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Caveat Emptor: Economics and Contemporary Philosophy of Science

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The relationship between economics and the philosophy of natural science has changed substantially during the last few years. What was once exclusively a one-way relationship from philosophy to economics now seems to be much closer to bilateral exchange. The purpose of this paper is to examine this new relationship. First, I document the change. Second, I examine the situation within contemporary philosophy of science in order to explain why economics might have its current appeal. Third, I consider some of the issues that might jeopardize the success of this philosophical project.

1. Introduction. A few years ago Ron Giere wrote the following words in the introduction to his *Cognitive Models of Science* (1992):

Within the philosophy of science one can detect an emerging specialty, the philosophy of cognitive science, which would be parallel to such specialties as the philosophy of physics or the philosophy of biology. But the reverse is also happening. That is, the cognitive sciences are beginning to have a considerable impact on the content and methods of philosophy. . . . Inspired by the work in the cognitive sciences, and sometimes in collaboration with cognitive scientists, a number of philosophers of science have begun to use the cognitive sciences as a *resource* for the philosophical study of science (1992, xvi, emphasis in original)

Now, less than a decade later, this quote would apply just as well if the word “economics” were substituted for “cognitive science”

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throughout. A few influential philosophers of science have recently begun to use *economics* as a *resource* for the philosophical study of science. After a brief discussion of this recent literature to convince readers of the presence of economics, I will turn to the important question of why this change has taken place. Unlike the previous cognitive turn, the change seems to be almost exclusively a result of recent developments within the philosophy of natural science and not because of any changes that have taken place within the science of economics. Finally, I will issue a bit of a warning (hence the title) to philosophers who pursue, or are considering pursuing, this economic path. The warning is not intended to discourage anyone from taking the path, but rather to make it clear that it is not a brisk and easy trail; the path is often steep, frequently treacherous, and there are even a few places where it is washed out entirely. Philosophers wanting to explore this path should heed these warnings and be prepared for what lies ahead. Before embarking I would also like to point out that given the breadth of the topic and the space limitations imposed on this paper, most of my “argument” will be in the form of compacted assertions with copious references to the literature where a more careful discussion can be found.

2. The Current Problem Situation. The idea of using economics as a resource for epistemology and the philosophy of science is certainly not new. Over a hundred years ago Charles Sanders Peirce (1879) employed microeconomic argumentation in the philosophical examination of scientific knowledge (see Wible 1994). Works by Polanyi (1962), Rescher (1989), and certain philosophers within the Popperian tradition (e.g., Bartley 1990) represent a few of the many other examples from the history of philosophy where one might find economics (or at least economic metaphor) involved in the investigation of science. But these philosophers are not the focus of this paper. This paper will focus on more recent examples such as Kitcher (1990, 1993) or Goldman and Shaked (1991) where economic arguments are applied directly to questions in the normative philosophy of natural science. These recent authors employ economics much more deeply—they employ the idiom as well as the ideas of modern (neoclassical) economics—and thus constitute an approach to the philosophy of natural science that I have elsewhere called “neoclassically naturalized epistemology” (Hands 1995, 615).

The basic philosophical problem situation in these papers is to explain how the right stuff (cognitively reliable scientific knowledge) can emerge from a social environment (the scientific community) where the individual scientists are pursuing their own self-interest and do not

intend to follow epistemologically approved norms for the pursuit of scientific knowledge. How possibly could cognitive virtue emerge from a context of such individual vice? For Kitcher, the general answer is an answer that is common to both economics and evolutionary biology: diversity, specialization, and competitive selection. The possibility of cognitive success is enhanced by a diversity of cognitive approaches; there is epistemic virtue in a “division of cognitive labor” (Kitcher 1990 and 1993). Not only can diversity lead to cognitive virtue, it is possible that scientists who behave as “ruthless egoists” will produce results that have more collective virtue than a community of “epistemically pure” scientist agents (Kitcher 1993, 349–351). In Kitcher’s own words,

The very factors that are frequently thought of as interfering with the (epistemically well-designed) pursuit of science—the thirst for fame and fortune, for example—might actually play a constructive role in our community epistemic projects, enabling us, as a group, to do far better than we would have done had we behaved as independent epistemically pure individuals. (1993, 351)

While Kitcher does not actually use the term, this is basically the same “invisible hand” argument that has been part of economic analysis since Adam Smith (Fuller 1994, Hands 1995, Mirowski 1996, Solomon 1994); it shows that it is possible, under certain institutional conditions, for self-interested individuals to produce outcomes that are socially efficient (cognitively efficient in this case, economically efficient in the standard economic story). The formal models presented in Goldman and Shaked 1991 or Chapter 8 of Kitcher 1993 demonstrate that such a result, an epistemic invisible hand, is possible. If the topic is how individual self-interest can achieve economic efficiency the field is microeconomics; when the topic is how individual self-interest can achieve epistemic efficiency the field is the contemporary philosophy of science of Kitcher, Goldman, and others.

Before moving on to the question of why economics seems so appealing, let me quickly list a few other contact points between economics and contemporary science theory. These other contact points are not the focus of this paper, but it is useful to mention them in order to demonstrate the breadth of the interpenetration and to differentiate these literatures from the direct applications that I am considering. First, there are many economic, and economic-sounding, arguments made in science studies and the sociology of scientific knowledge (see Hands 1994b, Mäki 1992). Second, there is developing literature on the “economics of science” which attempts to model the behavior of scientist agents in the same way that (micro)economists model economic agents (e.g., Diamond 1988, Stephan 1996, Wible 1992). This

research in the economics of science may seem to be much like the work under consideration, but in fact there is a significant difference: the economics of science claims to be purely descriptive and does not have the normative dimension that is the main objective of the recent economics-inspired work in the philosophy of science (see Hands 1994b). Third, there is a historical and philosophical literature that emphasizes the mutual co-determination of epistemic and political-economic values; an example of this is the recent work on Neurath (Cartwright, Cat, Fleck, and Uebel 1996; Cat, Cartwright, and Chang 1996; Uebel 1992). Finally, it is possible for philosophers of natural science to employ arguments from economic methodology and to use examples from economic science in the presentation of their more general philosophical program (e.g., Cartwright 1989; also see Hands 1994a, Sent 1996). None of these will be considered here, but they are certainly all fodder for future investigation.

3. Why Economics? The problem situation for contemporary (normative) philosophy of science is to find a middle ground between positivism and relativism. Everyone admits that theory-ladenness, under-determination, and a host of other difficulties have led to the abandonment of positivist and foundationalist inspired philosophy of science; on the other hand, relativist, purely descriptivist, and social constructivist approaches do not provide an acceptable replacement for the traditional philosophical project. The task is to find a philosophical approach that is sensitive to the critique of the Received View while still retaining an element of normative bite. As Kitcher puts it, to “replace both sleepy complacency and Luddite rage” (1993, 391). The consensus seems to be that such a philosophy will need to be *naturalistic*, *fallibilist*, and will recognize the *social* nature of the scientific enterprise.

One argument for involving economics in the philosophy of science is simply that naturalized epistemologies need to start with *some* scientific framework—behaviorism (Quine 1969), cognitive psychology (Giere 1992, Goldman 1986), or evolutionary biology (Hull 1988, Bradie 1986)—so why not economics? Economics may not have the scientific standing of evolutionary biology, but it would seem to be as legitimate a starting point as a discipline like cognitive psychology. While this argument has a certain appeal, the fact is that the decision to involve economics is not unrelated to these other forms of naturalization. Almost all naturalized epistemologies appeal, in one way or another, to evolutionary biology; in Kitcher’s words “naturalists view members of our species as highly fallible cognitive systems, products of a lengthy evolutionary process” (1992, 58). Even the approaches

that focus on psychology as a philosophical resource ultimately ground the notion of cognitive reliability on the evolution of our cognitive faculties; recall Quine's famous reference to "encouragement from Darwin" (1969, 126). Now while this is not the place to try to unpack the entire complex interrelationship between economics and evolutionary biology, it is certainly clear that the two fields have much in common. Kitcher in particular moves quickly from biological to economic metaphor and back, and it is clear that issues like specialization, competition, selection, and equilibrium are fundamental to both research programs. Much of the mathematics is also the same; population dynamics is modeled in the same differential (or difference) equation framework as macrodynamics in economics, evolutionary stable strategies are a type of Nash equilibria, and both employ maximization at the micro/genetic level and equilibrium at macro/species level. Invisible hand type arguments are also common to both fields; in fact, some evolution-based philosophical views such as Hull (1988), which do not explicitly involve economics, are often criticized for their use of the invisible hand concept (e.g., Sterelny 1994). The bottom line is that an economic approach to knowledge and an evolutionary approach to knowledge are sufficiently intertwined that it is very difficult if not impossible to separate out the component parts. In sum, the argument is that one reason for involving economics is that most naturalized epistemologies appeal in one way or another to evolutionary biology, and economics and evolutionary biology are deeply intertwined.

The second, and perhaps more important, reason for involving economics in the philosophy of natural science is that it seems to provide a way out of the relativism posed by Kuhn, the sociology of scientific knowledge, and others. Consider the standard debunking story from the sociology of scientific knowledge: scientists are supposed to follow a special rational/empirical method, the scientific method, but they do not; they make decisions that are in their own interest or in the interests of their group, and thus their inquiries are tainted, not epistemically privileged, and caused by the same factors that cause the beliefs and behavior of any other members of society. So how could economics help get around such an argument? Well, what economists do best is to explain how a society that is composed of aggressively self-interested individuals can, given the right institutions (usually "competitive" institutions), produce a result (usually a distribution of resources) that is socially optimal. The defining core of mainstream economics has always been, to show that rapaciously self-interested behavior (individual vice) need not be inconsistent with social optimality (public good). Now if economists can show that the (economically) right stuff can emerge from tainted behavior, then it should be a very short step, con-

tra the sociology of scientific knowledge, to show the (epistemically) right stuff can emerge from the behavior of tainted scientists. Not only is such public virtue possible, once the relevant mechanism is understood—once we can “identify the properties of epistemically well-designed social systems” (Kitcher 1993, 303)—it can be exploited for public policy purposes in order to facilitate the more effective organization of our scientific and educational institutions. Properly employed economics should be able to help us develop a philosophy of natural science that recognizes the social nature of science, admits the interest-laden behavior of actual scientists, repels relativism, preserves a normative component, and allows us to make useful recommendations about science policy. No wonder it is so appealing.

A final reason for employing economics is that it seems to be an OK science (at least at a distance). Economics has a core of highly formalized mathematical theory; it has a very sophisticated statistical approach to empirical analysis (econometrics); and economics students (at every level) learn their core theory from standardized textbooks, just like students in the natural sciences. Economists do “research” and get “results”; they talk assuredly about things like “hypotheses,” “causality,” “evidence,” and “testing”; and they seem to think that historical or philosophical study of the discipline is only of interest to those who cannot do (or are too old to do) serious work. They also seem to be able to use their science to make bold policy recommendations, and those in power actually seem to listen to what they say. There is a Council of Economic Advisors and there is a Nobel Prize in Economic Science. Economics thus seems to be a perfectly reputable science (particularly when judged against the other social sciences) and therefore a reasonable resource for philosophers to use in their efforts to naturalize epistemology.

4. *Caveat Emptor.* So what is the problem? The project of employing economics as a philosophical resource seems to be a perfectly reasonable, and given the current problem situation within the normative philosophy of science, a perfectly explicable, approach. The work of Kitcher, Goldman and others may not at this point provide the complete program—they may just be offering a few tentative first steps—but surely they are steps in the right direction. Well, perhaps they are, but let me discuss three of the many reasons that we might have to be skeptical.

The first problem is that economists do not have any notion of “the social” other than summing the individual agents. Since the social is merely the sum of the individuals, economists cannot accommodate any concept of the social that is *qualitatively different* from that which

is possessed by the individual economic agents. Since this is a relatively subtle point, let me briefly try to elaborate (see Hands 1996 for a related discussion). There are basically two notions of economic (social) efficiency in economics: the Pareto criterion and the compensation principle. An allocation of resources is Pareto efficient if the only way that one person can be made better off (have a higher level of utility) is for someone else to be made worse off (have a lower level of utility). The measure of social efficiency in the Pareto case is simply individual well-being (utility); the social is not different “stuff” than the individual, it is exactly the same stuff; efficiency is merely a matter of who has more or less of it and not any qualitative transformation of what it is. The compensation principle is perhaps more practical; it adds up winners and losers and allows for efficiency improving reallocations that make some agents worse off. If the gains to the winners are greater than the losses to the losers—if the winners could “compensate” the losers and still be better off—then, according to the compensation principle, it is an efficiency improving reallocation. The compensation is supposed to be in utility terms, but under certain conditions (technically the constancy of the marginal utility of money, or homothetic preferences, or some similar assumption) “money” can serve as a proxy for utility and the relevant compensation can be computed in dollars. Notice again that the social is not something different, not something with unique or emergent properties, it is ontologically the exact same stuff as the stuff that guides the behavior of the individual economic agents. Now consider the epistemic analog. We start with the subjective beliefs of the agents, and then, if all goes well, what emerges from the scientific endeavor is “knowledge”—that is justified true belief (or to be a bit more contemporary, fallible but reliable belief)—but it is something that is qualitatively different from the subjective beliefs of the scientist-agents in any case. If, as is the case in normative naturalized epistemology, one wants something to emerge (a special type of belief) that is qualitatively different from the beliefs of the individual agents, then economics will not help; you cannot get there from here (neoclassical economics).

The second issue is that even if one defines the social as the sum of the individuals, then economics still has problems. The standard consistency assumptions imposed on individual agents, the assumptions that generate the demand curves of those agents, impose effectively no restrictions on the aggregate (market) demand curves derived from adding up the individual demands (this is the so-called “Sonnenschein-Mantel-Debreu” result; see Hands 1994c for a discussion). This is just one of a series of problems associated with “individualism” in neoclassical economics; economists talk a good story about individualism

and having (individualistic) micro-foundations for all of the social and aggregate concepts employed in economic analysis, but serious gaps exist between the rhetoric and the systematic completion of the project (Kincaid 1996, 250–257). Even with a relatively impoverished notion of the social, the transition from the individual to the social in economics is not as fluid as it seems to be when the discipline is viewed from afar. This problem would obviously carry over to any philosophical application of economic theory.

Finally, there are serious questions about the cognitive status of economic science itself. The optimistic story that ended the previous section is not the story that most philosophical commentators tell about economics. In fact, most recent works in the philosophy of economics *start* with the *failure* of economics—its failure to show scientific progress with respect to prediction, explanation, or policy usefulness—as the *explanandum* for their work (Boylan and O’Gorman 1995, Rosenberg 1992). Despite the scientific sheen and the supreme confidence of the discipline’s most influential practitioners, there are, as most of the recent literature on economic methodology indicates, serious questions about the cognitive status of economic science. This is not to say that such problems are more severe in economics than in other social sciences, but it is to say that if philosophers believe that the discipline of economics is in great scientific shape and waiting patiently offstage to charge in and help clear up the disorder in late twentieth-century philosophy of natural science, they are in for a surprise. And it is a surprise that will reveal itself just as soon as philosophers (and historians and sociologists of science) start to look at economics with the same careful scrutiny that they have used to examine the natural sciences during the last thirty or so years.

5. Conclusion. I have argued that even though there are obvious reasons why economics might be a good resource for contemporary philosophy of science, there are also many reasons to be skeptical about its success. As I said in the introduction, the purpose is not necessarily to undermine potential research in the field; the point is simply to recognize the situation and to move ahead with caution, a healthy skepticism, and full information. Economists writing on “methodological” issues have certainly broadened their horizons in the last few years and philosophers employing economics can do the same; the real lesson is to have an in-depth understanding of the discipline that one intends to use as an intellectual resource. Remember, there are not any quick fixes in either philosophy or economics, and of course, *caveat emptor*.

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