

**Is Scientific Rationality the Slave of the Passions?  
Instrumental Rationality, Economics, and Contemporary Science  
Theory\***

D. Wade Hands  
Department of Economics  
University of Puget Sound  
Tacoma, WA 98416  
August 2002  
[Amended version July 2013]

\*Paper prepared for the Biennial Conference of the International Network for Economic Methodology, University of Stirling, Scotland, September 1-2, 2002. An earlier version of the paper was presented at the International Symposium "Science, Philosophy and Democracy: A Contemporary Debate" University of Catania, Catania Italy, October 2-3, 2001. Helpful comments on earlier drafts were received from John Davis, Steve Fuller, and Philip Mirowski.

[Dear reader, this paper is over a decade old. It was never published, although some of the ideas presented here did make their way into some of my research published during the first decade of the 21<sup>st</sup> century. Given this, you may ask: Why now? Why make a working paper available after a decade? The reason is simply that I think many of the general issues addressed in this paper are still alive – not only still alive, but currently hotly debated research topics within the philosophy of economics and the philosophy of science more generally. Three of these issues are:

1) the question of the relationship between descriptive and normative philosophy of science; philosophy of natural science has never really come to a consensus about the relationship between what (at least great) science "is" and what science "ought to be" (i.e. the ghost of Thomas Kuhn remains ),

2) developments in experimental economics, behavioral economics, behavioral welfare economics, contemporary revealed preference theory, and a host of other fields and approaches have re-opened the Pandora's box of "rationality" (once rationality meant rationality according to rational choice theory – expected utility theory in the case of risky choice and ordinal utility theory in the case of choice under certainty – but now that is much less the case), and

3) the question of the definition of, and relationship between, positive and normative economics has reappeared within economics and economic methodology (there are many reasons for this, but a one of the big ones is the influence of experimental psychology and behavioral economics, in particular the fact that while most of this research is quite critical of rational choice theory as a scientific – predictive and/or explanatory – theory, it seems to be quite comfortable accepting rational choice theory as a normative theory of what rational agents ought to do).

This paper certainly does not provide definitive answers to any of these big questions, but I think it does lay out some of the more important issues – and for some readers I suspect in a novel way – and points out some of pitfalls of various approaches that might initially seem to provide easy answers. I did not attempt to update, or otherwise revise the paper; it is as it was, but hopefully it is still interesting and useful.]

## 0. Introduction

It is now clear that the standard story about a revolution in mainstream philosophy of natural science – a revolution, in which the complacent positivist-based Received View was overthrown by a diverse band of insurgent historians and sociologists of science led by Thomas Kuhn – is a bit of an exaggeration. The story seems to be overstated with respect to both the degree of mainstream consensus that existed prior to the change (Cartwright, Cat, Fleck and Uebel 1996; Friedman 1999, 2001; Hacoheh 2000) as well as with respect to the role of the key revolutionaries (Fuller 2000). Nonetheless, despite a certain amount of embellishment, there clearly has been a displacement (though not replacement) of the set of widely accepted philosophical ideas about the character of scientific knowledge that had dominated scientific philosophy during the previous half century. As a result of these changes, the sciences are now generally seen to be more contingent; underdetermined; theory-, interest-, and social-laden; and generally messier, than had been the case in the philosophical literature – at least the philosophy of science literature – during the heyday of the Received View.

While most philosophers now accept, at least to some degree, the main lessons of the sociological critics – underdetermination, theory-ladenness, and the sociality of science – the *relativism* that often appears to follow from these concessions continues to be an ongoing concern. Of course the term "relativism" has a wide variety of different meanings, but the relativism that most concerns contemporary philosophers of science and the practicing scientists who have joined the fray (Gross and Levitt 1994; Gross, Levitt, and Lewis 1996) is the *epistemological relativism* that seems to follow from much of the post-Kuhnian science studies literature. The sociologist Harry Collins provides a clear definition of this type of relativism:

Epistemological relativism implies that one social group's way of justifying its knowledge is as good as another's and that there is no external vantage point from which to judge between them; all that can be known can be known only from the point of view of one social group or another." (Collins, 2001, p. 184)

Such relativism is very disconcerting to most philosophers of science. Epistemological relativism is viewed as a threat to the very *rationality* of science (or at least the standard philosophical characterization of the rationality of science) and overcoming this relativist threat – defending the rationality of science – seems to be the self-delegated main task of recent work within the philosophy of natural science. For some philosophers "overcoming the relativist threat" means simply attacking head-on the claims of the post-Kuhnian sociological literature; for most though, the task is less adversarial and more

conciliatory. For contemporary philosophers of science the goal is to find a new comfortable middle ground that *recovers rationality* while also accommodating certain aspects of the historical and sociological critique: in particular, admitting that science is more social and less rule-governed than previously believed. The goal, as Philip Kitcher put it, is to "replace both sleepy complacency and Luddite rage" (Kitcher, 1993, p. 391).

This paper will discuss *one particular strategy* that has been employed in these recent efforts to delineate a more socially sensitive version of scientific rationality. The strategy involves the use of *instrumental rationality* – rationality characterized solely in terms of using efficient means to achieve given ends – in the recovery of a more robust concept of scientific rationality. Since microeconomics constitutes the most sophisticated application of instrumental rationality in the human sciences, the philosophers who approach questions of scientific rationality in this way often employ economic ideas and tools in their philosophical research. In some cases the application of economics by these philosophers is quite intentional and explicit, and in other cases the economics seems to slip into the analysis quite unintentionally and/or unrecognized.

In the first section discusses the philosophical problem of recovering scientific rationality in a way that emphasizes the role of instrumental rationality (and possibly economic analysis) in such investigations. The second section examines the work of two philosophers of science who have explicitly employed instrumental rationality: Philip Kitcher and Larry Laudan. The third section examines a the problems encountered by these approaches – in particular the difficulties associated with the concept of instrumental rationality and the inability of any such instrumental-rationality-based approach to solve the problem of relativism in a way that most philosophers of science would find acceptable. The bottom line is that in order for microeconomic analysis to provide an effective vehicle for overcoming relativism in philosophy of natural science, it would be necessary to overcome the limitations of instrumental rationality in a way that neither economists, nor the philosophers who have employed such concepts, have done, or show any signs of doing. In a sense the features that make instrumental rationality so effective in microeconomics are precisely the features that make it so problematic in the campaign against epistemological relativism.

## 1. Instrumental Rationality, Scientific Rationality, and Economics

The post-Kuhnian science studies literature changed mainstream philosophy of science in a variety of ways, but by far the most significant was its contribution to the general recognition that science – science in general as well as specific theories, fields, and approaches within science – is fundamentally social, and that

much of what goes on in science is not unique, but rather is much like what goes on in a variety of other social contexts. Science, according to much of the sociological literature, is not the *unique* or *special* (particularly epistemically special) type of human activity that philosophers of science, and earlier sociologists of science in Mertonian tradition, had long presumed. What scientists do, the science studies literature argues, is just what countless other groups of people do. Scientists are socialized human beings, and should be studied in the same way that one would study the activities, beliefs, and social organizations of any other group of enculturated human beings: using the tools of the human and social sciences. It is not, as the early positivists would have it, that there are only two sets of human cultural practices, one scientific/meaningful and one metaphysical/meaningless; or that social activities can be divided into Popper's tripartite division of science, metaphysics, and muck (Hacking, 1979, pp. 384-5); or even that the relevant distinction is C. P. Snow's famous (1959) "two cultures" of the science and the humanities. Rather, for most of the science studies literature, the issue is not science *versus* some other culture; it is just science *as* culture. For a few of these authors this means that science should be studied solely as a form of rhetoric and persuasion; for others scientific knowledge is primarily a tool for the social and political domination; for still others, science studies is primarily about issues of reflexivity and relativism; but for the majority of those writing within science studies (particularly those closest to philosophy) science is not just persuasion, power, or the opportunity for exercises in reflexivity; it is one example (the most powerful example) of a particular type of culture – expert culture – and as such it is not essentially different from any other culture organized on the basis of expertise. As Trevor Pinch explains:

[T]he field of science studies breaks down this cozy relationship. Rather than treating science as the "exotic other" or just as a different animal, it levels the playing field – all animals are really the same, and they are not all that exotic. Within science studies, science is treated as another body of skilled practice, not unlike other areas of human endeavor. Science studies, rather than endorsing the parallelism of the two-culture thesis, asserts that there is *one culture* in the sense that both science and the humanities share the characteristics of being cultures of expertise.  
(Pinch, 2001, p. 18)

The response of most philosophers of science has been to admit the sociality of science – that science is in some sense a subset of expert culture – but to endeavor to retain the idea that science is also cognitively quite special; it gives us *knowledge* in a way that other cultures, even other expert cultures, simply do not. Science is more than just one of many successful crafts; it is a craft that is uniquely accomplished in serving our cognitive ends. For many philosophers

this means that science, unlike other expert cultures, produces something of absolute cognitive value; it produces *truth*. For others truth is too much to ask, but verisimilitude, or more recently reliability, distinguishes science from other, even expert, cultures. For those of a more pragmatic persuasion, it is not so much that science produces something that is different in kind from other skilled activities, but rather that it exhibits a difference in degree sufficient to warrant its special attention. In any case, for philosophers, science is *a different animal* – either different in kind, or simply, but substantially, different in degree – but it is a different, and a uniquely worthy, animal in any case. If science is just a particular culture – even expert culture – and does not have any of the extra-cultural epistemic virtues that philosophers have traditionally attributed to science, then it is condemned to epistemological relativism. Perhaps science studies can uncover various "norms" that prevail within the expert culture of science (in general, or within specific fields or research programs) and perhaps it would even be possible to discuss appraisal in such a context, but such appraisal and the norms it would be based on are entirely *local* and do not transcend the particular culture (branch of science) under investigation. Not only is this epistemological relativism, it reduces scientific methodology (previously the study of how we obtain universal knowledge about the world) to nothing but the study of particular class of social peccadilloes; a situation that is absolutely unacceptable to most philosophers of science. While old-time armchair philosophizing and totally asocial characterizations of science are also unacceptable, slipping back into the older philosophical idiom seems to be less of an immediate concern than repelling epistemological relativism and recovering the rationality of science.

So how does *instrumental rationality* enter into all of this? Actually it enters in a number of different ways, but the main motivation seems to be that while the notion of scientific rationality is currently in hot contention, the notion of instrumental rationality is quite clear and undisputed. Of course "clear and undisputed" does not mean that everyone is happy about the way that instrumental rationality is *used* in various disciplines – there are certainly many critics within economics – but there does seem to be agreement about what instrumental rationality *means*. Perhaps it would be possible to parlay this rather simple notion of instrumental rationality into a new more robust characterization of rationality in the acquisition of knowledge. Since we can agree about what it means to "be rational" in one sense, the argument is that perhaps we can extend out from this base to get a new consensus about the more difficult problem of scientific rationality.

According to *instrumental rationality*, *rationality* lies exclusively in the relationship between *means and ends*; being instrumentally rational simply means choosing appropriate means for achieving one's given ends. In particular, instrumental rationality has nothing whatsoever to do with the nature of the ends. The ends

could be immoral, self-destructive, or have any number of other prima facie undesirable characteristics, and yet be perfectly adequate antecedents for instrumentally rational action. Any ends will do as long as one acts appropriately in their realization. In the recent words of the philosopher of science Michael Friedman:

Instrumental rationality thus refers to our capacity to engage in effective means-ends deliberation or reasoning aimed at maximizing our chances of success in pursuing an already set end or goal. It takes the goal in question as given, and it then attempts to adjust itself to environmental circumstances in bringing this desired state of affairs into existence in the most efficient way possible. (Friedman, 2001, p. 54)

Instrumental rationality was raised to philosophical prominence by David Hume in the *Treatise of Human Nature* (1739) and numerous later works. Hume opposed universal notions of rationality – the idea that "rationality" necessarily required certain acts or beliefs for all humans in all places and all times. For Hume rationality was always a contingent affair; *given* the ends, certain means are rational, but rationality alone does not mandate any substantive ends.<sup>1</sup> In other words, all imperatives are hypothetical, not categorical; reason is "the slave of the passions" (Hume, *Treatise*, Vol. II, Book III, part 3).

While instrumental rationality is simply the relationship between effective means and given ends, it is often tied up with a particular kind of human end. In fact the Humean view of rationality can be broken down into two parts.<sup>2</sup> First, pure instrumental rationality (what philosophers often call "practical reason"): reasons for the end implies reason for the means. And second, that the relevant reasons reside in the subjective beliefs and desires of individual agents. For Hume the relevant desires were the actual wants of the agent, but modern economics adds another layer to the explanation of rational action by restricting the relevant beliefs in various ways. In economics the desires or preferences involved in a "rational choice" explanation must satisfy certain additional conditions. They must be "rational" desires: preferences that satisfy certain minimal conditions of "rationality" – usually transitivity, completeness, continuity, and some type of monotonicity; or that they satisfy the standard assumptions on ordinal utility functions. The rational choice explanations that are the bread-and-butter of microeconomics, thus involve at least three different notions of rationality. The first two from Hume: that actions are instrumentally rational, and that the

---

<sup>1</sup> This of course leaves open the question of whether the mandate to act in an instrumentally rational way given one's ends, is itself a categorical imperative, but I will skip this long-debated philosophical topic. The collection Cullity and Gaut (1997) contains a number of recent papers on this and related subjects.

<sup>2</sup> This argument follows Hubin (1999).

reasons for the actions are the beliefs and preferences of individual agents; and then the third that in order to be applicable for use in such rational choice explanations, the preferences need to be well-ordered in a way that contemporary economists find acceptable. But this is not the end of the restrictions that are required for microeconomic explanations; economists impose one more restriction rational choice explanations: *optimality*. Agents must not only act efficiently to achieve their given preferences they *must* do so optimally; they must use the most efficient means. Utility must be maximized.

So while instrumental rationality is an essential feature of all economic explanations of individual behavior, there is more to economic rationality than choosing appropriate means for one's given ends. As with Hume, the ends are subjective desires, but in addition these desires must be suitably restricted (well-ordered) and employing efficient means requires solving the relevant optimization problem. The economic rationality involved in microeconomic economic explanations of individual behavior – in either risk-free or risky – choice thus involve the four basic features:

- i) *Instrumental rationality*: the economic agent acts in a way that is appropriate to achieve his/her given ends.
- ii) *Desires as ends*: the relevant ends are the desires/preferences of individual economic agent.
- iii) *Well-ordered preferences*: the agents desires are given by a set of well-ordered preferences defined over the relevant choice space.
- iii) *Optimization*: the appropriate way to achieve the given ends is to optimize over the available choices subject to the relevant constraints.

Such an approach will of course provide a *description* of the behavior of the economic agent *if in fact* the agent has well ordered preferences, optimizes over them, and then acts on the basis of that optimization. Since these conditions for descriptive adequacy seem to fail – or are, at the very least, difficult, if not impossible, to empirically test – in any specific application, a variety of different methodological arguments have been offered to defend the adequacy of this microeconomic explanatory scheme even though it seems to fall short as an adequate description of the behavior of most economic agents. Such efforts constitute a large portion of the existing literature in economic methodology. Perhaps such explanations are normative; they characterize the rational way to act, which, like other norms, correctly describes some people some of the time, but not all people all of the time. Perhaps microeconomics is simply an efficient instrument for empirically predicting the behavior of economic agents and not literally a realistic description of the process that takes place inside their heads before they act (Friedman 1953). Perhaps this explanatory scheme is just one part of a broader program of equilibrium economics which can be defended along Millian lines as an inexact and separate science of economic behavior (Hausman



1992). Perhaps this approach to explaining individual action is just one component of the hard core of a progressive Lakatosian scientific research program in neo-Walrasian economics (Weintraub 1985). Perhaps such explanations can be defended as a particular, but successful, version of Popper's situational analysis approach to the social sciences (Caldwell 1991). Perhaps what economists provide is not really an explanation or description of individual behavior, but rather a defense of a specific type of contractarian political philosophy (Rosenberg 1992). Perhaps, ... and the list goes on and on.

Of course, while all economic explanations of individual behavior involve instrumental rationality, there is more to economics (even microeconomics) than the behavior of individual agents. Much of economics is less concerned with explaining individual behavior than with explaining the social phenomena that emerge from the *interaction* of a number of instrumentally rational agents interacting within the confines of particular social/institutional constraints/structures.<sup>3</sup> The classic example is the price of a particular commodity that emerges from a perfectly competitive market. No agent wants the price to be what it is; sellers would much prefer it to be higher and buyers would much prefer it to be lower. The competitive market price is an unintended consequence of the interaction of instrumentally rational buyers and sellers (optimizing over well-behaved preferences) who are acting within the context of a particular institutional structure: the perfectly competitive market. Different institutions and/or different agents (different preferences and/or different constraints) will produce different unintended consequences; each agent is instrumentally rational, but the explanandum phenomenon is not the immediate intention of any particular agent.

While a detailed discussion of how various philosophers have tried to parlay instrumental rationality into a new, more socially sensitive, characterization of scientific rationality is best deferred until the two examples are presented in the next section, it is possible at this point to get a rough idea why (at least the economic version) might appear to be a useful tool.

The fact is that economics, particularly microeconomics, appears to be well-suited for the task of explaining how a group of individuals can act in ways that are entirely self-interested and contrary (or at least indifferent) to collective interests, and yet (perhaps as if by an invisible hand) produce results that satisfy certain higher/normative goals such as efficiency, social optimality, and so forth. Economics is the social science that specializes in explaining the *unintended consequences* of individual rational action. Sometimes these unintended consequences are good (as in the case of the classical invisible hand) and sometimes these unintended consequences are bad (as is the case with externalities or certain game-theoretic results such as the prisoner's dilemma),

---

<sup>3</sup> Although this has been changing in recently.

but they are unintended consequences that go beyond – and are thus ostensibly a totally different animal than – the rational actions of the individual economic agents.

Thus it seems that economics might be able to help provide a way out of the relativism of science studies. Traditional philosophy of science defended science as a fundamentally different – and cognitively better – animal on the basis of a set of methodological rules (the scientific method) that were strictly adhered to by scientists, but not by those in other (even expert) cultures. But post-Kuhnian history and sociology of science successfully attacked this traditional defense of the uniqueness and non-relativity of science; scientists were not found to follow the strict rules of the scientific method, but rather to act in socially conditioned, and more importantly in "epistemically sullied" (Kitcher, 1993, p. 364) ways. While most of the science studies literature suggests that being sullied, not following the methodological rules, is sufficient to demonstrate that science is not really a different epistemic animal, economics might indicate something quite different. The lesson from economics is that it may be possible, if the scientific institutions are configured in the right way, that the unintended consequence of this sullied behavior is precisely the type of cognitive success envisioned by traditional philosophy of science. Of course, even economics, does not conclude that cognitive success must necessarily follow from such sullied behavior, but at least it is a possibility. Sullied motives and cognitive success need not be in conflict; scientific rationality may not be inconsistent with the type of interest-laden behavior that characterizes most sociological studies of science.

Since I have examined a number of the recent attempts to employ microeconomics in the philosophy of natural science in previous work (Hands 1994, 1995, 1996, 1997a, 2001), I will not review all of these various efforts in this paper.<sup>4</sup> The focus here is less on the difficulties associated with the explicit application of economics to the problem of refurbishing scientific rationality, than on the more general difficulties associated with employing instrumental rationality in this substantial philosophical endeavor. As the previous paragraphs make clear, philosophers seem to have good reason to believe that economics might be the right tool for the job – a tool that would allow philosophers to go from instrumentally rational (and perhaps sullied) scientists to scientific theories/activities that have at least some of the epistemic virtues

---

<sup>4</sup> A partial list of these works might include Bartley (1990), Goldman and Cox (1996), Goldman and Shaked (1991), Kitcher (1993, 1994), and Zamora Bonilla (1999a, 1999b). For related work by economists see the discussion and references in chapter 8 of Hands (2001). It is interesting that while certain practicing scientists have recently entered the fray against relativism in science – Gross and Levitt 1994; Gross, Levitt, and Lewis 1996; and others – these scientists have not expressed any interest in recruiting economics for their project. These scientists seem to rely almost exclusively on traditional philosophical arguments – particularly versions of scientific realism – for their attacks on relativism.

traditionally identified by philosophers of science – but as we will see economics is not explicit in every attempt to bridge this rationality gap. The problem is, I will argue, that whether economics is explicitly employed or not, instrumental rationality is insufficient for the job; in fact we will see that the main reason why economists have been drawn to an instrumental notion of rationality is precisely the feature that renders it ill-suited for the task of deflecting relativism in science theory.

Let us now turn to particular cases where contemporary philosophers have employed instrumental rationality in their efforts to save scientific rationality from epistemic relativism. I will examine two such cases: Philip Kitcher's approach in *The Advancement of Science* (1993) and Larry Laudan's "normative naturalism" (1987, 1996). Kitcher explicitly applies microeconomic theory – "an analytic idiom inspired by Bayesian decision theory, microeconomics, and population biology" (Kitcher, 1993, 305) – to the philosophy of natural science. Laudan, on the other hand, does not refer to economics, but he does explicitly apply instrumental rationality in his effort to overcome the creeping relativism of science studies, and ends up employing a substantial amount of argumentation that is, at least implicitly, microeconomic in character. Given my previous examination of these views (Hands 1994, 1995, 1996, 1997, 2001) the presentation here will be relatively brief and focus specifically on the role of instrumental rationality in the two approaches. The next section will present these two views, while the critical issues surrounding the use of instrumental rationality will be examined in section three.

## 2. Two Views of Instrumental Rationality in Recent Philosophy of Science

Philip Kitcher is clearly in search of a middle ground that recovers scientific rationality while recognizing that science is fundamentally social and needs to be analyzed as such. For Kitcher the main issue is not whether individual scientists have reliable beliefs about nature, but rather whether scientific institutions are designed in such a way that they systematically increase the distribution of reliable beliefs among the various members of the scientific community. Kitcher's problem is a problem in *social epistemology*.

The general problem of social epistemology, as I conceive it, is to identify the properties of epistemically well-designed social systems, that is, to specify the conditions under which a group of individuals, operating according to various rules for modifying their individual practices, succeed, through their interactions, in generating a progressive sequence of consensus practices.  
(Kitcher, 1993, p. 303)

From an economic perspective, Kitcher reduces the problem of scientific rationality to an economic planner's problem, but unlike the literature on macroeconomic planning, he is concerned with microeconomic issues like incentive compatibility and optimal institutional design: "good design of scientific communities for achieving epistemic ends" (1993, p. 303). Optimal institutional design of course depends on the behavior of the relevant agents, and Kitcher characterizes scientist-agents as *instrumentally rational*.

I conceive of rationality as a means-end notion. Concepts of rationality are generated by thinking of entities (people, groups of people, science as a whole, ...) as meeting some criterion of good design (maximization ...) relative to a set of goals (epistemic goals, practical goals, ...). Thinking of rationality as a means-end notion is hardly new ... Articulating that familiar idea in the context of an explicit investigation of the subjects of rationality, the goals and the criterion of good design transforms the debate about the rationality of science. (Kitcher, 1993, pp. 179-80)

If the traditional view of scientific rationality is translated into the language of instrumental rationality, then scientific rationality would reside in *individual scientists having the proper cognitive goals*; that "scientific behavior can and should be directed by the desire to attain cognitive (and only cognitive) goals" (Kitcher, 1993, p. 72). Kitcher adopts a broader interpretation of scientific behavior; scientists are instrumentally rational with respect to their particular goals, but those goals may be individually epistemic, collectively epistemic, nonepistemic, or any combination of the three. Scientists may seek reliable individual knowledge - they may "have the goal of improving their own cognitive state" (Kitcher, 1993, p. 72) - but they may also have the cognitive goal of improving the knowledge of their scientific community (or a wider community), or goals that are entirely noncognitive (status, prestige, promotion, etc.). One way to think about optimal cognitive design in a world of multi-goal scientist agents involves focusing on features - either individual norms/rules or social structures - that promote the achievement of the cognitive goals of individual scientists ("methodology" has traditionally been the study of such individual norms/rules). This is not Kitcher's approach. Rather than characterizing scientific rationality in terms of achieving individual cognitive goals, Kitcher's optimal design problem involves harnessing the instrumental rationality of scientists - epistemic, nonepistemic, sullied, or whatever - to promote the cognitive interests of the relevant scientific community. Kitcher discusses a variety of different topics within this framework, but his two main conclusions are that *cognitive diversity* (the division of cognitive labor) promotes *epistemic efficiency*, and that *sullied individual behavior* (contrary to most of the sociological literature) need not be epistemically problematic.

Much thinking about the growth of science is permeated by the thought that once scientists are shown to be motivated by various types of social concerns, something epistemically dreadful has been established. On the contrary, ... particular kinds of social arrangements make good epistemic use of the grubbiest motives. (Kitcher, 1993, p. 305)

Of course economists have argued since at least Adam Smith that the division of labor promotes *economic efficiency* and that the grubbiest of individual behavior may - as if by an invisible hand - lead to desirable social results. But economics is more than the invisible hand; in addition economists analyze various types of inefficiencies consistent with the rational behavior of individual economic agents (market failure, asymmetric information, incentive incompatibility, etc.) and discuss the appropriate social policy for each of these cases. Similarly, Kitcher is careful to point out that efficiency emerging from sullied behavior is only one possibility - a possibility that requires particular institutional conditions - he goes on to discuss various types of cognitive failures and appropriate institutional solutions. The point is not that Kitcher is advocating *laissez-faire* in science - he is not - but rather that whether the subject is cognitive success or failure, the tools of analysis come from economics and employ the same instrumental characterization of individual rationality.

Larry Laudan is also concerned with recovering scientific rationality from the clutches of relativism, and his approach also involves a direct application of instrumental rationality; but he does not explicitly employ the tools of microeconomic analysis. He also differs from Kitcher in that he focuses much more on traditional methodological *rules* for the behavior of individual scientists than on the epistemically efficient scientific institutions that are Kitcher's main concern. Laudan calls his approach "normative naturalism"; he is interested in *normative* methodological rules for the conduct of individual scientists and he wants to base those rules on broadly *naturalistic* investigation into the effectiveness of these rules. The methodological rules involve the relationship between our cognitive ends and sufficient conditions for promoting those ends, and naturalism requires that we approach the discovery of such rules in the same way that we would approach the discovery of any other feature of the world. Methodological norms are normative, but they are to be investigated empirically like any other feature of nature; the proper rules, when discovered, will be the ones that in fact (causally) promote our cognitive ends.

Once we recognize that methodological rules deal with the relationship between cognitive ends and means, we can recognize that it is an empirical question, ... which means promote which ends. There are epistemological and methodological "facts of the

matter," every bit as much as there are facts of the matter with respect to scientific claims. Whether certain proposed methods do in fact promote certain ends is generally a contingent question about cause-effect linkages in the natural world. Methodological claims, ... are no less "factual" than any claims made by natural or social scientists. (Laudan, 1996, pp. 16-7)

While Laudan is more concerned with traditional methodological rules than Kitcher, his characterization of individual decision making is precisely the same instrumental rationality employed by both Kitcher<sup>5</sup> and microeconomics.

Whatever else rationality or irrationality is, it is agent- and context-specific. When we say that an agent acted rationally, we are asserting minimally that he acted in ways which he believed would promote his ends. Determining that an agent acted in a manner that he believed would promote his ends may or may not be sufficient to show the rationality of his actions; ... But few would deny that it is a *necessary* condition for ascribing rationality to an agent's action that he believed it would promote his ends. (Laudan, 1996, p. 128)

Thus, like explanations involving economic rationality, Laudan makes the appropriateness of means to ends a necessary feature of rational behavior. Assuming such instrumental rationality, Laudan's desired methodological rules will take the form of *hypothetical imperatives*: "If one's goal is y, then one ought to do x" (Laudan, 1996, p. 132).

While Laudan emphasizes naturalism, instrumental rationality, and norms as hypothetical imperatives, he does not totally neglect the question of the proper scientific aims/ends/goals. If sound methodological rules are those which in fact lead to behavior that best promotes our scientific aims, then we certainly need to investigate those scientific aims. Laudan calls such an investigation – a study of the "right" scientific aims – "axiology," and considers it to be a necessary and important project, though one that is separate from normative naturalist's search for methodological rules.

On this analysis, the construction of a methodology of science is the development of a set of methodological rules, conceived as hypothetical imperatives, parasitic on a given set of cognitive or epistemic ends. Yet, although this is an attractive conception of

---

<sup>5</sup> While Laudan is more traditional (and thus less contemporary) than Kitcher on the question of individual methodological norms, he is in fact more contemporary (and thus less traditional) than Kitcher on the subject of naturalism. The traditional vs. contemporary score thus seems to be a tie.

methodology, it scarcely addresses the full range of epistemic concerns germane to science. I suppose that we all believe that some cognitive ends are preferable to others. Methodology, narrowly conceived, is in no position to make those judgments, since it is restricted to the study of means and ends. We thus need to supplement methodology with an investigation into the legitimate or permissible ends of inquiry. That is, a theory of scientific progress needs an axiology of inquiry, whose function is to certify or decertify certain proposed aims as legitimate.

(Laudan, 1996, p. 140)

Despite his stated need for such an axiology, Laudan devotes few pages to such an investigation (see Laudan, 1984, chapter 3), and it is not very clear how such an inquiry connects up with the naturalism and instrumental rationality of the normative naturalist approach. In any case what is clear is that Laudan employs instrumental rationality in a way that is consistent with both Kitcher and contemporary economic analysis. The potential problems with such an approach are the topic of the next section.

### 3. Ending Means, Meaning Ends

In this section I will examine the problems that arise when one tries to use instrumental rationality to defend scientific rationality against epistemological relativism. I will discuss Kitcher's approach first, and then turn to Laudan's, but many of the criticisms apply to any philosophical attempt to employ such tools in the recovery of scientific rationality. But before these philosophical considerations, it useful to review some economics.

Consider again how instrumental rationality is used in microeconomics. Instrumental rationality, as discussed above, is one component of the ensemble that constitutes *economic rationality*. The economic agent has well-ordered preferences (goals), and acts optimally relative to those preferences (chooses the best means) subject to the relevant feasibility constraints. For most of the twentieth century such economic rationality – or explaining individual decisions on the basis of such an explanatory scheme – has *defined economics*. The standard definition of economics, repeated in the first chapter of every elementary economics textbook for over a half-century, is that of Lionel Robbins first offered in 1932:

Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses. (Robbins, 1952, p. 16)

According to this definition of economics one could just as well investigate the economically rational way of committing murder as the economically rational way of choosing what to have for dinner. The goals are entirely arbitrary, and even when the preferences are assumed to be well-behaved, these restrictions – restrictions which essentially guarantee that the relevant mathematical optimization problem is well-behaved – still do not rule out antisocial, or other problematic, preferences. One can have complete, transitive, continuous, and monotonic preferences for the suffering of others. While economists in fact do macroeconomics, econometrics, and a variety of other professional activities that are only indirectly related to Robbins's definition of economic rationality, that definition remains the core characterization of the discipline, and within the realm of *microeconomics* it is a fairly accurate description of professional practice.

It is important to realize that this arbitrariness, this inability to evaluate the goals/preferences of economic agents, is generally considered to be a *virtue*. As Robbins and many others would explain it, the problem with classical economics – Smith, Ricardo, Marx, and others before the neoclassical revolution in the late 19th century – was that they picked sides on social issues (the capitalist class for Ricardo; the working class for Marx, etc.). According to this view, the "scientific" revolution in economics involved characterizing the discipline in such a way that it took individual preferences – at least as long as they were mathematically tractable – as entirely arbitrary. Economic policy in such a framework would either need to be conducted on the basis of the "Pareto criterion" – an allocation is socially optimal if there is no way to make one person better off without making someone else worse off – or it must be possible to somehow add-up winners and losers and then (at least conceptually) provide a way for the former to compensate the latter. In any case, Robbins's view – a view that continues to be echoed in textbooks and the discipline's rhetoric – is that one can not scientifically say anything about good (or desirable, or proper, or ...) preferences/goals, and that what makes modern economics scientific is this narrowly instrumental notion of rationality. This is of course separate from the question of whether economic rationality is descriptively accurate or whether explanations of economic behavior couched in such terms constitute valid scientific explanations. As indicated above, the standard answer for those who have examined the descriptive accuracy/explanatory adequacy question has been: "no, but." No, such explanations are not descriptively accurate, nor do they live up to the (at least D-N) standards for an adequate scientific explanation: but, there are reasons why microeconomics is OK anyway.

So how does all of this relate to relativism and rationality in science? The important thing to notice about the standard Robbins view of the scientific importance of instrumental rationality makes economics *scientific at least in part because it is epistemologically relativist*. Recall Collins's definition of epistemological relativism: "there is no external vantage point from which to judge between



them; all that can be known can be known only from the point of view of one social group or another" (Collins, 2001, p. 184). This seems to be precisely what is being claimed in the standard definition of economics; there is no external vantage point from which to know the economic efficiency or desirability of any particular situation, other than from the point of view of the relevant economic agents. The only difference seems to be that Collins (the sociologist) makes the perspective relative to the social group, while Robbins (the economist) makes it relative to the individual economic agent. Microeconomics is viewed as scientific precisely because economic rationality does not go beyond instrumental rationality and thus remains epistemologically relativist. This certainly seems to throw a spanner into the works of any philosophical effort to save scientific rationality from relativism by means of microeconomics, or for that matter any other version of instrumental rationality. How would it be possible to get beyond epistemic relativism on the basis of an approach to scientific agency that is grounded in such relativism?

Consider Kitcher's approach. As argued above, his philosophical program effectively mirrors the standard analysis of economic efficiency within mainstream microeconomics. Economists argue that the instrumentally rational actions of economic agents with sullied preferences need not, if the economic institutions are properly designed, lead to economic inefficiency; Kitcher argues that the instrumentally rational actions of scientific agents with sullied preferences need not, if the scientific institutions are properly designed, lead to epistemic inefficiency. So how could such an analysis get beyond epistemic relativism when it simply mirrors an economic argument that is fundamentally relativist? Kitcher's answer lies in his use of cognitive utility and epistemic goals. If we are restricted to a regime of instrumental rationality and yet want to have the epistemic emerge from the interaction of such instrumentally rational agents, the "epistemic" has to *go into* the goals of the agents in the beginning, or it *would not come out of* the equilibrium distribution in the end. Kitcher assumes that individual's have epistemic goals – in certain parts of the analysis he assumes the agents have purely altruistic epistemic intentions – but the main result comes from "epistemically sullied agents" who are "driven not only by a desire to solve the problem, but also by the quest for priority" (Kitcher, 1993, p. 310) or other sullied desires. One of his main results is to demonstrate that such sulliedness may, given certain constraints, produce a epistemically better final distribution of practices within the scientific community. The particular constraints, preference structures, dynamics, and (generally Nash) equilibrium concept may certainly produce a collectively desirable *distribution* of cognitive practices, but the cognitive utility must have been a preference of (i.e. be relative to) the initial agents. The ethical analog of this philosophical solution to the problem of scientific rationality would be to eliminate the tension between deontological and consequentialist ethics by putting goodness into the utility functions of some

individuals and then pointing out that goodness emerges as a property of some Nash equilibrium generated by the interactions of those agents.

Of course Chapter 8 of *The Advancement of Science* offers a number of different models to explain various features of science – many of which are not directly related to the core question of recovering scientific rationality from relativism – but in the cases where the scientist-agents are "thoroughly cynical" (Kitcher, 1993, p. 364) and do not get any utility from the truth of the theory, the result is an absence of the cognitive division of labor and epistemic inefficiency. As Kitcher explains it:

This conclusion is sobering. Even a community of sullied agents will not succeed in dividing the labor unless its members refrain from adopting a thoroughly cynical attitude to the way in which community decisions will be made. To put the point dramatically (but somewhat inaccurately) even if personal motivations (such as the desire for credit or enduring fame) play a major role in the decisions of individuals it is important that those individuals believe that they participate in an enterprise that is governed by devotion to truth. *Mauvaise foi* may be an essential part of *la condition scientifique*. (Kitcher, 1993, p. 364)

This is certainly no surprise; if what emerges is solely the result of the instrumentally rational actions of individual agents and nothing epistemic goes in, then nothing epistemic comes out. The cognitive efficiency of science is *relative to* the cognitive goals of the individual scientist-agents. Such epistemically negative results are avoided in most of Kitcher's models by means of the "idealistic computation" restriction (Kitcher, 1993, p. 364): the restriction that agents assume that the probability of acceptance (and therefore the resulting fame, fortune, etc.) is the same as the probability that it is true. Kitcher's scientific communities get the right equilibrium results out of the interactions of instrumentally rational scientists because the right results are arguments in the utility functions of at least some of these scientists. The right results are thus *relative to the preferences of those agents* and are thus *social*. This hardly seems to be the answer to epistemic relativism that most philosophers of science were looking for.<sup>6</sup>

Before moving on to Laudan, and in order to be fair to Kitcher, it should be noted that in his more recent work – particularly Kitcher (2001) – he seems to have

---

<sup>6</sup> There is an extensive critical literature on Kitcher's approach – including Downes (2001), Fuller (1994), Hands (1995, 1997a), Kincaid (1997), Mirowski (1995, 1996), Roorda (1997), Solomon (1995), and Wray (2000, 2001) – but the role of instrumental rationality has not been the main focus of these criticisms (present author included).

attempted to address some these (and other<sup>7</sup>) criticisms. He now draws on his earlier characterization in discussing "well-ordered science" but admits that it is "far too optimistic" (Kitcher, 2001, p. 113). The problems he identifies are not only the standard criticisms that might (and have) been leveled at economics – the necessary optimization problems cannot be solved (or adequately specified); transactions costs muddy the whole argument; there are problems associated with going from individual to collective goals (aggregation and social choice problems); and others – but also the instrumental rationality-based difficulties discussed above. It seems in order for well-ordered science to emerge from sullied behavior, it is necessary for some of the agents to have epistemic goals: the issue he now calls the problem of "local epistemic goals" (ibid.). His solution – and clearly his position is inchoate and still under development – is to shift the analysis away from Nash equilibria of scientific agents to questions of political philosophy. The invisible hand is not reliable, but nor is "vulgar democracy" (ibid., p. 117): voting based on *actual* preferences. Certain democratic values are necessary and those values would only be reproduced by an electorate with appropriately "*tutored* personal preferences" (ibid., p. 118). As he puts it: "Here I have in mind the idea that, as with war or politics, a distinctive 'public morality' might apply to scientific investigations" (ibid., p. 121). In the end Kitcher ends up with a notion of rationality emerging from a particular type of democratic environment – one that has much in common with the "ideal speech situation" of Jürgen Habermas (1984), the "idealized epistemic community" of Helen Longino (1994), Deirdre McCloskey's *Sprachethik* (1998), and a number of similar ideas posited by other authors – the individuals involved are still instrumentally rational, but their preferences are socially responsible in ways that go well beyond the actual preferences of Hume or the well-behaved preferences of economic theory. Kitcher hasn't so much solved the problems discussed above, as changed the subject. Despite these changes, the problem that what one wants to come out (epistemic goals, social responsibility, ...) needs to go into the goals of the instrumentally agent, seems to be as much a part of Kitcher 2001 as 1993.

Although Laudan's approach is less directly linked to economics, the problems of using instrumental rationality surface just as clearly in his analysis. Most importantly Laudan separates the question of naturalistically determining the most effective methodological rules *given* our scientific goals from the axiological question of determining the proper scientific goals, and provides very little in the way of a naturalistic framework for the determination of the proper goals. The same naturalistic approach that employs instrumental rationality to discover the causal links between various methodological rules and the effectiveness of the

---

<sup>7</sup> Without mentioning any of the various papers cited in the previous footnote, it is clear that Kitcher has gently shifted his position in a direction that that would help mollify many of these previous criticisms. The emphasis now seems to be much more social, less focused on the individual scientific agent, and even has a whiff of pragmatism.

scientific behavior they induce, seems to fall silent on the higher order (but for a naturalist no different in kind) question about what the right scientific goals should be. Like Kitcher, Laudan is unable to get the relativity-stopping right stuff out of instrumentally rational scientists without putting the right epistemic goals into those same agents, but for Laudan the problem is even worse. Because the epistemic efficiency emerges as an equilibrium condition for Kitcher, he does not require every agent to have the proper cognitive goals (in fact it is better that not all do). Laudan, on the other hand, is looking for more traditional methodological rules that can (must) be followed by all rational scientists, and thus requires the proper axiological goals be found (known and used) by all scientific agents. Laudan argues for a naturalistic approach for discovering the instrumentally rational "method" given that the proper epistemic goals are known and shared by all the relevant scientific agents, and yet provides very little information about how these goals are to be discovered (naturalistically), or why such instrumentally rational agents would adopt these particular goals.

Even though Laudan does not directly employ economic analysis, there is much to be learned from economics in his case as well. As pointed out many times above, microeconomics is based entirely on instrumental rationality and it is common to define economics essentially as microeconomics. Of course there are a number of contemporary philosophically-inclined economists who are quite critical of the profession's exclusive reliance on instrumental notions of rationality (Hargreaves Heap 1989 and Stewart 1995 for example), but more importantly, the belief that economists should have some way of deciding between good and bad preferences (though none is provided by standard economic theory) has a long history in economics. Among economists considered to be relatively mainstream, Frank Knight is a good example of someone who argued that the topic of appropriate and inappropriate preferences should not be outside of rational debate; the valuation of goals could be discussed rationally, and thus rationality was not just relative to particular goals.<sup>8</sup> Knight was content with Robbins's definition of economics, he just argued that there was much more to the "rational" analysis of social life than what was provided by economics.

An even more relevant lesson for Laudan's project comes from John Dewey and the American Institutional economists who sought to put Dewey's notion of "instrumental" reasoning to work in the economic domain. Laudan seems to be sympathetic to pragmatism and yet does not explicitly attempt to apply Dewey's instrumentalism – which was based in a Darwinian evolutionary view of the human condition and not just instrumental rationality – to the problem of rationality in science. Not applying Dewey's ideas to this subject seems to have both negative and positive implications for Laudan's project. On the negative side, Dewey seems to be the obvious place to go; Dewey was clearly concerned

---

<sup>8</sup> See Hammond (1991), Hands (1997b), and Stewart 1995 on this issue in Knight's work.

with scientific rationality (and wanted to radically extend its application beyond the domain of the natural sciences) and had a notion of such rationality that was social, naturalist, and prima facie quite consistent with the contemporary philosophical interest in recovering a broader, and yet distinctively nonrelativist, notion of scientific rationality. On the negative side, Dewey's approach never seemed to be entirely satisfactory; while he clearly thought that it was obvious that certain beliefs were rational – or as he would say, intelligent – it was never sufficiently or consistently clear to others what the standard was or how it should be applied. The difficulties with Dewey's notion of instrumental reason leaves neopragmatists such as Richard Rorty with little more than instrumental rationality (given the goal x, the best thing to do is y) and the epistemic relativism that such rationality seems to imply (what is rational is "relative to" one's goals).

If Laudan cannot solve the problem of finding the right scientific goals in a naturalistic way that gets beyond instrumental rationality then his normative naturalism is doomed to never overcome relativism. The instrumental rationality of normative naturalism at best tells us what is appropriate given our epistemic goals; this means that the resulting methodological rules are relative to those epistemic goals. Even if such naturalism allows nature to answer our methodological questions, the answers are totally dependent on, and thus relative to, our goals – goals that of course could be seen as the right ones for "us" because of the social conditioning of our particular epistemic culture. If we have a different episteme then we will have, via normative naturalism, different scientific methodologies. As with Kitcher, this hardly seems to be what philosophers are looking for in the war against relativism. As long as the only notion of rationality is instrumental, then that which is rational will always be relative to the goals of the relevant agents – the (perhaps contingent and/or socially conditioned/determined) goals of the individual scientists for Kitcher and the (perhaps contingent and/or socially conditioned/determined) goals of philosophical meta-methodologist for Laudan – but relative in any case. Scientific rationality cannot overcome epistemic relativism as long as the operative notion of rationality depends entirely on, and is thus relative to, the (culturally conditioned) goals of particular individuals or groups of individuals.

#### 4. Conclusion

I have discussed the filiation of the problem of relativism and rationality in recent philosophy of science; examined the concept of instrumental rationality in detail; explained why economics might be seen as an effective ally in the philosopher's struggle to recover scientific rationality; discussed the role of instrumental rationality in economics as well as the philosophical approaches of both Kitcher and Laudan; and finally, argued that these two approaches (and by

implication others who traffic in instrumental-rationality-based argumentation) fail to effectively repulse epistemological relativism in the way that they intend to do. If my critical arguments are correct then one or more of the givens of such work – I suspect instrumental rationality – will need to be abandoned.

While my argument has been critical, I realize that I have not provided a knock-down proof that instrumental rationality in general, or the microeconomic version of instrumental rationality in particular, cannot be successfully applied in the defense of scientific rationality against philosophical relativism. Perhaps they can. I just do not find existing efforts at all satisfactory, and at this point I simply do not see how instrumental rationality-based approaches will ever be able to get around the problems I have raised and actually provide the kind of defense of scientific rationality that most philosophers seem to be interested in. My point is simply that there are certain fundamental issues about instrumental rationality in this context that have not been, and need to be, seriously addressed. My hunch is that you simply "can't get there from here" – "there" of course being scientific rationality and "here" being instrumental rationality – but I also look forward to various efforts to do precisely this. Such work has certainly opened up an entirely new and extremely interesting field for the interaction of economics and science theory, and that will have a significant, if unpredictable, impact on both fields.

## References

- Bartley, William W. III (1990), *Unfathomed Knowledge, Unmeasured Wealth*. La Salle, IL: Open Court.
- Caldwell, Bruce J. (1991), "Clarifying Popper," *Journal of Economic Literature*, 29, 1-33.
- Cartwright, Nancy; Cat, Jordi; Fleck, Lola; and Uebel, Thomas (1996), *Between Science and Politics: The Philosophy of Otto Neurath*. Cambridge: Cambridge University Press.
- Collins, Harry (2001), "One More Round With Relativism," in *The One Culture? A Conversation About Science*. J. A. Labinger and H. Collins (eds.), Chicago: University of Chicago Press, 184-95.
- Cullity, Garrett and Gaut, Berys (eds.) (1997), *Ethics and Practical Reason*. Oxford: Clarendon Press.
- Downes, Stephen M. (2001), "Agents and Norms in the New Economics of Science," *Philosophy of the Social Sciences*, 31, 224-38.
- Friedman, Michael (1999), *Reconsidering Logical Positivism*, Cambridge: Cambridge University Press.
- Friedman, Michael (2001), *Dynamics of Reason*. Stanford, CA: CSLI Publications.
- Friedman, Milton (1953), "The Methodology of Positive Economics," in *Essays in Positive Economics*, Chicago: University of Chicago Press, 3-43.
- Fuller, Steve (1994), "Mortgaging the Farm to Save the (Sacred) Cow," *Studies in History and Philosophy of Science*, 25, 251-61.
- Fuller, Steve (2000), *Thomas Kuhn: A Philosophical History for Our Times*. Chicago: University of Chicago Press.
- Goldman, Alvin and Cox, James C. (1996), "Speech, Truth, and the Free Market for Ideas," *Legal Theory*, 2, 1-32.
- Goldman, Alvin and Shaked, M. (1991), "An Economic Model of Scientific Activity and Truth Acquisition," *Philosophical Studies*, 63, 31-55.

- Gross, Paul R. and Levitt, Norman (1994), *Higher Superstition: the Academic Left and Its Quarrels with Science*. Baltimore, MD: Johns Hopkins University Press.
- Gross, Paul R., Levitt, Norman, and Lewis, Martin W. (eds.) (1996), *The Flight From Science and Reason*. New York: New York Academy of Sciences.
- Habermas, Jürgen (1984), *Theory of Communicative Action*, Thomas McCarthy trans. Boston: Beacon Press.
- Hacking, I. (1979), "Imre Lakatos's Philosophy of Science," *British Journal for the Philosophy of Science*, 30, 381-410.
- Hacohen, Malachi H. (2000), *Karl Popper – The Formative Years, 1902-1945*. Cambridge: Cambridge University Press.
- Hammond, J. Daniel (1991), "Frank Knight's Antipositivism," *History of Political Economy*, 23, 359-81.
- Hands, D. W. (1994), "Blurred Boundaries: Recent Changes in the Relationship Between Economics and the Philosophy of Natural Science," *Studies in History and Philosophy of Science*, 25, 751-72.
- Hands, D. Wade (1995), "Social Epistemology Meets the Invisible Hand: Kitcher on the Advancement of Science," *Dialogue*, 34, 605-21.
- Hands, D. W. (1996), "Economics and Laudan's Normative Naturalism: Bad News from Instrumental Rationality's Front Line," *Social Epistemology*, 10, 137-52.
- Hands, D. W. (1997a), "Caveat Emptor: Economics and Contemporary Philosophy of Science," *Philosophy of Science*, 64 (Proceedings), S107-S116.
- Hands, D. Wade (1997b), "Frank Knight's Pluralism," in *Pluralism in Economics*, A. Salanti and E. Serepanti (eds.), Aldershot: Edward Elgar, 194-206.
- Hands, D. W. (2001), *Reflection Without Rules: Economic Methodology and Contemporary Science Theory*. Cambridge: Cambridge University Press.
- Hargreaves Heap, Shaun (1989), *Rationality in Economics*. Oxford: Basil Blackwell.
- Hausman, Daniel M. (1992), *The Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press.
- Hubin, Donald C. (1999), "What's Special About Humeanism," *Noûs*, 33, 30-45.



Hume, David (1739), *A Treatise of Human Nature: Being An Attempt to introduce the experimental Method of Reasoning into Moral Subjects*.

[<http://socserv2.socsci.mcmaster.ca/~econ/ugcm/3113/hume/treat.html>]

Kincaid, Harold (1997), "Individualism and Rationality," in *Individualism and the Unity of Science*, Lanham, MD: Roman & Littlefield, 119-42.

Kitcher, Philip (1993), *The Advancement of Science: Science Without Legend, Objectivity Without Illusions*. Oxford: Oxford University Press.

Kitcher, Philip (1994), "Contrasting Conceptions of Social Epistemology," in *Socializing Epistemology: The Social Dimensions of Knowledge*. F. F. Schmitt (ed.), Lanham, MD: Roman and Littlefield, 111-34.

Kitcher, Philip (2001), *Science, Truth, and Democracy*. New York: Oxford University Press.

Laudan, Larry (1984), *Science and Values: The Aims of Science and Their Role in Scientific Debate*. Berkeley, CA: University of California Press,

Laudan, Larry (1987), "Progress or Rationality? The Prospects for Normative Naturalism," *American Philosophical Quarterly*, 24, 19-31.

Laudan, Larry (1996), *Beyond Positivism and Relativism*. Boulder, CO: Westview Press.

Longino, Helen E. (1994), "The Fate of Knowledge in Social Theories of Science," in *Socializing Epistemology: The Social Dimensions of Knowledge*. F. F. Schmitt (ed.), Lanham, MD: Roman and Littlefield, 135-57.

McCloskey, Deirdre N. (1998), *The Rhetoric of Economics*, 2nd ed. Madison, WI: University of Wisconsin Press.

Mirowski, Philip (1995), "Philip Kitcher's *Advancement of Science*: A Review Article," *Review of Political Economy*, 7, 227-41.

Mirowski, Philip (1996), "The Economic Consequences of Philip Kitcher," *Social Epistemology*, 10, 153-69.

Pinch, Trevor (2001), "Does Science Studies Undermine Science? Wittgenstein, Turing, and Polanyi as Precursors for Science Studies and the Science Wars," in *The One Culture? A Conversation About Science*. J. A. Labinger and H. Collins (eds.), Chicago: University of Chicago Press, 13-26.

Robbins, Lionel (1952), *An Essay on the nature & Significance of Economic Science*, 2nd. Ed., London: Macmillan [1st edition 1932].

Roorda, Jonathan (1997), "Kitcher on Theory Choice," *Erkenntnis*, 46, 215-39.

Rosenberg, Alexander (1992), *Economics - Mathematical Politics or Science of Diminishing Returns?*. Chicago: University of Chicago Press.

Snow, C. P. (1959), *The Two Cultures and the Scientific Revolution*. Cambridge: Cambridge University Press.

Solomon, Miriam (1995b), "Legend Naturalism and Scientific Progress: An Essay on Philip Kitcher's The Advancement of Science," *Studies in History and Philosophy of Science*, 26, 205-18.

Stewart, Hanish (1995), "A Critique of Instrumental Reason in Economics," *Economics and Philosophy*, 11, 57-83.

Weintraub, E. Roy (1985), *General Equilibrium Analysis: Studies in Appraisal*. Cambridge: Cambridge University Press.

Wray, K. Brad (200), "Invisible Hands and the Success of Science," *Philosophy of Science*, 67, 163-75.

Wray, K. Brad (2001), "Science, Biases, and the Threat of Global Pessimism," *Philosophy of Science*, 68 (Proceedings), S467-S478.

Zamora Bonilla, Jesus P. (1999a), "Elementary Economics of Scientific Consensus," *Theoria*, 14, 461-88.

Zamora Bonilla, Jesus P. (1999b), "Verisimilitude and the Scientific Strategy of Economic Theory," *Journal of Economic Methodology*, 6, 331-50.