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ECONOMIC SCIENCE WARS

BY
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"Thomas Kuhn told us years ago that natural scientists treated their own history the way the Ministry ofTruth treated its news archive: infinitely pliable, unconstrained by evidence, always attuned to make Oceania appear like the very apotheosis of human progress. Combine this with the neoclassical penchant for an ahistorical account of human rationality, and no one should expect a neoclassical economist to do anything but repudiate the history of economics as a scholarly subject" (Mirowski 2004a, p. 504).

It is not news that the history of economics is disesteemed by most economists. There have been almost annual discussions at professional meetings about the institutional role of the history of economics. Indeed, a conference in 2001 documented the precarious state of the field in North America, and its even more perilous position in the United Kingdom and the Antipodes (Weintraub 2002b). With the exception of Duke University there are no longer any regularly scheduled graduate courses, let alone programs, in the history of economics at any "top" university in North America (Gayer 2002).[1]

To begin to consider this problem, if indeed it is a problem, consider a paper (Blaug 2001) published around the time of that 2001 conference, whose title "No History of Ideas Please, We're Economists" nicely frames some important issues.[2] In it Mark Blaug, addressing an audience of economists, began by noting the disappearance of the history of economics over the post-war period from university economics curricula. To appraise that loss Blaug reprised several traditional arguments made by historians of economics for having economists study the history of economics,

Department of Economics, Duke University, Durham, NC 27708-0097. An earlier version of this paper was delivered as the Jérome Adolphe Blanqui Lecture to the European Society for the History of Economic Thought on April 30, 2006 in Porto, Portugal. With the usual caveat, I am grateful for extensive comments on earlier drafts from Roger Backhouse, Simon Cook, Neil De Marchi, Paul Dudenhefer, Deirdre McCloskey, Phil Mirowski, Alex Rosenberg, Margaret Schabas, Barbara Herrnstein Smith, and two anonymous referees.

[1]That the role for, and status of, the history of economics is different in Europe and Japan from that in the U.S. and UK is apparent, but I have nothing to say in this paper about their different histories—I leave it to others more informed than I am to illuminate the cross cultural differences (see though Weintraub 2002b, and the contributions there which discuss the traditions of HET in Italy, France, Germany, Japan, Spain and Portugal, and the Netherlands.).

[2]The paper appeared in the American Economic Association's *Journal of Economic Perspectives* and probably had the largest circulation of any piece written by a historian of economics in the last decade.

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noting specifically the opportunity of discovering forgotten ideas, and the pedagogical advantage in teaching material with historical connections. Referring to Stigler's and Schumpeter's views he suggested that there is an enhanced "gut level" understanding of economics when there is some history attached to it. He then went on to describe the distinction between rational reconstruction and historical reconstruction, where by the former he meant presenting the history of economic thought as a history of economic analysis and representing past work in current terms, while by historical reconstruction he meant presenting work, past work, in the context of then contemporary ideas and settings. He argued that rational reconstruction has little or nothing to do with historical understanding. Thus he had to face the problem: how is understanding the past, in terms of the past, of value for modern students and teachers of economics? He argued "no idea or theory in economics, physics, chemistry, biology, philosophy, and even mathematics is ever thoroughly understood except as the end-product of a slice of history, the result of some previous intellectual development. I never understood the calculus I learned in school until I read the accounts of the Newton-Leibniz disputes . . ." Blaug's argument that he himself understands intellectual products better by knowing something of their genesis is one supporting element of what he believes on other grounds as well, that the history of economics is too important to be so neglected by economists. His argument may even convince some individuals, but opportunity costs may render it nugatory. Moreover, that argument which focuses on understanding current economics requires constructing presentist histories, which are historically thin and mostly uninteresting to historians.

It is however a curiosity that Blaug's discussion, indeed most discussions, of the proper connection of the history of economics with economics has been so a-historical. That is, if to write history is to write context, we have had virtually no historical narrative examining the break in the history-economics connection that we observe today.[3] In what follows, I will attempt to provide that contextualization.

My narrative is inflected by two interconnected conflicts, namely the "Two Cultures," and the "Science Wars" (Guillory 2002). The phrase "Two Cultures" dates specifically to C. P. Snow's Rede Lectures on the *Two Cultures and the Scientific Revolution* (1959), but the humanities' vision of the scientist as "other" goes back much earlier. Indeed, the development of economics, sociology, and anthropology in the late nineteeth century reflects a scientism that suggested that the important and useful knowledge of the world and its peoples could best be gained by science, not by philosophy, history, the classics, or literature. Commenting on the controversy associated with Snow's lectures, in which opposition to the idea of a scientific culture was led by the literary scholar F. R. Leavis, sociologist C. Wright Mills observed, "In the past, literary men as critics and historians made notes on England and on journeys to America. They tried to characterize societies as wholes, and to discern their moral meanings. Were Tocqueville or Taine alive today, would they not be sociologists?" (1961, p. 17).[4]

[3]But see Backhouse (2004) for a partial reconstruction of the UK story. An essay by Craufurd D. W. Goodwin (forthcoming) titled "The History of the History of Economics" will be published in *The New Palgrave, Second Edition*.

[4]As quoted in Guillory (2002, p. 483).

This humanities-science cultural divide is central to disagreements about the proper role of the history of economics. But also central to such disagreements is the second conflict, the "science wars," shorthand for a set of intellectual controversies that erupted, initially in the United States, in the late 1980s. It appeared to involve some scientists' belief that the emerging field of science studies, as an ally of "postmodernism," was in league with those who wanted to de-legitimize science. Although this controversy was artificial, and misguided on the scientists' part, it touched on some real matters of fact and institutional power.[5]

Where can we locate the loss of history in economics? Looking back at the journals in the 1930s, it is apparent that history was "employed" rhetorically in economics. Knowing what the canonical texts had to say about wage differentials, tariff policy, or monopoly and employing those texts in support of an economic analysis was common practice. In the post war period, however, this style of argument began to disappear. This period also saw the emergence of the history of science as a new and separate sub-discipline; its creation was associated with a perceived need to make science more important in the national interest, which of course entailed that interests opposed to science were to be cast aside. History of science, not scientific training *per se*, was to be the vehicle by which an informed citizenry could understand the nature and importance of science in society. That is, following World War II, a time during which successful big science had promoted the war effort, both scientists and the new Cold War prosecutors sought to maintain that large scientific establishment. From the prewar period in which the scientists did not command public resources, but rather were engaged in private investigations, the idea of what science was, and how it was best done, changed dramatically. As Bird and Sherwin (2006, p. 549) observed, colleges, universities, and the private sector supported scientific research in the first three decades of the twentieth century, but "For several decades, American scientists had been leaving the academy in droves for corporate jobs in industrial research laboratories. In 1890, America had only four such labs; by 1930 there were over a thousand. And World War II only accelerated this trend." In the postwar period the scientific elite needed public support for science resources, and meeting that need required a public sufficiently literate about science, and identified with science's own objectives, so that they would continue to provide scientists with access to public money for scientific research.[6] As has been well documented, in the United States the major force associated with these kinds of attempts to change the public mindset—to

[5]The most fair-handed and cleanly argued discussion of this is appears in chapter 8 ("Microdynamics of Incommensurability: Philosophy of Science Meets Science Studies") of Smith (1997). Her new book (Smith 2006) enlarges on and extends the arguments made there.

[6]It might be thought that a similar role could have developed for the history of economics, namely one of attuning citizens to the importance of economics and its success in shaping the modern world. Unfortunately, however, economics was not so understood, possibly because many individuals believe that they themselves know more than economists about the issues that economics deal with. Despite Keynes's line about "madmen in authority," the discipline of economics never did have the prestige of physics in that post Great Depression era. By the time there were Nobel awards given to economists, suggesting an "arrived science," the "history of economics" had been replaced by the "principles of economics" as the site for general education of those informed citizens. And despite attempts to use some history of economics in such a course, neither the Samuelsonian "potted" historical textbook inserts, nor secondary reading in Heilbroner's *The Worldly Philosophers* (1953), ever really shaped the intellectual content of the large introductory course.

educate the public to the needs for public resources for scientific research—came from those who were connected to the scientific *cum* military establishment in WWII, and to the emergent Cold War anti-communist establishment. From Vannevar Bush and associates who created the National Science Foundation, to the increased number of university courses that attempted to explain science to students who would become the future decision makers in society, science became a matter of public concern, and hence a public enterprise. Indeed, "For a few years after World War II, scientists had been regarded as a new class of intellectuals, members of a public-policy priesthood who might offer expertise not only as scientists but as public philosophers" (Bird and Sherwin 2006, p. 549).[7]

But from a prewar period in the university in which "science education" meant the training of new scientists, science education became part of the general education programs in the liberal arts. These new courses were designed to teach students about science using the history of science as the educational material. The two-volume *Harvard Case Histories in Experimental Science* (1957) by Harvard's chemist-President James B. Conant was, for instance, developed for the Harvard General Education curriculum but had wide success as the feature selection and free membership gift for joining the Science Book Club in the 1950s. Science was no longer characterized as open to the average citizen's understanding, as Karl Pearson's *The Grammar of Science* (1892) had earlier maintained. In Great Britain, the history of science had similar characteristics. From the 1930s when the Cambridge History of Science Committee was organized by scientists Joseph Needham and Walter Pagel, to the 1940s and 50s when it came to be dominated not by scientists but by historians like Herbert Butterfield, the history of science had come to play a service role in a liberal humanism that was associated with "a positivistic protocol" (Mayer 2000, p. 665). From those years on, British history of science was no longer dominated by scientists, but rather as history it became part of the culture of the humanities. This occurred, as Mayer (2000, 2004) notes, despite the deeply contextualized historical work that scientists like Needham had pioneered. It was not so much a turn to "real history," non-Whiggish history, as the history of science disciplinary creation myth maintains, but rather that the history of science was reconstructed to serve "a public discourse [in the Cold War period] praising science as an embodiment of liberal values" (Mayer 2000, p. 682).

Elite university students at, say, Harvard and Cambridge (UK) were thus taught that scientists were members of a professional community who could be trusted to serve the public interest provided that the public provided both support and non-interference.[8] From Pearson's vision of the scientific method as a bulwark of enlightened citizenship in a democratic state, scientific thought was now located in "the scientific community."

As has been described by Fuller (2000), Mirowski (2004b), and many others, one of the more interesting successes belonged to the physicist turned philosopher Thomas

[7]This passage continues (p. 549) by noting, however, that "With [J. Robert] Oppenheimer's defrocking [in 1954], scientists knew that in the future they could serve the state only as experts on narrow scientific issues."

[8]This differed from the public's general perception of economists, who in that period were characterized for instance by President Harry Truman as offering policy advice of the "on the one hand, on the other hand" variety. Unlike physicists, economists disagreed, and were seen to disagree, on many matters of fact and theory and policy.

Kuhn and his slender volume, *The Structure of Scientific Revolutions* (1962). That book, on nearly every list of the one hundred most important books of the twentieth century, provided a vocabulary, a language, for talking about science. Its intellectual origins were in Ludwig Fleck's (1935) idea of "thought collectives," Alexandré Koyré's (1961) ideas about "revolutions," and Jean Piaget's ideas on "genetic epistemology" (summarized in Piaget 1970). But its social origins were in the Harvard general education program in which Kuhn had begun his teaching career. From the period of time in which scientists were seen as lonely investigators searching fearlessly, conquering ignorance and the unknown, Kuhn presented the new view of the scientific *community* as central to scientific research. No longer were scientists characterized as bold and creative individualists. Rather they were members of a community whose work served vital national interests, and whose values were those shared by both the political leaders and the informed community. This emergent view characterized the scientific community's standards as appropriately policed by the scientists themselves, and located expertise about the allocation of resources for science not with public officials but within the community of scientists. This shift of science from an individual enterprise to a collective enterprise, an idea well-developed by Fleck, together with Kuhn's characterization of normal science, reframed the process by which young scientists were understood to be socialized into the best practices of the particular scientific communities.[9]

Such a view of science made the history of science only indirectly important to the actual practitioners of science, its role limited to providing exemplars of past successes and failures. The community's shared understanding of history, learned from the potted histories of the textbooks used to socialize the young into modern science practices, differed from the histories of professional historians, and thus produced different understandings for scientists, and those who studied science—historians, philosophers, and sociologists—of what science had been and had become. But such exemplars were becoming problematical as Kuhn's ideas about science challenged the older view of scientific (or natural-philosophical) knowledge as moving from error to truth. As we shall see, some elements of the science wars developed from particular interpretations of Kuhn, specifically those that focused on Kuhn as proffering a vision of scientific knowledge as intertwined with particular paradigms, incommensurable in some ways one with another. But even more curious, given that Kuhn himself was ambivalent about whether economics was a science,[10] was that economists *and* historians of economics fell in love with Kuhn's ideas in the 1960s and 70s, reframing the history of economics as the development and overthrowing of particular paradigms.

[9]It was in this period of course that the Rockefeller Foundation, and then later the Ford Foundation, began to fund giant programs involving teams of economists.

[10]See Kuhn's response to a comment by Joseph J. Spengler about "crises" in economics: he wrote: "In the physical sciences disagreement about fundamentals is, like the search for basic innovations, reserved for periods of crisis. It is by no means equally clear that a consensus of anything like similar strength and scope ordinarily characterizes the social sciences" (Kuhn 1977, pp. 221–22). In his *Structure* (1962, p. 160) however he was to note: "It may . . . be significant that economists argue less about whether their field is a science than do practitioners of some other fields of social science. Is that because economists know what science is? Or is it rather economics about which they agree?"

Beginning in the post-war period then, publicly funded science operated relatively autonomously. Public scrutiny attached only when there were colossal community failures, like the catastrophic attempts to launch U.S. rockets into orbit in the immediate post-Sputnik period, etc. And if there were attempts by the larger polity to subject science to restraints, they were noticeable for their ineffectiveness: the "socialized medicine" complaint about large scale trials for the Salk anti-polio vaccine in the 1950s, and the "big government" tampering with "our precious body fluids" associated with mandatory fluoridation of public drinking supplies confirmed the scientific community's belief that "the Public was an ass" (Curtis 1861, p. 414).

The histories of science in the post war period were often "internalist" with science presented as a coherent endeavor in which problems or puzzles arose from incomplete or incorrect prior scientific work. The community of scientists was portrayed as doing science to solve science's own questions, solving science's own unsolved puzzles. In this way science progressed, and was associated with the liberal "progressive" mindset. As Mayer (2004, p. 41) frames it, "Anti-Marxism formed a defining feature of the process by which the image of scientific work as a disinterested journey of the mind came to be institutionalized."[11] Science was a paradigm of a content improving-increasing process. This set of ideas, to be sure, had roots in the Enlightenment ideas of a progressive movement from error to truth. The sociology of science, following Merton (1938), concerned itself with questions of rewards and status and socialization and networking in the scientific community. At the same time philosophy of science, having in the positivist period privileged scientific knowledge epistemically, was concerned both with understanding how science produced such valuable knowledge claims and in suggesting how other enterprises (for example, the social sciences) could correct their intellectual deficiencies by attending more closely to successful gambits in the physical sciences (Hempel 1965, Brodbeck 1968).

All of this should, of course, be quite familiar to economists. In the post-war period, economics refashioned itself as economic science (Weintraub 2002a, Mirowski 2002), and the curriculum and socialization processes of young economists began to emphasize developing and testing theory (or at least searching for confirmations), developing and testing hypotheses, and understanding the economics community as one comprised of those who developed, tested, and applied economic analyses. The tools economists used were those that had been employed in the physical sciences themselves and which had proved so successful beginning in the middle of the nineteenth century (Mirowski 1989). From the perspective of the general public, economists' claims that, as a result of enhanced macroeconomic understandings and interventions there would never again be a Great Depression, minimized public awareness of economists' disagreements.

Similar to the historians of science who wrote about the progress of science from error to truth, those who wrote in economics about economics in those years, individuals we would call historians of economics or methodologists of economics, told

[11]It is a bit paradoxical to note that the Marxist communitarian view, opposed to seeing scientific geniuses as the engineers of scientific progress, was taken over and stripped of its ideological wrapping. Now "community" was to be a historically contingent actor, not a historically necessary one. But of course the "freedom" that defined the liberally human scientists, and their community, was often contrasted to, say, Lysenko-ism.

stories of the development of economic knowledge, and how modern ideas had developed from older ideas, and how conceptual and logical error gave way to the modern understandings that economics, as an intellectual discourse, had achieved. The history of economics was a narrative of progress in economics; both Joseph Schumpeter's *History of Economic Analysis* (1954) and Mark Blaug's (1962) *Economic Theory in Retrospect* were well-articulated expressions of the accretion of knowledge in economics as error was eradicated. Similarly, methodologists of economics, looking to the philosophers of science, began talking about positivism, instrumentalism, and all manner of application of philosophy of science ideas to economics, in search of answers to the demarcation question "Is economics a science?" and its uncomfortable corollary "If not, why not?" Economists were thus challenged (Hutchison 1977, Blaug 1980) to attempt to refute theories, to develop more precise methodological dicta, etc. From outside economics though, most philosophers of science writing about economics found that economics, while on the right track compared to sociology and political "science," didn't quite measure up to the physical sciences (Rosenberg 1976, 1992). And of course in this period many sources of public funds became available to those doing scientific economics, as the National Science Foundation, as well as other Federal agencies like the Departments of Defense and Health and Welfare and Agriculture and Interior and Commerce provided grants and contracts for economists to do research on issues important to the grantors. Allocating those funds often required peer review and other processes similar to those well-established in the natural sciences.

This was the context in which, at the beginning of the last quarter of the twentieth century, some kind of peace reigned between Snow's *Two Cultures*. Scientists tolerated historians and philosophers because they were mostly supportive of the scientific enterprise and didn't get in the way of massive public support for that enterprise. And people in the humanities, particularly the historians and philosophers, viewed science as an estimable model for scholarly discourse. The two cultures were in balance of a sort. Science, representing progress, was in the national interest, while the humanities were to receive grudging support (but only in the academy) for elevating the human spirit. The social sciences meanwhile were busy emulating science proper, in order to have an effect on the public purpose. Simplifying greatly, all was well with the world, even as the economics community was beginning to marginalize historical thought, quarantining it to particular courses in the history of economics. It was no longer possible to teach a course say in Price Theory by employing historical texts.[12] What Marshall or Knight said was no longer relevant to any analysis. Increasingly looking to science, economics began requiring its graduate students to master statistics, mathematics, and econometrics. Graduate programs began dropping their foreign language requirements since the quantitative skills were seen to be more important for success in economic science. And as historical reasoning began to disappear in economic analysis, the requirements that students study economic history and the history of economics began to disappear. The first History of Economic

[12]It was in the 1970s, for instance, that Sidney Weintraub, at the University of Pennsylvania, was no longer able to teach his Macroeconomics graduate course using only Keynes's *General Theory* as the text, or the graduate Price Theory with required readings from Marshall. Indeed, from that period on to his death he was allowed to be a teacher of undergraduates only.

Thought Conference, at Sussex in the UK, was held in 1968, while the creation of the first journal in the history of economics, *History Of Political Economy*, in 1969, and the formation of the History of Economics Society in 1974, were at least in part responses to the difficulty historians of economics were finding in both getting their papers published in mainstream economics journals,[13] and the marginalization of their concerns and interests in the larger economics professoriate.

The science-humanities "peace" arrangements nevertheless began to unravel in the 1970s. The story of the Edinburgh group and the Strong Program in the Sociology of Scientific Knowledge (SSK) is well-known, and has been narrated to philosophers and historians of economics quite well on several occasions (Hands 2001). Whereas Popper had refused to countenance a naturalistic theory of science,[14] one which looked at science itself to construct a theory of science, Edinburgh took that naturalistic turn and looked to the practices and processes of science to explain the construction and development of scientific knowledge. SSK asked questions like "what are the practices that lead scientists to make claims about their objectivity?" which contrasted to earlier questions like "what is scientific objectivity?" Unlike the earlier explorations of the objectivity question which suggested that science was (or should be) interest-free and asocial, the newer answers from SSK embedded both the scientific community and its products, including scientific knowledge itself, in the larger social world. Scientific knowledge was irredeemably social and political and historical. This set of concerns led SSK scholars to studies of issues like the mutual stabilization of clinical diagnoses and clinical categories, the processes whereby scientific disputes "were closed," the role of laboratories in grounding and constraining knowledge practices, the processes by which rewards are apportioned for scientific work, etc.

That is, science came to be described, in these new writings in the humanities and social sciences, as a product of human communities. Consequently, for those non-scientists writing about science, there was diminution of interest in the special nature of scientific knowledge products at the same time there was an increased interest in how those products were developed and how the community constructed vocabularies, practices, and rewards in its evolution over time. This view of contextualized science suggested that there was great purchase in understanding it as a cultural product in the same way that art, music, cooking, and sport were cultural products, contingent in time and in place. And as the interests of some historians, sociologists, anthropologists, and rhetoriticians of science began to fuse, the boundaries between these separate disciplinary approaches to studying science became fully permeable. What emerged were new ways of writing about science in the last quarter of the

[13]Mark Perlman tells of how, early in the history of the *Journal of Economic Literature* (it began in 1969), he dramatically increased the number of *JEL* citations in history of economics by unilaterally deciding that articles about the meaning of Keynesian economics would be moved from the Macroeconomics classification to the History of Economics classification. See also Backhouse (2004) for the detailed story of these moves in the UK.

[14]*The Logic of Scientific Discovery* (1934), Chapter 2, section 10 is titled "The Naturalistic Approach to the Theory of Method." In it Popper argues against naturalism, and concludes "Thus I reject the naturalistic view. Its upholders fail to notice that whenever they believe themselves to have discovered a fact they have only proposed a convention. Hence the convention is liable to turn into a dogma. This criticism of the naturalistic view applies not only to its criterion of meaning, but also to its idea of science, and consequently to its idea of empirical method" (p. 53).

twentieth century, and the assemblage of such approaches came to be known as *science studies*.[15]

Looking at science and its practices closely (as a set of particular craft activities) was not always congenial to scientists who wished to see their work as a heroic progressive struggle against, in Keynes's words, "the dark forces of time and ignorance" (Keynes 1936, p. 155). This tension between the older scientists' view of science and the perspectives of those who studied science in this new SSK was one thread of what was to become the "science wars." It was, though, a strange war in which only one side was fighting. Historians and science studies scholars were by and large baffled at the accusation that they were "attacking" science, or wanted to de-legitimize it, since the richness and complexity and importance of science in modern society is what drew those historians to devote their careers to understanding science and its larger social context.[16]

It was not until the late 1980s and early 1990s that these kinds of ideas began to surface in writings about economic science as well. Although this literature was never hegemonic among contributions in history and methodology of economics journals, individuals like Deirdre McCloskey, Arjo Klamer, Phil Mirowski, Esther-Mirjam Sent, and me began writing about the history and philosophy of economics from a science studies perspective. The vocabulary of historians of economists and methodologists fairly quickly began to reflect the use of these ideas in an ever-wider set of contexts (for example, Leonard (1998), Fontaine (2002), Backhouse (2004), etc.) These shifts are admirably documented in D. Wade Hands' (2001) book *Reflection Without Rules*. Yet just as the sciences studies approach to the history of science discomfited scientists, so too did the science studies approach to writing the history of economics discomfit both economists and the majority of those writing in the history of economics.[17]

A number of scientists were openly critical if not hostile to the history of science done this new way: "In recent years, a relatively small number of scientists suspicious of historians' non-linear, non-whiggish, and non-celebratory histories and of their increasing independence from science, see historians and those with whom they intellectually associate as somehow attacking the 'moral stature,' 'epistemological authority,' and therefore the legitimacy of science" (Hughes 1997, p. 22).

But there is one other skein to untangle. It has a different source, the "culture wars," although it traces its lineage back through the "Two Cultures" debates to earlier conflicts both in and outside of the academy. To document the sequence of moves here is difficult, for a complete narration of the role of the humanities in both education and public life would begin long before the nineteenth century. But variously over that century science became more central to more intellectual projects, and increasingly intellectual cachet accrued not only to the classically learned, but to science and scientists.

[15]To scan the ideas and approaches encompassed, see Bagioli (1999).

[16]There were, to be sure, some people involved in science studies who were "debunkers" in one or another sense. Their own use of language mocking or undermining the normal ways of talking about science was quite in evidence. Feminists who deplored the unwittingly gendered accounts of, say, human reproduction, or Marxists who argued that modern science was a feature of the Cold War's military-industrial complex, allied themselves often with science studies. The larger point remains, though, that science studies itself was not an "anti-science" movement even as some who wished to de-legitimize science's role in the modern world (Ross 1996) embraced elements of the science studies analyses.

[17]What is not so often understood is that science studies scholars themselves (for example, Ashmore et al. 1989, Yonay 1998) began writing about economics in this period.

Economists of course know this, for it was in that period, late in the twentieth and then through the first decades of the twentieth century that economics sought to become, and indeed did become, a science. And as the social sciences developed and began to define how the world was to be understood in its complexity—social structures, demographic details, economic activity, family and church and all manner of human institutions—the importance of the humanities, those proto-disciplines which had traditionally been the sites for understanding the world, began to share their explanatory authority with the social sciences. And over the course of the twentieth century, within the university the humanities became one of the three divisions of humanities, natural sciences, and social sciences.

The culture wars that "broke out" in the United States in the late 1980s had their roots in a number of concerns variously political, religious, and cultural. Nevertheless the realization that some literary scholars were not "teaching Shakespeare" but were rather exploring the gendered biases of television sitcoms induced frenzy in otherwise sane individuals who seemed to be threatened, and who by projection believed the modern nation state was threatened, by English professors. The strange idea that "political correctness" resulted from openness to racial and gender and religious diversity would be simply curious were it not the case that real damage was done to real people by those who believed that newly emergent work in the humanities, or "literary theory," or "cultural studies" presented a clear and present danger.

The other component of the science wars then was the belief, held by many scientists as well as those who saw science as a model of epistemological rectitude, that treating the products of science and scientific knowledge as local and contingent and historically situated, as it appeared to be from the perspective of science studies, undermined the scientific enterprise. That some science studies scholars employed tools and techniques also employed in studying political communities and literary productions suggested to some scientists and their allies that science was being treated too casually. That issues of race, class, power, and gender were introduced into analyses of the way science operated was unnerving to working scientists. "Reading" the book of nature was no longer a harmless metaphor. For most working scientists endowed with an uncritical epistemological stance, a casual realism, the notion that "those people" did not believe in "the reality of gravity" made it impossible to take those people seriously. The result was the public commotion produced by those opposed to science studies—scientists like Alan Sokol, Paul Gross, and Norman Levitt (1994)—and the actual science wars.

In fact, it is even a bit worse than that, which brings me full round to my title "The Economic Science Wars." The antagonism of working scientists for those who write about science but don't practice it is akin to a phenomenon one sees in televised sports in the United States. In any three-person team of television broadcasters, be it for American football, soccer football, baseball, basketball, etc., one of the three commentators must have been a former athlete in the sport under view. There seems to be a public need to have comments done by someone who has "played the game."[18] Much as scientists think little of historians

[18]One of the most distinguished mid-twentieth century American sports commentators, who trained as a lawyer, was reviled by a large fraction of the American television public for his outspoken commentary which was

of science who were not mature scientists themselves, so too economists appear to have little respect to offer historians of economics who have not, through their work in economics, made "serious" contributions to the discipline: the proof of this pudding would include George Stigler, Lionel Robbins, Joseph Schumpeter, Takashi Negishi, and Paul Samuelson. This ancient dualism of those who do versus those who criticize, or those who "merely" talk versus those who do, is a *leitmotif* of the science wars. The belief that there have not been science wars in economics thus represents a curious blindness. The slow starvation of resources for history and methodology of economics, for any kind of science studies in economics, is an exact manifestation of economic science wars no different in kind, no different in effect, from the physics science wars. In economics departments in the U.S., doing the history and methodology of economics came to be seen as doing no economics at all. It was even worse, of course, for those doing history and methodology of economics were generally seen as critics, often hostile critics, of mainstream economics.

This last point is the crucial one. It explains why the science wars in economics are not associated with that quite small band of Mirowski, McCloskey, Sent, Weintraub, and their Economics-science studies allies. Unlike in the natural sciences, where science studies was seen as the de-legitimizer, in economics it is heterodox economics that is considered de-legitimizing in the sense that in numerous ways it challenges the epistemic authority of "neoclassical" or "mainstream" economics.[19]

There are a variety of impulses that lead professional economists to "take up" the history (and methodology) of economics. It cannot be surprising that many historians who begin as critics of mainstream economics link their historical projects with critical appraisals of modern economics. As Backhouse (2004, pp. 313–16) has argued, as heterodox economics took form in the 1970s, groups of scholars loosely described as Post Keynesians, neo-Austrians, neo-Ricardians, neo-Marxians, and institutionalists defined their identities historically, in relation to a person or body of work that mainstream economists considered to be "in the past." Constructing these heterodox identities meant constructing historical narratives (Mata 2004), and consequently the history of economics they produced became associated with what mainstream economics considered to be low quality or out-of-date theory. This association of heterodoxy with history of economics is real: a quick examination of the program of any History of Economics Society meeting will locate two dominant poles: work on "old" economics (Physiocracy, Adam Smith, etc.), historically engaged in contextualization, and work on issues treated in mainstream

frequently critical of the players, coaches, and practices of the sports he commented on. Indeed, Howard Cosell's (1985) autobiography was called *I Never Played the Game*.

[19]As good evidence of this distinction, Mirowski's historical work is not denigrated by economists nearly to the extent that his critique of modern economics elicits ire. McCloskey's (1986) analytic work on rhetoric was not a flash point in the same way that her use of rhetorical analysis to criticize mainstream econometric practices produced forcefully argued and finally compelling rebuttals (Hoover and Siegler 2005). And I, no critic of mainstream economics, am understood by economists to be a somewhat offbeat historian, but friendly. As Maskin (2004, p. 173) commented, reviewing Weintraub (2002a): "The book's organization is rather unusual, at least for a history of economic thought (perhaps books on the natural sciences in the Science Studies tradition by which Weintraub acknowledges being influenced also follow unorthodox formats)."

economics today, in which there is a critical component, heterodox economics if you will.[20]

This observation is not meant to denigrate critical work, or work generally done in heterodox economics. Nevertheless such work appears to many mainstream economists as having been produced by historians of economics (Post Keynesians are still writing about Keynes, and neo-Austrians are still writing about Hayek) and thus they associate the history of economics as a subdiscipline with heterodoxy. To the degree that the science wars had involved scientists' beliefs that science studies was hostile to mainstream science, just to that same degree are the economic science wars associated with economists' beliefs that heterodox economics is hostile to mainstream economic science. And as a consequence the history of economics, *as and to the extent* that it is associated with heterodoxy, is taken to be (as it is often meant to be) critical of if not hostile to mainstream economics.[21]

Such issues would be innocuous were these simply different approaches or opinions, but unfortunately they can have some real consequences. With the stabilization of economics as economic science by the end of the second third of the twentieth century, there appeared a disjunction in economics between mainstream scientists' views of how to characterize and appraise their enterprise and its intellectual products, and how heterodox economists—and by guilty association historians, sociologists, and methodologists of economics—characterized and appraised that same science. However, the reward system within economics, and the fact that heterodox economics scholars have directly to compete for resources with those whom they criticize, meant that the war was over very quickly. And as "collateral damage," historians and methodologists have been greatly injured. For while physicists cannot de-list a history of physics course from the history department offerings, economists have managed to first de-require, and then eliminate, the history and philosophy of economics from professional-graduate education. While mathematicians at the Institute for Advanced Study (IAS) needed to lead a public campaign (McMillan 1997) to discredit the sociologist of science Bruno Latour (Levitt undated), and the historian of science Norton Wise, to deny them academic posts in the School of Social Sciences at IAS, economists simply appropriate without public comment or notice the faculty positions and tenure lines of retiring historians and use those resources for more "mainstream" work. Sometimes of course the "retirements" are academic executions: the hostility of "real" economists for the non-ceremonial science studies work played some part in the involuntary removal of historians and philosophers of economics from the Economics Department, and graduate teaching opportunities, at Notre Dame University.[22] The resources thus "freed up" allowed the Economics Department to hire "real" economists in their stead (Monaghan 2003). That episode was noisy, but the larger

[20]A quick and dirty count, using titles and personal knowledge of participants, from the 2005 HES Meeting produced fifteen sessions on "old" economics, fourteen sessions which were decidedly heterodox in nature, and nine sessions of "other" which included things like my own session on "Life Writing." The 2006 ESHET Conference in Porto had roughly nine "old," seventeen "heterodox" and nineteen "other."

[21]A referee worried that there are many other reasons for individual economists to have distanced themselves from the history of economics. Of course that is the case, but it would appear that such other "reasons" are unsystematic, perhaps idiosyncratic, but certainly ahistorical.

[22]This of course is reminiscent of Paul Davidson's "exclusion" from the graduate program at Rutgers, discussed in Mata (2005).

economic science war had already rendered historians of economics *hors de combat* on the university battlefield.

Economists' projects and those of historians, philosophers, and sociologists of economics—collectively economics science studies scholars—are different. Blaug's belief about the importance of the latter to the former is simply not credible to mainstream economists. Thus economics does not need someone like an Alan Sokal who can make an intellectual fortune mocking science studies scholars, because historians and methodologists of economics are not seen as any "threat" to mainstream economists. Indeed they are invisible to economists. The science war ended years ago in a science peace in economics. The argument, the war, is over except that the losers still say "you need to deal with us, why won't you pay attention to us?" The economic science war is over and, institutionally speaking, historians of economics have lost. Tarred with the heterodox brush, it could not have been otherwise for them.

But perhaps out of the loss of institutional vitality in economics, a new kind of historical scholarship might emerge. Beginning in the early 1990s (Schabas 1992, Porter 1992) some historians of economics, and other historians of science, have been arguing that it is time for economics science studies scholars, particularly historians of economics, to make new alliances. There are many scholars, and communities of scholars, with whom historians of economics have natural affinities. In particular there are communities in which historical thought styles are valued and interpretative insights are treasured. The science studies community has been "open" to the history of economics, as has the community of intellectual historians who themselves have reemerged in history departments as social history wanes.[23] If historians of economics can shed their professional identification with the community of economists and thus refuse to take sides in the mainstream-heterodox controversies, their return to history, to constructing narratives of context, might brighten their institutional future. The science studies disciplines and subdisciplines, broadly understood to include the history and philosophy and sociology and anthropology of science as well as science and technology studies more narrowly construed, are then alternative sites of institutional support.[24] The institutional challenge remains to find academic sites for programmatic activity in the history of economics, not merely places that individual scholars can "sit and write." The history of economics needs to locate academic sites in North America that can provide *more* support than do research departments of economics. Since a number of historians of economics have intellectual affinities to the science studies community

[23]Mirowski, for example, argues that since the modern university is fragmenting under the pressures of commercialization, there is growing room for interdisciplinarity, like economic science studies, in the emergent interstitial formations (Mirowski 2005). Whether one agrees with his analysis or not, the evidence for his claim is real, and we both are exemplars of it: even as his own position in the "standard" department of economics has disappeared, Mirowski retains his chair in the history and philosophy of science. And for my own part, in addition to teaching science studies as well as the history of economics, I co-edit a book series on Science and Cultural Theory with the eminent (and disciplinarily unclassifiable) Barbara Herrnstein Smith.

[24]For instance, Mary Morgan at LSE has her primary appointment in the Department of Economic History; Kevin Hoover is now a Professor of Economics AND Philosophy at Duke University; Erik Angner is in the Department of Philosophy at the University of Alabama at Birmingham; Margaret Schabas is a Professor of Philosophy at the University of British Columbia, etc. Such a list though misses individuals who have, or have had, careers "outside": for example, Evelyn Forget in Community Health Sciences, and Tom Humphrey at the Richmond Federal Reserve Bank.

broadly understood, that realization can lead to their evolving connection with a potentially more welcoming scholarly community. But historians of economics will have to earn that welcome, since for example many historians consider work in the history of economics under-researched and over-interpreted, while many philosophers believe that too much work in "methodology" is insufficiently nuanced. Training in economics after all prepares one to do economics, not history or philosophy or sociology or anthropology or cultural studies. But early economics training can be an interesting base from which new skills, prized by other communities, can develop. And it is this kind of renewal that holds out some possibility of a more positive institutional future for scholarship in the history of economics.

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