

This is the text of the Jérôme Adolphe Blanqui Lecture as delivered to the European Society for the History of Economic Thought on April 30, 2006 in Porto, Portugal. It was drawn from a much longer paper, of the same title, that is currently (May 2006) under revision. The delivered Lecture is made available here to promote discussion and debate.

Economic Science Wars

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The fact that the history of economics is not held in high regard by most economists is hardly a surprise to you. Indeed, I directed a conference at Duke in 2001 on the future of the history of economics and we documented the precarious state of the field in North America, and its even more perilous position in the United Kingdom and the Antipodes. 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.

In my discussion today, I intend to contextualize the low regard in which history of economics is held. To introduce my argument, I want to draw your attention to Mark Blaug’s paper published around the time of that conference, whose title “No History of Ideas Please, We’re Economists” nicely frames some important issues. Blaug wanted economists to face the following problem: how is understanding the past in terms of the past of any 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. [He says] 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.

It's a curiosity though that Blaug's discussion of the proper connection of the history of economics with economics, like that of others, was so a-historical. That is, if to write history is to write context, we have had virtually no narrative contextualizing the break in the history-economics connection that we observe today. Thus given this opportunity to address such a distinguished collection of historians of economics, I would like to suggest how the divide occurred and to thus show that the rift is likely to be permanent.

I want to position my narrative with respect to two particular conflicts which I shall argue are in fact connected, namely the "Two Cultures" divide, and the "Science Wars". Let me take these up in turn. The phrase "Two Cultures" dates specifically to C. P. Snow's 1959 Rede Lectures on the *Two Cultures and the Scientific Revolution*. But the humanities' vision of the scientist as "other" goes back much earlier. Indeed, the development of economics, sociology, and anthropology in the late 19th century reflects a scientism which suggested that the important and useful knowledge of the world and its peoples could best be gained by science, not by philosophy, or history, or the classics, or literature.

This humanities-science cultural divide will help as I go along to explain disagreements about the proper role of the history of economics. But a second conflict is relevant too, namely the "science wars," a 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.

Looking at the journals in the 1930s, we see that employing historical references was a fully legitimate rhetorical style 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 mode of argument began to disappear. This period also saw the emergence of the history of science as a new and separate sub discipline. The postwar needs of the scientific elite for public support for increased science resources required a public sufficiently literate about science, and identified with science's

objectives, so that they would continue to provide scientists with access to public money for scientific research. History of science, not scientific training, was to be the means by which an informed citizenry could be brought to understand the nature and importance of science in society. 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 practiced, changed dramatically. As has been well documented, in the United States the major force associated with these kinds of attempts to change the public mindset, to educate the public to the needs for public resources for scientific research, came from those who were connected to the scientific-military establishment in World War II, and to the emergent Cold War anti-communist scientific establishment. From Vannevar Bush and his associates who created the National Science Foundation, to the increased number of university courses which 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.

Thus from a prewar period in the university in which “science education” meant the training of new scientists, science education now 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. Harvard’s chemist-President James B. Conant’s 1957 two volume *Harvard Case Histories in Experimental Science* 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 as practice was no longer characterized as open to the average citizen’s understanding, as Karl Pearson’s turn of the century *The Grammar of Science* had maintained. Instead the history and philosophy of science were to be the road to understanding science’s role in society.

In Great Britain, the history of science had similar characteristics. The Cambridge History of Science Committee was organized in the 1930s by scientists Joseph Needham and Walter Pagel. But in the 1940s and 50s it came to be dominated not by scientists but by historians like Herbert Butterfield. In England too the history of science began to play a service role in a liberal humanism that was associated with “a positivistic protocol”. 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 despite the deeply contextualized historical work that scientists like Needham had pioneered. It was not a turn to “real history”, non-Whiggish history, as the history of science disciplinary creation myth maintains. Instead, the history of science was reconstructed to serve “a public discourse [in the Cold War period] praising science as an embodiment of liberal values”.

Elite university students at Harvard and Cambridge (UK) were taught that scientists were members of a professional community who could be trusted to serve the public interest provided that the public provided both financial support and non-interference. Instead of Karl 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” which necessarily excluded other communities.

One of the more interesting successes here was connected to the physicist turned philosopher Thomas Kuhn and his slender 1962 volume *The Structure of Scientific Revolutions*. That book, on nearly everybody’s list of the one hundred most important books of the 20th century, provided a vocabulary, a language, for talking about science. Its intellectual origins were in Ludwig Fleck’s idea of “thought collectives”, André Koyré’s ideas about “revolutions”, and Jean Piaget’s ideas on “genetic epistemology”. 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 in a readable volume this new view of the scientific *community* as central to scientific research. No longer were scientists to be seen as bold and creative individualists, but rather as 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 about the nature of the scientific community presented that community’s standards as appropriately policed by the scientists themselves. It located expertise about the allocation of resources for science not with public officials but with 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.

Such a view of communitarian science made the history of science only indirectly important to the *actual* practitioners of science, even as the history of science was being pushed onto those enlightened citizens whose role was to provide scientists with money. For working scientists, history's role was now 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. It thus produced different understandings for on the one hand scientists, and on the other hand for those who studied science – historians, philosophers, and sociologists – who had different notions of what science had been and had now become.

Yet most histories of science in the early post war period were still written by those who had trained in the prewar period. They were “internalist histories” 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 presented 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 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”. Science was a paradigm of a content improving-increasing process. This set of ideas to be sure had roots in Enlightenment ideas of a progressive movement from error to truth. The sociology of science in this period, following Merton, 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, became more concerned with understanding how science produced such valuable knowledge claims. It also started suggesting how the social sciences could correct their intellectual deficiencies by attending more closely to successful gambits in the physical sciences.

All of this should be quite familiar to economists. In the early post war period, economics refashioned itself as economic science, and the curriculum and socialization processes of young economists began to emphasize theory and data-based reasoning. It also redescribed the economics community as one comprised of those who developed theories,

and those who tested or applied them. The tools for economists to use were those that had been employed in the physical sciences. Economists' claims that because of their expertise there would never again be a Great Depression served to minimize public awareness of economists' disagreements.

Like the historians of science who wrote about the progress of science from error to truth, historians of economics and methodologists of economics told stories of the development of economic knowledge. They told stories of how modern ideas had developed from older ideas, and how conceptual and logical error gave way to the modern understandings that economics had achieved. The history of economics was a narrative of progress in economics; Mark Blaug's (1962) *Economic Theory in Retrospect* was a well-articulated expression 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?" Methodologists challenged economists to attempt to refute theories, to develop more precise methodological dicta, etc. Yet 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. And of course in this period many sources of public funds in the United States 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 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 20th 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 activity to be emulated in less developed scholarly discourse. The two cultures were in a sort of balance. Science, representing progress, was in the national interest, while

the humanities were to receive some modest support in the academy for elevating the human spirit.

The social sciences meanwhile were busy emulating the natural and life sciences in order to have a similar 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. 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 econometrics was now seen to be more important for success in economic science. And as historical references began to disappear in economic analysis, the requirements that students study economic history and the history of economics began to disappear. 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 responses to the difficulty historians of economics were finding in getting their papers published in mainstream economics journals, and in the marginalization of their concerns and interests in the larger economics community.

The science-humanities “peace” 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. Whereas Popper had refused to allow a naturalistic theory of science, 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 “how does science provide the best model of objectivity?” Unlike the earlier answers to the objectivity question which suggested that science was interest-free and a-social, 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 study 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, and the processes by which rewards are apportioned for scientific work.

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 nonscientists writing about science, there was reduced 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. This view of contextualized science suggested that it was a cultural product in the same way that painting, music, cooking, and sports were cultural products, contingent in time and in place. And as the interests of some historians, sociologists, anthropologists, and rhetoricians of science began to fuse, the boundaries between these separate disciplinary approaches to studying science became fully permeable. What emerged from the older SSK work were new ways of writing about science in the last quarter of the 20th century, and the assemblage of such approaches came newly to be known as *science studies*.

Looking at science and its practices closely gave a perspective on science that 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”. This tension between the older scientists’ view of science and those who studied science from the science studies perspective was a feature 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.

It was not until the 1980s that such ideas began to surface in writings about economic science as well. Although this literature was never a dominant one when compared to all contributions in history and methodology of economics journals, individuals like Deirdre McCloskey, Arjo Klamer, Philip Mirowski, Esther-Mirjam Sent , and me began writing about the history and philosophy of economics from a science studies

perspective. These shifts are admirably documented in Wade Hands' 2001 book *Reflection Without Rules*. Yet just as the sciences studies approach to the history of science upset scientists, so too did the science studies approach to writing the history of economics upset both economists and the majority of those writing in the history of economics.

But before I go more deeply into that unhappiness, there is one other knot 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 inside and outside 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 19th century. But variously over that century science became more central to more intellectual projects, and increasingly intellectual cachet was seen to accrue not only to the classically learned, but to science and scientists.

We economists of course know this, for it was in that period, from late in the 19th century through the first decades of the 20th century, that economics sought to become, and indeed did become, a science. The social sciences developed and began to define how the world was to be understood in all its complexity. They sought to understand social structures, demographic details, economic activity, family and church and all manner of human institutions. And so the humanities, those proto-disciplines which had traditionally been the sites for understanding the world, began to share explanatory reach with the social sciences. Over the course of the twentieth century, within the university the humanities became merely one of the three divisions of humanities, natural sciences, and social sciences.

The culture wars that "broke out" in the United States in the 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 professors of literature. 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 a particular set of beliefs held by many scientists as well as those who saw science as a model of epistemological rectitude. One such belief was that science studies, in treating scientific knowledge as local and contingent and historically situated, undermined scientific authority. Another belief was that using scholarly tools and techniques also employed in studying political communities and literary productions meant 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 a public commotion produced by those opposed to science studies. This became the actual science wars.

In fact, it is even worse than that, which brings me full around to my title “The Economic Science Wars”. The antagonism of working scientists for those who write about science but don’t practice it is like the phenomenon one sees in televised sports in the United States. In any two or three-person team of television broadcasters, be it for American football, soccer football, baseball, basketball, etc., one of the 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.” Much as scientists think little of historians 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, Takashi Negishi, Joseph Schumpeter, 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. I submit that the belief that there have not been science wars in economics 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. For mainstream economists, 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 in my view the crucial one. It explains why the science wars in economics are *not* associated with that quite small band of Mirowski, McCloskey, Sent, and Weintraub and their allies. Unlike in the natural sciences, where science studies was seen as the de-legitimizing, in economics it is heterodox economics that is considered de-legitimizing in the sense that in numerous ways it challenges the epistemic authority of mainstream economics.

There are a variety of impulses that lead professional economists to “take up” the history (and methodology) of economics. It cannot be a surprising claim that many historians who begin as critics of mainstream economics link their historical projects with critical appraisals of modern economics. A quick examination of the program of these meetings in Porto, or any History of Economics Society meeting, will locate two primary intellectual themes: work on “very old” economics, historically engaged in contextualization, and critical work on issues treated in mainstream economics today, which work is called heterodox economics.

There are many books on the history and philosophy of economics that have appeared in recent years all of which take an anti-mainstream economics line. More are forthcoming. For instance, I note that there seems to be one fairly consistent theme reprised namely the overuse, inappropriateness, and sheer irrelevance of mathematics or formal argumentation in economics. In contrast for instance to my attempts in the book you have honored, *How Economics Became a Mathematical Science*, to contextualize, that is write one kind of history of, the interconnection of mathematics and economics, a recent book has its entire Part Five concerned with the misuse of mathematics and statistics. The titles of three of its chapters, “Can mathematics be used successfully in economics?” (The author says no), “Can we expect anything from game theory?” (The author believes not), and “Improbable, incorrect, or impossible: the persuasive but flawed mathematics of microeconomics,” give the flavor of the contributions in this one volume.

I do not want to be misconstrued here. I am not denigrating critical work, or work generally done in heterodox economics. I do however insist that such work appears to many mainstream economists, like my economics

department colleagues, as often having been produced by *historians* of economics, 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 is 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 hostile to mainstream economics.*

Since this is the central point I want to make to you today, let me repeat this: *The economic science wars are associated with economists' correct 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 considered to be hostile to mainstream economics.*

Such true beliefs have some real consequences. I have argued that although this economic science war developed in economics in the immediate post World War II period, it has only been in the past couple of decades that the costs of that war have become apparent. Just as had been the case in the natural sciences, in economics there was a disjunction between mainstream economist-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 – would characterize and appraise that same economic science. However the reward system within economics, and the fact that in North America, Australia, New Zealand, and the United Kingdom 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 needed to lead a public campaign to discredit the sociologist of science Bruno Latour, and the historian of science Norton Wise, to deny them academic posts in the School of Social Sciences at the Institute, 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 Mirowski and Sent being excluded from the Economics Department, and graduate teaching opportunities, at Notre Dame University. The resources thus “freed up” allowed the Economics Department to hire “real” economists. That episode was noisy, but the larger 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 what I have called economics science studies scholars – are different. Mark 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 some of 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 we historians of economics have lost.

And yet, and yet.

My own hope is that out of that loss, a new kind of scholarship may emerge. For over a decade now I have been arguing that it is time for economics science studies scholars, particularly historians of economics, to make new alliances. There are numerous scholars, and communities of scholars, with whom we have natural affinities. In particular there is a community in which our thought styles might eventually be valued, our insights might someday be treasured, and the rewards of community might be reaped before our professional demise. If historians of economics can shed their professional identification with the community of economists, can refuse to act as critics of economics, can refuse to take sides in the mainstream-heterodox controversies, but can instead turn to history, to constructing narratives of context, their future – our future – might look brighter. Thus were I to be optimistic about the history of economics, as this venue and this assembled community would undoubtedly encourage me to be, I submit that 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 the way for us to achieve perhaps greater institutional support. But even more importantly, we have much closer intellectual affinities to those communities of scholars than we do to mainstream economists, and that realization can lead us to an evolving connection with a large – and potentially more welcoming – scholarly community. But we will have to earn that welcome. So I say: Forsake the love of economists. Go forth and seek connection instead with that eclectic community of science studies scholars. Thus may our tribe increase.