sociologist Elizabeth Popp Berman has accurately described (2013) how “The policies that changed the organization of science in the United States included some that intervened in markets and others that expanded their reach, and were promoted by some groups who were skeptical of free markets and others who embraced them.” Historians of economics like my colleague Bruce Caldwell have been writing about the various meanings, and histories of those meanings, of “neoliberalism”, for many years. Perhaps as more attention is paid to the confusions about the nature of economic analysis produced by use of that word, the STS community can engage more usefully with the large intellectual enterprise, the science if you will, of economics.

We historians of contemporary economics would quite enjoy having that conversation.