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REVIEW ESSAY: MAKING BETTY CROCKER ASSUME THE POSITION

BY

THOMAS C. LEONARD

Philip Mirowski and Esther-Mirjam Sent, eds. Science Bought and Sold: Essays in the Economics of Science (Chicago: University of Chicago Press, 2002), pp. ix, 573, $33. ISBN 0–226–53857–5.

I. INTRODUCTION

The economics of science is a curious enterprise. Few economists and fewer scientists know of it. The use of economic ideas in the study of science was pioneered not by economists but by philosophers and sociologists. Even among economic methodologists, approaches informed by economic ideas remain relatively rare. (Who exactly, John Davis once asked me, is the audience for this stuff?) And, to the extent that the economics of science can be said to have an intellectual home, it resides in science studies, a field populated by scholars as poorly disposed to market economies as they are to contemporary economic thought. So perhaps it is fitting that *Science Bought and Sold* is itself a curious enterprise.

Mirowski and Sent's anthology is the first to gather and reprint papers under the rubric, "economics of science." Though sprinkled with the contributions of science-studies stalwarts such as Steve Fuller and Michel Callon, the volume first and foremost collects "classic" articles in the economics of science—Kenneth Arrow, Robert Nelson, Partha Dasgupta and Paul David, Philip Kitcher, and Michael Polanyi among them. The anthology is correct enough to include the prescient Charles Sanders Peirce paper, "On the Theory of the Economy of Research" (1879), unearthed by Jim Wible.

But wait. What are the "classic" economics of science papers doing in *this* volume? Didn't the editors convene a 1997 conference entitled *The Need for a New Economics of Science* before anyone knew there was an old economics of science in need of replacement? And isn't the economics of science of all ages—

Department of Economics, Princeton University, Fisher Hall, Princeton, NJ 08544.

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several leading statements of which are reprinted here—the same literature that Mirowski and Sent, in their introduction as elsewhere, vigorously criticize as wrongheaded? Well, yes. One blurber calls this arrangement "balance," but the effect is more one of incongruity. The incongruity is enough to make one wonder if this is the book the editors set out to publish. On the other hand, the editors' method—establishing a canon so as to attack it—is oddly right. For, as Allan Walstad has observed, if the economics of science can even be judged a coherent body of thought, its critics, Mirowski and Sent among them, have been the first to notice. And therein lies a tale, a tale of how fundamental changes in the way in that scholars regard science has come to influence the theory and practice of the history of economics.

II. WHICH ECONOMICS OF SCIENCE?

The editors, you will know, are economists who do intellectual history inspired by the sociology of scientific knowledge. This wide-ranging expertise informs their fine introductory essay, which runs to short monograph length, and the framing of the six sections into which the nineteen papers (which I will not attempt to individually summarize) are collected. The title makes clear that you are not holding a handbook. And *Science Bought and Sold* is not a really a conference volume—the reprints outnumber the conference papers—nor is it a reader, exactly. So what is it? Ultimately, it is an assemblage of papers on quite different topics: science policy, scientific motivation, scientific practices, scientific knowledge, intellectual property, and science studies itself, especially the economics corner of it.

The enriching benefits of this broad canvas come at the inevitable cost of some infelicities. The editors sometimes imply, for example, that the economics of science is *laissez-faire* with respect to science policy—it preaches don't worry, be happy. But, of course, Arrow's 1962 paper on the allocation of resources for invention, reprinted in part two, is a landmark precisely because it is neoclassical and not *laissez-faire*: Arrow treats scientific knowledge as a durable public good, and he argues that this market failure requires non-market solutions.

Elsewhere, we are told (p. 30) that the increasing commercialization of U. S. universities, which is real enough (and assumed to be a Bad Thing), has arisen because university administrators are in thrall to the economics-of-science take on science as a market process. Not only is this notion implausible on its face, it would be unfair were it true, since economics of science scholars have been among the first to propose replacing the old pure-science/applied-science dichotomy with one that emphasizes the difference in the incentive structures of academic science and industrial science, and emphasizes their fragility (as do Dasgupta and David, expurgated in part three).

While it is a pleasant fantasy to imagine university provosts reading in economics of science, the more likely culprit is the lure of profit from markets hugely expanded by technological change. Universities were once happy to let professors privately appropriate the returns to publishing their lecture notes as textbooks. No more. With the advent of the World Wide Web and greater interconnection, webbed lecture notes—indeed webbed lectures—are a profit center worth fighting over. (David Noble's paper casts as villain the technology itself, rather than the unseemly scramble for technology-created rents.) However one judges these trends, they are not caused by the economics of science. Harvard Law School did not get an injunction that enjoined Arthur Miller from peddling his Con Law lectures elsewhere on grounds that the academy should be a commerce-free zone. It was, rather, an old-fashioned tussle over who gets the rents from commerce.

And the old "marketplace of ideas" warhorse is trotted out for a good flogging, though I have yet to encounter a serious proponent of the notion, and the editors can't find anyone either.

No one denies that scientific claims sometimes compete, and few deny that scientific competition is ordinarily wholesome. What is found objectionable is the marketplace-of-ideas claim that rivalry among ideas leads to "truth," that is, that the epistemologically superior theory will ultimately win out. But to identify this marketplace-of-ideas chestnut with the economics of science is to confuse truth production with preference satisfaction. Economics does not claim that functioning markets produce substantive outcomes, only that they produce what consumers want. It is an open question as to whether "consumers" of scientific knowledge always want the truth—the truth can be inconvenient, even career-threatening, and the very success of science hangs on the extent to which its institutional structure promotes the production of uncomfortable, funding-threatening truths.

These commonplace solecisms in *Science Bought and Sold* will irritate the specialist, but they are comparatively small beer. The bigger prize is the grandly conceived historical narrative that unifies the volume's disparate papers by conceiving of them as landmarks along the way. The editors' introduction, which functions as a kind of raw-footage trailer for Mirowski's extraordinary feature, *Machine Dreams*, sketches the narrative.

The narrative goes like this: the exigencies of Cold-War American science decisively shape both the natural sciences and neoclassical economics—shape them in tandem, no less—and, moreover, this heretofore secret history can also explain the history of the halting and various attempts to use economic ideas in the study of science. This is an audaciously ambitious thesis for which I commend the editors. I'm not convinced of its last component—that the size and scope of Pentagon Big Science has determined the trajectory of the economics of science literature—but no matter. Mirowski and Sent are after bigger game.

III. THE DIFFERENT ICONOCLASMS OF ECONOMICS AND SOCIOLOGY

Some background will be useful, not least because of the unacknowledged centrality of the sociology of scientific knowledge to the editors' method and to their appraisal of the economics of science. Mirowski (2001) has described science studies of the last century as consisting of two strains: (1) the economics of science, which says science operates like a market, so best leave it be, and (2) the view that science is unfathomably mysterious, so best leave it to the priests. This is a bit like saying that Cold War geopolitics were dominated by two great powers, the Soviet Union and . . . Portugal. The economics of science (Portugal) is, in fact, a latecomer to the science studies derby, and is still a poor cousin. And the received-view notion that the study of science is a matter of foundational philosophy propounded by armchair law-givers (Soviet Union) expired mid-to-late twentieth century. The death of the received view may not be the end of history, but we are all naturalizers now—that is, those who study science believe that the study of science should itself be scientific, in particular that the empirical strategies of science should be used in study of science.

The missing great power in Mirowski's summary of science studies is the sociology of science, and its cognates in history and anthropology of science—the very hegemon that dominates science studies and that most informs Mirowski and Sent's method and appraisal. I'm not sure why Mirowski, no shrinking violet he, is so demure on the origins of his own thought. But give Mirowski and Sent credit, they reprint a Paul Forman article that, once translated from the Postmodern, argues that the contours of the sociology of science can also be explained by something like the editors' grand historical narrative.

Motive to one side, I argue that the economics of science and the sociology of science have enough in common that critiques of economics apply with equal (or greater) force to sociology. And, where the economics of science and the sociology of science part company, the sociology of science runs into fundamental epistemological difficulties of the kind that have given rise to what the newspapers call the Science Wars. Let me suggest why I think it matters for history of economics.

The economics of science and the sociology of science are both iconoclastic; they both attack the traditional image of science as a unique social realm. Both approaches, for example, treat scientists not as disinterested truth seekers but as worldly, interested actors. But from here, economics and sociology attack very different idols, disagreeing most profoundly on the consequences, for scientific knowledge especially, of a more realistic conception of scientific motivation. (A more scholarly discussion is available in Leonard 2002.)

The economics of science treats the process by which scientific knowledge is created as a market process, that is, as one where scientists respond to (pecuniary and non-pecuniary) incentives that promote (or hinder) the creation of scientific knowledge. Science retains its epistemological distinction, but economics depicts the process of producing scientific knowledge as no less partisan, grubby and shallow than the market processes that produce breakfast cereals or broadcast television. Natural scientists are less aristocratic in their attitude toward markets than is the humanities professoriate but, let's face it, the economists' portrait of scientific motivation is less flattering than the traditional image of selfless truth seeking.

The sociologists' leveling move is, remarkably, even more radical. Science studies denies science its epistemological distinction by denying that empirical evidence plays any substantive role in science. Science Studies epistemology asserts that non-evidential considerations determine *entirely* which among rival theories will prevail, as they determine the criteria by which one theory is judged better than another. Reason and empirical evidence are, goes the claim, utterly toothless—they serve only to rhetorically pretty up the means by which dominant in-groups come to prevail in science. For compactness, let us call Science Studies epistemology the Position.

If the Position sounds radically skeptical, it is. (If it sounds somewhat familiar, it is that, too—the notion that empirical evidence does not affect what scientists believe merely substitutes a credo of "it's all social" for the "it's all natural" position of received-view philosophy of science). Students of science have long known that social influences matter. Even Cold War-era sociology of science acknowledged indirect effects on scientific knowledge from social influences such as research project selection. Funding and the opportunity to publish can clearly affect the prospects for obtaining evidential support, as Brock and Durlauf note in their contribution. The issue is not *whether* non-evidential considerations affect scientific knowledge (what science studies, per the Position, calls scientific "content"), but *how*, and *to what extent*.

But the Position treats "science as social" as an argument-ending claim in epistemology, not as a Kuhnian mandate to inquire into how the social character of science promotes or undermines knowledge. Science studies no longer believes, as Thomas Kuhn did, that "observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science" (1962, p. 4). And, as a result, science studies regards contributor John Ziman's argument as moot: "The key point is that market-like mechanisms at various points in the academic research process facilitate the operation of the Mertonian norms, and thus keep the whole system open, flexible, progressive, relatively impartial, and self critical. . . . [T]he nature of the knowledge produced by this system is closely bound up with its social structure" (p. 323).

So the economics of science says to the exact scientists—*you* scientists are no different and no better than we are. If your product is superior, it only because: (1) exact sciences are easier than inexact sciences such as economics, and or (2) you face more virtuous incentives, which promote epistemologically better outcomes. The sociologist of science says to the exact scientists—*your product* is no different and no better than ours. Since, per the Position, scientific knowledge does not exist, your product cannot be superior to ours, and the need to consider how social factors influence knowledge production is obviated. Which do you, dear reader, regard as the worse slander?

IV. WHY SHOULD HISTORIANS CARE?

Why does this fundamental epistemological disagreement between economists and sociologists matter for historians? Three reasons. First, it puts into context Mirowski and Sent's historical sketch of the economics of science. The editors' introduction argues that economists wishing to consider science were caught in a fourfold intellectual bind: economists suffered from physics envy; their work was scorned as unscientific by physical scientists; the use of economic ideas in science studies leads to paradoxes of self-reference, and economics has little to add on matters of truth and cognition. These factors, whether or not they usefully describe the history of the economics of science, are plausible enough. They also are plausible when applied to the known alternatives in science studies. Substitute "sociology" for "economics" in the sentence above, and it reads even better.

In fact, professional envy to one side, it is the sociologists and fellow travelers in science studies who have most run afoul of the physics establishment—witness the dumping of Bruno Latour's appointment to the faculty of The Institute for Advanced Study. Moreover, whatever physical scientists think of economics, their contempt for science studies comes not from a beef with invisible-hand reasoning or with the idea that scientists are people, too. The scientists' contempt for science studies, I would argue, comes from a deep aversion to the epistemological position that science studies takes. Physicists may not know from Adam Smith, but they do know that they regard the epistemological status of their scientific claims—make that, laws—in ways profoundly different than does the Position.

Second, this is why the editors are, I think, misguided when they pooh-pooh the so-called Science Wars, as exemplified by the Sokal Hoax, which the editors dismiss in a footnote. I agree with Mirowski and Sent that the Science Wars have produced more heat than light, and that Sokal's success has wrongly been used to indict the whole of science studies. But the origins of the Science Wars have their roots in the same fundamental epistemological disagreement.

For those who did not read the newspapers in the late 1990s, an NYU physicist named Alan Sokal submitted to the journal *Social Text* a preposterously incoherent paper (1996) full of howlers, but one with an ideologically correct line. The bogus paper, was, of course, accepted. Sokal's goal was not to send up the gobbledy-gook obscurantism that fills such journals, nor even was it to expose the intellectual dishonesty of editors willing to publish errant gibberish. Sokal, with all the earnestness we might expect from an expatriate Sandinista school teacher, thought he was saving the Left from the relativist perils of the Position. How can the Left remake capitalism, Sokal wondered, if our epistemology denies to us the reason and evidence that we require to make our case?

Sokal's argument is that the Position is self-destructive. Why bother with a mountain of empirical evidence offered to support the thesis that empirical evidence cannot explain what people take to be true? The position is self-destructive because it is everywhere destructive. It undermines the capitalist and the socialist argument alike, just as it undermines the history of science—empirical claims about what has happened—and science alike.

Third, though harder to see, because it has been bundled into a larger and mostly persuasive historiographic critique of traditional history of economic thought, the Position has been exported to the history of economics. Some of our leading scholars—Roy Weintraub and Margaret Schabas among them—argue that bad old history of thought will remain moribund unless it conducts historical research that is, well, more like what real historians do. Out with all the ideological axe-grinding, and the Whiggish business of constructing lines of intellectual descent, and in with writing histories that better situate ideas in the personal, social and intellectual contexts in which they were created. Well, yes. These historiographic injunctions are unobjectionable, even laudable. The idea that historical writing should be thicker, richer ... and creamier, can be called Betty Crocker historiography.

But this critique of traditional history of economic thought ordinarily bundles these wholesome historiographic injunctions with a claim that historians should also embrace science studies epistemology. Sokal's lament, which I share, is that critical perspectives on the history and organization of science (and, for that matter, on the organization of the economy and economics) not only don't require the Position—they cannot go forward when burdened with the Position. If all claims about the world (how it is or how it was) really *are* epistemologically indistinguishable, why should anyone entertain, for example, the compelling new work that connects the history of economics to Cold War science imperatives?

Making Betty Crocker assume the Position thus can do real harm. It can lead historians of science, for example, to think that their historical narratives must find a usable past for science studies epistemology, which makes debunking the pretensions to knowledge the paramount aim. History as debunking is, of course, a venerable practice. But if, in the paradoxical name of the Position, we invest too much in detailing what science (or economics) is not, we can lose sight of what science (or economics) is. The centrality of debunking in science studies, required by the Position, may help explain why, as the sociologist Stephen Cole (1992) argues, the sociology of scientific knowledge, even taken on its own epistemological terms, has not succeeded in providing an account of the social causes of belief.

Don't get me wrong. History of economics needs its Mirowskis and Sents, and not just for their excellent work, but in the sense that better history will nearly always result in a kind of debunking; the overturning of received wisdom practically follows. But debunking should be a byproduct of good history of science, not the whole project of history of science. The same Position that requires historians to make debunking central, also requires their readers to treat these histories as bunk.

Thus, I argue, does a fundamental epistemological disagreement help explain historiographic differences among historians of science and economics. It also helps explain why this anthology's papers seem less balanced than awkwardly juxtaposed. The economics of science and the sociology of science have much in common but, because the latter assumes the Position and the former does not, they can seem to be talking about entirely different things. This is not an illusion, nor a rhetorical effect—different theories of knowledge will affect how one treats the knowledge production business.

The epistemological divide may even shed light on the history of economics boom in the study of mid-century information science, cybernetics, and the effects of Pentagon science funding, a boom to which the editors have contributed greatly. Why are historians and sociologists drawn to cybernetics? It could be, of course, that John Von Neumann fathered both cybernetics and neoclassical economics, and that the former explains the latter, full stop. But Andrew Pickering, a leading science studies scholar, admits to a more, well, social influence. Pickering is drawn to cybernetics because it is a place where the scientists themselves seemed to adopt, or at least move closer to, a kind of science studies epistemology.

As such, "the history of cybernetics can help us to break still further away from the representational idiom," by which Pickering means, break further away from the idea that science is "an epistemological project aimed at knowledge production" (2002, p. 10). Think that scientists are never really persuaded by the evidence? Well, here's some, er, evidence that suggests the players themselves might have been led—*by their science, mind you, not by some philosopher in a beret*—to share the debunking aims of the Position.

This new line of inquiry is fascinating, original, and important. But the same Position that might have motivated the inquiry will not permit its conclusions. For the conclusions are impossible by the Position's own lights: you can't ask history of science to provide evidence for a theory of science when that theory of science argues that evidence is bunk. If it were true, as a historical matter that cyberneticists moved toward a kind of science studies epistemology—the same science studies epistemology precludes taking it seriously. This should be an unacceptable bargain. Shouldn't it?

I am quite sympathetic to Jeff Biddle's (2001) proposal, in this journal, that historians of economics—tired of epistemology, neologisms, and prolix "theorization"—call a moratorium on debate over how scholars should treat scientific knowledge, and let partisans of the Position show what they can do, as historians. Then, by its fruits, Biddle suggests, we shall know it. But, the Position rules out even this modest proposal: for the Position insists that by history's fruits we shall know nothing. Why not, instead, stop assuming the Position, and free our histories, critical and other, to go forward?

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